History of RSB Interview: Giorgio Parisi

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PC: Good morning, Giorgio. Thank you very much for sitting down with us. As we've discussed ahead of time, we'll be mostly discussing the work you've done and your ideas around replica symmetry breaking, which we roughly bound from 1975 to 1995. But before we get to that, there's a few background questions we'd like to ask. In particular, can you describe how you got to work with Nicola Cabibbo[1]. How did the process of finding an advisor then took place at La Sapienza?

GP: [0:00:42] You want to know how I got to work with Nicola?

PC: Yes. How you got to choose him, and also how generally were people finding advisors at that point.

GP: [0:00:50] If you look to the atmosphere of that period, '69—because I took the thesis with Nicola Cabibbo in '69—people considered [that] the physics of high energy was the best. The top of all theoretical physics [was] in high energy. And Nicola was clearly the most known, famous, distinguished [in that field]. He was clearly at the top. Therefore, it was natural to ask Nicola. And I think that Nicola maybe spoke with Francesco Calogero[2] and Massimo Testa. Anyhow, Nicola decided to be the thesis advisor.

I remember that we had many discussions [about] physics with Nicola, but he was undecided at that moment which should be my thesis. Nicola often said: "Well, this is interesting, but this may be not a good thesis." At a certain moment, Gianni Jona[3], [who] was here [at La Sapienza], said:

[1] Nicola Cabibbo: https://en.wikipedia.org/wiki/Nicola_Cabibbo
[2] Francesco Calogero: https://en.wikipedia.org/wiki/Francesco_Calogero
[3] Giovanni Jona-Lasinio: https://en.wikipedia.org/wiki/Giovanni_Jona-Lasinio

"Maybe you should look to a paper that was done by Migdal and Polyakov[4]."

The paper by Migdal and Polyakov was not on phase transitions, but was on the Higgs phenomenon. Using Ward identities[5] they were checking that, if you do scattering of particles, you have no pole corresponding to the exchange of Goldstone Bosons. This was not evident, because if you quantize the model in Fermi gauge both the Goldstone Boson and a singularity—$1/p^2$ in the Gauge propagator—remains there. In that work, they showed that the two poles cancel one with the other in the scattering [leaving non massless particles in the physical sector of the model]. Gianni Jona said: "That was done in the Fermi gauge. Maybe you can do the computation in the Landau gauge to see if you obtain the same result." In the Landau gauge you have a double pole, things were more complex, but fortunately [at the end of the computation there was a cancellation]. Now the question seems ridiculous, but at the time (end of the '60s) people were not fully convinced that the Higgs mechanism was gauge invariant. So controlling in a different gauge that massless excitations do decouple from the physical spectrum would support the soundness of the theory.

Higgs[6]—I met him at the Nonino prize[7]—told me that very often, when he was invited to give a seminar, at the middle of the seminar somebody started to say: "No. This is not gauge invariant. It's not possible." There were many, many discussions about this point. So I started to do this work with the help of Massimo Testa under the supervision of Nicola. We were also collaborating with Nicola and with Massimo Testa on other issues—on the parton model[8]—and that was the starting point of my research.

[4] A. A. Migdal and A. M. Polyakov, "Spontaneous breakdown of strong interaction symmetry and the absence of massless particles," *Sov. Phys. JETP* **24**, 91 (1967).
[5] Ward-Takahashi Identity: https://en.wikipedia.org/wiki/Ward%E2%80%93Takahashi_identity
[6] Peter Higgs: https://en.wikipedia.org/wiki/Peter_Higgs
[7] Nonino Prize "An Italian Master of our Time" for 2006 was awarded to Giorgio Parisi.
[8] N. Cabibbo, G. Parisi, M. Testa and A. Verganelakis, "Deep inelastic scattering and the nature of partons," *Lett. Nuovo Cimento (1969-1970)* **4**, 569-574 (1970). https://doi.org/10.1007/BF02755316

After the thesis, there were two very good friends of mine, Marco d'Eramo and Luca Peliti, that were doing their thesis with Carlo di Castro[9] and Jona-Lasinio on the renormalization group applied to statistical mechanics, to second-order phase transitions. I was interested to look [at] what people around me were doing. The papers by Wilson [had] not [yet] been written at that moment[10]. I started to study this stuff and I came very interested because it was a strong coupling problem of the kind that was also present in strong interaction theory.

At that time, I was spending a lot of time in the library, looking at what was new in the journals. There were many journals that I was checking, among them also the Soviet journals were good[11]. There was an interesting paper by Polyakov where he was suggesting conformal invariance for second order phase transitions[12]. He was arguing that second-order phase transitions should be not only scale invariant, but also conformal invariant. Conformal invariance was also studied by people in high-energy physics.

At the beginning of '71, I had just moved to Frascati[13]. At Frascati, there were, among other people, Aurelio Grillo[14] and Sergio Ferrara[15], who were working with Raoul Gatto[16] on conformal invariance. So I started to work on conformal invariance. I wrote a paper on the line of the paper by Polyakov with d'Eramo and Peliti[17]. Unfortunately, the computation that we did was wrong. There was a 0/0 that we did not pay attention to, so the result was not correct.

[9] Carlo di Castro: https://www.lincei.it/it/content/di-castro-carlo (Accessed March 15, 2022.)

[10] See, *e.g.*, K. G. Wilson, "Renormalization group and critical phenomena. I. Renormalization group and the Kadanoff scaling picture," *Phys. Rev. B* **4**, 3174 (1971). https://doi.org/10.1103/PhysRevB.4.3174; "Renormalization Group and Critical Phenomena. II. Phase-Space Cell Analysis of Critical Behavior," *Phys. Rev. B* **4**, 3184 (1971). https://doi.org/10.1103/PhysRevB.4.3184

[11] Soviet Physics, Journal of Experimental and Theoretical Physics: https://en.wikipedia.org/wiki/Journal_of_Experimental_and_Theoretical_Physics; V. P. Pastukhov, "50 years of *JETP Letters*" *Phys. Usp.* **58**, 407 (2015). https://doi.org/10.3367/UFNe.0185.201504h.0441

[12] A. M. Polyakov, "Conformal symmetry of critical fluctuations," *JETP Lett.* **12**, 381 (1970). [*Pis'ma Zh. Eksp. Teor. Fiz.* **12**, 538 (1970).]

[13] Laboratori Nazionali di Frascati: https://en.wikipedia.org/wiki/Laboratori_Nazionali_di_Frascati

[14] G. Parisi, "Personal and scientific recollections of Aurelio Grillo," In: *VULCANO Workshop 2018 Frontier Objects in Astrophysics and Particle Physics*, *Frascati Physics Series Vol. 66*, R. Fusco-Femiano, G. Mannocchi, A. Morselli, G. C. Trinchero, eds. (Frascati: Istituto Nazionale di Fisica Nucleare, 2018), 1-12. http://library.lnf.infn.it/volumi-pubblicati/ (Accessed April 25, 2022.)

[15] Sergio Ferrara: https://en.wikipedia.org/wiki/Sergio_Ferrara

[16] Raoul Gatto: https://it.wikipedia.org/wiki/Raoul_Gatto

[17] M. d'Eramo, L. Peliti and G. Parisi, "Theoretical predictions for critical exponents at the λ-point of Bose liquids," *Lett. Nuovo Cimento (1971-1985)* **2**, 878-880 (1971). https://doi.org/10.1007/BF02774121

Simultaneously we started with Gatto, Ferrara and Grillo to do some computation of the four-point function[18]. The idea was to do the conformal bootstrap—what is known as conformal bootstrap these days by Slava Rychkov[19] and others. We wrote the basic formula (i.e. the contribution of one operator to the four point function), but after having written that basic formula it was clear that we needed some new ideas to obtain a symmetric four point function.

[The proposal was similar to dual model in high energy physics.] You know that there are some singularity that corresponds to an exchange of one operator. You write the four-point function as the sum of diagrams that looks like two vertices and an exchange of some field having a certain value of dimensions. We wanted that the sum of all *s* channel diagrams are equal to the sum of the *t* channel, which is the standard approach of dual model. (We should get a symmetric four point function by summing non symmetric contribution[20].) However, what happened was clear: we did a very long computation to compute that diagram involving a lot of use of Gradshteyn[21], that was a bible for doing integrals. We also found in the middle that one of the formula in Gradshteyn was wrong. (It was clear that it was wrong). At the end of the day, we did the computation, but it was not so useful because we had to do an infinite sum on one kind of terms to get the other one. What we missed completely is the idea that instead of looking near the singularity, we should start to look around the symmetric point. That was the main physical idea that was done by Slava together with the bounds from positivity. There were also lots of technical tricks. It's a magnificent work. Also [even] if we were smart enough we could not have used the computer facilities (linear programming and Mathematica) that were not available at that time and played a very important role in their work.

FZ: If I understood well, your interest in statistical mechanics dates back to your *laurea* thesis[22]?

GP: [0:10:44] Just after. A few months later. Just because of this friend of mine was doing… There was this paper of '71 that we did with Peliti and there

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[18] S. Ferrara, A. F. Grillo, G. Parisi, and R. Gatto, "Covariant expansion of the conformal four-point function," *Nucl. Phys. B* **49**, 77-98 (1972). https://doi.org/10.1016/0550-3213(72)90587-1

[19] Vyacheslav Rychkov: https://en.wikipedia.org/wiki/Vyacheslav_Rychkov

[20] **GP:** The idea is simple : you have a four point function of the a given field that is a symmetric function of *x,y,z,t*. You write a Taylor expansion around *x-y* small. If you keep only a finite number of terms the resulting expression is not symmetric. However, you want to gather some information from the knowledge that the final result is symmetric.

[21] Gradshteyn and Ryzhik: https://en.wikipedia.org/wiki/Gradshteyn_and_Ryzhik

[22] Laurea: https://en.wikipedia.org/wiki/Laurea

we were computing the exponents of the Ising model, but there was this one point that was technical, that was…

FZ: So your friendship from Peliti, d'Eramo and Massimo Testa comes from university?

GP: [0:11:13] Well. [We overlapped at University.] Massimo was one year older, but we had friends in common. Also, [with] what happened during '68[23], there was a lot of mixing from people of different years. During the normal courses, you know only the students of your year. When you go on, you start to know students from different years.

This was '71. I started to do some work on conformal field theory. Also, with Nicola we did something that was related to hadron production in electron-positron collisions. I was in Frascati. In Frascati, they had this big experiment with a colliding beam of e^+ and e^-. Frascati was the only place where they had the highest energy. The Orsay machine had up to 1.2 GeV, and Frascati had a machine up to 3GeV. Also, for a technical problem, they could not go to lower energies, to the region of Orsay's. They had to work in the high-energy [regime]. The things that people were expecting at that moment… There was a technical expectation that you should see after only a few resonances, like rho, phi, and so on. When you passed the energy of the resonances, the cross section should be very small. This was not [what] happened. They found some sustained cross section that was relatively high. Much higher than what people were predicting using the common folklore. I mean, if you stick to models where you don't have constituent particles—you don't have partons and so on—everything is soft. A e^+ and e^- high energy [beam] should not produce anything at high energies, because it is something hard and the matter is soft. From form factors, you know that the cross section would go to zero very fast. Indeed, one of the papers that we did with Nicola and Massimo Testa was to compute in the parton model the cross section and find that the cross section should be stable. It should be quite high. It should be proportional to the sum of the square of the charges of the partons. So I stayed in Frascati, I was certainly interested to see the scaling behavior of strong coupling theories.

PC: Were you physically at Frascati? Or you were traveling from La Sapienza?

GP: [0:14:35] I was physically at Frascati.

PC: So you were talking to Nicola…

[23] 1968 in Italy: https://en.wikipedia.org/wiki/1968_movement_in_Italy

GP: [0:14:40] Look. I was spending one day in Frascati, and one day in Rome. Or, the typical thing is that I was coming to university at 9 o'clock, taking the bus at 11:30 from here that goes to Frascati and speak to people at Frascati, and come back with the bus here. Or one day I go to Frascati, one day I come here. I was spending more or less half of the time here, half of the time in Frascati.

In Frascati, also, I started to look at the details of the experiments. They were doing electromagnetic experiments. So they have e^+ and e^- that were going in e^+ and e^- gamma, one gamma bremsstrahlung[24]**.** One thing that I did with Francesco Zirilli—at the suggestion of Nicola—was to do the computation of these things—the full computation—by conventional Feynman diagrams[25]. To simplify the computation, we used what people call the Weizsäker-Williams [approximation][26]. The Weizsäker-Williams approximation is essentially that you may have after or before the collision – also before or after the collision in a Feynman diagram sense – an emission of a gamma by the incoming electron, and after or before an interaction, and you want to factorize the cross section—which is not correct, but you try to do factorization of the cross section in a part that corresponds to the emission and in a part that corresponds to the finite interactions. That was important because the Weizsäker-Williams is a formula that clearly contains the correct term for soft gamma emission. When you do this thing, you get logs automatically from the Weizsäker-Williams, because you have some log corrections. The corrections are not of order a, but of order a log (E/m_e), energy divided by the mass of the electron. The log of ratio of the energy and the mass of the electron is 7 or 8 at these energies, so you have an enhancement of a factor 7 or 8 with respect to a naïve evaluation. This Weizsaker -Williams approximation was giving the bulk of the cross section that was dominated by the log correction. So it was the start of the computation of log correction.

Of course, I was extremely interested in this kind of arguments. At that moment, what started was also the renormalization group, the Callan–Symanzik equation[27]. The Callan–Symanzik equation led to a paper by Symanzik, [where] he had very much the meaning of the renormalization group in high energy. Also, it was shown how you can use or relate all the

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[24] Bremsstrahlung: https://en.wikipedia.org/wiki/Bremsstrahlung

[25] G. Parisi and F. Zirilli, "A simple method for computing electrodynamic processes of high order," *Nuov. Cim. A (1965-1970)* **11**, 37-44 (1972). https://doi.org/10.1007/BF02722776

[26] C. F. v. Weizsäcker, "Ausstrahlung bei Stößen sehr schneller Elektronen," *Z. Physik* **88**, 612–625 (1934). https://doi.org/10.1007/BF01333110; E. J. Williams, "Nature of the high energy particles of penetrating radiation and status of ionization and radiation formulae," *Phys. Rev.* **45**, 729 (1934). https://doi.org/10.1103/PhysRev.45.729

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

scaling laws of deep inelastic scattering to the anomalous dimension of the composite operator. One part [of the work] was done by Brandt and Preparata[28]. After, there was this Callan-Symanzik treatment that was analyzing things with [Feynmann} diagrams, proving things, [so] the picture was very clear[29]. It was clear at that moment that there was a problem, therefore I started to reflect on how the Bjorken scaling law[30] was satisfied. Deep inelastic scattering was a big experiment at that time that produced partons. That was clearly one problem. At that time, I also became friend of Symanzik[31]. I met him on a few occasions. And I was reflecting also on the renormalization group. At that moment, it was more or less known that if you take a Yukawa [interaction the] coupling [increases at high energy]. The point is the following. What happens to the charge, to the effective coupling constant, when it is weak [at low energies]? If you increase the energy, does the coupling constant increase or does the coupling constant decrease? The old theories had this type of properties: increasing the energy, the coupling constant increases. What we would say in modern language [is that it is] not an asymptotically free field theory. There was only one example of asymptotically free field theory. (That was done by Symanzik.) It was a Landau ϕ^4 theory with negative coupling constants. Of course, negative coupling constants are usually bad, because that means an instability and so on. But formally, if you work with that, this works. Therefore, I started to study this type of problems. Also, I was interested in violations of Bjorken scaling laws at SLAC.

Sorry. What I forget is [that] we did with Peliti a nice paper in which a computation of the critical exponent for the O(N) model at the $1/N$ leading term, in the $1/N$ expansion using the conformal bootstrap[32]. There were two conformal bootstraps: conformal bootstrap *à la* Polyakov—just resumming diagrams—and the conformal bootstrap à la Slava. Also, I was starting to have discussions on resummation of diagrams, different renormalization groups and so on.

Indeed, it was certainly interesting to see if one could find a theory which was asymptotically free. Also, there were different viewpoints[33]. For

[28] R. A. Brandt and G. Preparata, “Operator product expansions near the light cone,” *Nucl. Phys. B* **27**, 541-567 (1971). https://doi.org/10.1016/0550-3213(71)90265-3

[29] G. Mack and K. Symanzik, "Currents, stress tensor and generalized unitarity in conformal invariant quantum field theory," *Comm. Math. Phys.* **27**, 247-281 (1972). https://doi.org/10.1007/BF01645514

[30] James Bjorken: https://en.wikipedia.org/wiki/James_Bjorken

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

[32] G. Parisi and L. Peliti, "Critical indices for the spherical model from conformal covariant self consistency conditions," *Phys. Lett. A* **41**, 331-332 (1972). https://doi.org/10.1016/0375-9601(72)90914-0

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

example, there was some argument by Wilson in ’72[34]—and I agree with him philosophically—that it would be natural to have a non-asymptotically free field theory for strong interactions, because this means that the theory at high energies has a non-trivial fixed point and so on. Anyhow, apart from that, you see when you start to make naturalness arguments, sometimes it works sometimes it does not work. There was a problem finding an asymptotically free theory. It was interesting. The Yukawa theory and so on, they were not asymptotically free. The only point was to understand gauge theory, because gauge theory was the only possibility.

I remember that at that moment I tried to look to gauge theory. I said: ”Look, gauge theory should work more or less like quantum electrodynamics. The self-energy would have the same sign as in quantum electrodynamics because it should be positive, and the vertex should not give a correction because of gauge invariance.” I did not try to do the computation, because I was convinced. However, both things were completely wrong. I knew that there were Fadeev-Popov ghosts[35], therefore with unitarity one must be careful. Because when you do the quantization you have ghosts in the quantization. And the identity of the vertex are not sufficient to guarantee that there is no vertex [correction] in the renormalization [of the coupling]. What happened is that in the summer of ’72, ‘t Hooft did the second computation of the beta function[36]. He found a pure gauge theory [that] was asymptotically free. I say the second computation because the first computation was done in ’66 or ’67 by a Russian [Iosif Benfsionovich Khriplovich] and nobody was so much interested in the arguments in that paper[37]. He did a beautiful computation—a few lines computation that was very interesting—and it disappeared from science.

Anyhow. I was told that ‘t Hooft[38] presented this result at the Marseille conference in August ’72[39]. He made just a five-minute remark on that. Ten

[34] K. G. Wilson and J. Kogut, "The renormalization group and the ϵ expansion," *Phys. Rep.* **12**, 75-199 (1974). https://doi.org/10.1016/0370-1573(74)90023-4 A preprint, *Institute for Advanced Study Lecture Notes COO 2220-2*, was circulating in 1972 as, for instance, cited in G. Parisi, “Deep inelastic scattering in a field theory with computable large-momenta behaviour, *Lett. Nuov. Cim. (1971-1985)* **7**, 84–88 (1973). https://doi.org/10.1007/BF02728276

[35] Fadeev-Popov ghosts: https://en.wikipedia.org/wiki/Faddeev%E2%80%93Popov_ghost

[36] G. ‘t Hooft, “When was asymptotic freedom discovered? or The rehabilitation of quantum field theory,” *Nucl. Phys. B* **74**, 413-425 (1999). https://doi.org/10.1016/S0920-5632(99)00207-8

[37] I. B. Khriplovich, "Green’s functions in theories with non-abelian gauge group," *Yadern. Fiz.* **10**, 409-424 (1969) [*Sov. J. Nucl. Phys.* **10,** 235-242 (1969).]

[38] Gerard ‘t Hooft: https://en.wikipedia.org/wiki/Gerard_%27t_Hooft

[39] *Conference on Renormalization of Yang-Mills Fields and Applications to Particle Physics*, C. P. Korthals-Altes, Centre de Physique Théorique, CNRS, Marseille, France, June 19-23, 1972. Proceedings:

people were present. Nobody understood what 't Hooft said but Symanzik. Symanzik, I [learnt] later from Tini Veltman[40], did not want to tell me the result because he told Veltman: "Parisi is so wild that [he] could publish something, quoting 't Hooft, obviously." But if you publish something quoting someone unpublished usually after you get quoted instead of the one unpublished that cannot be quoted. So he said me nothing for a few months. After a few months, 't Hooft was not writing anything because he was trying to do quantum gravity at one loop and he had no time to write this thing. At the end, I [learnt] that thing. I went to CERN. I had a short discussion with 't Hooft on this matter. And this was one of the most stupid things in my life. I was really an expert of deep inelastic scattering. For example, I was trying to interpret the violation of Bjorken scaling with the theory with a non-asymptotically free field theory and so on. I knew that there was a proposal by Gell-Mann of having quarks colored: QCD[41]. (QCD was proposed by Gell-Mann, but I did not like too much the proposal by Gell-Mann, maybe for psychological reasons.) In the end, when we started to discuss with 't Hooft, the main problem was to look for the gauge group. One idea that we put forward was [that] the gauge group was the flavor group. But the flavor group cannot be a gauge group for strong interactions without destroying the renormalizability of weak interactions and electromagnetism, because it means that gluons are charged. If you use SU(2)xU(1) for electromagnetism, you can put on the top SU(3) [only] for the color stuff. However, at the end of the game, we said: "Look. We are not able to do a viable theory that is asymptotically free." Of course, if someone was present in the room and would have [asked] us: "Why don't use Gell-Mann's theory?" The immediate answer, in five seconds [would have been]: "Yes. You are right. With Gell-Mann's theory everything is fine. We are done! At the end the theory is asymptotically free." The point is at that moment I was interested in computing the critical exponents of the Ising model with a perturbative expansion, not with the epsilon expansion, but directly in three dimensions. 't Hooft was interested in quantum gravity, I was interested in critical exponents, so we both missed the thing. The responsibility is more mine, because I was the one [who] was really an expert on phenomenology and all that. Anyhow.

FZ: If I can summarize what you said until now, at that point, you had a network of collaborators that were some of your friends…

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https://inis.iaea.org/collection/NCLCollectionStore/_Public/05/092/5092103.pdf (Accessed April 18, 2022.)

[40] Martinus Veltman: https://en.wikipedia.org/wiki/Martinus_J._G._Veltman

[41] Murray Gell-Mann: https://en.wikipedia.org/wiki/Murray_Gell-Mann

GP: [0:31:49] Not very much. There was the paper that was written with Ferrara and others, but most of the papers at that [time]—like the one of deep inelastic scattering—[were not]. The first version of the Altarelli-Parisi[42] [equation for valence quarks] was done in ’73; that was done by myself[43]. I started to have a network of collaborations after I went for one year, ’73-’74, to Columbia university.

FZ: So before ’73, you were mostly working by yourself, but you still had this network of people with whom you were talking.

GP: [0:32:44] Yes. I was talking to a lot of people.

FZ: You were interested in statistical mechanics from the point of view of the renormalization group and calculation of critical exponents, but you were also interested in high-energy physics. The goal was to formulate…

GP: [0:33:01] I was oscillating between the two fields. I went to Columbia University in ’73-’74. When I came back from Columbia University, I started to work with people in Rome—that was Petronzio, Ellis and so on—to try to understand better the scaling relation of deep inelastic scattering[44]. That was essentially some of things that I was doing. And I was responsible in fact of the first two students—that were Guido Martinelli and Roberto Benzi—on the problem of statistical mechanics[45].

After that I went to Paris, and when I went to Paris I started to collaborate with many other people in Paris.

PC: Just before we get to that. I’m trying to understand what was your working style. You said you were working a lot on your own. Or, at least, you published a lot on your own. So you were having conversations with the community by going to meetings, by exchanging letters? And then you were mostly doing computations on you own, writing papers? You have a series a papers throughout the ‘70s…

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[42] DGLAP evolution equations: https://en.wikipedia.org/wiki/DGLAP_evolution_equations

[43] G. Parisi, **“**Detailed predictions for the p-n structure functions in theories with computable large momenta behaviour,” *Phys. Lett. B* **50**, 367-368 (1974). https://doi.org/10.1016/0370-2693(74)90692-3

[44] See, *e.g.*, G. Parisi and R. Petronzio, "On the breaking of Bjorken scaling," *Phys. Lett. B* **62**, 331-334 (1976). https://doi.org/10.1016/0370-2693(76)90088-5; R. K. Ellis, R. Petronzio and G. Parisi, "Mass dependent corrections to the Bjorken scaling law," *Phys. Lett. B* **64**, 97-101 (1976). https://doi.org/10.1016/0370-2693(76)90366-X

[45] See, *e.g.*, R. Benzi, G. Martinelli and G. Parisi, "Anomalous dimensions from a high temperature expansion without a lattice," *Phys. Lett. B* **64**, 451-453 (1976). https://doi.org/10.1016/0370-2693(76)90119-2; “High temperature expansion without lattice,” *Nucl. Phys. B* **135**, 429-444 (1978). https://doi.org/10.1016/0550-3213(78)90347-4

GP: [0:34:29] There were in a sense two problems that I was interested in. In the end of ’69, field theory was considered something that is useless to do computations [in strong interactions]. You can do computation in a perturbative way, and then in a non-perturbative way. What I was interested in, in a sense, is if one could find some key to understand field theory in a not-so-weak coupling regime. Therefore, from one side, there was high-energy physics, because at the beginning one was assuming that some of the interactions are strong. And in the other regime was phase transitions, because phase transitions in three dimensions were also strong interactions. In the end, it turned out that strong interactions were weak at high energy, and therefore I was trying to understand more precisely all the types of scaling behavior, the determination of the color coupling constant and so on. Also, I was interested to understand how one could do a [second order phase transition] theory in three dimensions, without resorting to the 4-epsilon expansion.

When I went to Paris in ’76, I started to work on many different subjects.

FZ: Can you explain to us why you decided to go to Paris? Why Paris? Who invited you there? What was the connection?

GP: [0:36:45] One year, I spent in Columbia University. Well, Paris, I think that Luigi Radicati di Brozolo[46]—[who] died two years ago at the age of 100—was on the scientific panel of the *Institut des Hautes Études Scientifiques*, in Bures-sur-Yvette. He was a very good friend of Louis Michel[47], who was a high-energy physicist who had done a famous paper on muons in the ’50s. He invited me. He asked me to go there for one year. I accepted. I was very happy with the invitation, because I was interested to go to a city with a good life. Certainly, Paris was a very interesting point.

Also, because my wife—at that moment she was not my wife, but we were planning to marry in a short while—was studying Greek literature and found that Paris was an excellent place to study Greek literature. Therefore, she decided [to] enroll for the doctorate at the Maison [des Sciences] de l’Homme[48]. (I don’t remember exactly. It was with Pierre Vidal-Naquet[49]. The relevant thing was the thesis [was] with Pierre Vidal-Naquet[50].) I would go to Paris, and we should remain there for two years.

[46] Luigi Radicati di Brozolo: https://en.wikipedia.org/wiki/Luigi_Arialdo_Radicati_di_Brozolo
[47] Louis Michel: https://en.wikipedia.org/wiki/Louis_Michel_(physicist)
[48] Maison des Sciences de l’Homme: https://fr.wikipedia.org/wiki/Fondation_Maison_des_sciences_de_l%27homme
[49] Pierre Vidal-Naquet: https://fr.wikipedia.org/wiki/Pierre_Vidal-Naquet
[50] Daniella Ambrosino, *Mythe et comédie chez Aristophane : le mythe comique des "Nuées"*, Thèse de 3e cycle, École des hautes études en sciences sociales (1984).

[She would not have enough time at Paris to finish] the thesis, but to do the DEA[51] and the other stuff other than the thesis. So we decided to go there.

When I got there, I was already a friend of Édouard Brézin[52]. I started to go often to Saclay. I met Itzykson[53], Zuber[54] and others. Sourlas[55] was at that time at ENS. One year, there was Altarelli[56] there, so I worked also with Altarelli. Therefore, at that time I was oscillating between École Normale Supérieure, Bures-sur-Yvette and Saclay.

FZ: And Brézin, you met him in Cargèse in '73[57]?

GP: [0:40:07] No. Brézin invited me to give a seminar before '73. He invited me to give a seminar in Paris in October of '72. Somewhat after the seminar, he invited me to go to Cargèse in '73. He wanted to check, I guess, what I was telling, but [he] also probably wanted to check me.

So I was in Paris. I started to work on many things. One of the things I started to work on was high-order perturbation theory. There was a Lipatov paper in '76[58], who was doing the computation in high-order perturbation theory and doing the tunneling from negative coupling. I was quite struck by that paper, because I knew more or less the same computation was done by Langer a few years before, and I [had] read the Langer paper[59]. I "read" is [saying] too much; I looked at the Langer paper very fast. I was looking to a lot of papers. When I read the Langer [paper], my impression was: "No. It cannot be so easy to do the computation."

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https://www.sudoc.fr/052994341 **GP :** My recollection was wrong. it was l'École des Hautes Études en Sciences Sociales La Maison de l'Homme existed at that time but it was a different establishment.

[51] Diplôme d'études approfondies: https://fr.wikipedia.org/wiki/Dipl%C3%B4me_d%27%C3%A9tudes_approfondies

[52] 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

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

[54] Jean-Bernard Zuber: https://en.wikipedia.org/wiki/Jean-Bernard_Zuber

[55] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Nicolas Sourlas*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 23 p. https://doi.org/10.34847/nkl.2a55p6c3

[56] Guido Altarelli: https://en.wikipedia.org/wiki/Guido_Altarelli

[57] *Cargèse Summer School on Field Theory and Critical Phenomena*, E. Brézin and J. Charap, Cargèse, Corsica, France, July 1973.

[58] L. N. Lipatov, "Divergence of the Perturbation Theory Series and the Quasiclassical Theory," *Sov. Phys. JETP* **45**, 216-223 (1977); *Zh. Eksp. Teor. Fiz.* **72**, 411-427 (1977). See, *e.g.*, J.C. Le Guillou and J. Zinn-Justin, eds., *Large-Order Behaviour of Perturbation Theory* (Amsterdam: Elsevier, 1990).

[59] J. Zittartz and J. S. Langer, "Theory of bound states in a random potential," *Phys. Rev.* **148**, 741 (1966). https://doi.org/10.1103/PhysRev.148.741

However, when Lipatov made the things clear, I started to work on high-order perturbation theory. I wrote some papers by myself[60], and some papers with Brézin [and Zinn Justin][61]. Afterwards, Itzykson and Zuber started to work on fermions, and I wrote two papers on fermions[62]. One of the most amazing things, in retrospect, is that I was doing a computation of this coupling constant, the asymptotic behavior of high-order perturbation theory, by doing a cavity computation. It was a cavity computation taking a Feynman diagram, just the type of computation that you do for computing the free energy on Bethe [lattices]. When we did things on Bethe [lattices] I [had] completely forgotten the computation that I did at that moment, but that was a notion that was introduced [with] these things. There was a technical difference. We spent a lot of [time] looking to this type of large order behavior, also because the large order behavior was important to get more precise numbers from the renormalization group for the critical expansion.

Also, I was working with Nicolas Sourlas and Drouffe on perturbative expansions, on what happens in high dimension, corrections to high dimensional things and so on[63].

One thing that I was working [on]—I never published the paper—was looking if one can introduce, define the non-integer order in perturbation theory. When you look to some computation in perturbation at first order, second order, third order and so on, [it is always at integer order]. The idea is that if you could generalize the perturbation theory to a non-integer value. The idea essentially is that you cannot do a non-integer value computation of a diagram by scratch, but if you know what you can argue that in certain cases the value of the coefficient of the perturbative expansion defines some analytic function that may have some poles in certain region and therefore you can do an analytic continuation. You can think of where the pole of the coefficient [is], and this pole controls the behavior at large values of the coupling constant. In some sense, the value at one, two, three and so on controls what happens at small *g*. The position

60 See, *e.g.*, G. Parisi, "Asymptotic estimates in perturbation theory with fermions," *Phys. Lett. B* **66**, 382-384 (1977). https://doi.org/10.1016/0370-2693(77)90020-X

61 See, *e.g.*, E. Brézin, G. Parisi and J. Zinn-Justin, "Perturbation theory at large orders for a potential with degenerate minima," *Phys. Rev. D* **16**, 408 (1977). https://doi.org/10.1103/PhysRevD.16.408; E. Brezin and G. Parisi, "Critical exponents and large-order behavior of perturbation theory," *J. Stat. Phys.* **19**, 269-292 (1978). https://doi.org/10.1007/BF01011726

62 **S**ee, *e.g.*, C. Itzykson, G. Parisi and J.-B. Zuber, "Asymptotic estimates in quantum electrodynamics," *Phys. Rev. D* **16**, 996 (1977). https://doi.org/10.1103/PhysRevD.16.996; R. Balian, C. Itzykson, J.-B. Zuber and G. Parisi, "Asymptotic estimates in quantum electrodynamics. II," *Phys. Rev. D* **17**, 1041 (1978). https://doi.org/10.1103/PhysRevD.17.1041

63 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

of the poles controls the thing at large *g*. It's essentially a trick that I learned with Symanzik in order to re-sum some complex Feynman diagrams and so on. Anyhow, I was working on these things, and also because I learned some tricks of integer functions, all this stuff for analytic functions, when I was doing that work on Fermions with Itzykson—because we had to construct the integer function that corresponds to the determinant of Fermions and so on.

Therefore, when I went back to Rome…

There was some work that I started to do with Drouffe and Nicolas Sourlas that was on gauge theory. The idea is that if you solve the strong coupling expansion on the lattice of lattice gauge theory, you have that the things that is going to dominate are like a surface. You have a Wilson loop, and you look at the expectation of this Wilson loop and what happens is that you have to do a high-temperature expansion where this Wilson loop is connected by plaquettes and so on. However, the point is what else happens, because the theory does not possess any type of excitations [at the leading order]. The thing that we could say: "Well, you can have some excitation like a cube that is added on the surface. Other than this cube, we can add some other cube over there. And there you can have some kind of polymer from the cube, which could have some bifurcations and so on. This polymer cube may interact at some point—maybe some attraction, repulsion by these things. Here, that may be." What was clear is that if you said that in some particular regime [at] beta d^4 [fixed at large d], in some particular regime, these diagrams were dominating. We did some things on these diagrams, and after the idea was that we could do something more.

I started to look for a paper in the literature and by chance I went to a paper, I think, by Lubensky, in which he was doing this type of computation using the replica method[64]. Coming to the replica method. The replica method is doing *n* going to 0 in some sense, in some theory. That idea was first formulated by Brout[65], in the '60s[66], but many people were not aware of the Brout thing.

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[64] 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 **GP:** I discovered the replica approach in this paper, and I noticed that for unknown reasons it gave the wrong results in spin glasses. Maybe the material was not in this paper but in a paper quoted or a longer chain of papers.

[65] Robert Brout: https://en.wikipedia.org/wiki/Robert_Brout

[66] **PC:** Brout did not use the replica *trick*, but clearly articulated the distinction between quenched and annealed averages in disordered systems. See, *e.g.*, R. Brout, "Statistical Mechanical Theory of a Random Ferromagnetic System," *Phys. Rev.* **115**, 824 (1959). https://doi.org/10.1103/PhysRev.115.824

FZ: In what context did Brout do this?

GP: [0:49:38] In solid state physics, to do statistical mechanics, to do diagrammatics, perturbations on metals of I-don't-know-what kind of metal in solid state physics with quenched impurities. He says: "Well, technically, the simplest thing is to introduce *n* copies of that and after to send *n* equal to zero." But this was just a tool for constructing some kind of perturbation theory with the correct diagrammatic rule without having to do by hand all the computations.

The thing that was very popular was by de Gennes, [who] discovered that polymers can be [described] as a field theory with O(*n*) symmetry in the limit where *n* goes to 0[67]. Therefore, this was just understanding all the result of the computation of exponents for the Ising model for polymers. This was very important, because there was some debate in the polymer [world], if the radius of the polymer should be like $N^{1/2}$—that was the mean-field theory [prediction]—or if it should be something different. De Gennes was pushing for something different. After, he decided: "Look, I've computed this 0.58 [exponent] that just fits the data." Because when you have something slightly higher that 0.5, people were saying: "Well, maybe this is [a] subleading thing." If you have a prediction of [an exponent of] 0.58 and it fits very nicely with the data [things change]. Therefore, this was already known, and Lubensky was doing polymers of different sorts. (There was essentially this research going on with these things mainly originating from de Gennes' theory.) And he was doing this computation of branched polymer.

Because I was not looking anymore at anything that was written [about] statistical mechanics from the end of '74, I discovered that from the end of '74 to '78 there were a lot of problems that people had started to study: some things connected to impurities, some things with disordered systems, or not disordered but like polymers, and so on. There was a lot of things.

One thing that I noticed that was in the Lubensky paper is… Maybe I can tell you what I remember was written. (I read the paper at the end of '78 and I never looked again. I looked again in '81, but after '81, for 40 years, I never looked at the paper.) You must be careful because there is a mistake [in the application of replica method to Sherrington-Kirkpatrick model]. Because the results in the case of spin glasses are not consistent. So the

[67] P.-G. De Gennes, "Exponents for the excluded volume problem as derived by the Wilson method," *Phys. Lett. A* **38**, 339-340 (1972). https://doi.org/10.1016/0375-9601(72)90149-1

replica method maybe, in one case, gives inconsistent results. In principle, that should not concern me too much, because I knew well de Gennes' theory. I understood that what they were doing with branched polymer stuff was just a counting and the replica technique that was slightly different from de Gennes' but was essentially doing the counting in the correct way. So the fact that spin glasses were not so correct was not so interesting.

But I was [thinking]: "Well, we cannot remain with something wrong that is written in the literature. That some method gives some wrong result and we don't know why." I said: "I think that I should read the literature and I think that it can be fixed easily. There must be some mistake that should be fixed." That was just before the Christmas vacation of '78. I was in Frascati, I went to the library, I started to look back to what was written in the literature. I made Xerox copies of a few papers. I remember for certain: the paper by Sherrington and Kirkpatrick[68]; the original by Anderson and Edwards that introduced the replica method for spin glasses[69]; and also the very interesting lectures of Anderson in a book that was published. It was the Les Houches school on Ill-Condensed Matter[70].

PC: So you had this already in the winter of '78? But the school was in the summer of '78.

GP: [0:56:22] I think it was in '77.

PC: No. It was July-August of '78.

GP: [0:56:29] Or maybe I read that later. I'm not sure. Maybe I read that one year later. For me, it's possible. You're right that it cannot [be]. The school was in '78 and at the end of '78 I could not have *La Matière condensée malade, comme on dit*.

PC: *La Matière mal condensée.*

GP: [0:56:50] So maybe not the paper by Anderson. Anyhow.

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[68] D. Sherrington and S. Kirkpatrick, "Solvable model of a spin-glass," *Phys. Rev. Lett.*, **35**, 1792 (1975). https://doi.org/10.1103/PhysRevLett.35.1792

[69] S. F. Edwards and P. W. Anderson, "Theory of spin glasses," *J. Phys. F* **5**, 965 (1975). https://doi.org/10.1088/0305-4608/5/5/017

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

FZ: Before you tell us this story. Your interest in this replica problem came for this study of perturbation theory and resummation at all order, and then through the paper by Lubensky you went…

GP: [0:57:28] Exactly. The paper of Lubensky was saying that there is something that was not going correctly with the replica method. It said something like: "In some cases, the replica method gives the wrong result."

FZ: So you decided to look into that case.

GP: [0:57:48] It was also by curiosity. I thought it should be fixed relatively easily. I started…

FZ: But you also said that you were interested in other problems of disordered systems.

GP: [0:58:08] No! When I looked to Lubensky's paper, they were saying that there was a lot of papers that were using the replica method. After looking at Lubensky's paper, I realized—I just gave a look to the *Journal of Physics A*[71]—that there was a lot of work that was done on disordered systems, which I was not at all aware of.

FZ: Had you not already started to work with Nicolas Sourlas on supersymmetry in the random field Ising model?

GP: [0:58:52] Not at all. That was in '81 or '80.

FZ: I think you had a paper that was submitted in '79[72].

GP: [0:59:13] Maybe in '79.

FZ: So in '78 you still had not worked on this.

GP: [0:59:12] In '78, I was not working[73]…

[71] See, *e.g.*, J. R. L. de Almeida and D. J. Thouless, "Stability of the Sherrington-Kirkpatrick solution of a spin glass model," *J. Phys. A* **11**, 983 (1978). https://doi.org/10.1088/0305-4470/11/5/028

[72] G. Parisi and N. Sourlas, "Random magnetic fields, supersymmetry, and negative dimensions," *Phys. Rev. Lett.* **43**, 744 (1979). https://doi.org/10.1103/PhysRevLett.43.744

[73] **GP:** I completely messed the chronology. In '77 Brézin explained to me the random field Ising model. At that time dimensional reduction was proved only at the one loop. After one or two hours of discussions at the blackboard, we did a compact derivation of dimensional reduction at all loops. For complicated reasons we never published the result, that was found independently by A. P. Young more or less at that time [A. P. Young, "On the lowering of dimensionality in phase transitions with random fields," *J. Phys. C* **10**, L257 (1977). https://doi.org/10.1088/0022-3719/10/9/007]. However the genesis of the Parisi-Sourlas paper was different. We knew that in field theory, *n* Fermions are equal to *-2n* Bosons, so we got the a

FZ: And when you were in Paris, you never…

GP: [0:59:18] When I was in Paris, I was interested in high-order perturbation theory, planar diagrams[74]. I was working with Guido Altarelli. We did the Altarelli-Parisi equation[75], and we were doing with Guido Martinelli some application of these things to hadrons that produced m^+/m^- pairs[76].

FZ: So you never met Blandin, who was in Paris?

GP: [0:59:56] Yes. I met him later.

FZ: But not before '78?

GP: [1:00:01] No. Maybe I met him for lunch at Saclay, but we never discussed physics. We were interested at that moment at the high-energy physics perspective. Therefore, the planar diagrams were important, the high-energy things. It was the typical thing that were doing Brézin, Zinn-Justin[77], and Altarelli. In that occasion maybe we wrote a paper also with Nicola. It was that year that Nicola Cabibbo was there and [with] Luciano Maiani [we wrote] on some bounds on the Higgs coupling constant[78]. I was doing all high-energy physics, up to the moment that I went to look to the paper by Lubensky. From the paper by Lubensky, I could make that there was a lot of interesting things that were done in four years. (You can imagine that in four years a lot of interesting things [had been done].) My feeling in '74 was that having understood the critical exponents of spins, all other transitions were in the same universality class. I was surprised that there were a lot of things that were left over, [including] disordered systems. And there was this replica trick that was used to do the computation of systems with disorder. In some cases, the replica trick was a modification on the standard O(n) theory. For the problem of random potential it was also n=0,

theory with D Bosonic coordinates plus 1 Fermionic coordinates should be equivalent to a theory with D-2 Bosonic coordinates. I do not know why we started to study the problem. I think that Nicolas was interested to see how it worked. Only at the end, after having spelled out the D-dimensional theory with Fermions and Bosons, I recognized that it was the problem I had looked at with Brézin.

[74] See, *e.g.*, E. Brezin, C. Itzykson, G. Parisi, and J.-B. Zuber, "Planar diagrams," *Comm. Math. Phys.* **59**, 35–51 (1978). https://doi.org/10.1007/BF01614153

[75] G. Altarelli and G. Parisi, "Asymptotic freedom in parton language," *Nucl. Phys. B* **126**, 298-318 (1977). https://doi.org/10.1016/0550-3213(77)90384-4

[76] See, *e.g.*, G. Martinelli and Parisi, "Testable QCD predictions for sphericity-like distributions in e+ e− annihilation," *Phys. Lett. B* **89**, 391-393 (1980). https://doi.org/10.1016/0370-2693(80)90150-1

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

[78] N. Cabibbo, L. Maiani, and G. Parisi, "Bounds on the number and masses of quarks and leptons," *Nucl. Phys. B* **136**, 115-124 (1978). https://doi.org/10.1016/0550-3213(79)90167-6

but with the wrong signs of the coupling constant and it was complex. Anyhow, I started to look…

FZ: Sorry, but we want to understand in a bit of detail how these ideas developed in that period. Around ’78 already, the de Almeida-Thouless paper was out.

GP: [1:03:01] I don’t remember the moment I made a copy of the de Almeida-Thouless paper, whether it was before Christmas or after Christmas. What I’m certain was very clear—because they were the papers that were quoted by Lubensky—[is that] there were the paper by Sherrington and Kirkpatrick and by Kirkpatrick and Sherrington[79]. Two papers.

FZ: We have been told, for example, that Thouless[80] was going around, giving seminars on the breaking of replica symmetry. So you never crossed paths?

GP: [1:03:47] No. I didn’t cross path [with him]. I mean, I was not interested at that moment at all in solid state physics. If there was a seminar in Paris by Thouless I would not go. I was impressed by some of his results. I knew the work by [Kosterlitz] and Thouless[81]. That was well known and very important—with connections with the renormalization group and with [the] XY [model] in two dimensions—but I was not at all interested in this kind of matter, so I did not go to any seminar[82].

FZ: When you were in Paris, you didn’t meet Cirano[83] and the people who were already a bit involved in spin glasses?

GP: [1:04:30] I met Cirano. I was a good friend of Cirano. I don’t know when he started to work [on spin glasses], but I’m certain I did not discuss [this] with Cirano [before the MECO conference of April 1979 in Trieste].

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[79] S. Kirkpatrick and D. Sherrington, "Infinite-ranged models of spin-glasses," *Phys. Rev. B* **17**, 4384 (1978). https://doi.org/10.1103/PhysRevB.17.4384

[80] 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

[81] Kosterlitz-Thouless transition: https://en.wikipedia.org/wiki/Kosterlitz%E2%80%93Thouless_transition

[82] **GP:** I must have met Thouless or Kosterlitz (or had mail contacts), after the introduction of the functional order parameter. In Ref. 138, it is written: “We are very grateful to Dr G. Parisi for various communications about his work.” The paper is dated February ’80, about eight months after mine. The first day I was in a workshop at Les Houches, in February ’80 (see Ref. 122), I had a discussion with Thouless and Kirkpatrick on spin glasses.

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

FZ: Cirano had a paper in '78 about dynamics and replicas[84], but you didn't discuss about that?

GP: [1:04:55] I didn't discuss about that[85].

FZ: So it was really the Lubensky work.

GP: [1:05:00] I did not discuss with Cirano. I mean, we went to eat together with Cirano at the *cantine de Saclay*[86], but…

FZ: So the community working on spin glasses and disordered systems and the high-energy community were quite disconnected…

GP: [1:05:20] I think that when we had Cirano at the table, the main argument was about the position of the communist party. There were lots of political discussions on *l'union de la gauche*[87] and all this kind of problems. So it's clear I had never heard the name—if I had, I had forgotten—when I looked at the Lubensky paper and later on to the Sherrington-Kirkpatrick paper. The only thing that I can remember is that one day Édouard [Brézin] told me that Kirkpatrick is a smart guy, but I don't remember why he told me that Kirkpatrick is a smart guy.

I looked at the papers by Sherrington and Kirkpatrick and Kirkpatrick and Sherrington. I went home and I started to do some computations. Because I started to do computations and so on, I'm not exactly… I think that I reproduced a certain proof and that I did some computation. After I went to Frascati, I looked back at the literature and I discovered that some of the computations that I was doing were already done.

(It's possible that the lectures of Anderson were floating around as a preprint. That may be possible. At that time, there were a lot of preprints around. That's possible that [was the case with] the Anderson paper. I should look back to see if I find the photocopy of that paper.)

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[84] 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

[85] **GP:** After further thinking: I did discuss at length his work on dynamics in general, Langevin equations, the dynamic renormalization group work with Brézin, the diagrammatical expansion for the time dependent correlations, but not replicas.

[86] Ana Bela de Araujo, *Auguste Perret - La cité de l'atome - Le centre d'études nucléaires de Saclay* (Paris : Éditions du Patrimoine, 2018); Olivier Delaittre, Daniel Moulinet and Julie Delaittre-Vichnievsky, "Le Restaurant du CEA Saclay : Une Réinterprétation contemporaine de l'œuvre d'Auguste Perret – Visite du 25/06/2014," *Commissariat à l'énergie atomique et aux énergies alternatives* (2014). https://www.cea.fr/presse/Documents/DP/2014/Dossier-presse-reinterpretation-oeuvres-Auguste-Perret.pdf (Accessed February 4, 2022.)

[87] L'union de la gauche: https://fr.wikipedia.org/wiki/Union_de_la_gauche

At that moment, there was the de Almeida-Thouless. It was clear that the breaking was in that direction, so I started to do some work on breaking the replica symmetry. The typical thing—that was similar to the one that I think was done by De Dominicis and maybe some others—was to do the breaking with m=2[88]. This is the standard breaking with m=2. There were two points essentially that were crucial in the Sherrington-Kirkpatrick paper with the SK model. One, it was evident that the entropy has to be positive, non-negative, and that you don't need anything special. The second thing is that there were good evidence in the second paper that the ground state energy was not -0.798, but was around -0.76. Anyhow, I need not count the standard deviation. Also, the specific heat was different from the other one. I took for granted that not only the entropy at zero temperature was wrong, but also the internal energy at zero temperature was wrong. By a small amount, [but] it should be wrong. For example, I looked at the breaking with m. Also, I tried to look to the limit $m \to \infty$ of breaking. What I realized is that when you do the breaking with m—for the standard thing, for m integer, for m>1—the entropy at zero temperature is proportional to $1/m$, so the entropy problem is cured when m goes to infinity, but the internal energy does not move. The reason is essentially that you… Well, you do the computation. Therefore, one could cure the thing with m going to infinity for the entropy, but for the energy it was not good. Also, I discussed with De Dominicis on their paper [but not in Paris].

Sorry. I forgot about [something]. At the end of March '79 I went to a conference in Trieste, to a MECO conference. I think it was the fifth or the sixth MECO conference[89]. (Tosatti[90] [recently] sent me the program of that conference.) One of the speakers was De Dominicis. He was talking about spin glasses[91]. I presented a poster, where I was showing how to do this computation, I think for general integer m[92].

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[88] **GP:** The case m=2 was done by Blandin (see Ref. 100). The case with generic integer m was done by C. De Dominicis and T. Garel, "A solution of Sherrington Kirkpatrick model for Ising spin glass with physically acceptable entropy," *J. Phys. Lettres* **40** 575-578 (1979).

[89] Sixth International Seminar on Phase Transitions and Critical Phenomena (MECO), March 26 – 28, 1979, International Center for Theoretical Physics, Trieste, Italy.

[90] Erio Tosatti: https://en.wikipedia.org/wiki/Erio_Tosatti

[91] C. De Dominicis, "Systems with quenched Random Impurities Including Spin Glasses," Sixth International Seminar on Phase Transitions and Critical Phenomena (MECO), March 26 – 28, 1979, International Center for Theoretical Physics, Trieste, Italy.

[92] **GP:** The contents of De Dominicis' talk and my poster were very similar. There were only some differences I cannot remember. In any case, the conclusion was clear: one step replica symmetry breaking with integer m was not the solution. In the limit $m \to \infty$, the entropy crisis was solved, but the value of the internal energy was not correct.

PC: Was this conference the first time you were presenting that work?

GP: [1:12:29] Yes. But it was something that did not work. It was not working. I was concluding that one could try to break the replica symmetry this way. I think that De Dominicis presented for m=2. I think that I did the computation for all m. (I think, but I'm not sure[93].) I presented this thing; I had a poster. There was someone from the physics department of Rome—Luciano Mistura[94], I believe, but I have to check the name—who mentioned: "Look, this does not make sense. You have a free energy and the free energy should be minimized. Now, you have one parameter q_0 and q_1" –or q and p, nowadays we say q_0 and q_1— "and now for one parameter you minimize and for the other parameter you maximize." Because you see the trace of Q^2 is something like $2q_1^2$-q_0^2. Only when n becomes less than one, do all terms have the same sign. When m>1, one term has one sign, but you have a m-1 factor and that is going to change the sign. So he said: "Look, you are going to minimize one parameter and to maximize another one. You should minimize with respect to the two." It was clear that I have to maximize, because it was already clear that in the high-temperature phase one has to maximize the free energy. But it started me [thinking], with that suggestion, that there was something strange, that you have to maximize with one and minimize with the other one. In reality, there is nothing strange with that, because if you follow the correct computation that was done by Amit, Sompolinsky and Gutfreund for the Hopfield model[95]—this was done later on—you have one parameter which you have to maximize and one parameter which you have to minimize. So the fact that in this type of approach you don't treat minimization and maximization in a common way for the order parameter is not necessary. Probably because they are auxiliary parameters depending on a number that is either positive or negative and so on. Anyhow, when he did this [comment] I started to think, and I said: "How can I have the two terms of the same sign?" The answer was quite simple: I should take m<1. At least for the quadratic term, it's quite clear that at the quadratic level, if m<1, one term is $mq_0^2+(1-m)q_1^2$, so this has the same sign. So I said: "Now, if I have m<1, which value should it take?" The only possibility was that I should maximize also with respect to m. I think that it was relatively easy to check. The $T \to 0$ limit in this case was not easy to be done by hand—in the other case, it was done by hand—so I had to write a computer program to do the computation.

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[93] **GP:** I now recall that both where for generic m.

[94] Luciano Mistura (December 3, 1938--) was faculty in the Dipartimento di Scienze di Base e Applicate per l'Ingegneria at La Sapienza Università di Roma. See, *e.g.*, "Luciano MISTURA," *Aracne editrice* (undated). http://www.aracneeditrice.it/aracneweb/index.php/autori.html?auth-id=10784 (Accessed May 19, 2022.)

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

Fortunately, at that time I [had done] a few computer programs, so I was able to do computations with computers. And we had a connection with some CDC [7600][96]. We could have some time on the CDC [7600] from Frascati. Also, there was a nice program from CERN—everything was in FORTRAN—that was called—I think it [still] exists, but it's different today from that time—MINUIT[97]. MINUIT is something that makes a minimization. It was something that was used by experimentalists to do fit, so it was quite well organized. So I defined my functions. Because I already had the formula, I only had to check that the integrals were correct. The formula were written with mathematical work. So I gave it to MINUIT and MINUIT was going to minimize. When MINUIT was minimizing—this could be done at different values of the temperature—the result was that the entropy was much, much smaller: 0.01. The other was 0.17, therefore I was gaining a factor of 20 in the entropy. It was much nearer to zero. And the energy was -0.765 or something like that. That was [close to] the [computational] one, which was -0.76+/.01. It was clearly… Anyhow, it was different from -.798. And the specific heat had a shape that was much better than the other thing. I was extremely satisfied by this thing, because it was the first time that I could see something that more or less solved, or nearly solved, both problems. I wrote a letter to *Physics Letters*[98].

However, it was quite evident that the result was not the correct [one], because the entropy was still slightly negative. If you look from the point of view of symmetry, in essence what was broken was the O($n \to 0$) group. When you have this broken symmetry, you have still an O(0) group that remains unbroken, so you could break again that group, which corresponds to the hierarchical construction that we know. I was familiar—from high-energy physics, from my thesis work—with the idea that you have a group, a breaking of the symmetry group, Goldstone bosons and all this type of coset group that corresponds to the breaking. All the group theory was clear to me. Therefore, I sent a paper to *Physics Letters A*, saying that there was this breaking of the symmetry, and saying at the end, in the last sentence[99], that because the entropy is negative this is not the correct result, that you can break the [symmetry] another time in the same direction. What happened is that…

[96] CDC 7600: https://en.wikipedia.org/wiki/CDC_7600
[97] MINUIT: https://en.wikipedia.org/wiki/MINUIT
[98] G. Parisi, "Toward a mean field theory for spin glasses," *Phys. Lett. A* **73**, 203-205 (1979). https://doi.org/10.1016/0375-9601(79)90708-4
[99] "The solution with two order parameters […] works much better than the solution with only one order parameter […]. It is quite likely that an infinite number of order parameters is needed in the correct treatment […] and that the neglected order parameters have small effects at not too small temperature."

FZ: Can I just summarize up to this point? If I understand well, there was this moment in the Christmas of '78 when you started going through the literature, then you went to Trieste. The *m*=2 case had been done…

GP: [1:21:37] By De Dominicis

FZ: And also by Blandin[100], because in the paper you cite…

GP: [1:21:49] Blandin, De Dominicis, or both. I don't remember. Or [actually] by Blandin. It was made by some French people. I'm not sure exactly what De Dominicis said, but…

FZ: Probably you learned about the result for *m*=2 from De Dominicis in Trieste?

GP: [1:21:59] No. I think that Blandin did the computation in '78. I did the computation by myself in February ['79] and then learned that it was already done by Blandin.

FZ: You learned the Blandin result by reading the literature?

GP: [1:22:24] By reading the literature.

FZ: So De Dominicis in Trieste was speaking about something else.

GP: [1:22:33] What I presented was the generalized Blandin stuff for any value of *m*. [This was also the content of De Dominicis's talk.]

FZ: So you were discussing the limit $m \to \infty$?

GP: [1:23:16] I was saying that $m \to \infty$ was solving the entropy, but not the energy thing. [I think also Cirano found the same thing.]

FZ: Then the crucial step was to understand that *m* could take values…

GP: [1:23:28] Then someone said to me: "Look. It's strange that the quadratic form is not anymore positive definite." So I started to reflect: "Let's try to see if *m* is non integer, because it's the only way in which a quadratic form may become positive definite." I did the computation, and the computation gave me a good result. That I sent to *Physics Letters A*.

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

The referee report of *Physics Letters A* was quite interesting. He said: "The construction is completely incomprehensible, but as long as the formula gives the correct result, the result goes in the right direction—the energy is correct and so on—the paper should be published. But the last part, in which the author suggests that you can break the thing in a hierarchical way is not [worth] the paper on which it is written." So I took out that part.

FZ: But it is in the paper. You say: "It is quite likely that an infinite number of order parameters is needed in the correct treatment."

GP: [1:25:10] But you go on. What is the title of the paper?

FZ: This is the first one, no? It's the *Physics Letters A*.

GP: [1:25:31] Number 3?

FZ: It should be the first one. This is the first one. It's the *Physics Letters A,* received April '79. You have the sentence, but maybe you had something more that was cut.

GP: [1:25:52] Sorry. You're right. Maybe I was discussing in a little more details [about] how the construction was done. That was the part that was cut[101].

PC: While we're on this paper. At the end, you thank useful discussions with Cirano, Natoli[102] and Peliti. How were these discussions helpful?

GP: [1:26:33] Well, I met these people. The thing usually that I was doing at that time, when I was working on something, [is that] I was going inside the office of someone else and say: "Look, I am studying this problem. I have not understood all of the things. Do you want to hear what I'm doing?" And they usually said yes. It's clear that speaking with these people [was useful to me]. Also, this was very useful because when one had to explain something to someone else [I had to formulate ideas in a clearer way]. I was discussing with these people. I don't remember [what I discussed with] Natoli at all. [I think that he suggested to me some of the reviews to look on]. It was someone who was in Frascati. I was one or two offices of distance from him. Cirano, I discussed with him in Trieste, and maybe in Paris. I don't remember if I went to Paris in this period or not. Maybe I also spent some time in Paris and I discussed it in Paris. I'm not

[101] **GP:** Quite likely, I was mentioning that the permutation group of zero elements has itself as a subgroup.

[102] Calogero Rino Natoli. See, *e.g.*, D. Sébilleau, K. Hatada and H. Ebert, eds. *Multiple Scattering Theory for Spectroscopies: A Guide to Multiple Scattering Computer Codes—Dedicated to CR Natoli on the Occasion of His 75th Birthday* (Cham, Switzerland: Springer, 2018).

sure because it's possible that I spent a month in Paris in that moment, that year. It's quite likely that I was in Paris for nearly one month, in February, but I'm not 100% sure. However, when I was in Paris [I do not think I discussed so much spin glasses.] There were discussions [with] Claude Itzykson, Jean-Bernard Zuber [and so on]. [We'd discuss] how to generalize the thing for planar diagrams or other type of SU(N) and U(N) things. Also, I was interested in the problem of computing the 1/N corrections to the formula that we had written. It's possible that I spent one month, in February, at Ecole Normale, in '79. It's possible that I was there. But I think that I was mostly worried by the SU(N) theory…

FZ: In June '79, you submitted with Sourlas the work the supersymmetry, so you were also working on that in parallel.

GP: [1:31:10] When was it submitted?

FZ: In June '79.

GP: [1:31:13] Yes. Probably at that time when I was there, I was working with Sourlas on the supersymmetry stuff, because[103]… You see, at that moment, I was starting to think of analytic continuation and so on[104]. Probably the thing had been started and this was probably during my visit in Paris. Also, I was looking to… For example, you know that in zero dimension you have the sum of all planar diagrams. This was the thing that was done with Brézin, Itzykson and Zuber. We wanted to do the 1/N and 1/N^2 computation and so on. I remember, for example, that I went to Saclay. There was one seminar by [Daniel] Bessis, in which he was doing this 1/N correction, writing a formula for orthogonal polynomials that was derived in a very complex way[105]. I remember that I gave a two-line derivation on the formula that finally was published [by Itzykson and Zuber, quoting me] At that time, I did not have time to write the computation. It was written as "Itzykson, Zuber, Parisi, to be published" [or something like that][106]. But

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[103] **GP:** No, I worked on Sourlas on supersymmetry in general the following year.

[104] **GP:** We all knew that in the path integral formulation a negative number of Bosons is a Fermion. This was the explanation friends gave to me in Paris for having trivial critical exponents for the O(N) model for N=-2; -2 Bosons are 1 Fermion, and quadrilinear Fermionic interaction has not effects. The term in the Hamiltonian is zero. More generally a theory with N Bosons and 1 Fermion is equivalent to a theory with n=N-2 Bosons. Nothing deep. At a certain moment we decided to see if the same argument works with the dimensions].

[105] See, *e.g.*, D. Bessis, "A new method in the combinatorics of the topological expansion," *Comm. Math. Phys.* **69**, 147-163 (1979). https://doi.org/10.1007/BF01221445

[106] See, *e.g.*, D. Bessis, C. Itzykson, and J.-B. Zuber, "Quantum field theory techniques in graphical enumeration," *Adv. Appl. Math.* **1**, 109-157 (1980). https://doi.org/10.1016/0196-8858(80)90008-1 "[…] considers for the first time the nonplanar topology by introducing the method of orthogonal polynomials which was further simplified by an unpublished remark due to G. Parisi."

it was never published. Anyhow, I'm certain I was working in Paris on these things. After, when I went back from Trieste, in April, in Rome, I did in Frascati this type of computation. And you had the content of that letter.

Now, there were two things that I was trying to do. First, I had to try to do the computations with magnetic field, a more systematic analysis of one-step [replica symmetry breaking]. After, I started to do two-step, three-step near the critical temperature. That was a lot of complex computation because the algebra is painful when you're doing two steps, three steps, four steps. Also, you get to Q^3 and Q^4. [You have to include some Q^4 terms on the top of the Q^3 term]. I remember that sometimes I was checking that the algebra was correct, that what I was doing was correct, by assigning numerical values to the variables and seeing if the results were correct [with a small pocket calculator]. At that moment—I don't remember exactly when I had the intuition that when you go to $k=\infty$ the interval becomes smaller and smaller and the function becomes a continuous function. What was extremely surprising is that, when you write the formula near the critical point for a continuous function, everything simplifies a lot. The computation becomes much, much simpler when you go to [$k\to\infty$], because instead of having a complex [expression] of 10 variables to take care, you have a function of $q(x)$ and a simple formula that you derive by doing two derivatives: linear plus flat. Therefore, everything simplifies. That was more or less the situation before the summer, when I realized that the whole thing goes to a continuous function. I hoped that in the continuum limit the entropy should be zero.

I think that was the beginning of August of '79, because you have one paper that is sent from August '79[107]. There was a little [problem] at that time, because when I had to write a paper in August, most of the people in Frascati were on vacation. At that time, the [journal] wanted to get the figure in Chinese ink[108]. You had to do semi-transparent paper and on this semi-transparent paper, you had to do the thing in China ink. Of course, you have already something that you can push to get the numbers. You don't have the numbers in [Chinese ink], but you have something that sticks to have numbers on. Normally, the thing that one was doing… Well, of course, there was also no printer with computers. There was no printer at all. Therefore, one was getting the numbers, was taking millimeter paper and had to make a graph with pencil, interpolating by hand or interpolating with some curvilinear [tool][109] to make the drawing. After you gave the

[107] G. Parisi, "Magnetic properties of spin glasses in a new mean field theory," *J. Phys. A* **13**, 1887 (1980). https://doi.org/10.1088/0305-4470/13/3/042 (Submitted August 7, 1979.)
[108] India Ink: https://en.wikipedia.org/wiki/India_ink
[109] Flexible curve: https://en.wikipedia.org/wiki/Flat_spline#Other_curve_drawing_tools

drawing to the man that was a specialist in making drawing. (There was a lot of technical drawing that had to be done for experiments. For engineering things, there was two or three people, I guess, in this institute (at least two) that were doing this job in Frascati. If you want to produce something that should be done by a factory—it should be cut and so on—you have to provide the technical drawings.)

However, I remember that at the end of July, there was no people in Frascati, because all of these people were on vacation. Therefore, I decided to do the technical drawing by myself. If one looks in one of the papers, there is some *sbaffatura* [smudging], *i.e.*, the ink is not neat, not perfect. There is something not perfect, because it was done by myself, but I'm pretty proud that they were at a reasonable technical level to be accepted by the [journal].

FZ: Actually, you have three papers. After the one submitted in April '79 to *Physics Letters A*, you have one in June, one in July and one in August: June 22[110], July 31[111], and August 7[112]. So you were working on all these papers in parallel?

GP: [1:39:25] Yes. I was working in parallel on the different problems, and when I got enough material I wrote it. I had a lot of material but I decided that putting all the things in one paper was not good. Also, because I wanted to [finish] some things, I was working on these things more or less in parallel.

FZ: Were you stressed by some potential competitor? Or was it for pedagogical reasons, to better explain the ideas that you broke up the material?

GP: [1:40:07] Yes. To explain better. I mean, to write a long paper [takes time] The words that I was using in writing the paper, I was typewriting by myself, putting in the formula, leaving the things to the people in the office, and they were typewriting in a professional way, doing some corrections on that and so on. To do this on a long paper altogether was quite painful. It was more easy, from my viewpoint, to finish something, to do the whole typographic stuff for something [short].

There was no competitor. As you know, there were [many others people]. [But] there was not the feeling that there were competitors around [for

[110] G. Parisi, "Infinite number of order parameters for spin-glasses," *Phys. Rev. Lett.* **43**, 1754 (1979). https://doi.org/10.1103/PhysRevLett.43.1754
[111] G. Parisi, "The order parameter for spin glasses: a function on the interval 0-1," *J. Phys. A* **13**, 1101 (1980). https://doi.org/10.1088/0305-4470/13/3/042
[112] See Ref. 107.

the non-integer *m* stuff]. It was just to force me [to write things in a clear way and having well written formulae that I could use]. There was also one other paper in *Philosophical Magazine*, in which I did Monte Carlo [simulations][113]. Therefore, I sent all these papers and after I went on vacation. At the end of ’79, I was finishing the study. It took me some time to [have the final expression]. Well, at that moment I realized that one could have a compact expression for the free energy. (The one that you know everyone call the Parisi formula.) Therefore, I wrote the Parisi formula[114]. Also, I did the check with k=2 two-step replica symmetry breaking. But at that moment, after the summer, most of things were done for spin glasses. Because there was this last paper that was a little longer to do the minimization with five parameters and not with three parameters on MINUIT, but at the end it was working well. And I started to work on other disordered systems, for example, I started to look [at Anderson localization[115]].

FZ: Before we move to that, we have a few other questions on this period. You talked about Monte Carlo simulations. How did you do that?

PC: Was this your first time using Monte Carlo computations?

GP: [1:42:40] Yes.

FZ: What kind of computer were you using?

GP: [1:42:45] It was a CDC [7600]. We had a connection from Frascati to a CDC [7600]. I’m pretty sure that [it was that model]. I think that at that time I was using punched cards. We already had terminals [available] to write programs and so on, [but I was lazy and I did not want to learn new procedures]. I think they were done by punched cards. Anyhow, I did a short program. In Frascati, we had access to a CDC that was in Bologna without any problem. It was a very short Monte Carlo, just to get [some rough numbers]. Because there was nothing particularly difficult I don’t remember that there were [prior] computations in magnetic fields. (Kirkpatrick-Sherrington was in zero magnetic field.) I just did some computation in a magnetic field to verify that the susceptibility was essentially one [or much smaller]. Anyhow, the details of the paper and the

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[113] G. Parisi, "The magnetic properties of the Sherrington-Kirkpatrick model for spin glasses: Theory versus Monte Carlo simulations," *Philo. Mag. B* **41**, 677-680 (1980). https://doi.org/10.1080/13642818008245416 (Submitted October 10, 1979.)

[114] G. Parisi, "A sequence of approximated solutions to the SK model for spin glasses," *J. Phys. A* **13**, L115 (1980). https://doi.org/10.1088/0305-4470/13/4/009 (Submitted January 4, 1980.)

[115] GP: G. Parisi, " Some remarks on the electronic states in disordered materials,” *J. Phys. A* **14**, 735 (1981). https://doi.org/10.1088/0305-4470/14/3/020

paper I have forgotten. Most of the things were solved for the summer. I was convinced that the paper on the Parisi formula was written earlier, but I think it was submitted at the beginning of ’80.

FZ: The one entitled: “Sequence of approximated solutions to the SK model”?

GP: [1:44:30] No. The short one with the Parisi formula with the integro-differential equation.

FZ: Then it must be: **“**Mean-field theory for spin glasses” in *Physics Reports*? You have the one in June, “Infinite number of order parameters”, then July and August, you have the two papers on the order parameter.

GP: [1:44:58] Maybe this one.

FZ: “A sequence of approximate solutions…” Yes. That one was submitted in January 1980.

GP: [1:45:08] I think that most of the material was probably done before, but it took some time, because I was starting to work on the problem of random potential.

FZ: One last thing on this. You had a paper in collaboration with Vannimenus and Toulouse[116]. This is the first collaborative paper you wrote on this topic.

GP: [1:45:38] When was the paper with Vannimenus?

FZ: In October 1980.

GP: [1:45:45] Probably I spent some time in the beginning of 1980 in Paris[117].

FZ: This is the first paper where you collaborated with someone else on this SK model. How did it happen?

GP: [1:46:23] Yes. Well, when I was at École Normale we were going to eat together with other people. [We] started speaking. We had a lot of discussions. I had a lot of discussions in Paris with people around. (I had some problem to localize.)

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[116] G. Parisi and G. Toulouse, "A Simple hypothesis for the spin glass phase of the infinite-ranged SK model," *J. Phys. Lett.* **41**, 361-364 (1980). https://doi.org/10.1051/jphyslet:019800041015036100; J. Vannimenus, G. Toulouse and G. Parisi, "Study of a simple hypothesis for the mean-field theory of spin-glasses," *J. Phys.* **42**, 565-571 (1981). https://doi.org/10.1051/jphys:01981004204056500

[117] **GP:** I spent one week in March. The first paper is with Toulouse, and was submitted in April ’80.

I remember that Édouard came to Rome for one week in December ’79, and we did the paper on the tail of the density of [localized] states in the random potential[118].

At the same time, I was working on [localization in general]. I remember that I was working on that problem and my impression was, I arrived to the conclusion that there was this breaking of the O(*N*) theory, that there was something like [spontaneous symmetry breaking]. In reality, the group was not O(N) [as for the density of states where] you have a real field. If you want to study localization, you have to compute [the average of modulus squared]. If you want to study the density of states, it is sufficient to have O(*N*) theory. If you want to study localization, you have to put two complex fields—or if you want two real fields—and the symmetry group is O(*N*)xO(*N*), [which becomes O(2N) in the interesting limit. In reality, you must] have the relativistic O(*N*,*N*) group. I remember that I did this computation on these things. However, I was very late to write it. I think it took me one year to write it down[119]. In the meanwhile, this thing was also done by Wegner along that direction[120].

I definitely thought that with the paper in August [‘79], the last paper, that after it was published in January ’80[121], but most of the things were written before, it was only a problem of writing things in details. The spin glass problem was more or less solved. I mean the part that I was interested [in]: to find the correct use of replica. So I started being interested from one side on other systems, like the Anderson problem, and on the other side I was strongly interested to lattice gauge theory.

PC: Before we move on, can you give us a feeling of what was the reaction to that first series of papers? When did you start getting feedback from the community?

GP: [1:49:52] I think that most of the people that I was speaking [to] were very… Well, there were some people that said that the thing is too crazy. Other people were quite interested. Certainly, I remember that in ’80 there

[118] E. Brézin and G. Parisi, "Exponential tail of the electronic density of levels in a random potential," *J. Phys. C* **13**, L307 (1980). https://doi.org/10.1088/0022-3719/13/12/005

[119] **GP:** I’m not sure, but I think it was written in January ’80, and that I started to work on it after the summer of ‘79.

[120] **GP:** To be clear, Wegner was considering the “wrong” case of O(2*N*). I was the first to realize that the group was the non-compact O(*N*,*N*) group. However, this was forgotten. See: F. Wegner, “The mobility edge problem: Continuous symmetry and a conjecture,” *Z. Phys. B* **35**, 207–210 (1979). https://doi.org/10.1007/BF01319839

[121] **GP:** The “last” paper was actually submitted in January ’80.

was a winter school in Les Houches[122], and during the winter school in Les Houches I made an after dinner talk [on February 22][123]. I remember that I had the impression that most of the people present there were convinced by these things. [There was a quite strong applause after the talk.] I remember that Leo Kadanoff was there, and I remember that he strongly congratulated [me] with the things.

On the other hand, the other thing that I was trying to understand better—because there was a paper by Rebbi and others, maybe '78-'7[9][124], on Monte Carlo in lattice gauge theory—was how to do Monte Carlo in lattice gauge theory, and how to do Monte Carlo at all. So I remember that I started to do [computations for the non-linear sigma term also] with Guido Martinelli and [Petronzio][125]. That was an analytic computation [that I did by myself]. I was interested in all problems of these types. We started to do some Monte Carlo for the O(N) field theory in two dimensions. And there was the problem of trying to understand how to put fermions in the theory. There were two things that we were doing. One thing that was done by us—in the thesis of Enzo Marinari—used the technique of pseudofermions[126]. That was to use some bosons that played the role of fermions, but using the Langevin equation with the wrong sign. The other thing that we started to do in '81 was to do computations with fermions in the *quenched approximation*[127]. (I gave myself the name because you take a [zero] number of fermions and the gauge fields are frozen. If you think of spin glasses at zero magnetic field, you have a gauge symmetry and the coupling are the fields of the Z_2 symmetry. The magnetic fields are the spins and in the quenched case the gauge fields are frozen and you have the magnetic field that evolves in presence of the quenched coupling.) The idea is the same [as what] you have in gauge theory. You have the real gauge field that is quenched and you have the fermion field that evolves in presence of…

[122] *Common trends in particle and condensed matter physics*, É. Brézin, J.-L. Gervais, G. Toulouse, February 1980, Les Houches, France. Proceedings in *Phys. Rep.* **67**(1), (1980). https://www.sciencedirect.com/journal/physics-reports/vol/67/issue/1 (Accessed March 27, 2022.)
[123] G. Parisi, "Mean field theory for spin glasses," *Phys. Rep.* **67**, 25-28 (1980). https://doi.org/10.1016/0370-1573(80)90075-7
[124] M. Creutz, L. Jacobs and C. Rebbi, "Experiments with a gauge-invariant Ising system," *Phys. Rev. Lett.* **42**, 1390 (1979). https://doi.org/10.1103/PhysRevLett.42.1390; "Monte Carlo study of Abelian lattice gauge theories," *Phys. Rev. D* **20**, 1915 (1979). https://doi.org/10.1103/PhysRevD.20.1915
[125] G. Martinelli, G. Parisi and R. Petronzio, "Monte Carlo simulations for the two-dimensional O(3) non-linear sigma model," *Phys. Lett. B* **100**, 485-488 (1981). https://doi.org/10.1016/0370-2693(81)90610-9
[126] F. Fucito, E. Marinari, G. Parisi and C. Rebbi, "A proposal for Monte Carlo simulations of fermionic systems," *Nucl. Phys. B* **180**, 369-377 (1981) https://doi.org/10.1016/0550-3213(81)90055-9
[127] E. Marinari, G. Parisi and C. Rebbi, "Computer estimates of meson masses in SU (2) lattice gauge theory," *Phys. Rev. Lett.* **47**, 1795 (1981). https://doi.org/10.1103/PhysRevLett.47.1795

FZ: So these went from spin glass to QCD, not the other way?

GP: [1:53:13] The name, it went from spin glass to QCD, but it was not so clear how much spin glass [was in it]. But certainly the name went from spin glass to QCD.

FZ: So the [lattice] QCD problem was not a motivation for you to study spin glasses. This came after?

GP: [1:53:32] No. I was interested in QCD. I understood that there was a very nice paper by Rebbi, Creutz and [Jacobs], in which they started to do Monte Carlo for lattice gauge theory, but [they] had a lot of problems to do the correct computation of the masses, and to do the correct computation of masses… I knew very well from Symanzik and others the difference between Euclidean and Minkowski[128] field theories, [and] the various theorems to connect one to the other one. There was a problem, first of all, to do the correct computation of masses. The computation of masses was delicate because you had the propagators, you had to control the correlations in the regions where they are quite small [i.e., the region where they decay exponentially]. Also, there was some representation that was written by Symanzik, which is in my book of statistical mechanics[129]. [In chapter 16,] "Particle field duality", there is this thing in which you can write something with the propagator in a gas of closed trajectories, which have a weight proportional to *n*. These things were essentially done by Symanzik in '69[130]. If you look to this type [of correlations], it's something that if you want to do to the ϕ^4 theory to compute the propagator. In order not to use the field, you can write the field theory as a one-line propagator–one line that goes from one point to another point—in a background of loops, and each loop has a weight that is proportional to *n*. Therefore, if you put n=0 you remain with a self-avoiding walk. For this self-avoiding walk, the problem was very clear. I think that the idea of quenched was more coming from the self-avoiding walk; to say that I want to do a self-avoiding walk and [that] this self-avoiding walk is something like a ϕ^4 theory when you put n=0. Therefore, the idea of quenched [applied to] QCD was coming essentially from the self-avoiding walk, [about which] I was very familiar because of all the things of De Gennes [and also from Anderson localization]. I discussed a lot of self-avoiding walk also with des

[128] Minkowski space: https://en.wikipedia.org/wiki/Minkowski_space

[129] Giorgio Parisi, *Statistical Field Theory* (Redwood City, CA: Addison-Wesley, 1988).

[130] K. Symanzik, "Euclidean Field Theory" in: *Local Quantum Theory*, Res Jost, ed. (New York: Academic Press, 1969), 152-227. Proceedings of the *International School of Physics "Enrico Fermi"* XLV, August 12-24, 1968, Varena on Lake Como, Villa Monastero, Italy.

Cloizeaux[131], in Saclay, who was doing the other way around, without having the *n* theory. I knew the stuff of Symanzik, so the idea of the quenched approximation came probably from the old things that were solved. At the moment I gave it a name, the name was taken from spin glasses, because the idea just corresponded.

You see, [at that] moment there was the work that was done with Marinari, [for whom] I was the thesis advisor, on pseudofermions. We did with Marinari and Rebbi, I think, the computation for two-dimensional pseudofermions. At the same time (in '81), we did the computation for quenched fermions with SU(2) Marinari[132], and with SU(3) we did with Hamber[133]. I met [Herbert] Hamber in '81[134], when I went to a school in Santa Barbara. He had a program already working for lattice gauge theory for SU(3)—we had the program for SU(2), but why not SU(3)—so I said: "Look, we can add for SU(3) the fermions to your program." After, I went to Brookhaven for one week and we were working also from home with Hamber. It was not easy work because I think that we had a very slow connection to the lab. I think it was 300 bauds or something like that. We were using vi[135] to make some… Anyhow, in the end we succeeded to finish the paper. At that moment, I was starting to do other things, for example, the things with Nicolas that you mentioned in stochastics differential equations. Therefore, in '80-'81 I was working on many other problems connected to lattice gauge theory and …

FZ: In the last of the '79-'80 series of papers[136], at the end you write: "I believe that we are on the right track and that we have found the solution for the Sherrington-Kirkpatrick model." And then you write: "The computations of the fluctuations induced corrections, of the Goldstone modes and of the lower critical dimension are only technical problems which may be solved with a serious effort." It looks like you had the feeling that the problem was kind of solved and it was just a matter of technical efforts.

GP: [2:00:42] The part of the technicality in the end were very, very complex. Indeed, it took a lot of time. That was a magnificent work that was done by

[131] See, *e.g.*, Jacques Des Cloizeaux and Gérard Jannink, *Les polymères en solution: leur modélisation et leur structure* (Les Ulis, France: Les Éditions de Physique, 1987).
[132] E. Marinari, G. Parisi, and C. Rebbi. "Computer estimates of meson masses in SU (2) lattice gauge theory," *Phys. Rev. Lett.* **47**, 1795 (1981). https://doi.org/10.1103/PhysRevLett.47.1795
[133] H. Hamber, and G. Parisi. "Numerical estimates of hadronic masses in a pure SU (3) gauge theory," *Phys. Rev. Lett.* **47**, 1792 (1981). https://doi.org/10.1103/PhysRevLett.47.1792
[134] Herbert W. Hamber: https://academictree.org/physics/peopleinfo.php?pid=448876 (Accessed March 28, 2021.)
[135] vi: https://en.wikipedia.org/wiki/Vi
[136] See Ref. 113.

Kondor and later by Temesvári[137], first of checking that there was stability. That was not evident.

PC: Isn't that the de Almeida-Thouless-Kosterlitz computation from 1980[138]?

GP: [2:01:21] De Almeida-Thouless did the one-step computation[139] [and] maybe also did the large (or all) *k* computation. I remember that someone proved that near the critical temperature the negative modes were proportional to 1/(*k*+1)^2[140]. Someone proved this thing, but I don't remember who.

FZ: There is this paper by Thouless, de Almeida and Kosterlitz, where they study the stability of your solution near the transition and they show that it's stable.

GP: [2:02:24] Exactly. However, there was the problem… They have that 1/(*k*+1)^2. They [obtained] a formula saying that the unstable mode is proportional to 1/(2*k*+1)^2, but after there was the problem to prove it at all temperatures[141]. Thouless, de Almeida and Kosterlitz did the computation near T_c at the first non-zero order. [The stability at all temperatures] was done by De Dominicis and Kondor, and later on there was the problem of computing the propagator[142]. That was very complex.

FZ: Why didn't you work on all these things immediately after the '79 work?

GP: [2:03:06] As you see from my sentence at the end, I thought that it was possible to be done. But, on the other hand, you remember that the work on spin glass was a diversion from the work on lattice gauge theory. The mission was completed, the thing understood, I could get back to high energy physics. The period [starting] in '81, I started to do work on lattice

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[137] C. De Dominicis and I. Kondor, "Eigenvalues of the stability matrix for Parisi solution of the long-range spin-glass," *Phys. Rev. B* **27**, 606 (1983). https://doi.org/10.1103/PhysRevB.27.606.

[138] D. J. Thouless, J. R. L. De Almeida and J. M. Kosterlitz, "Stability and susceptibility in Parisi's solution of a spin glass model," *J. Phys. C* **13**, 3271 (1980). https://doi.org/10.1088/0022-3719/13/17/017

[139] J. R. L. de Almeida and D. J. Thouless, "Stability of the Sherrington-Kirkpatrick solution of a spin glass model," *J. Phys. A* **11**, 983 (1978). https://doi.org/10.1088/0305-4470/11/5/028

[140] **GP:** In de Almeida *et al.*, it is for *k*=1. The 1/(*k*+1)^2 formula should be in one of Kondor and De Dominicis papers, where they also compute the $k \to \infty$ limit.

[141] **GP:** My memory of this point is not good. I am not sure if there is a paper with the 1/(2*k*+1)^2 formula. I believe that there is a paper with 1/9 for *k*=1, but I do not remember the authors. I knew from my computation that the corrections to the free energy were 1/(2*k*+1)^4. When I saw 1/9 for *k*=1, I immediately interpreted it as 1/(2*k*+1)^2 , maybe no one did the explicit computation for higher values of *k*.

[142] C. De Dominicis and I. Kondor, "Gaussian propagators for the Ising spin glass below Tc," *J. Phys. Lett.* **46**, 1037-1043 (1985). https://doi.org/10.1051/jphyslet:0198500460220103700

gauge theory. Also, the other thing that took me a lot of time at that point is that Lorenza was born in August of ’80.

FZ: Exactly. I was looking at the birthdate of Lorenza. Was this at the same time?

GP: [2:04:05] Lorenza was born in the August of ’80 and Leonardo was born in January ’82.

FZ: Did they have an impact on your work?

GP: [2:04:15] They had some impact.

PC: We both have two children. We understand.

GP: [2:04:25] I remember that one night with Leonardo, I think, for some problem woke me up in the night and I succeeded to have him sleep. However, I started to work [on a new problem] at the same time. It certainly had some impact also because I was quite present with [childrearing, not only] because my wife was working.

Anyhow, what happened is that I was interested in lattice gauge theory, in the problem of Monte Carlo, in the problem of computing the connections between the renormalized coupling constant, the coupling constant on the lattice on these things, and in how to do Monte Carlo with fermions. Therefore, we started to do Monte Carlo with fermions and there was a long period, from ’81 to ’90, during which we did different sorts of Monte Carlo with fermions, first on a simple computer and after on different things, on a Cray, and we built up and so on.

PC: Before we move on to this, there’s one conference in Rome, in 1981, on disordered systems and localization[143], where you presented.

GP: [2:05:56] Yes. I presented these things on disorder[144]. And there’s a nice picture floating around of me, Sherrington, Toulouse[145] and maybe Peliti,

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

[144] G. Parisi, “Mean field theory for spin glasses,” in: C. Castellani, C. Di Castro and L. Peliti, eds. *Disordered Systems and Localization*, *Lecture Notes in Physics* **149** (Berlin: Springer, 1981). https://doi.org/10.1007/BFb0012548

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

on the front. I presented these things. And I did a second paper with Toulouse and Vannimenus[146], I think.

PC: In the proceedings of that conference, you mentioned that Nicola Cabibbo had studied the random energy model before Bernard Derrida[147]. Do you know in what context this work had taken place?

GP: [2:06:40] Yes. I can explain. When I was doing this work with spin glasses, Nicola was interested in what people were doing. Also, I was interested in speaking with people. So I was speaking with Nicola about the problem of spin glasses and so on. Different story. One day, I came to Nicola and Nicola [said]: "Ah! I have done a blitz solution of this model of spin glasses. You suppose that the energies are not given by the standard Hamiltonian, they are [instead] Gaussian distributed and you take the same variance as the original one. If you do the computation, you have a transition. At a certain temperature the system freezes, and the energy becomes zero and the entropy becomes zero." It was stupid of me. "Well," I said to Nicola, "it's too simple."

PC: So this would have been in '79 or 1980?

GP: [2:08:06] It was in the spring of '79. I said: "No, Nicola. I think it's too simple, because you miss the other phase transition." Anyways, I said that the model was too simple. It's clear that the very interest of the model was that it was really simple, and he showed how you could get this phase transition from the high-energy phase, the entropy catastrophe, that there was no state. I [thought] that it is certainly important, and that it [was] very interesting, but I [thought] that it is too simple. The problem that I [was] looking at [was] more complex. Certainly, I regret I never suggested to Nicola to write down the finding. Of course, the work of Derrida was much more complex than that, because he was estimating everything with greater details, corrections and so on. But in a nutshell the idea that you have an exponentially large number of states, and at a certain moment when you go [toward] zero temperature, you stop to find these states. If you go on with the analytic formula for high temperature you enter the negative entropy regime, but at just the moment when you enter the negative entropy regime, you have a phase transition that [corresponds to what] that was done by Nicola. But I never stressed to him that the thing

[146] See Ref. 116.

[147] 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

was important, [that] it should be published. I thought it was more or less a curiosity. I was more interested to solve the algebraic problem.

The problem is that I was not so much interested with the physics. If you see [the papers], there's no mention of the physical characteristics of spin glasses. I had no idea of the physics. Of course, I looked into the final formula, and I reflected a lot of time on the final formula, but I was not able to understand its meaning. Of course, there was this *x* that was from 0 to 1, but one of the things that I was thinking of [is that] *x* may be a percentage of spin. Say you take a system which is small and you increase the size of the system. The *x*=0 is trivial, it's Gaussian, and it tells you how it evolves with *x*, by increasing the percentage of spin. But it was not clear at all to me how can we do [such a computation]. Indeed, I think that one cannot derive this thing to interpret *x*. Probably, the idea was that you should at least have some temperature, so *b*, with *bx*, but you only take a fraction *x* of the spins, you change… But I never succeeded in interpreting the formula in that way. Maybe it can be interpreted, but I don't know. Of course, I was repeating what I did, discussing with people. There was Vannimenus, Toulouse and so on. But presenting things in a different context, it was clear that the result was in perfect agreement with Monte Carlo, so most people believed it was correct. But I was not anymore working on it, because I had a lot of [other] things.

In '82, I went to Les Houches. There…

FZ: Giorgio, maybe it's a good point to take a break.

GP: [2:12:43] Yes. Fine.

PAUSE

PC: When we stopped, we were talking about 1982.

GP: [2:15:05] 1982. What I said is that I went to Les Houches[148], and I had to speak of disordered systems. I presented the things on spin glasses[149]. After, I went to Paris for two months, where a lot of people that were in Bures-sur-Yvette were interested in spin glasses: De Dominicis and Young

[148] Les Houches Session XXXIX August 2-September 10, 1982. Proceedings: *Recent advances in field theory and statistical mechanics*, Jean-Bernard Zuber and Raymond Stora, eds. (Amsterdam: North-Holland, 1984).

[149] G. Parisi, "An Introduction to the physics of amorphous systems," In: *Recent advances in field theory and statistical mechanics*, Jean-Bernard Zuber and Raymond Stora, eds. (Amsterdam: North-Holland, 1984), 473-523.

were working on the interpretation of spin glasses[150]. I remember that [Eliott] Lieb was there and I had a long discussion with [him][151]. Lieb is interesting, because I remember that the first version of the TAP—Thouless Anderson and Palmer—paper was signed also by Lieb[152].

FZ: The preprint?

GP: [2:16:10] The preprint. [I was told that] what happened is that they had written this thing after discussing with Lieb, [but] without asking Lieb. Lieb said that he had not contributed enough and asked that his name be removed. Probably you can find it. So Lieb was interested in spin glasses. I remember we had a lot of discussion with people in Paris about spin glasses. I think that someone—maybe Itzykson, but I'm not sure—noticed that the integral of $q(x)$ to the power k is equal to the sum over different sites of the square of the k-spin correlations. You take $\int q(x)^3 \mathrm{d}x = \langle \sigma_i \sigma_k \sigma_l \rangle^2$.

That was a key point, because I studied also for high-energy physics all the discussion that there were about the decomposition in pure states. There was a way to discuss—generally speaking—symmetry breaking:t he symmetric vacuum is not clustering [i.e. the correlation functions do not satisfy the cluster decomposition property], that you break it into [the sum of] clustering states. You have a different way. Consider the vacuum as a linear functional. Given a linear functional, you can define a pure state [as a state] that cannot be decomposed as a sum of convex combinations, then you can decompose each state in term of pure states. I was quite familiar of the possibility from my university courses of Algebra. Because people that were doing mathematics on [spontaneous symmetry] breaking in particle physics—or generally speaking of symmetry—were doing all complex constructions that were not necessary for the standard things. However, I reflected and I said: "Well. This is something that cannot be, and it means that the connected correlation functions do not go to zero at infinity. Otherwise, it would not break [the symmetry]." Therefore, I noticed that if you write the state as a sum of many states, you can just write a formula interpreting $x(q)$—the inverse function of $q(x)$—as the probability distribution for the overlap of the function. Therefore, that was [quite clear at the end]. Something similar was done by Young and De Dominicis, but there were some slight differences, because we were

[150] C. De Dominicis and A. P. Young, "Weighted averages and order parameters for the infinite range Ising spin glass," *J. Phys. A* **16**, 2063 (1983). https://doi.org/10.1088/0305-4470/16/9/028
[151] Eliott Lieb: https://en.wikipedia.org/wiki/Elliott_H._Lieb
[152] D. J. Thouless, P. W. Anderson and R. G. Palmer, "Solution of 'solvable model of a spin glass'," *Philo. Mag.* **35**, 593-601 (1977). https://doi.org/10.1080/14786437708235992. On p. 595, for instance, an unpublished report by Thouless, Anderson, Lieb and Palmer is mentioned.

discussing this with the second moment and so on. They were not discussing the things in a clear way, in an explicit way, [so the connection with my solution was not clear]. Anyhow, it was something that was in the air, certainly.

I remember I wrote a paper to *Physical Review Letters*[153], and I remember that there were two referee reports that were quite different. One that was saying that I was not aware of some of the recent results of spin glasses, because I was not quoting them. There was certainly support, and there was some discussion about the whole thing, the meaning and so on. The second one was a four-line report saying: "This paper [unveils] the mystery. The mystery of spin glasses is removed. Publish." My feeling was that the four-line thing was by Anderson, and the other was by some of the authors that were not quoted. I answered to *Physical Review Letters*: "Well. I think that you should put a law that a referee cannot ask to be quoted." Anyhow, I added the [citation] and made some remarks. Finally, I added: "Well, you have so different referee reports. It's clear that the only thing that you have [to do] is to weigh the referee with the prestige it has and take the viewpoint of the one that has the better prestige." The paper was published and they said that the point that the referee should not ask to be quoted was well taken.

That was done. I was still in the middle of the time doing a lot of problems in lattice gauge theory. When the summer [of '83] came, I remember that I started again to look at the problem. I was curious to see if one could have some idea about other [probability distributions]. The moment of $P(q)$ was clear, but after you can compute with the replica method which was, for example, the probability distribution [for three replicas, $P(q_{12},q_{13})$]. Now, the thing that one can get using the fact that the sum of the elements of the matrix q is constant—it does not depend on the line, one line is a permutation of the other one—I remember that I derived the first or one of the first relations which is [part of] the Ghirlanda-Guerra relations[154]: that the average value of the product of this P is one half the delta function of the P with the delta function of the q, and after the disconnected term with the product of the P, i.e., $\overline{P_J(q_{1,2})P_J(q_{2,3})} = \frac{1}{2}P\big(q_{1,2}\big)P\big(q_{2,3}\big) + \frac{1}{2}P\big(q_{1,2}\big)\delta(q_{1,2} - q_{2,3})$. I think that I was in some train that was going from Roma to Milan or Milan to Roma—that took six hours at that time. I remember that when I arrived at the final formula, which was one of the

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

[154] S. Ghirlanda and F. Guerra, "General properties of overlap probability distributions in disordered spin systems. Towards Parisi ultrametricity," *J. Phys. A* **31**, 9149 (1998). https://doi.org/10.1088/0305-4470/31/46/006 **GP:** When you realize that you have assumed this relation, you are quite surprised.

simplest Ghirlanda-Guerra identity, my first reaction [was]: "That is completely nonsense. The whole approach should be wrong." After thinking again… I remember that there was a long exchange of letters—I don't known exactly with whom. Certainly with people in Paris…

After, we started to work with the people in Paris: Nicolas, Toulouse, Marc and Virasoro[155]. Virasoro[156] was quite a strange story. When he was in Rome, we never talked about spin glasses [only string and gauge theories], but when he was in Paris, he became interested in spin glass theory. Therefore, we discovered the whole story. We discovered ultrametricity. After I did a very long and complex work to pin down all the probability distributions and so on. I remember that it was essentially Toulouse that was stressing [to me] that ultrametricity was implying a tree-like classification of states, and therefore a taxonomy; that it could be put on a taxonomic tree. This pointed to the fact that you have an infinite number of states, that you have a taxonomic classification. That was the thing that made some connection with complexity. A system [that has an] infinite [number of] states can be put on an infinite tree. The tree has an infinite number bifurcations at each level. That was clearly revealing the complexity of the whole object. That was not the reason for which I started. Maybe Anderson had some idea that the thing was complex…

FZ: What is the connection with the Ghirlanda-Guerra identities with what you did on the train?

GP: [2:28:07] [What I did on the train is one of the simplest Ghirlanda Guerra identities.] The Ghirlanda-Guerra equations [are what you need] if you want to understand the statistics of the *P*. The first thing [to compute] was written in [my PRL] paper. It is easy to compute is $P(q)^k$. The second thing that you can compute [is] the average of two *P* of different arguments. (They can have one replica in common or zero replica in common.) These overlaps come from the Ghirlanda-Guerra identity, but it was not [obtained] using the Ghirlanda-Guerra [identity]. It was just an explicit computation [using replica theory]. All the Ghirlanda-Guerra can be derived from the assumption that the elements of one line of the matrix *Q* are a permutation of those of another line]. The result was quite strange. At the beginning, I thought that the result was so strange that it was

[155] 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

[156] 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

nonsense. Only after some time, was it clear that the thing made sense. Especially, what it tells us is that because of ultrametricity—as was stressed by Toulouse—one can put the things on a taxonomic tree. It's an example of a property of a typical complex system and so on. So we wrote the two papers—a letter and a long version. [For that,] the contribution of the others was very important.

After, the thing that was crucial, however—in order to go on with the theory—was that Marc came to Rome. Marc was in Rome from '84 to '86, for two years. He started to work with Miguel and myself. I was working with other things, with APE[157] and so on, but I was also discussing with them. Marc and Miguel made a long effort to put the whole approach in terms of cavity. So we wrote two papers with the cavity[158]. The second is a simplification of the first, because the first was more complex. [It was a more complex construction, and under this form it was possible to do a cavity computation]. Finally, it was clear that this work was mostly done by themselves. [My contribution was minor.] They also did together a very nice paper "The Microstructure ultrametricity"[159].

Now, the things were very clear. In order to compute the free energy you have to assume a certain probability distribution of the overlap. Therefore, you have to say that you have infinitely many states, you have a probability distribution of the weights, you have a probability distribution of the overlaps—which are the values of the overlaps, how the overlap are correlated to weight of the state and all this type of things. Once you have these probability distributions over these objects, from these probability distributions, you can compute the free energy [using the cavity approach].

What the replica method was providing [was an explicit expression for all those probabilities]. Also, one particular example of this probability distribution [had] all the properties that were described [and] it was ultrametric and there were random free energies at each level of this thing. The second paper was crucial, because it was a computation of the free energy where there was no replica at the end. There was only probability theory. Therefore, that was something that could be well understood, and

[157] APE: https://en.wikipedia.org/wiki/APE100

[158] **GP:** The first paper was not on cavity, only on the probability distributions of the weight of states. The first is M. Mézard, G. Parisi and M. A. Virasoro, "Random free energies in spin glasses," *J. Phys. Lettres* **46**, 217-222 (1985). https://doi.org/10.1051/jphyslet:01985004606021700 ; the second is M. Mézard, G. Parisi and M. A. Virasoro, "SK Model: The Replica Solution without Replicas," *Europhys. Lett.* **1**, 77 (1986). https://doi.org/10.1209/0295-5075/1/2/006

[159] M. Mézard and M. A. Virasoro, "The microstructure of ultrametricity," *J. Phys.* **46**, 1293-1307 (1985). https://doi.org/10.1051/jphys:019850046080129300

can be followed by mathematicians, while for the replicas it's not clear—up to the present day—how to formalize the method. It could be done by mathematicians, in principle. What happened is that there was a lot… But maybe we stop here and about the mathematicians we'll speak later?

PC: Yes. We'll get back to this point. What was the reaction to those papers? As you said, this series of papers was really important to obtain a physical understanding of the solution.

GP: [2:33:35] I think that everybody—the people that I was speaking with—believed that it was a clear physical understanding. Anderson was extremely interested in all these things, because he had this idea that spin glasses could be the starting point of other optimization problems. I reread recently the paper—you've seen the seven-part column in *Physics Today*[160]—and there's one that says that this can be, and [that] spin glasses are a cornucopia for many other things. I think that was taken after Marc and maybe Sherrington. (Marc, certainly, in Cargèse '14 spoke about the spin glass cornucopia. You were present, I guess[161].) I think that this was very well received as far as the Sherrington-Kirkpatrick [model] was concerned.

FZ: Did you have direct interactions with Anderson? Letter or other exchanges?

GP: [2:35:10] He went in '89 or '87 in a Cargèse school[162], but most of the interactions—maybe I met him at some place, but not too much. Maybe what happened is that he was spending nearly every year some of his time in Paris. That was quite frequent. He had a connection with other people, and he was clearly very interested in the problem.

[160] P. W. Anderson, "Spin glass I: A scaling law rescued," *Phys. Today* **41**(1), 9-11 (1988). https://doi.org/10.1063/1.2811268; "Spin Glass II: Is There a Phase Transition?," *Phys. Today* **41**(3), 9 (1988). https://doi.org/10.1063/1.2811336; "Spin glass III: theory raises its head," *Phys. Today* **41**(6), 9 (1988). https://doi.org/10.1063/1.2811440; "Spin glass IV: Glimmerings of trouble," *Phys. Today* **41**(9), 9-11 (1988). https://doi.org/10.1063/1.881135; "Spin glass V: Real power brought to bear," *Phys. Today* **42**(7), 9 (1989). https://doi.org/10.1063/1.2811073; "Spin glass VI: Spin glass as cornucopia," *Phys. Today* **42**(9), 9 (1989). https://doi.org/10.1063/1.2811137; "Spin Glass VII: Spin Glass as Paradigm," *Phys. Today* **43**(3), 9 (1990). https://doi.org/10.1063/1.2810479

[161] *Spin glasses: An old tool for new problems*, Florent Krzakala, Giorgio Parisi, Federico Ricci-Tersenghi and Lenka Zdeborova, Institut d'études scientifiques de Cargèse, Cargèse, France, August 25-September 6, 2014. http://www.lps.ens.fr/~krzakala/WEBSITE_Cargese/home.htm (Accessed March 31, 2022.)

[162] Both Parisi and Anderson were at: *Common trends in statistical physics and field theory*, C. Itzykson *et al.*, Cargèse Advanced Research Workshop, Cargèse, France, 23 May - 4 Jun 1988. Proceedings: *Phys. Rep.* **184** (2-4). https://www.sciencedirect.com/journal/physics-reports/vol/184/issue/2 (Accessed April 1, 2022.)

FZ: One more thing on this: When did you first meet Marc, and how?

GP: [2:36:22] Marc came in '83 or '84. The work with the French people was done by phone, by letter and so on. Marc came to Rome—you can check when—for one week, and we wrote a paper[163]. The problem was the following. The magnetizations, and how the magnetization projects on the eigenvalue of [the matrix of couplings] *J*, so the spectral projection of the magnetization on the eigenvalues of *J*. We wrote a paper together, which is not in the book. I guess that that paper was written somewhat after the visit of Marc in Rome. I have forgotten exactly. Also, it's quite likely that I met Marc a few times in Paris, because he was working with [Claude] Bouchiat[164] in high-energy physics. He did graduate with Bouchiat[165].

PC: In 1985-1986, Gérard Toulouse and you were both invited to give the Loeb Lectures at Harvard[166]. How did that come about?

GP: [2:38:24] They asked me to just describe this construction, that was done in four or five lectures, I guess. A lot of people, mathematicians and so on, were interested. I remember that I was living on the campus, in the Faculty Club, but apart from that there was not so much interaction…

PC: With the physicists, Halperin[167], Nelson[168], Martin[169]?

GP: [Also with mathematicians, like Jaffe[170].] I was speaking with people. There was local interest, but apart from local interest to learn what I was doing, there was not any productive interaction.

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[163] M. Mézard and G. Parisi, "Self-averaging correlation functions in the mean field theory of spin glasses," *J. Phys. Lett.* **45**, 707-712 (1984). https://doi.org/10.1051/jphyslet:019840045014070700
[164] Claude Bouchiat: https://en.wikipedia.org/wiki/Claude_Bouchiat
[165] Marc Mézard, *Test de QCD et observables inclusives dans la diffusion inélastique de neutrinos*, Thèse de 3e cycle, Université de Paris 6 (1980). https://www.sudoc.fr/042326508; *Etude de la théorie de champ moyen des verres de spin et de son interprétation physique*, Thèse de 3e cycle, Université de Paris 6 (1984). https://www.sudoc.fr/174095813; 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; C. Bouchiat and M. Mézard, "Parity violation in metals," *J. Phys.* **45**, 1583-1598 (1984). https://doi.org/10.1051/jphys:0198400450100158300
[166] "Loeb and Lee Lectures Archive: 1953-1990," Harvard University, Department of Physics https://www.physics.harvard.edu/loeblee3 (Accessed April 1, 2022.)
[167] 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/10.34847/nkl.7ac326ng
[168] David R. Nelson: https://en.wikipedia.org/wiki/David_Robert_Nelson
[169] Paul C. Martin: https://history.aip.org/phn/11606015.html (Accessed April 1, 2022.)
[170] Arthur Jaffe: https://en.wikipedia.org/wiki/Arthur_Jaffe

PC: You worked briefly on neural networks and optimization as well around that time. What interested you to those ideas? And then why did you leave these ideas?

GP: [2:39:35] There are two completely different histories. On neural networks, there was the Hopfield model[171], and [then] there was a solution of the Hopfield model. The computation was done by Daniel [Amit], Gutfreund and Sompolinsky[172]. Therefore, this was an interesting subject about which I had a few ideas, [and] I wrote into a short paper about neural networks[173]. But after I stopped, because I had the feeling that there were so many people that were interested in neural networks. I started to try to understand if the idea of the neural network could be extended to immunology. There was a lot of discussions that you have something like a neural network in immunology. I wrote also one or two papers on the subject, but at the end there was not really convincing evidence of a functional immunological network[174]. If you have one antibody that interacts with one antibody, there are two possibilities. This is something that just follows from chemistry, but it's not important in that case. The other possibility is that this has a functional role. There was somebody that was pushing for a functional role, but the functional role of these antibody-antibody interactions was never clear. I worked for two, three years on that and I stopped.

On the other hand, the optimization problem was something natural. Also, Anderson and Fu were speaking about optimization problem and deriving other things[175]. I don't remember who had the idea—I don't think myself—to look for matching. From the beginning, I think that there were discussions between Miguel and Marc. At the end, for some reason, I worked only on it with Marc, but I can't remember the origin of the interest

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

[172] D. J. Amit, H. Gutfreund, H. Sompolinsky, "Storing infinite numbers of patterns in a spin-glass model of neural networks," *Phys. Rev. Lett.* **55**, 1530. 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

[173] G. Parisi, "A memory which forgets," *J. Phys. A* **19**, L617 (1986). https://doi.org/10.1088/0305-4470/19/10/011; "Asymmetric neural networks and the process of learning," *J. Phys. A* **19**, L675 (1986). https://doi.org/10.1088/0305-4470/19/11/005

[174] G. Parisi, "Networks in immunology," *Phys. Rep.* **184**, 283-287 (1989). https://doi.org/10.1016/0370-1573(89)90047-1; "A simple model for the immune network," Proc. Nat. Acad. Sci. U.S.A. **87**, 429-433 (1990). https://doi.org/10.1073/pnas.87.1.429

[175] 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; G. Baskaran, Y. Fu and P. W. Anderson, "On the statistical mechanics of the traveling salesman problem," J. Stat. Phys. **45**, 1-25 (1986). https://doi.org/10.1007/BF01033073

in matching[176]. [Maybe matching was a proxy of the traveling salesmen problem.] I think that we did the theory—the theory was the replica symmetric one—and apparently this theory was essentially correct, as it could be seen just from the first simulation. Indeed, this was in agreement with the following idea that has never been proven in a completely consistent way.

Replica symmetric breaking can only happen in problems that are computationally hard, either NP-hard or NP-complete, while matching could be computed [in the bipartite matching case] in N^3 time, [in general N^4]. This idea was floating around. Maybe someone has written it in an explicit way[177]. Technically it's not clear, because you may have some smart algorithm which is polynomial, but there was this idea… Anyhow, we did the computation for matching and I was after all type of matching: bipartite matching, non-bipartite matching. After we did more complex stuff, the traveling salesman [problem][178]. The traveling salesman was a much harder problem that finally we solved with population dynamics for looking at the thing. We tried another way [to tackle] the question, but we've never been able to. Also, looking nowadays at the original process with replicas seems too crazy. We would never have been able to. There were some deformations in the complex plane that had to be done: something should be deformed. Anyhow, the results with replica were correct. We also did the computation for the bipartite matching.

Finally, the thing some years later was proven to be correct by Aldous[179]. Later on, Wästlund proved that also for [the] traveling salesman the solution was correct[180].

FZ: Two things on that period. One, you said you didn't work much on neural networks, because there were many people working on it, but were you

[176] M. Mézard and G. Parisi, "Replicas and optimization," *J. Phys. Lett.* **46**, 771-778 (1985). https://doi.org/10.1051/jphyslet:019850046017077100; "Mean-field equations for the matching and the travelling salesman problems," *Europhys. Lett.* **2**, 913 (1986). https://doi.org/10.1209/0295-5075/2/12/005

[177] **GP:** I think that this is quite natural if you use minimal descent of simulated annealing as algorithm.

[178] M. Mézard and G. Parisi, "A replica analysis of the travelling salesman problem," *J. Phys.* **47**, 1285-1296 (1986). https://doi.org/10.1051/jphys:019860047080128500; "On the solution of the random link matching problems," *J. Phys.* **48**, 1451-1459 (1987). https://doi.org/10.1051/jphys:019870048090145100; "The Euclidean matching problem," *J. Phys.* **49**, 2019-2025 (1988). https://doi.org/10.1051/jphys:0198800490120201900

[179] D. J. Aldous, "The ζ(2) limit in the random assignment problem," *Random Struct. Alg.* **18**, 381-418 (2001). https://doi.org/10.1002/rsa.1015

[180] J. Wästlund, "The mean field traveling salesman and related problems." *Acta Math.* **204**, 91 – 150 (2010). https://doi.org/10.1007/s11511-010-0046-7

nevertheless following the work of Elizabeth Gardner[181], Haim Sompolinsky[182], Bernard Derrida?

GP: [2:45:38] Yes. I was following the things, but it seemed [that there was] some flux of new ideas that one had to work out to catch up. We certainly knew Gardner; we knew all these things. The Gardner computation, the Derrida [work], and all [the] other things, I was following, but I was not working too much [on it], because I had no good ideas. That was also the core moment when we had a lot of things to do with APE.

The other thing that maybe we should speak [about] later is that there were some numerical simulations. [For] the numerical simulation, what is important is that there was in ’82 or ’83, just after the paper that I said with the interpretation and the fact that $P(q)$—because there was a moment when the prediction was more or less in De Dominicis and Young… The point was that the P(q) was analytically computed in terms of $q(x)$, so there was a precise prediction of $P(q)$ and there was a first Monte Carlo that was done by Young and Mackenzie[183]—that is also in the book[184]—which shows that the $P(q)$ computed from the theory was in agreement. Of course, there was not a delta function [in] finite-size systems. The real thing that was important was to try to understand what happens in finite-volume systems. One has the problem to see what happens in dimension three and in dimension four.

PC: Before we move there, I wanted to ask you about that book, *Spin Glass Theory and Beyond*.

GP: [2:48:08] The book was started to be done in Rome. Certainly, the idea was in Rome and after it was finished maybe later. We agreed was that the structure of the book should have self-contained text [and use] the published papers as appendices. After, we decided that we would divide the work. I think that I have written one chapter, Marc wrote another, Miguel wrote another, some we wrote together. Therefore, we had this idea to put all things together, because I think that at that moment lots of things about the Sherrington-Kirkpatrick [model] were understood, and there were some ideas of possible applications that were in the region of optimization and the region of neural networks and so on. (By the way, the

[181] Elizabeth Gardner: https://en.wikipedia.org/wiki/Elizabeth_Gardner_(physicist)
[182] Haim Sompolinsky: https://en.wikipedia.org/wiki/Haim_Sompolinsky
[183] N. D. Mackenzie and A. P. Young, “Statics and dynamics of the infinite-range Ising spin glass model,” *J. Phys. C* **16**, 5321 (1983). https://doi.org/10.1088/0022-3719/16/27/015
[184] Marc Mézard, Giorgio Parisi and Miguel Angel Virasoro, *Spin glass theory and beyond: An Introduction to the Replica Method and Its Applications* (Singapore: World Scientific, 1987).

version of the book that one can find on libgen[185] does not contain part of the book. The guy that made things was not interested in it.)

PC: What made it such that it should be written then, and not before or after?

GP: [2:50:06] I think that at that moment we had a good understanding of the structure of spin glasses. Also, we had a good understanding of how one could start to use these things for looking to simple optimization problems. There was also the Anderson and Fu thing and neural networks. It was at the same moment that Anderson wrote the columns. It was clear that we were at least relatively confident that that was the correct solution, and all the simulations with SK were in agreement with these things, although we can never be sure by numerical simulations. I think that was a good moment because just after there started [to be] a lot of applications. All types of new problems, finite-dimensional, other optimization problems and so on. The book was relatively fast to be written, because most of the book was material that [we had] published together. Therefore, we had to do 60 or 70 pages. I think it was a useful book, because it was a collection of all or nearly all relevant papers.

PC: A year or two after that book came out, you published another book, *Statistical Field Theory*[186], which was based on your lecture notes.

GP: [2:52:06] Yes, but I think that most of the book was finished probably in ’84 or something like that. First, I asked some professional—because part was written by hand—to type everything, and I wrote later the formula. After I sent the manuscript there, they had to produce the book. They sent me a very long proof, with queries for all formula that they could not understand, how to correct them and so on. That was sent again. After one has to correct again the second proofs. Meanwhile, I realized that something was wrong or could not be understood, therefore I added something. So it was taking a lot of time. There was a first version of the book of 100 pages that I wrote immediately after my course in ’79-’80, and I distributed [it] to the students. Probably, Enzo [Marinari] or myself have a version of that thing. That was something that was a project. For example, I remember that when I was in Bures-sur-Yvette, in ’82, I gave the secretary of Bures-sur-Yvette some of the manuscript to be printed, so I was writing the book in ’82. That was something that was done [at a] low

[185] Two versions of the book can be found on libgen: (i) one with 317 pages, https://libgen.li/edition.php?id=135794926; and (ii) one with 475 pages, https://libgen.li/edition.php?id=137036996. (Accessed February 18, 2022.)
[186] See Ref. 129.

pace. The book on spin glasses was something that we decided to do fast. It was short [somewhat longer that 100 pages if you exclude the reprints].

You see, for *Statistical Field Theory* I did deviations [or some wandering]. I started to think of something, I added [some material]. You know, this thing can also be important for other things. There are many footnotes. I wanted to have people aware that there were some other applications of this stuff to other things. Therefore, it was a slow book to put something proper. I think it was a good idea, because people can refer to the book. Usually, also I was referring to the book, not to my original publications, because everything important was there. I think this was quite useful.

PC: In the *Statistical Field Theory* book, you did not include anything about spin glasses.

GP: [2:55:58] No, because that was done completely in a different direction. The title *Statistical Field Theory* was essentially to describe the relation that were from Euclidean statistical field theory, where you can do Monte Carlo, you can do quantum mechanics, and also to try to understand the path integral from quantum mechanics first of all in the lattice and in the Euclidean context where everything is a probability, before going to the much more difficult problem of doing with path integrals with [imaginary exponents]. Also, the limit of the continuum is much easier to understand, I believe, from the viewpoint of phase transitions than from the viewpoint of quantum statistical physics. Otherwise, if I should start to speak of spin glasses, I would never end.

FZ: Before we move to the next subject, and since we were talking about neural networks, can you make a digression to talk about Daniel Amit[187] a little bit? How did you meet him? How did you recruit him to Rome?

GP: [2:57:31] Amit was coming often to Rome.

FZ: When did you first meet him?

GP: [2:57:45] In Cargèse summer school of 1973. One of the courses at 9 o'clock was boring and both of us where going to swim in front of the school. In '82, everybody in the business of spin glasses was speaking about Hopfield, because it was considered to be a big important progress, something which was important also to understand the brain. There were all these people who were doing connectionism—as it was called at the time—that were doing artificial intelligence that was distributed. There

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

was a lot of ideas about distributed artificial intelligence, to have many objects that are relatively simple [working] together. The Hopfield model clearly fit in these things, and it was clear that the Hopfield model was strongly connected to spin glasses. Therefore, everybody was interested in following what was happening there. Toulouse, for example, Miguel Virasoro was also interested in the Hopfield model and so on. Also, he was trying to understand if [there existed] some version of a spin glass for neural networks. The fact that the brain has a tendency to classify, and that it classifies in a taxonomic way… Because something that is not taxonomical is also very complex. The idea is that you could classify things in a taxonomical way. Spin glasses also naturally provide you a background for taxonomy. There was Miguel, for example, who was interested to explore this part of the thing[188], so there was a lot of interest in this business. Also, because there was a first application of spin glass replica theory that was going beyond material science and so on. That was the reason that people were strongly interested.

FZ: And Daniel Amit, where did you meet him first, and why did he come to Rome? Did you have anything to do with it?

GP: [3:00:19] First of all, Daniel Amit was going often to Paris. You remember that also Daniel wrote a book on the renormalization group[189]. He was certainly interested, and he was a frequent visitor in Paris [in Saclay I believe]. He was also a frequent visitor in Rome, not only for physics, but also for political reasons. He was interested to have a contact with the left wing [of *Democrazia Cristiana*][190]. The [*Democrazia Cristiana*] party in Italy had a left wing. The left wing was strongly philo-Arab. It was an inheritance of Enrico Mattei[191] and so on to have a good relation. Therefore, there was a certain good connection with the Palestinian movement, the anti-occupation part of Israel with the left part of the Italian government.

FZ: So this was his political motivation for coming to Rome?

GP: [3:01:39] I think for coming one or two times per year to Rome, yes. Also, he liked very much the Roman way of life in the department of physics. So we invited him for coming for one year. He first came for one year to Rome. After he went back to Israel. After the political situation in Israel was quite heavy for him, so we offered him to have a permanent job in Rome. That

[188] N. Parga and M. A. Virasoro, "The ultrametric organization of memories in a neural network," *J. Physique* **47**, 1857-1864 (1986). https://doi.org/10.1051/jphys:0198600470110185700
[189] Daniel J. Amit, *Field theory, the renormalization group, and critical phenomena* (New York: McGraw-Hill 1978).
[190] Christian Democracy: https://en.wikipedia.org/wiki/Christian_Democracy_(Italy)
[191] Enrico Mattei: https://en.wikipedia.org/wiki/Enrico_Mattei

took some time but was technically possible. Finally, he found this way to spend half of the time in Rome, half of the time in Israel, doing the trip from Israel to Rome with a car. Typically, he was putting the car on a ferry going to Athens, spending one week with his friends in Athens [among them Maria Becket[192]], and putting [his car] on another ferry to Italy. By road, it would have been quite a long and difficult journey. He had very good friends in Greece. Among them, he was friend of the archbishop of Patmos, the island where St. John died, or St. John wrote the Apocalypse[193]. There's a big monastery there. Dan was friend of the bishop of the monastery. Sometimes he was spending time there. He had a good connection with it. For about 15 years, he was a member the Committee planning and organizing the Religion Science and the Environment symposia[194].

PC: We now want to go back to the finite-dimensional simulations that were being…

GP: [3:04:28] The finite-dimensional simulations is something we started to do around '89. I don't remember exactly the order. Certainly, we did some simulations that were done with Nicolas Sourlas and Sergio Caracciolo[195]. We started to do this type of things. We tried to write faster computer code. Nicolas Sourlas spent six months in Rome, about '86-'87, where we discussed simulations. At a certain moment, also Enzo was starting to work on this type of simulations. He was at a university in America, [Syracuse] maybe.

PC: I wanted to step back to the motivation for this. I think that the work with Enzo started in the '90s,.

GP: [3:06:00] Of course, I did lot of work on QCD with Enzo. I don't remember exactly, but at that moment we started to do some computations for spin glasses also with Enzo. That was for three-dimensional spin glasses. I remember that we started to have a lot of discussions with Enzo to try to

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[192] Neal Ascherson, "Maria Becket: Resistante who fought the Greek junta then became an ecological activist," *The Independent* (November 21, 2012). https://www.independent.co.uk/news/obituaries/maria-becket-resistante-who-fought-the-greek-junta-then-became-an-ecological-activist-8336227.html ; "Maria Hary Becket (1931-2012)," *Maria Becket Report* (2012). http://www.mariabecketreport.com/home.html (Consulted August 18, 2022.)

[193] Cave of the Apocalypse : https://en.wikipedia.org/wiki/Cave_of_the_Apocalypse

[194] "Religion Science and the Environment," *RSE Symposia* (s.d). http://www.rsesymposia.org/index.php (Constulted August 18, 2022.)

[195] S. Caracciolo, G. Parisi, S. Patarnello, and N. Sourlas, "Low temperature behaviour of 3-D spin glasses in a magnetic field," *J. Phys.* **51**, 1877-1895 (1990). https://doi.org/10.1051/jphys:0199000510170187700; "3d Ising spin-glasses in a magnetic field and mean-field theory," *Europhys. Lett.* **11**, 783 (1990). https://doi.org/10.1209/0295-5075/11/8/015

understand how to improve Monte Carlo, because Monte Carlo is quite slow. The first thing that we invented was simulated tempering[196]. That was a real mess to write and run the program. However, we [worked for] two or three years with that method. Finally, there was a very big advantage [to the parallel tempering] method that was discovered by Hukushima and Nemoto[197]. That was much [more] straightforward to simulate, and therefore we moved to that. I can remember two papers from that period. One paper in which we did very careful [work], because there was a lot of discussions[198]. Also not everybody was convinced that there was a transition in three dimensions. They would say: "Maybe the transition is slightly rounded." That is always difficult to convince [someone] that it's not rounded, because in a magnetic field it's rounded. You have to go to very small magnetic fields. Therefore, we did also large-scale simulations to convince ourselves that there was a real transition. [That should be '88 or something like that.] We did something on that to show the transition. The other things that we did also [were] because Juan Jesus Ruiz-Lorenzo – JJ—came to Rome, and we started to work on these things[199].

FZ: Did all these things happen during the '80s?

GP: [3:09:39] Look. I slightly confuse the '80s and '90s. Because in the '80s, up to '89, most of the computational effort was [for] lattice gauge theory.

FZ: Were you following the developments on the numerical simulations of spin glasses even if you were not working on it?

GP: [3:10:02] Well, the numerical simulations of three-dimensional spin glasses were not so much in the literature.

PC: There was Ogielski's in '85 with a special purpose computer[200].

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[196] E. Marinari and G. Parisi, "Simulated tempering: a new Monte Carlo scheme," *Europhys. Lett.* **19**, 451 (1992). https://doi.org/10.1209/0295-5075/19/6/002
[197] K. Hukushima and K. Nemoto, "Exchange Monte Carlo Method and Application to Spin Glass Simulations," *J. Phys. Soc. Jpn.* **65**, 1604-1608 (1996). https://doi.org/10.1143/JPSJ.65.1604
[198] E. Marinari, G. Parisi and F. Ritort, "On the 3D Ising spin glass," *J. Phys. A* **8**, 2687 (1994). https://doi.org/10.1088/0305-4470/27/8/008
[199] E. Marinari, G. Parisi, J. Ruiz-Lorenzo and F. Ritort, "Numerical evidence for spontaneously broken replica symmetry in 3D spin glasses," *Phys. Rev. Lett.* **76**, 843 (1996). https://doi.org/10.1103/PhysRevLett.76.843
[200] See, *e.g.*, 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

GP: [3:10:11] I read that paper many, many times. However, it was on the dynamics. There were a few ones at equilibrium, but they were not on large lattices and there was not a clear analysis. We started to work on this maybe in ’90. I have to look to the paper.

FZ: The simulated tempering paper is from ’92, and you have a paper with Caracciolo and Sourlas in ’90. During the ‘80s, there was this debate with the droplet model and RSB. Was this done on a purely theoretical basis?

GP: [3:10:52] Mostly on a theoretical basis, because there was not any very large scale numerical simulations. Well, there were small simulations, but they had to be interpreted. There was a proposal, and I think that it was mostly a theoretical approach. I don’t remember exactly the debate. Also, there was some discussions theoretically if you could have many states in finite dimensions and also be consistent and so on. The point attracted the attention of mathematicians, because the first version was a little too fast in the definitions of what does one means with the infinite volume limit. Essentially, when we wrote the paper where we discuss states in the infinite volume [limit] we did not paid attentions to details. The infinite volume limit is quite complex. You have to do the construction of states in finite volume, and then from the finite volume [determine] how to construct states. Therefore, the wording of the first paper had to be polished. The mathematicians did, not that they were not correct, but they were unprecise. It’s clear that on a finite Ising model you have two states also on a finite volume, but the precise definition of this is something [about which] you have to be a little careful. There was some debate on how one could formulate…

But probably you are right that up to ’88-’89 I was essentially doing most of the things for numerical simulations for QCD. We had APE working. We had a lot of things that we were doing. After a certain moment, in ’88-’89, people decided that APE was a nice machine but it was not [anymore] at the top for high energy physics, so we decided to build a new computer that was APE100 that was 100 times faster than APE. I worked on the project and so on, but at a certain moment I realized that this was too technical. At that moment, I left that direction. I was involved only in the project [in that] I was discussing with people about the construction, but I left to others the construction. Also, the one that was organizing the physics on APE100 was essentially Guido Martinelli.

After having left APE in ’89, we started to be interested in other problems. For example, there was a heteropolymer paper that we did with Marc in

1990, I guess[201]. I think that the paper was mostly done in Heraklion. There was a two-week school in Heraklion, in Greece, and during this school we started to discuss this problem and we nearly finished the paper for random heteropolymers. Also, what took a lot of theoretical interest at that time was to compute finite-size corrections for the optimization problems[202]. The structure was quite complex in one case. Unfortunately, the paper was also long. Finally, it was found out to cure it.

Therefore, I probably started in the '90s to do simulations for spin glasses, after the Caracciolo-Sourlas paper that was written in '89. I think that some of the simulations were also written on APE, because APE could also manage real numbers. I think that Federico Ricci-Tersenghi did some simulation on APE for spin glasses[203]. One paper that we did was done on simulated tempering and that was quite a heavy technique, but in '92 the thing started to run much better with parallel tempering. Finally, after a long effort, we had a very clear picture of what was happening on lattices from 4^3 to 16^3, where one can do finite-size scaling. And we [saw] that everything was compatible with replica symmetry breaking. Of course, it's one thing to say that it's compatible, [and] it's [another] thing to do the extrapolation to infinite volume.

This was a period when we started to have a lot of people who came from Spain to Rome. There was some way to get a fellowship from Spain to come to Rome. People came for a one-year fellowship from Spain and after we'd add another year fellowship from Rome. We had Tarancón, we had Fernández, who [both] started to work on APE[204], [and] who started to work on spin glasses later. I think that Félix Ritort and Juan started directly to work on spin glasses.

PC: What was you connection with Spain? How did you know the group?

GP: [3:18:05] They knew us. They'd get fellowships from the Spanish government to come to Rome[205].

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[201] M. Mézard and G. Parisi, "Interfaces in a random medium and replica symmetry breaking," *J. Phys. A* **23**, L1229 (1990). https://doi.org/10.1088/0305-4470/23/23/008

[202] See Refs. 176 and 178.

[203] G. Parisi, F. Ricci-Tersenghi and J. J. Ruiz-Lorenzo, "Equilibrium and off-equilibrium simulations of the Gaussian spin glass," *J. Phys. A* **29**, 7943 (1996). https://doi.org/10.1088/0305-4470/29/24/018

[204] See, *e.g.*, P. Bacilieri, E. Remiddi, G. M. Todesco, M. Bernashchi, S. Cabasino, N. Cabibbo, L. A. Fernández, E. Marinari, P. Paolucci, G. Parisi, G. Salina, A. Tarancón, F. Coppola, M. P. Lombardo, E. Simeone, R. Tripiccione, G. Fiorentini, A. Lai, P. A. Marchesini, F. Marzano, F. Rapuano, and W. Tross, "Order of the Deconfining Phase Transition in Pure-Gauge QCD," *Phys. Rev. Lett.* **61**, 1545 (1988). https://doi.org/10.1103/PhysRevLett.61.1545

[205] The postdocs obtained a number of fellowships, but many were supported by a Formación de Personal Investigador (FPI) grant. See, *e.g.*, Programma FPU: https://es.wikipedia.org/wiki/Programa_FPU;

FZ: Why was it computer oriented? Was it a fellowship to do computer work? Why did all these people work on APE and other numerical projects?

GP: [3:18:26] Well. No. These two worked on APE, because they were interested in high-energy physics. Juan, I think was more interested both the lattice gauge theories and to statistical mechanics. I don't think it was something related to APE. Also, Félix [Ritort] was interested in the theory[206]. Of course, we did a lot of simulations also with Félix, but he was interested in the theory. Fernández and Tarancón were more interested in working with APE because that was a big group that was needing a lot of people do to [some] analysis. There were a few people that came in from outside. For example, there was one Czech engineer, Jarda [Pech], that was working there.

The date [of these events] ,you should correct by looking at the papers. We started in the '90s to work on the three-dimensional lattices. Therefore, first of all we did this first analysis with Juan. After, we wrote maybe a paper with five people—Enzo, Juan, myself, Federico Ricci-Tersenghi and Zuliani—that was a quite long paper, which established the theoretical situation for finite [dimensional spin glasses][207]. (This was maybe the only paper with Zuliani that I had[208].) There was [in this paper] the most [extensive] summary of the things that we were doing, and all numerical verification of the Ghirlanda-Guerra identities. And there was a lot of discussion on what was the physical meaning, some kind of response to the critics—what was done with the metastate[209]—that it was not possible to do spontaneous symmetry breaking, clarifying the meaning of that

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"Archivo Central de Educación: 10.03. Formación del profesorado y personal investigador (1972-1996)," *Gobierno de España, Ministerio de Educación y Formación Profesional* (2015). https://www.educacionyfp.gob.es/dam/jcr:415d666f-46d7-4cc6-b842-48799bad9506/10-03-cuadro.pdf (Accessed May 19, 2022.)

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

[207] E. Marinari, G. Parisi, F. Ricci-Tersenghi, J.J. Ruiz-Lorenzo and F. Zuliani, "Replica symmetry breaking in short-range spin glasses: Theoretical foundations and numerical evidences," *J. Stat. Phys.* **98**, 973-1074 (2000). https://doi.org/10.1023/A:1018607809852

[208] **PC:** Not quite. See also, *e.g.*, E. Marinari, C. Naitza, F. Zuliani, G. Parisi, M. Picco and F. Ritort, "General method to determine replica symmetry breaking transitions," *Phys. Rev. Lett.* **81**, 1698 (1998). https://doi.org/10.1103/PhysRevLett.81.1698

[209] See, *e.g.*, C. M. Newman and D. L. Stein, "Spatial inhomogeneity and thermodynamic chaos," *Phys. Rev. Lett.* **76**, 4821 (1996). https://doi.org/10.1103/PhysRevLett.76.4821; G. Parisi, "Recent rigorous results support the predictions of spontaneously broken replica symmetry for realistic spin glasses," arXiv:cond-mat/9603101. https://doi.org/10.48550/arXiv.cond-mat/9603101

work. That was a quite long paper. I think it was 40-50 pages. That was a quite serious effort, this thing.

In the meanwhile, at that moment people discovered—not us, but Rieger[210]—that it was possible to do simulations with multispin codes. Multispin codes were introduced by [Claudio Rebbi[211] and used in systems with disorder by Heiko Rieger[212]]. With Caracciolo and Sourlas we used Rebbi version of multispin coding, but the advantage was marginal. Rieger version was extremely efficient, due to a wonderful idea. Finally we were using [them][213]. Multispin codes [have] an advantage of a factor of 20 above the standard simulation. I think that up to [the year] 2000 we remained on a relatively small lattice, 16^3. We did some work on magnetic fields, trying to identify things in a magnetic field[214]. What was clear from that paper was that there was evidence that some correlation lengths were large, but it was not so clear how large they were if you didn't do finite-size scaling. The susceptibility has to go to infinity at a second-order transition. If the susceptibility goes from 1 to 100, you can say that you're seeing some sort of critical behavior. But we know that, for example, in the two-dimensional O(3) model—the Heisenberg model[215]—you have a strong increase of the susceptibility, [yet] at the end you find only an exponential increase to the susceptibility. Therefore, you don't have a transition; the transition temperature goes to zero. So there was a lot of work on this direction.

(There was one machine, but I don't remember the year, that was done [using] Transputers[216] by the Spanish friends, they started to do did very large simulation Spain[217]. Later on they constructed SUE [218] that was the

[210] M. Creutz, L. Jacobs and C. Rebbi, "Experiments with a gauge-invariant Ising system," *Phys. Rev. Lett.* **42**, 1390 (1979). https://doi.org/10.1103/PhysRevLett.42.1390. See also: R. Zorn, H. J. Herrmann and C. Rebbi, "Tests of the multi-spin-coding technique in Monte Carlo simulations of statistical systems," Comp. Phys. Comm. **23**, 337-342 (1981). https://doi.org/10.1016/0010-4655(81)90174-0
[211] Claudio Rebbi: https://de.wikipedia.org/wiki/Claudio_Rebbi
[212] H. Rieger, "Fast vectorized algorithm for the Monte Carlo simulation of the random field Ising model," J. Stat. Phys. **70**, 1063–1073 (1993). https://doi.org/10.1007/BF01053609
[213] E. Marinari, G. Parisi and F. Zuliani, " Four-dimensional spin glasses in a magnetic field have a mean-field-like phase," *J. Phys. A* **31**, 1181 (1998). https://doi.org/10.1088/0305-4470/31/4/008
[214] F. Krzakala, J. Houdayer, E. Marinari, O. C. Martin and G. Parisi, "Zero-temperature responses of a 3D spin glass in a magnetic field," *Phys. Rev. Lett.* **87**, 197204 (2001). https://doi.org/10.1103/PhysRevLett.87.197204
[215] Heisenberg Model: https://en.wikipedia.org/wiki/Classical_Heisenberg_model
[216] Transputer: https://en.wikipedia.org/wiki/Transputer
[217] V. Azcoiti *et al.* "Experience on TRN" in: *Proceedings of the International Conference on Computing in High Energy Physics '92, Annecy, France, 21-25 September 1992*, C. Verkert and W. Wojcik, eds. (Geneva: CERN, 1992), 353-360. http://dx.doi.org/10.5170/CERN-1992-007; Cited in: F. Belletti *et al.* "Ianus: an adaptive FPGA computer," *Comp. Sci. & Engineer.* **8**, 41-49 (2006). https://doi.org/10.1109/MCSE.2006.9
[218] https://doi.org/10.1016/S0010-4655(00)00170-3

most advanced machine for spin glass simulations at that time; it was the progenitor of JANUS.

I was always trying to understand how to get more details about, for example, looking to the transition [in a magnetic field], looking to some way—because it was discovered that you can define different cumulants. [At the same moment we started to be interested in glasses and in the KTW[219] approach.]

FZ: Before we talk about glasses, I want to be sure that I understand. Did you start to work on the numerical simulations of spin glasses more systematically because you kind of disengaged from the APE project?

GP: [3:25:45] The APE experience was closed for me. However, we were doing simulations mostly on workstations that were not very fast at that moment. We were interested to see is whether there was a qualitative agreement or not with the theory, and how this qualitative agreement was moving. The behavior of $P(q)$ in the SK model and the $P(q)$ in finite-dimensional [spin glasses] as a function of the volume was quite similar. There was interest to have this type of comparison

Of course, there was also interest of discussing with experimentalists about the way that people were doing experiments. I had a lot of discussions with people in Paris, with [Miguel] Ocio, [Eric] Vincent and the other ones in Paris, about these things. Again, looking to spin glasses, we were doing some aging. For example, we studied the theory of aging properties on spin glasses[220]. That was certainly important in order to be convinced of the correctness of the theory. In Rome, Andrea Baldassari did some numerical and the Sherrington-Kirkpatrick to see aging[221]; similar simulations on the Bethe lattice and on the three dimensional lattice were done with SUE[222]. Because at a certain moment the dynamics also started to be important.

PC: This computational effort started more than 30 years ago, and within five to seven years you were already pretty convinced that the answer was

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

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

[221] A. Baldassarri, "Numerical study of the out-of-equilibrium phase space of a mean-field spin glass model," *Phys. Rev. E* **58**, 7047 (1998). https://doi.org/10.1103/PhysRevE.58.7047

[222] S. Jiménez, V. Martín-Mayor, G. Parisi and A Tarancón, "Ageing in spin-glasses in three, four and infinite dimensions," *J. Phys. A* **36,** 10755 (2003). https://doi.org/10.1088/0305-4470/36/43/006

what you were expecting. Yet this has kept on going. What was the drive to get to Janus, to get to large-scale computations?

GP: [3:27:52] The drive to understand [whether] the RSB could work in finite dimensions was considerable. Now, we have to come back to the theory. The theory was quite clear that you had long-range correlations inside the broken phase. These long-range correlations were supposed to interact and to create a change; the correlators should be different when we're in less than six dimensions. Unfortunately, the full-loop computations were too difficult to [do]. There was only some parts that could be done. The best thing that one could hope was that the replica symmetry breaking was surviving in 6-epsilon, at least at zero magnetic field. (In magnetic field everything was more complex.) However, it was not clear if this was surviving in four [dimensions].

We all know that most of the critical theories have a lower critical dimension where the transition disappears. It was very clear that in two dimensions there is no spin glass transition. People noticed also that the ground state is polynomially soluble in two dimensions. Anyhow, there's no transition in two dimensions. Not being in two dimensions, the first guess was that the lower critical dimension was three. Therefore it was possible that the situation could be that in four dimensions we have standard replica theory, and the replica theory disappears in three dimensions. Three dimensions is the lower critical dimension. If three dimensions were the lower critical dimension, that would be interesting because you could—like for the O(N) model—say that at T=0, the theory is just the mean-field theory, and we have to do the expansion from T=0 to higher loop. However, in order to do this thing, you have to be convinced that three dimensions is the lower critical dimension and [that] there, [there] was no transition.

However, the more we were looking to this model, [the more] we [were getting] good arguments that three was not the lower critical dimension. First of all, because there was a transition and that was indisputable. Also experiments [gave] a very sharp transition. And below the critical temperature in a magnetic field, everything has the signature of replica symmetry breaking. At the time, we were working theoretically. There was that paper that we did with Virasoro and Silvio, on the interface between [phases] that we had the lower critical dimension was 2.5[223]. That was a very puzzling result, because this was a non-perturbative computation. Part of the non-perturbative computation was verified numerically after

[223] S. Franz, G. Parisi and M. A. Virasoro, "Interfaces and louver critical dimension in a spin glass model," *J. Phys. I* **4**, 1657-1667 (1994). https://doi.org/10.1051/jp1:1994213

with Maiorano[224]. This was in some sense orthogonal to the perturbative computation by De Dominicis et al., which gives no hint of a lower critical dimension[225]. If you think of diagrams it is difficult to get 2.5 Well, it could happen, but it's somewhat strange to have diagrams that diverge in dimension 2.5. One wanted to get the scope, the range of validity of the replica broken theory in zero magnetic field. The interest was to understand if the three-dimensional [system] at finite temperature was included in the theory, or if one should start from zero temperature. That's very important.

The other thing is that [later] on there was all this connection with the dynamics, and we started to see if the connection with dynamics—the work of Cugliandolo-Kurchan on fluctuation-dissipation[226]—was something that was compatible. At a certain moment, we moved in that direction. On one side, we computed the $P(q)$ from static measurements, and on the other side we computed correlations, we looked to scaling and so on. That was another important point. In the end, what happened is that for fluctuation-dissipation the experiment was done in the group of Ocio in Paris[227]. It was technically very complex, a very long experiment that went on for one year, I believe. They had to do many cycles of cooling and heating of the sample. That was a very nice experiment. The scaling law of correlations and responses of Cugliandolo-Kurchan were going to universal curves when the time was going to infinity. Of course, in Cugliandolo-Kurchan the universal curve was connected to the static $P(q)$ that experimentally they cannot measure. So they wanted to say that they measured the $P(q)$ by doing the experiment, if you accept that the theory was correct. However, there was the problem of understanding. There were a lot of problems of this type.

[224] A. Maiorano and G. Parisi, "Support for the value 5/2 for the spin glass lower critical dimension at zero magnetic field," Proc. Nat. Acad. Sci. U.S.A. **115**, 5129-5134 (2018). https://doi.org/10.1073/pnas.1720832115

[225] C. De Dominicis, I. Kondor and T. Temesvari, "Ising spin glass: recent progress in the field theory approach," *Inter. J. Mod. Phys. B* **7**, 986-992 (1993). https://doi.org/10.1142/S0217979293002134

[226] See, *e.g.*, L. F. Cugliandolo and J. Kurchan, "Analytical solution of the off-equilibrium dynamics of a long-range spin-glass model," *Phys. Rev. Lett.* **71**, 173 (1993). https://doi.org/10.1103/PhysRevLett.71.173; L. F. Cugliandolo and J. Kurchan, "On the out-of-equilibrium relaxation of the Sherrington-Kirkpatrick model," *J. Phys. A* **27**, 5749 (1994). https://doi.org/10.1088/0305-4470/27/17/011; L. F. Cugliandolo, J. Kurchan and L. Peliti, "Energy flow, partial equilibration, and effective temperatures in systems with slow dynamics," *Phys. Rev. E* **55**, 3898 (1997). https://doi.org/10.1103/PhysRevE.55.3898

[227] D. Hérisson and M. Ocio, "Fluctuation-dissipation ratio of a spin glass in the aging regime," *Phys. Rev. Lett.* **88**, 257202 (2002). https://doi.org/10.1103/PhysRevLett.88.257202

I think that in 2000, we started to be less interested to this. There was all the new theoretical ideas that came on the Bethe lattice[228]. I was more interested in that.

PC: That's exactly what we wanted to touch upon next. You mentioned optimization problems in the mid-80s, and then you did not work at all on this family of problems. In the early 2000s, you worked with Marc on survey propagation[229] as well as on random satisfiability problems[230].

GP: [3:36:55] Although we did the first paper with Marc on analytic theory, in which we put… One of the things we tried in the past was to do analytic theory on the Bethe lattice. On the Bethe lattice, there was this work by De Dominicis, which looked at one step [replica symmetry breaking] in the large [connectivity] z limit[231]. In the large z limit, they could do the computation, they could do the $1/z$ expansion, because the large z limit of the Bethe lattice coincides with the analytical [solution to the] Sherrington[-Kirkpatrick] model. So they can do perturbation of the Sherrington[-Kirkpatrick] model, they can do a $1/z$ [expansion]. But I think the finite z—z=6 or z=4—Bethe lattice was something that we were thinking [about] and that we were not able to go on [with].

At a certain moment, there was something that was quite strange. I went to some conference, that was in reality a conference that I organized with Luciano Pietronero[232] at ICTP, around 1999 or 2000—I don't remember exactly—on complexity, on different issues of this type. I remember that one day I had lunch with Riccardo Zecchina[233]. I knew him, but I had never spent much time with him. He said: "Look, there is this k-SAT problem that people in statistical mechanics are very interested to understand, and one should do something." (Indeed there was some paper of his on the k-SAT[234].) "One should study these things more carefully. However, this is

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

[229] M. Mézard and G. Parisi, "The cavity method at zero temperature," *J. Stat. Phys.* **111**, 1-34 (2003). https://doi.org/10.1023/A:1022221005097

[230] 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

[231] C. De Dominicis and Y. Y. Goldschmidt, "Replica symmetry breaking in finite connectivity systems: a large connectivity expansion at finite and zero temperature," *J. Phys. A* **22**, L775 (1989). https://doi.org/10.1088/0305-4470/22/16/003

[232] Luciano Pietronero: https://en.wikipedia.org/wiki/Luciano_Pietronero

[233] Riccardo Zecchina: https://it.wikipedia.org/wiki/Riccardo_Zecchina

[234] See, *e.g.*, R. Monasson and R. Zecchina, "Entropy of the K-satisfiability problem," *Phys. Rev. Lett.* **76**, 3881 (1996). https://doi.org/10.1103/PhysRevLett.76.3881; R. Monasson and R. Zecchina, "Statistical mechanics of the random K-satisfiability model," *Phys. Rev. E* **56**, 1357 (1997). https://doi.org/10.1103/PhysRevE.56.1357; R. Monasson, R. Zecchina, S. Kirkpatrick, B. Selman and L.

something like a Ising [spin glass model] on a Bethe lattice. Therefore, the first thing that you should understand is the Ising [spin glass model] on the Bethe lattice. But I know that you have never done one-step replica symmetry breaking on the Bethe lattice." And he says: "But why are there some difficulties?" I said: "Well, there are some difficulties." And I started to explain why counting the different powers of the different order parameters could be [difficult], and which is the order parameter. At the moment that I was trying to explain why it could not be done, I realized that the counting of the order parameter, the counting of the probability, that if you interpret the one step replica symmetry as a probability distribution of probability distribution the counting was correct and the computation could be done. At the beginning I was telling him that it was impossible to do the computation; at the end I said: "Well. No. Maybe the computation should be done. I should ask Marc." When I came back to Rome, I sent an email to Marc explaining the whole idea. Marc said that it was feasible. However, there were many computations to be done.

The paper on Bethe lattice spin glass revised was quite a complex paper. We did all the computations in details, because it was better to understand how can one do in spin glasses before going to *k*-SAT. Because one has to do with cavity, and there were two parts of the functional kind. There was a paper that we did not know at the beginning that explains when you do cavity you have to do the cavity coming from one link addition if you want to get the free energy. If you just want to get the distribution of the probability of probability, you don't have to compute the free energy, but if you want to get the free energy you must be more careful. Also, there were a lot of tricks to be done. Finally, we did this paper with Marc. After we did the second…

FZ: Is this the first paper on the Bethe lattice with Marc?

GP: [3:42:38] No. We did a few papers on the replica symmetric [solution]. We wrote papers with Marc in '87-'88[235].

FZ: Yes, but this is the 2000 paper.

GP: [3:42:50] The 2000 [paper]. We first wrote the paper with Marc on the finite temperature…

Troyansky, "Determining computational complexity from characteristic 'phase transitions'," *Nature* **400**, 133-137 (1999). https://doi.org/10.1038/22055

[235] See Refs. 158 and 176.

FZ: If I understand well, you're saying that this paper was motivated by this discussion with Riccardo and the goal was to understand the *k*-SAT.

GP: [3:43:08] It was independent of the *k*-SAT at that moment. The discussion with Riccardo Zecchina made it clear to me that it was possible to make a computation for the Ising [spin glass model] on the Bethe lattice at one step [replica symmetry breaking], which was not evident at that moment. I was convinced that it was impossible. Indeed we never did it, because we were convinced that it was too difficult. At the end, we succeeded. Therefore, we started to do these computations. I went through some results over some period and finally we finished the paper on the Bethe lattice at finite temperature.

We decided at that moment that—because we wanted to do *k*-SAT at zero temperature—we should do also [the] Ising [case] at zero temperature, because if you stay at zero temperature there were some simplification. And we started to work after with Riccardo on the analytic computation. The part of survey [propagation] was essentially done by Marc and Riccardo. It was included in the short paper, but after the long paper on survey was written by themselves[236]. Why? I was involved on the analytic computation of the transition point and the complexity nearby. We did the computation in two ways. They were doing the computation directly at zero temperature. I preferred to do the computation at finite temperature. But at the end the results were the same. We published only the part at zero temperature. They were interested in survey as a tool to find the ground state configurations on a given instance of the model.

In the meanwhile—now I come back to spin glasses—we were interested in three-dimensional spin glasses, in the computation of the ground state[237]. It was clear to us that at finite temperature you have thermal noise and so on. At zero temperature, things are quite clear. There were very clear predictions about what should happen to a spin glass if you do some kinds of change on the Hamiltonian: how the ground state is going to change; how it's going to change in one spin, two spin and so on. At that moment we left Monte Carlo simulations for three, four finite dimensions

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[236] A. Braunstein, M. Mézard, and R. Zecchina, "Survey propagation: An algorithm for satisfiability," *Rand. Struct. & Algo.* **27**, 201-226 (2005). https://doi.org/10.1002/rsa.20057. See also https://doi.org/10.48550/arXiv.cs/0212002

[237] See, *e.g.*, E. Marinari and G. Parisi, "Effects of changing the boundary conditions on the ground state of Ising spin glasses," *Phys. Rev. B* **62**, 11677 (2000). https://doi.org/10.1103/PhysRevB.62.11677; "Effects of a bulk perturbation on the ground state of 3D Ising spin glasses," *Phys. Rev. Lett.* **86**, 3887 (2001). https://doi.org/10.1103/PhysRevLett.86.3887; F. Krzakala, J. Houdayer, E. Marinari, O. C. Martin, and G. Parisi, "Zero-temperature responses of a 3D spin glass in a magnetic field," *Phys. Rev. Lett.* **87**, 197204 (2001). https://doi.org/10.1103/PhysRevLett.87.197204

to move to ground state computations. We did most of the computations up to 14^3. I think that we found the ground state, just for fun, of a lattice with 16^3, but this took three days and we could not get any useful statistics.

Apart for this computation of the ground state on the lattice, we were interested in these properties of *k*-SAT[238], also in the Hessian of *k*-SAT. This took the first years of the 2000s.

PC: There are a couple more papers, up to 2005, in this direction. There is a paper with Federico and Andrea Montanari[239], but then you largely left that area.

GP: [3:47:57] Yes. The one thing with Federico and Andrea was to find where was the threshold for breaking symmetry. The things that I was most interested in, and is not completely clear at the moment, is [what] is the energy you get in off-equilibrium simulations. If you just do a Monte Carlo at high temperature and you just do a cooling, typically you arrive to some energy in the low-temperature phase that is not the static energy, but is the dynamic energy. How can these things be computed? That is something on which I was working. We did many simulations for the *p*-spin model on the Bethe lattice and so on that we never published because it was not clear how to do the analytic computation.

For example, I remember that Federico did simulations doing fast cooling of *p*-spin interactions on the Bethe lattice. He was obtaining a given value for the internal energy. I was convinced that these things were dependent on the dynamics. You do the Kalos-Lebowitz way of dynamics in continuous time[240]. You have to look to what is the probability of a spin to flip, and I was convinced from this [computation] that empirically it is extremely efficient for the *p*-spin model. It could not be done in parallel temperatures, but it was essentially as fast as parallel tempering. Because of one stupid reason. When you do standard dynamics, [for] the energy of activation, DE, the probability to activate one spin is $exp(-\beta\Delta E)$. If you do Kalos-Lebowitz dynamics the probability is $exp(-\beta\Delta E/2)$, therefore you get a factor of two in the barrier. The factor of two in the barrier is

[238] See, *e.g.*, G. Parisi and M. Ratiéville, "On the finite size corrections to some random matching problems," *Eur. Phys. J. B* **29**, 457-468 (2002). https://doi.org/10.1140/epjb/e2002-00326-3; A. Crisanti, L. Leuzzi and G. Parisi, "The 3-SAT problem with large number of clauses in the ∞-replica symmetry breaking scheme," *J. Phys. A* **35**, 481 (2002). https://doi.org/10.1088/0305-4470/35/3/303

[239] A. Montanari, G. Parisi and F. Ricci-Tersenghi, "Instability of one-step replica-symmetry-broken phase in satisfiability problems," *J. Phys. A* **37**, 2073 (2004). https://doi.org/10.1088/0305-4470/37/6/008

[240] A. B. Bortz, M. H. Kalos and J. L. Lebowitz, "A new algorithm for Monte Carlo simulation of Ising spin systems," *J. Comp. Phys.* **17,** 10-18 (1975). https://doi.org/10.1016/0021-9991(75)90060-1 See also, kinetic Monte Carlo: https://en.wikipedia.org/wiki/Kinetic_Monte_Carlo

immediately compensated by the fact that the excited spin is the first on the list to be flipped, because it has a higher energy. This way you bring to a higher energy not only that spin, but the flipping down maybe from a nearby spin. So you may have a movement of spin up-spin down that goes to spin down-spin up keeping the same energy, which with standard Monte Carlo is $exp(-\beta\Delta E)$ and with Kalos-Lebowitz dynamics is $exp(-\beta\Delta E/2)$, because it goes up and after a very small time – of order $exp(-\beta)$—if you look to the system at a given time the probability of having a up [spin] is exponential with a small exponent. Anyhow, I think that the simulations and the value of the internal energy was exactly the same—four digits, essentially the same—as the value computed by Federico. We studied a lot of things that way.

Also there was all the problem of understanding—that was a mess from the theoretical viewpoint—the computation of the number of metastable states and so on. For example, a paper that I found very difficult to have done with Irene [Giardina] and Andrea Cavagna was and old computation of 1980. You know that there is the old paper by Bray and Moore[241] and some [other group[242]] on the number of states, the number of TAP solutions. The real problem was how to do it with cavity. Doing it with cavity was much more complex. In the end, we succeeded to do it with cavity, but there was all these discussions because we had to use some sort of supersymmetry of fermionic invariance with Cavagna, Mézard and others[243]. No. In reality, this fermionic symmetry was used in a paper by Cavagna-Giardina-Juan P [Garrahan][244], but they did not pay attention to the fact that it was general. So there was a lot of work that was interesting to find analytic things for this kinetic computation of the high-value part. Not only for the ground state, but for the excited states [as well].

I think that we came back to 3D simulations around 2006-2007, because the Spanish people and also Lele Tripiccione… (I don't know if you met him. You know that he died? It was eight in the morning at the institute in Padova and he fell down from the fourth floor. Poor Lele disappeared 10-

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[241] A. J. Bray and M. A. Moore, "Metastable states in spin glasses," *J. Phys. C* **13**, L469 (1980). https://doi.org/10.1088/0022-3719/13/19/002; "Broken replica symmetry and metastable states in spin glasses," *J. Phys. C* **13**, L907 (1980). https://doi.org/10.1088/0022-3719/13/31/006

[242] C. De Dominicis, M. Gabay, T. Garel, 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

[243] A. Cavagna, I. Giardina, G. Parisi and M. Mézard, "On the formal equivalence of the TAP and thermodynamic methods in the SK model," *J. Phys. A* **36**, 1175 (2003).

[244] A. Cavagna, J. P. Garrahan and I. Giardina, "Quenched complexity of the mean-field p-spin spherical model with external magnetic field," *J. Phys. A* **32**, 711 (1999). https://doi.org/10.1088/0305-4470/32/5/004

15 days ago[245]. A real tragedy.) Lele and people in Madrid had this idea that you can do some chip on which you can put all the gates that you [want]. They are something like a sea of gates. So the chip is produced, and after you put [it] under a lamp and you just burn some of the connections with high intensity. You can have all the connections and the functional things that you want. You can do one chip that contains memory for spin glass and so on. The hardware for doing one spin flip is very simple so you can put 100 or 200 of them on the same chip. And you can have a very fast speed. This was the first version of the Janus computer[246]. (At the beginning we wrote Janus with a *i*, Ianus, which is the normal Roman spelling. Ianus, in the same way as the god, was bifront. It had one part that was connected to the user computer, one part that had hardware. We wrote Ianus, but after I found on the net somebody that said: "Was this a registered name of Apple: I-anus?" In order to avoid this joke, we decided that it was better to use a *j*. Anyhow, this was a much faster machine.) After we built a second version, Janus II, [which] was still faster[247]. Then, we had the possibility to do something that could not be done with conventional computers. Or that could [only] be done with an incredible amount of time on a very large mainframe [computer]. That worked very well. Therefore, we started to redo all the simulations up to 32^3. In the meanwhile, with people in Bologna[248], we did up to 20^3, but that was done with 32^3. After, [we had] to do a lot of analysis with discussion of dynamics, understanding well the dynamics, so there was lots of very detailed work that was done by these people[249]. We are still working because we have most of the configurations. The analysis could not be done online, so the thing that was done was to save the configurations. We have about 33 TB of configurations that from time to time we analyze, looking into details.

PAUSE

GP: [3:59:52] [What else is there to discuss?] There is structural glasses and…

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[245] See, *e.g.*, L. Bianchini, "L'ultimo saluto al professor Tripiccione: 'Addio Lele, eri uno dei migliori'," *estense.com*, Nov 18, 2021. https://www.estense.com/?p=938677 (Accessed April 10, 2021.)

[246] F. Belletti *et al.* "IANUS: Scientific Computing on an FPGA-based Architecture," In: *Parallel Computing: Architectures, Algorithms and Applications*, C. Bischof, M. Bücker, P. Gibbon, G. R. Joubert, T. Lippert, B. Mohr and F. Peters, eds. (Amsterdam: IOS Press, 2008), 553-560. https://ebooks.iospress.nl/volumearticle/26236

[247] See, *e.g.*, M. Baity-Jesi *et al.* "Janus2: an FPGA-based supercomputer for spin glass simulations," In *Proceedings of the Future HPC Systems: the Challenges of Power-Constrained Performance (FutureHPC '12)* (New York: Association for Computing Machinery, 2012), 1–11. https://doi.org/10.1145/2322156.2322158

[248] P. Contucci, C. Giardina, C. Giberti, G. Parisi and C. Vernia, "Structure of correlations in three dimensional spin glasses," *Phys. Rev. Lett.* **103**, 017201 (2009). https://doi.org/10.1103/PhysRevLett.103.017201

[249] See, *e.g.*, Janus: http://www.janus-computer.com/index.html (Accessed April 10, 2022.)

PC: Mathematical physics.

GP: [3:59:58] The idea that such glasses were important, could be relevant for the physics, was always a possibility. However, it was unclear how to do [it]. There was the KTW paper in the '80s that showed the phenomenology of the glass transition was compatible with the one-step replica symmetry breaking.

PC: Did you follow that work at the time?

GP: [4:00:49] Not so much.

FZ: How did you learn about it?

GP: [4:00:55] I don't remember. [I think] you [want to] ask me how much I was influenced by the paper that I wrote for these proceedings in Copenhagen.

FZ: The Oskar Klein Centennial symposium[250], in Stockholm, yes.

GP: [4:01:29] I don't remember how much I read at the time about Oskar Klein's things. I think that [the reason why] I personally started to [work] with glasses is that there was at a certain point [the] Bernasconi problem[251], or [rather] one version of the Bernasconi problem, the one with periodic boundary condition. This was a deterministic problem. We realized that one could argue that for that deterministic problem the solution done by replica was certainly correct for the dynamics. The idea was that we can construct a model which is not the original Bernasconi model, but was similar in some sense, with some modifications of the Bernasconi model, which could be studied by replicas and could be solved by replicas. Most of the properties of the original Bernasconi model and this lightly modified Bernasconi model were similar. There was this paper that we did with Félix and Enzo, in '94, I guess[252]. Klein is '92?

PC: September '94.

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[250] G. Parisi, "Gauge theories, spin glasses and real glasses," In: *Proceedings of the Symposium The Oskar Klein Centenary*, U. Lindström, ed. (Singapore: World Scientific, 1995), 60-71. https://doi.org/10.1142/9789814532549. Held on September 19-21, 1994 in Stockholm, Sweden.

[251] J. Bernasconi, "Low autocorrelation binary sequences: statistical mechanics and configuration space analysis," *J. Phys.* **48**, 559-567 (1987). https://doi.org/10.1051/jphys:01987004804055900

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

GP: [4:03:33] I think that was done after we the paper on Bernasconi model. What we realized is that there was this model that was deterministic, that [had] no random Hamiltonian, [yet] that had many features that could be studied analytically with replicas. Therefore, I think that these things made clear [sense] to me that we could study real glasses. The old difficulties in thinking about glasses is that glasses had no disorder in the Hamiltonian. In another viewpoint, [in] spin glasses it is fundamental to have disorder, because if you don't have disorder you cannot have replicas that interact. However, there was some realization that if you have some deterministic problem—for example you look to TAP equations—the disorder is in some sense in the solutions, in the way that atoms [come together]. Therefore, if you want use replicas—for example, the typical way that you do replica computations—[then] let's take an equilibrium configuration which is random. Now, you start to look around the equilibrium configuration, and after you do something self-consistent about that—it's an old idea to proceed in a self-consistent way. Therefore, at just the moment that we were doing this deterministic model with Félix and Enzo, also we were doing other deterministic models with Cugliandolo, Kurchan and in one case also Félix[253]. (They were doing work on a deterministic model of a different type.) What we found is that the dynamics was essentially the same [as] spin glasses and also the statics was the same [as] spin glasses, [except for] the possibility that for certain values of the size of the system there was something that could look like a crystallization. Therefore, the idea was that one could use this approach in glasses. At the same time, there was the work of Cugliandolo-Kurchan on the question of the dynamics. Also, apart for the fact that they were off-equilibrium, the equilibrium dynamics was also similar to the one of Sompolinsky[254]. At that moment, it was quite clear that they were looking very similar to the equations of Götze's mode-coupling [theory][255]. Therefore, there was also this stuff. So I was interested in this type of things.

I was invited to this Oskar Klein thing on symmetries, however, I did not have anything to speak about symmetry. Therefore, at the end I twisted the thing to talk about this vague idea. Now, I cannot remember how much

[253] L. F. Cugliandolo, J. Kurchan and G. Parisi, "Off equilibrium dynamics and aging in unfrustrated systems," *J. Phys. I* **4**, 1641-1656 (1994). https://doi.org/10.1051/jp1:1994212; L. F. Cugliandolo, J. Kurchan, G. Parisi and F. Ritort, "Matrix models as solvable glass models," *Phys. Rev Lett.* **74**, 1012 (1995). https://doi.org/10.1103/PhysRevLett.74.1012

[254] 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

[255] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Lennard Sjögren*, transcript of an oral history conducted 2021 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 19 p. https://doi.org/10.34847/nkl.382d6bmv

I was influenced by KTW. Certainly, after having written this thing I realized that there was a strong overlap with KTW, but what I had in mind at the moment when I have written this thing should be in the written text. The point [is that] I was not very familiar with the KTW stuff. I looked back later on, so I had some idea that this could be done, that you could have one-step replica symmetry breaking that was studied in details by many other people and could be used also in this context. At that moment, I was clearly influenced by them, but I don't remember the exact time[line].

Anyhow, the thing that we tried to do with Marc—and also with Barbara Coluzzi, Paolo Verrocchio—was to perform this type of replica approach that should work to get results in the glassy phase[256].

FZ: Before we talk about that, I had another question. It seems that around 1993-1994, there was an explosion of interest in the community on the problem of equivalence between random and non-random Hamiltonians. There were the two works you mentioned with Ritort and Enzo, there was also the paper of Rémi Monasson in 1995[257], and there was your work with Silvio on the Franz-Parisi potential[258]. Why precisely at that time was there an explosion of interest for this particular problem? Was it all driven by the Cugliandolo-Kurchan solution?

GP: [4:10:24] No. It was independent. There was the Bernasconi problem that we were interested [in]. It was not clear for the Bernasconi problem how to compute the [free energy] in the high-temperature phase. Indeed, the first thing—that was really a mess—was to compute the high temperature phase. We did some [analytic computation] and we were also [obtaining] the coefficients of the expansion in powers of beta to check that the expansion was correct. The main interest is, on one [hand], [the] Bernasconi problem and, [on] the other [hand], the papers by Silvio and Monasson about the potential, that the old discussion for this type of things may be done with a potential. The thing in this potential was essentially also due to the fact that it was definitely clear from the

[256] See, *e.g.*, M. Mézard and G. Parisi, "A tentative replica study of the glass transition," *J. Phys. A* **29**, 6515 (1996). https://doi.org/10.1088/0305-4470/29/20/009; B. Coluzzi, M. Mézard, G. Parisi and P. Verrocchio, "Thermodynamics of binary mixture glasses," *J. Chem. Phys.* **111**, 9039-9052 (1999). https://doi.org/10.1063/1.480246; B. Coluzzi, G. Parisi and P. Verrocchio, "Thermodynamical liquid-glass transition in a Lennard-Jones binary mixture," Phys. Rev. Lett. **84**, 306 (2000). https://doi.org/10.1103/PhysRevLett.84.306

[257] R. Monasson, "Structural glass transition and the entropy of the metastable states," Phys. Rev. Lett. **75**, 2847 (1995). https://doi.org/10.1103/PhysRevLett.75.2847

[258] See, *e.g.*, S. Franz and G. Parisi, "Recipes for metastable states in spin glasses," *J. Phys. I* **5**, 1401-1415 (1995). https://doi.org/10.1051/jp1:1995201; S. Franz and G. Parisi, "Phase diagram of coupled glassy systems: A mean-field study," *Phys. Rev. Lett.* **79**, 2486 (1997). https://doi.org/10.1103/PhysRevLett.79.2486

simulations that we did with the Bernasconi that the system was freezing before any possible type of transition. There was a freezing phenomenon of the model. Therefore, there was a problem of finding the metastable states. I think that it is clearly stated in the paper with Silvio that metastable states are local minima of some kind of potential, and analytical continuations—in some sense—from the region of stability. It was certain that this type of deterministic models were clearly examples of models for which there was a dynamic transition and the fact that you go to a metastable [state]. When we did these computations, it was after Monasson and after marginal stability. (I remember that we were trying to do with Félix a marginal stability computation.) I should look to the chronology of the work. I have some difficulty to realize exactly what happened in the chronology. The fact that the Potts model and the *p*-spin with *p*>4 had a one-step solution that was quite clear. The Potts model with *p*>4 was [done] in Sompolinsky.

FZ: For the *p*-spin, there was the Crisanti-Sommers series of papers[259].

GP: [4:14:50] I don't remember who was first.

FZ: Crisanti-Sommers is from '92 to '93. There are a series of papers where they do the statics and the dynamics for the spherical *p*-spin.

GP: [4:15:10] The Gross-Kanter-Sompolinksy

FZ: The Potts is before, from 1985[260].

GP: [4:15:30] Just after Gross was in Paris, because it was before the Gardner transition[261].

FZ: It was at about the same time.

GP: [4:15:37] No, before. Gardner quotes them. They made the observation that with *p*>4 there was one-step replica symmetry breaking, but the low-temperature solution was full-replica [symmetry] broken, and therefore there should be somewhere a transition from [full] replica symmetry

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[259] A. Crisanti and H. J. Sommers, "The spherical *p*-spin interaction spin glass model: the statics," *Z. Phys. B* **87**, 341-354 (1992). https://doi.org/10.1007/BF01309287; A. Crisanti, H. Horner and H. J. Sommers, "The spherical *p*-spin interaction spin-glass model," *Z. Physik B* **92**, 257-271 (1993). https://doi.org/10.1007/BF01312184

[260] 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

[261] 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

breaking to one-step [replica symmetry breaking]. Later, in Gardner's paper that quotes Gross-Kanter-Sompolinksy, she's able to do the precise computation. The transition is there so people quoted that. I think that this thing was before KTW.

FZ: KTW was '87-'89.

GP: [4:16:40] Crisanti and Sommers were just after. I don't remember too much of that paper. The idea that there was a random first-order transition—or a transition at order one and half—was clearly old. This idea of applying this to glasses was KTW. After, there was this deterministic model. I think that the idea of the deterministic model came out of the computation of the Bernasconi model. At the beginning, when we looked at the Bernasconi model, it was not clear at all that there was a transition. After that, we did the computation, we found that the high-temperature phase of the Bernasconi model had a negative entropy. Therefore, it had the same problem that we know, that there should be a breaking of replica symmetry. After making the comparison with the random [version of the Bernasconi] model, we had this thing that was one-step replica symmetry breaking but there was a problem of crystallization before.

PC: That eventually gave rise to the replicated liquid computations with Marc. What led to this effort? Were you seeing Marc regularly at that point? How did the two of you get to work on this particular issue?

GP: [4:19:01] For the replicated liquid, we had the idea that we should now transform this idea of the low energy phase to do some kind of numerical computation in the liquid. I knew from reading Huang[262]—or another book on statistical mechanics—that there were hypernetted chain approximations which within more or less 20% for the liquid phase give the correct result. Therefore, the idea that we had for soft spheres [was] that we should be able to do the same thing for liquids. For the replicated liquid theory the idea was relatively simple, in the sense that you want to just construct the main standard approach. In the replica symmetric phase, you have just a correlation between the same replica and in the replica broken phase you have a matrix of correlations between things. You can use the standard structure of the matrix to do the computation. The thing was not so successful because we were seeing that there was a transition, but the properties of the transition were pretty bad. After, we realized that there was some problem at low temperature, that the correction was becoming

[262] Kerson Huang, *Statistical Mechanics* (New York: Wiley, 1987). **PC:** This book does not discuss HNC, but others do, such as Jean-Pierre Hansen and Ian Ranald McDonald, *Theory of Simple* Liquids (London: Academic Press, 1976).

more and more singular in going to low temperature. This was not good. We left that problem and at a certain moment we realized—but I don't remember exactly how and when—we had the other version—the version that Francesco knows well—with the small cage, or molecular, [construction][263]. How we arrived at this version is something that I cannot remember. Maybe Marc remembers. I don't have the slightest idea how we did.

(I fear that there was some old email—before Gmail—scattered in different places. I tried to recover the old mail, but I have a big hole. Some email I can recover but not all.)

Anyhow, with Marc, we had this idea of molecular replicas—I don't know how it came to us the idea—that was clearly much better than the previous one, because it made sense also at low temperature. What was most interesting is that *bm* was roughly constant at low temperature. That was something that could be expected from… I don't remember why we were happy that *bm* was nearly constant. I think that it's somewhat natural, because *bm* (or *bx*) is *y* and we know that in SK [when] we have a sensible zero-temperature limit there should happen something at constant *y*, so that was much better. In the meanwhile, I did also some numerical simulations for glasses in order to check the fluctuation-dissipation theorem and there was a good agreement with *bm*, but maybe this was done much better. Anyhow, with Marc, we did this thing and there was a lot of complications because we had to do loops and so on. There was some part of the work that was quite complex. There was this mess with the correlations with more than four indices.

PC: The dynamical susceptibility calculation?

GP: [4:24:57] No. The first paper on molecular replicas probably was more messy.

FZ: The one of 1996 with Marc, where you have HNC and you have the two functions?

GP: [4:25:05] In paper with molecular replicas, we had a lot of correlations that had a tensor nature. Here, I think that in HNC it was relatively simple.

[263] M. Mézard and G. Parisi, "Thermodynamics of Glasses: A First Principles Computation," *Phys. Rev. Lett.* **82**, 747 (1999). https://doi.org/10.1103/PhysRevLett.82.747; "A first-principle computation of the thermodynamics of glasses," *J. Chem. Phys.* **111**, 1076-1095 (1999). https://doi.org/10.1063/1.479193; "Thermodynamics of glasses: A first principles computation," *J. Phys.: Condens. Matter* **11**, A157 (1999). https://doi.org/10.1088/0953-8984/11/10A/011

FZ: Yes, in the small cage treatment, the numerical part was easier.

GP: [4:25:15] The thing was quite easy. We had some expansion at small cage and so on, but that was certainly much better. Of course, we had the problem that hard spheres were hard to study. I was quite fortunate that I had a smart doctorate student to solve the problem.

PC: In Les Houches, in 2002, you gave lectures on glasses, and you wrote at the end of the lectures that "the extension to the case of hard spheres seem to be particularly interesting"[264]. Why was it particularly interesting? Why were you focused on hard spheres at that point?

GP: [4:26:10] I don't know. I think maybe because is such an old problem. First of all, it's the simplest possible problem, the one of hard spheres. It's more simple than $1/r^{12}$ and so on. So I think it may have the possibility of discovering new ideas. [Francesco,] when did you start?

FZ: We discussed about this problem in 2002 in Les Houches. That was the first time that I heard about it. That was the moment when we started working on it.

GP: [4:27:02] Ok. Of course, I had no idea about jamming in this [system][265], but there was some work on the problem.

FZ: I was wrong. It was in 2004 that we started working, in another Les Houches school[266]. In 2002, it was still not…

GP: [4:27:29] I don't know. I had the feeling that hard spheres were interesting, but I cannot remember now. Look, it was a problem that we could not solve. A problem that we could solve, well, it's a problem that we can solve. A problem that we cannot solve, that needs new ideas, that's certainly interesting. The fact that we were unable to solve [it] was…

PC: Before moving on from structural glasses. You had a paper also on dynamical heterogeneity from the mean-field standpoint. That was a

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[264] G. Parisi, "Glasses, replicas and all that" in: *Slow Relaxations and nonequilibrium dynamics in condensed matter, Les Houches Session LXXVII, 1-26 July, 2002,* Jean-Louis Barrat, Mikhail Feigelman, Jorge Kurchan, Jean Dalibard, eds. (Berlin: Springer, 2003). https://doi.org/10.1007/978-3-540-44835-8_6

[265] See, *e.g.*, C. S. O'Hern, S. A. Langer, A. J. Liu and S. R. Nagel, "Force distributions near jamming and glass transitions," *Phys. Rev. Lett.* **86**, 111 (2001). https://doi.org/10.1103/PhysRevLett.86.111; "Random packings of frictionless particles," *Phys. Rev. Lett.* **88**, 075507 (2002). https://doi.org/10.1103/PhysRevLett.88.075507

[266] *Applications of random matrices to physics*, Marie Curie Training Course, École de Physique des Houches, Les Houches, France, June 6–25, 2004.

collaboration with Silvio Franz and Sharon Glotzer[267], in 1999[268]. Can you tell us what led to that work?

GP: [4:28:23] The idea was in a sense that we knew that marginal stability implied long range correlations. There were other papers in which we were computing propagators. That was also with other people at a first-order transition. It was clear that on the marginal line—supposing, which could be debated, that the thing could be at equilibrium on the marginal line—there was a $1/q^2$ singularity in the propagator. If you do this translation to time-dependent correlations, you should get some heterogeneities. There was Sharon who was looking at heterogeneities from a numerical viewpoint. We had an analytic framework in which one could compute heterogeneities. So we decided to put things together.

PC: How did you get to know about each other's work and about each other?

GP: [4:29:56] I went a few times. There was some meeting in Copanello[269]. There were a lot of meetings on glasses, where I was invited, in Italy[270]. I think [one was] in Copanello, which is in Calabria. Maybe one meeting in Messina[271], maybe another one in some volcano island or something like that[272]. (Or Lipari, I don't remember.) Therefore, I knew of all these works that people were doing on glasses. Therefore, I was going. Indeed, you may find on the paper that there's something presented in Copanello, something presented and so on. Therefore, I met Sharon, Walter Kob and

[267] Sharon Glotzer: https://en.wikipedia.org/wiki/Sharon_Glotzer

[268] S. Franz, C. Donati, G. Parisi and S. C. Glotzer, "On dynamical correlations in supercooled liquids," *Philo. Mag. B* **79**, 1827-1831 (1999). https://doi.org/10.1080/13642819908223066; C. Donati, S. Franz, S. C. Glotzer and G. Parisi, "Theory of non-linear susceptibility and correlation length in glasses and liquids," *J. Non-Cryst. Sol.* **307**, 215-224 (2002). https://doi.org/10.1016/S0022-3093(02)01461-8

[269] *First International Conference on Scaling Concepts in Complex Fluids*, Francesco Mallamace, Copanello, Catanzaro, Italy, July 4-8, 1994. Proceedings: *Il Nuovo Cimento D* **16**(7-9). See, in particular, issue 8 https://link.springer.com/journal/11544/volumes-and-issues/16-8 (Accessed April 18, 2022.)

[270] For instance, *Seventh International Workshop on Disordered Systems,* A. Fontana, G. Parisi, G. Ruocco, G. Viliani and M. Wagner, Molveno, Italy, early March 1999. Proceedings: *Philo. Mag. B* **79**(11-12). https://doi.org/10.1080/13642819908223052; see also Eric J. Amis and Bruno M. Fanconi, *Polymers 1999 Programs and Accomplishments* (Washington: Department of Commerce, 2000) https://tsapps.nist.gov/publication/get_pdf.cfm?pub_id=853693 (Accessed April 18, 2022.)

[271] *International Conference on The Morphology and Kinetics of Phase Separating Complex Fluids*, Francesco Mallamace, Sow-Hsin Chen and P. Tartaglia, Messina, Italy, June 24-28, 1997.

[272] See, *e.g.*, *Second Workshop on Non-Equilibrium Phenomena in Supercooled Fluids, Glasses and Amorphous Materials*, Marco Giordano, Dino Leporini and Mario Tosi, Pisa, Italy, 27 September to 2 October 1998. Proceedings: *J. Phys.: Condens. Matter* **11**(10A) (1999). https://doi.org/10.1088/0953-8984/11/10A/001; *Conference on Unifying Concepts in Glass Physics*, Sharon Glotzer, Silvio Franz, Srikanth Sastry, ICTP, Trieste, Italy, September 15-18, 1999. https://indico.ictp.it/event/a03247/ (Accessed April 18, 2022.)

so on that were doing simulations on glasses there[273]. Also, I started to read papers on glasses, so I realized the connections. Because also at that moment there was arXiv it was much easier to find the papers that you could possibly use.

PC: Moving on to the mathematical physics of replica symmetry breaking. In your interview with Luisa Bonolis[274], you said that you hesitated in your undergraduate studies between mathematics and physics. In the work that you did, the topic that probably had the biggest tension between mathematics and physics was on replica symmetry breaking.

GP: [4:31:51] Yes, I was certainly interested in mathematics. However, the thing that was interesting was to understand what was is correct. I think that one of the reasons that the contributions of mathematicians was very important is that if you look to the way—at the end—that you have—as I said before—a free energy functional that you can compute in terms of the probability distribution of overlap, why should I take an ultrametric one? The ultrametric solution is stable, but it may not be the best one. It was also marginally stable, so it could have had something that was slightly better. And maybe there could be something more complex than the ultrametric one that was the correct solution. There was no reason whatsoever to have that. The space of all the probability distributions of the overlap distributions, q_1 and so on, is incredible. It's really large. I could formulate as a variational principle from some free energy of this type. It was a problem that I could formulate but not in a rigorous way. It was something about which I could not say how it could be proven. Therefore, I was believing that to find a proof of the SK free energy would be something extremely difficult. Now, there have been two really spectacular contributions by Guerra. The first contribution by Guerra were the Ghirlanda-Guerra identities that were extended by Contucci-Aizenman[275]. (Which identities are the original Ghirlanda-Guerra, which are the Contucci-Aizenman, I always get a bit confused.) All of these type of identities could be obtained by the replica method by saying that there was the matrix Q and the powers of the matrix Q were such that one line was one permutation of the other. This is something very abstract and powerful. On the other hand, the Ghirlanda-Guerra were coming out from a clear physical principle that the system was stable against small random

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[273] See also *Sixth International Workshop on Disordered Systems*, A. Fontana and G. Viliani, Andalo, Trento, Italy, 3-6 March 1997. Proceedings: *Philo. Mag. B* **77**(2). https://doi.org/10.1080/13642819808204944

[274] G. Parisi, "Giorgio Parisi," in: *Fisici italiani del tempo presente. Storie di vita e di pensiero*, Luisa Bonolis and Maria Grazia Melchionni, eds. (Venice: Marsilio, 2003), 291-335.

[275] M. Aizenman and P. Contucci, "On the stability of the quenched state in mean-field spin-glass models," *J. Stat. Phys.* **92**, 765-783 (1998). https://doi.org/10.1023/A:1023080223894

perturbations, what was called later on stochastic stability. (I think that *stochastic stability*[276], the name, was mine, but I'm not sure if it was used by someone before[277].) This means that if I have stochastic stability it may be hard to be true nearly everywhere for the SK model, apart from some zero-measure sets or things like that. It was clear that at least for the SK model, as far as the free energy is concerned, you can add a very small bout of three-body coupling, four-body coupling and so on and that it [remained] like a standard SK model, stochastically stable. This meant that there is a lot of constraint on the possibility of the distribution at equilibrium. If you think of a problem out of equilibrium, it's a different mess. But if you think of a problem at equilibrium, it was clear that the problem was strongly constrained by these things. Therefore, there was not all the possible forms of the probability distributions, but it was something that was very constrained.

One thing that I think we discovered with Juan Ruiz-Lorenzo was that if you put ultrametricity plus stochastic stability you fix the whole solution[278]. Therefore, the space may be really large, but with stochastic stability the space was really becoming much smaller. The second thing was this idea of Guerra [and] Toninelli to do Gaussian interpolation[279]. Guerra, very fast proved [a] sequence [of] theorems [with] that. The first is that the infinite volume limit exists, which was not proven because in principle you could have oscillations with *N*[280]. Of course, it makes no sense [otherwise], but you have to prove [it]. The most interesting thing was that my solution was an upper bound to the real free energy[281]. This was very interesting because it was a rigorous proof that the two things were connected. At least, this was an upper bound, therefore there's something mathematically that puts the solution in a mathematical framework, so that to derive that this was an upper bound. Very soon Talagrand, who was

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[276] GP used the term starting in 1999. See, e.g., S. Franz, M. Mézard, G. Parisi and L. Peliti, "The response of glassy systems to random perturbations: A bridge between equilibrium and off-equilibrium," *J. Stat. Phys.* **97**, 459-488 (1999). https://doi.org/10.1023/A:1004602906332; G. Parisi, "Stochastic stability" *AIP Conference Proceedings* **553**, 73-79 (2001). https://doi.org/10.1063/1.1358166

[277] The expression had a prior usage in the study of dynamical systems. See, *e.g.*, Harold J. Kushner, *Stochastic stability and control* (New York: Academic Press, 1967).

[278] E. Marinari, G. Parisi, F. Ricci-Tersenghi, J. J. Ruiz-Lorenzo and F. Zuliani, "Replica symmetry breaking in short-range spin glasses: Theoretical foundations and numerical evidences," *J. Stat. Phys*. **98**, 973-1074 (2000). https://doi.org/10.1023/A:1018607809852

[279] F. Guerra and F. L. Toninelli, "The thermodynamic limit in mean field spin glass models," *Commun. Math. Phys.* **230**, 71-79 (2002). https://doi.org/10.1007/s00220-002-0699-y

[280] F. Guerra and F. L. Toninelli, "The infinite volume limit in generalized mean field disordered models," *Markov Proc. Rel. Fields* **9**, 195-207 (2003). http://math-mprf.org/journal/articles/id963/ (Accessed April 18, 2022.) See also, arXiv:cond-mat/0208579 https://doi.org/10.48550/arXiv.cond-mat/0208579

[281] F. Guerra, "Broken replica symmetry bounds in the mean field spin glass model," *Commun. Math. Phys.* **233**, 1-12 (2003). https://doi.org/10.1007/s00220-002-0773-5

working for a long time on the spin glass problem—he had written already one book—was able to prove with a *tour the force* that the upper bound was also the lower bound[282].

PC: How closely were you following this work as it was happening? Were you talking with Francesco Guerra regularly?

GP: [4:39:14] Well, Francesco Guerra was two rooms from here. (My office was somewhere else at that time, but I was talking often with Francesco Guerra.) Francesco Guerra, from 1979 (I think), was saying that spin glass theory was the most interesting thing that was in statistical mechanics[283]. He had followed a lot. We had a lot of discussions with him. He was very interested in the problem. At the moment Francesco came out with that interpolating method, that was really very successful. He could prove many things, but it was an algebraic method. Later on, Talagrand was able, as I told you before, with a *tour de force* to flip the argument and go from a lower bound to an upper bound and vice versa.

PC: How did you react, when you first found out?

GP: [4:40:34] I was extremely happy, but I did not believe that it could be provable in some way. That, of course, closed the whole doubts that I had on the correctness of the solution. It's clear that if that you have solution that is 10^{-4} different and so on…

The other thing that was very important was that there was another paper by Aizenman and Starr[284], which was doing in a rigorous way what I was having intuition [about], that in reality you can use the cavity method to prove that the free energy is given by the max of a function that can be written with the cavity with respect to all possible distributions. That was more similar to the original idea. The proof of Guerra-Talagrand was very nice, but it was more a *tour de force* algebraically. That was more clear.

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[282] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Michel Talagrand*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 20 p. https://doi.org/10.34847/nkl.daafy5aj

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

[284] M. Aizenman, R. Sims and S. L. Starr, "Extended variational principle for the Sherrington-Kirkpatrick spin-glass model," *Phys. Rev. B* **68**, 214403 (2003). https://doi.org/10.1103/PhysRevB.68.214403; "Mean-Field Spin Glass models from the Cavity—ROSt Perspective," in: *Prospects in Mathematical Physics*, José C. Mourao, Joao P. Nunes, Jean-Claude Zambrini and Roger Picken, eds. (Providence, Rhode Island: AMS, 2007). See also: arXiv:math-ph/0607060 https://doi.org/10.48550/arXiv.math-ph/0607060

While it was clear that one could restrict oneself only to stochastically stable distributions, the thing that was missing was to prove that the thing was ultrametric. There were a few papers that were saying that if the structure of replica symmetry breaking is only k-level—a finite number of levels—then everything should be ultrametric, but it was not clear it was valid in the continuous case. Finally, there was a very interesting proof by Panchenko[285], which proved that the Ghirlanda-Guerra identities imply ultrametricity. I never completely understood the proof, but the main line of argument was in a sense the following. Suppose that there is a triangle which is not ultrametric, after he elaborates on this triangle, he finds that the probability of finding N times this ultrametric triangle when N becomes large is negative, from Ghirlanda-Guerra. Because negative probabilities are not allowed, this is a contradiction. This is a fact of life. The Ghirlanda-Guerra identities are just the ultrametric tree that we presented. This ultrametric tree may be constructed by a mathematician in a very clear way. One is the formulation of Ruelle[286], or maybe—as people like now—in the formulation of Bolthausen and Sznitman[287]. Therefore, now the argument for the proof is relatively simple. You take this functional, you prove Ghirlanda-Guerra, and Ghirlanda-Guerra gives ultrametricity. Ultrametric and Ghirlanda-Guerra compliant probabilities are parametrized by $q(x)$. You explicitly write the functional, you evaluate for $q(x)$ and that's the end of the story. This means that nowadays we have a very nice compact proof. However, it's missing the original one, the one with non-integer stuff and so on.

PC: In 2002, you wrote[288]: "The existence of alternative routes clearly shows that the results make sense. In any case the replica method has a very strong heuristic value and it would be surprising if we could not assign a precise mathematical meaning to such a useful method." Are you still hoping that there will be a RSB-adjacent demonstration?

GP: [4:46:22] I have written one or two papers, in which I was suggesting some way in which one could develop a rigorous way[289]. The point is that in some

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[285] D. Panchenko, "The Parisi ultrametricity conjecture," *Ann. Math.* **177**, 383-393 (2013). http://doi.org/10.4007/annals.2013.177.1.8

[286] D. Ruelle, "A mathematical reformulation of Derrida's REM and GREM," *Commun. Math. Phys.* **108**, 225-239 (1987). https://doi.org/10.1007/BF01210613

[287] E. Bolthausen and A. S. Sznitman, "On Ruelle's probability cascades and an abstract cavity method," *Commun. Math. Phys.* **197**, 247-276 (1998). https://doi.org/10.1007/s002200050450

[288] G. Parisi, "Two spaces looking for a geometer," *Bull. Symb. Logic* **9**, 181-196 (2003). https://doi.org/10.2178

[289] G. Parisi, "The Mean Field Theory of Spin Glasses: The Heuristic Replica Approach and Recent Rigorous Results," Lett. Math. Phys. **88**, 255 (2009). https://doi.org/10.1007/s11005-009-0317-4; M. Campellone, G. Parisi and M. A. Virasoro, "Replica Method and Finite Volume Corrections," *J. Stat. Phys.* **138**, 29-39 (2010). https://doi.org/10.1007/s10955-009-9891-1

sense what we know is that the Sherrington-Kirkpatrick model is defined for all n, not only integer n. You have Z^n, and you can take any possible value of n in the real plane. Now, what you have for integer n [is that] you can write an integral or a matrix of q values. That, of course, coincides with the other. That evaluation is obviously equivalent to the other one. Therefore, this means that the function that you get for the free energy is something that satisfies all the Froissart-type bound to be unique—continuous and so on[290]--because when you know the function with integers it may be continued to non-integer values under some conditions. Now, the point that is a well-posed mathematical problem is suppose that you take a pick for n integer, the approximation in which Z [is done using] an integral. You just approximate the integral with a saddle point with a certain method, but not [only] putting the dominant one, also the sub-dominant one. This will give another function, which is different from the original one. However, one can ask if this is also analytically continued in n. If this is analytically continued in n—this would be a big theorem to prove—you can now start to say: "Well, now I have to study the saddle point of $Z(q)$, and maybe under the saddle point of view I can do an analytic continuation and so on. It's something that in principle could be done, but there is some structure theoresm that are missing that I [don't have] the slightest idea how to prove. Also, I [don't have] the slightest idea what kind of mathematician might have the tools to prove it. Maybe after the Nobel, some mathematician could try to solve the problem. I think that I should write another paper suggesting how. It could be [about] what are the different conjectures to be done.

PC: This sounds like a great proposal for your chapter for the book[291]. At Tor Vergata, at La Sapienza, or elsewhere, did you ever teach a class about spin glasses or replica symmetry breaking? If yes, could you detail?

GP: [4:50:29] Not really. I had, in La Sapienza, a few lessons. Of course, in Les Houches ,I had a course on spin glasses. We started to do, in La Sapienza, six, seven lectures, but at a certain moment I stopped.

PC: Would this have been taught in the '90s?

GP: [4:51:04] I stopped, because I said we should meet next week, next week was not possible and you lose momentum,

PC: So it was not a class, but a discussion group.

[290] Froissart bound: https://en.wikipedia.org/wiki/Froissart_bound

[291] *Spin Glass Theory and Far Beyond*, P. Charbonneau, E. Marinari, M. Mézard, G. Parisi, F. Ricci-Tersenghi, G. Sicuro, F. Zamponi, eds. (Singapore: World Scientific, 2023).

GP: [4:51:14] It was a discussion group with 20 people, in which I was teaching the things. Probably there were some notes that were taken, but at the end I never had a course because it was clearly too advanced for the university. Of course, I could have also a course for the doctorate, but in the end I preferred not to teach in the doctorate. I never did.

PC: So you never had the opportunity to write a pedagogical set of notes?

GP: [4:51:53] Some pedagogical notes were [made] for Les Houches' summer school. [The book was supposed to be pedagogical.]

PC: But almost 20 years after the fact.

GP: [4:52:02] There was something that was started. The idea was that I should do the course at the university and after there should be some pedagogical notes, but this never [happened]. I should look back to the notes, because they should be somewhere but at some moment it stopped. The point is that the more time passes, the more you can understand—also because of mathematical results—what is the essence of the main thing, what was the hypothesis. At the beginning, people were doing a lot of conjectural parts. The most surprising thing that I always found in that business …

PAUSE

GP: [4:55:32] So you were asking…

PC: We were asking about pedagogical notes. You said that as time advances you were getting more insight into what were the assumptions initially made.

GP: [4:55:42] Yes. Things become more different from what one should write in a different time. It's something that maybe we should do, but I don't know if I'll do it now or later on.

PC: One final question. Do you still have notes, papers, correspondence from that epoch? Have you kept your notes, your papers, your correspondence over the years? If yes, do you have a plan to deposit them in an academic archive?

GP: [4:56:31] Not too much, because most of the correspondence from '84-'85 [on] was by email. I remember that written correspondence was not too much. I remember that there was once 10 pages of computation by Marc, which I answered by another 10 pages of computation, sometime by fax.

We were trying to understand the corrections to matching[292]. We were doing the computation with different techniques. Marc was doing it algebraically, I was doing it with cavity, and we wanted to check that the two were the same. I don't have too much. Maybe I have some things, but I have to look back. There's some place where I put some of the correspondence, but I don't think that there's too much correspondence on this subject, because most of things were done after email. Email, the main server changed all the time, so they are not saved sometimes.

PC: Did you save your notes?

GP: [4:58:14] No. I was not well organized with notes. Maybe I have some notebook where I did the computation of replica symmetry breaking in the '80s, but I have to see if I find it. The point is that many times I did computations on scrap paper. Also, I write many things together so one page of this, one page of that is not so useful.

PC: In any case, whatever you do have I encourage you to deposit it in an academic archive.

GP: [4:59:01] Ok. I started to look on for this, but I have had not [enough] time. I have some notes of what I was doing at the beginning of '80, but that is not so much interesting because I was not working on spin glasses. I was convinced that I had something written from '79, but my first attempt to find it, found only the thing from '80. I do not understand if the '79 went in a different position, or if I had a bad memory and only '80 was [preserved].

PC: Is this here, in your Sapienza office, or at home?

GP: [4:59:53] At home. At home, I have a lot of stuff.

PC: Giorgio, thank you very much.

GP: [5:00:02] You're welcome. I think that there's the whole part on jamming that you can cover by yourselves[293].

FZ: Yes!

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[292] See Refs. 176 and 178.

[293] See, *e.g.*, P. Charbonneau, J. Kurchan, G. Parisi, P. Urbani and F. Zamponi, "Glass and jamming transitions: From exact results to finite-dimensional descriptions," *Annu. Rev. Condens. Matter Phys.* **8**, 265-288 (2017). https://doi.org/10.1146/annurev-conmatphys-031016-025334; Giorgio Parisi, , Pierfrancesco Urbani and Francesco Zamponi. *Theory of simple glasses: exact solutions in infinite dimensions* (Cambridge: Cambridge University Press, 2020).