History of RSB Interview: Enzo Marinari

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PC: Thank you very much, Prof. Marinari, for sitting with us. The theme of this interview series is the history of replica symmetry breaking in physics. Before we dive into this topic, we have some questions on background. First, can you tell us a bit about your family and your studies before starting university?

EM: [0:00:26] Sure. I come from the south of Italy, Avellino, which is a small town. My parents[1] were in humanistic studies, both in Italian literature. My mom eventually became an expert of ancient Greek and translated both the Odyssey and the Iliad to Italian (when she was older than 80, around 2010). We moved to Rome when I was 14. I believe they did it somehow for me. They had cultural relations in Rome, in the university environment, in humanistic studies, so they kind of enjoyed it themselves, but I think that was a perfect thing for me. I studied in the Liceo Tasso,[2] which is in these days very much in the newspapers,[3] and then I came to Sapienza. Sapienza was at the time the only physics university in Rome.

PC: What led you to pursue physics at La Sapienza?

EM: [0:01:22] Good point. As I told you I was coming from a humanistic family, so this was not straightforward. For some reasons people expected me to be a lawyer. I was talkative. But then I started to realize that I kind of loved

[1]Dora Tomasone Marinari, https://it.wikipedia.org/wiki/Dora_Tomasone_Marinari;
Attilio Marinari, https://it.wikipedia.org/wiki/Attilio_Marinari

[2] Liceo Tasso: https://it.wikipedia.org/wiki/Liceo_ginnasio_Torquato_Tasso_(Roma)

[3] See, *e.g.*, R. Gressi, "Il liceo Tasso occupato e le punizioni ai figli della Roma bene, per i genitori è l'ora dell'imbarazzo. Protestano anche i ragazzi anti-occupazione," *Corriere Della Sera* (January 19, 2024). https://roma.corriere.it/notizie/politica/24_gennaio_19/liceo-tasso-occupato-punizioni-scontro-bfa20125-b394-4dc7-8bab-edaf238caxlk.shtml

mathematics. It was a not so good professor that [led to] my decision. There was this lady, very nice, but she didn't know much. Then, I realized I was kind of a bit faster than her. No big deal, just very normal. But then I said: "Maybe." Still, I had a feeling that mathematics was not my life. Also, I knew about the physics school in Rome. This is important, because it tells you that this kind of names that go around eventually help people take initiatives. I knew about it, I was reading about it, and then I said: "Maybe this could be the right way." What happened is that I went to my daddy, and I told him: "I'm thinking about studying physics." I did not expect that at all, but he said: "Yes, I'm very happy. But Enzo, do you realize you will never be rich?" I said: "Yes, I do realize it." "Then, go ahead!" Then, I came here and right ahead I loved it so much. I was coming from classical studies. We started calculus here the first year. For me, it was completely new. It was just fantastic. I bought some skis, because I thought: "Now, I'm at university, so I will be very free and spending my time on the mountains." Then, for three years, I didn't go to mountains at all because I was studying all the time, because I loved it.

PC: You mentioned the reputation that physics in Rome had. What was this reputation? How did you encounter to it?

EM: [0:03:23] To be a very good school. I'm from '57, so we're talking about '74-'75, the moment I was deciding, the [middle of] the '70s. Already, you'd go to the newspaper, and you'd read about it, this tradition. Maybe about Amaldi[4] founding CERN, about physics becoming important in Italy thanks to these people in Rome; maybe something about Touschek;[5] this kind of things. People would tell that this was really an excellent environment. I had a feeling—I don't know how it came to me because I was a kid—that an excellent environment could help.

PC: Were there particularly memorable physics courses?

EM: [0:04:32] Luciano Maiani[6] gave a wonderful course of quantum mechanics. The first course of quantum mechanics was Luciano, and it was really nice, honestly. I didn't have Guido Altarelli,[7] who was supposed to be a very good teacher too. Then, I got Nicola Cabibbo[8] for theoretical physics.

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[4] Edoardo Amaldi: https://en.wikipedia.org/wiki/Edoardo_Amaldi
[5] Bruno Touschek: https://en.wikipedia.org/wiki/Bruno_Touschek
[6] Luciano Maiani: https://en.wikipedia.org/wiki/Luciano_Maiani
[7] Guido Altarelli: https://en.wikipedia.org/wiki/Guido_Altarelli
[8] Nicola Cabibbo: https://en.wikipedia.org/wiki/Nicola_Cabibbo

Indeed, it was Nicola Cabibbo [who] brought me first to Francesco Guerra,[9] then to Giorgio when we started to talk about a thesis project. I loved very much this course of Nicola. There were a lot of drawbacks, because he was teaching it at 8 o'clock in the morning—that was already kind of a problem—and he was smoking a cigar. At the time, you would still smoke even inside the university. So, he would come, he would go on his chair, and he would put the cigar here. Since I was a very active student, I was sitting always in the first row, and I had this cigar one meter from me. I survived it! Then, I had Giorgio. This was also exciting because Giorgio was finalizing his book about Euclidean field theory.[10] It was first time he was teaching Euclidean field theory and statistical mechanics. He was even finishing the book, so we kind of helped him. We were looking for mistakes.

FZ: So, Giorgio was already here?

EM: [0:06:11] No. He was not professor here. He was a very young researcher in Frascati,[11] but everybody knew that he was extraordinary. Giorgio's name was really incredibly known around already when he was 24.

FZ: So, he was volunteering as a teacher?

EM: [0:06:47] Yes. As many people from INFN[12] teach now. We have many courses that are given by people from research institutions, CNR,[13] etc. He clearly was going to become a professor—he became a full professor very young—but then he went to Tor Vergata, where eventually I was hired as an assistant professor. He first came here to the engineering department, and then went to Tor Vergata. This was incredibly exciting. Indeed, it brought us to his work, as we'll discuss later on.

FZ: Giorgio was teaching Euclidian field theory and statistical physics and Cabibbo was teaching…

EM: [0:07:28] Theoretical physics. It was basically the first course, plus or minus, not really the renormalization group, [but] just up to the renormalization group. It was the basis, the electroweak model. Then,

[9] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Francesco Guerra*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 27 p. https://doi.org/10.34847/nkl.05bd6npc
[10] G. Parisi, *Statistical Field Theory* (Redwood City, Calif.: Addison-Wesley Pub., 1986).
[11] Laboratori Nazionali di Frascati: https://en.wikipedia.org/wiki/Laboratori_Nazionali_di_Frascati
[12] Istituto Nazionale di Fisica Nucleare: https://en.wikipedia.org/wiki/Istituto_Nazionale_di_Fisica_Nucleare
[13] Consiglio Nazionale delle Ricerche: https://en.wikipedia.org/wiki/National_Research_Council_(Italy)

there were something later on. I don't know because I was still young. Gamma matrices[14]…

FZ: So, statistical physics was taught by Giorgio in this course and then…

EM: [0:08:03] This was new. Then, Carlo Di Castro[15] was teaching the main course. The basics were together with quantum mechanics. I got it taught by Luciano Maiani, who did it very well. I would not be able to tell but obviously his heart was on the quantum mechanics part. And he was really fantastic. I was just young enough that I didn't meet people like Touschek. I met Amaldi, but I never had a personal relation with him. [But] Salvini[16] was very present. I had been talking to Nicola about my thesis work. Then, I had been talking to Francesco Guerra who was starting to tutor me. Even as I was doing my thesis with Giorgio, he was working a bit with us. But then, Giorgio, during the course, one day I was starting my thesis told me: "I have this new problem. Would you like to work there?" (He was already in touch with Francesco Guerra about this, and Francesco was enthusiastic and was very active in our interaction). I said: "Yes!" I knew who Giorgio was, because people knew. Then, Giorgio—this is just amazing—came to me and said: "You know, Enzo, I want to apologize. You know I am not a full professor, so I'd offer you to tutor you, but I don't know if it is appropriate. I don't want to put you in any difficult situation. Maybe you would prefer someone else." I said: "Giorgio, how could you imagine that?"

FZ: So, Di Castro, in his course was teaching stat mech and some disorder?

EM: [0:10:07] No. There was nothing about disorder in the Parisi sense. I can tell you more. It will come. I spent maybe 10 years of my scientific career without knowing much about disorder. There will be a story with Alain Billoire and David Gross,[17] but this comes… I was working in particle physics that is in statistical mechanics because I was working on Wilson's ideas. Then, very soon, I liked numerics very much, so I started to do a lot of numerics, and then there were other projects. But Giorgio, at this point, was still a particle physics guy, very happily. He was just starting. Then, you could see all the signs of statistical mechanics that came.

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[14] Gamma matrices: https://en.wikipedia.org/wiki/Gamma_matrices
[15] "Carlo di Castro," *Accademia Nazionale dei Lincei* (n.d.). https://www.lincei.it/it/content/di-castro-carlo (Accessed February 7, 2024.)
[16] Giorgio Salvini: https://en.wikipedia.org/wiki/Giorgio_Salvini
[17] See, *e.g.*, P. Charbonneau, *History of RSB Interview: David Gross*, transcript of an oral history conducted 2022 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 16 p. https://doi.org/10.34847/nkl.dd4f3kf4

PC: How was the group functioning? Were there group meetings or seminars? How were your interactions? Were you just talking one-on-one with Giorgio?

EM: [0:11:09] A lot was one-on-one. Giorgio was a young guy. In '76, he was less than 30. Then, two things started to happen slowly. One, people from abroad started to come for spending one year or two years with Giorgio. Marc Mézard[18] and Yi-Cheng Zhang[19] came. It was fantastic because this guy was coming from abroad to Italy. He had spent already one or two years in Germany, so he was already very accustomed then [to Europe], but still. Yi-Cheng Zhang was getting notes in Chinese on the Chinese paper written by Giorgio,[20] when he wrote up stochastic quantization, because stochastic quantization was written with Wu during a Chinese trip of Giorgio, so it was published in Chinese. For us, this was exotic. You are talking about '79 or '80. The Spanish friends started to come; Luis Antonio Fernandez was the first,[21] then everybody followed. Then, in Rome, we started to have a little group, because some researchers from particle physics decided to start to work with Giorgio: Mariella Paciello[22], Massimo Falcioni, Bruno Taglienti.[23] So, this was obviously the start of a group. But at the start, much of that was really a personal interaction. Giorgio had many personal interactions, but in my research, I was mainly working with Giorgio.

FZ: Around Cabibbo there was a group of people, but was there no formal group?

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

[19] "Yi-Cheng Zhang - Curriculum Vitae," *Academia Europea* (2022) https://www.ae-info.org/ae/Member/Zhang_Yi-Cheng/CV (Accessed March 1, 2024.)

[20] G. Parisi and Y Wu, "Perturbation theory without gauge fixing," *Sci. Sin.* **24**, 483-496 (1981). https://www.openaccessrepository.it/record/18105/files/LNF_81_017.pdf

[21] 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

[22] See, *e.g.*, Mariella Paciello, "Poche donne in carriera," *Sapere* (August), 52-56 (2002). https://web.infn.it/CUG/images/alfresco/CPO/Sito_CPO/contributi/contributi/articolo_sapere.pdf (Accessed March 1, 2024.)

[23] See, *e.g.*, M. Falcioni, E. Marinari, M. L. Paciello, G. Parisi, and B. Taglienti, "Complex zeros in the partition function of the four-dimensional SU (2) lattice gauge model," *Phys. Lett. B*, 108(4-5), 331-332.

EM: [0:13:18] I was talking to you about the group that was created around Giorgio. The story here started with Gatto.[24] He had very good students, who were called the Gattini, the little cats. That was Maiani, Altarelli, and Cabibbo. Then, Gatto went away and Cabibbo stayed. Luciano Maiani was already a crucial presence here. Nicola has always been on his own. Again, many collaborations in different situations, many talking, many doing things together, but he never had a real…. Ok. Then, he did the APE project.[25] That has been a huge group, obviously. There was an organization. Close to Nicola there were researchers in particle physics, but they have never really been part of my interactions. (Not before Juan [Juan Jesús Ruiz Lorenzo], but that was 8-10 years later.) For particle physics, I started, but for many of my interactions I would go—because it is statistical mechanics—to Saclay.[26] When I moved as a postdoctoral fellow to Saclay, I started to work with the group there, to talk to many people, but maybe you want to go over that later.

PC: You mentioned how you started working with Giorgio for your Laurea thesis.[27] What project were you pursuing?

EM: [0:15:12] It has been fantastic. The project was solving QCD, an ambitious program. Giorgio was really understanding Wilson's ideas.[28] Somehow, Wilson had done his work by having the presence of computers very clear [in his mind]. His work was potentially numerical from the start. Giorgio had that clear. Giorgio had taken the point and this idea that he could do it. So, we started. We started with punched cards to program numerical simulations by Monte Carlo methods of lattice QCD. That was completely new. (Spin glasses will enter in one moment.) When we started, we said: "Ok, we do QCD." We started working and indeed it was very difficult. Eventually, we just said: "Fine. Let us do this little approximation." We took away internal loops. So, we take away the internal fermion loops, we just leave the external lines, and we see how this works. In my thesis, we proposed this kind of approach that is called quenched QCD.[29] Why is that called quenched QCD? Giorgio had started his flirt with glassy systems in some sense and then, with this idea of quenching. So, quenched QCD is a

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[24] G. Battimelli, F. Buccella and P. Napolitano, "Raoul Gatto, a great Italian scientist and teacher in theoretical elementary particle physics," *Quaderni di Storia della Fisica* **22**, 145-169 (2019). https://doi.org/10.1393/qsf/i2019-10065-7

[25] APE100: https://en.wikipedia.org/wiki/APE100

[26] Service de Physique Théorique: https://en.wikipedia.org/wiki/Institute_of_Theoretical_Physics,_Saclay

[27] Enzo Marinari, *Simulazione Numerica di Teorie con Fermioni (Numerical simulation of Theories with fermions*], tesi de laurea, Università di Roma ``La Sapienza'' (1980).

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

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

QCD which has this feedback eliminated. Again, I was saying: “I'm doing something mad for my thesis.” This seemed absolutely ridiculous, but then we did that. Somehow, it was numerical simulations of QCD, also trying to include fermion loops, but introducing the idea that you can take them away and computing observables.

PC: For the subsequent decade or so, you worked on various problems in lattice gauge theory and QCD. What was driving the research program? What questions were you pursuing?

EM: [0:17:56] In our kind of pioneering stuff, it was really a new problem of statistical mechanics. There have always been two basic motivations. As the start, the crucial one was [that] this is something new. Can we understand this kind of Monte Carlo simulation? There were good people in molecular dynamics here. For example, Giovanni Ciccotti[30] was a very important and fascinating person. Giorgio was talking to him very much. I became his friend. When I went to Paris, I was living with him for a few months. There was interaction. The use of Langevin equations in some situations, a few ideas. Also—we will go back to it probably—there were all these tempering ideas for Monte Carlo simulations. They are umbrella sampling, basically. These were things that were a little bit in the field of molecular dynamics and Monte Carlo and were well known and used. This is also thanks to Giorgio very much. He has always been able to talk to many people. That was the first motivation. But behind, there was also: will it be one day a phenomenological tool? Now, it is. In this sense, it has been a triumph. These ideas now really work like a practical phenomenological tool. When I started here, you would go into the department, you would hear Maiani, Cabibbo, and confinement of quarks was really a maybe. People were really not sure at all. All the stories about Giuliano Preparata’s[31] ideas, for example. He was not convinced. He was here at times. When I started to talk to Francesco Guerra, when Francesco Guerra came here from Princeton, he was working on constructive field theory. Cabibbo told me about Francesco Guerra, saying: “He is the person who will explain confinement.” Because of Giorgio, he was led astray, but still. The one of the confinement is not an issue anymore and is well understood because of numerics. The fact is that it is clear now that it is confined. Nobody would discuss it anymore. Still, from the start, I started to also work on problems in statistical mechanics and disorder. The first

[30] Giovanni Ciccotti: https://en.wikipedia.org/wiki/Giovanni_Ciccotti
[31] Giuliano Preparata : https://en.wikipedia.org/wiki/Giuliano_Preparata

were the two papers with Ruelle[32] in Paris.[33] Maybe I can elaborate later if you want.

PC: Please.

EM: [0:20:45] You see, I never got a PhD, because we didn't have a PhD in Italy. Our Laurea thesis was longer than now, and you would go out of it writing two or three papers, and then you would go as a postdoc. That's what I did. So, I went to Saclay. Saclay was generically a wonderful place. It had Roger Balian, Edouard Brézin,[34] Cirano de Dominicis, Jean Michel Drouffe, Claude Itzykson[35], Jean Zinn-Justin, Jean-Bernard Zuber[36] and many others. I was more in the particle group, so working with Alain Billoire, André Morel,[37] Robert Lacaze,[38] Hannah Kluberg-Stern. But I was talking to people. For example, the interaction with Itzykson was very useful. I will comment about his paper on analyzing phase transition by numerically determining Lee-Yang zeros.[39] I did that first when I was there, in Saclay.

Giorgio was visiting and he was somehow going to IHES,[40] because he spent a few months there, and he started to work with David Ruelle. Then, David Ruelle had some ideas about the Sinai potential. So, we wrote a paper together about the power spectrum of the Sinai potential. I met Gleb Oshanin maybe ten years ago in Santa Barbara, and he was working on that.[41] Slightly more recently, but 30-40 years later to anomalous diffusion

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[32] P. Charbonneau, *History of RSB Interview: David Ruelle*, transcript of an oral history con- ducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 4 p. https://doi.org/10.34847/nkl.5330p51b

[33] E. Marinari, G. Parisi, D. Ruelle, P. Windey, "Random walk in a random environment and 1/*f* noise," *Phys. Rev. Lett.* **50**, 1223 (1983). https://doi.org/10.1103/PhysRevLett.50.1223; "On the interpretation of 1/f noise," Comm. Math. Phys. **89**, 1-12 (1983). https://doi.org/10.1007/BF01219521

[34] 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

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

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

[37] See, *e.g.*, M. Bauer, "Disparition d'André Morel," *Institut de Physique Théorique* (July 31, 2013) https://www.ipht.fr/Phocea/Vie_des_labos/Fait_marquant/index.php?id_news=516 (Accessed March 3, 2024.)

[38] "Disparition de Robert Lacaze, physicien théoricien," *Communication CNRS Physique* (June 29, 2020). https://www.inp.cnrs.fr/fr/cnrsinfo/disparition-de-robert-lacaze-physicien-theoricien (Accessed March 3, 2024.)

[39] C. Itzykson, R. B. Pearson and J.-B. Zuber, "Distribution of zeros in Ising and gauge models," *Nucl. Phys. B* **220**, 415-433 (1983). https://doi.org/10.1016/0550-3213(83)90499-6

[40] Institut des hautes études scientifiques: https://en.wikipedia.org/wiki/Institut_des_Hautes_%C3%89tudes_Scientifiques

[41] See, *e.g.*, D. S. Dean, S. Gupta, G. Oshanin, A. Rosso and G. Schehr, "Diffusion in periodic, correlated random forcing landscapes," *J. Phys. A* **47**, 372001 (2014). https://doi.org/10.1088/1751-8113/47/37/372001

analysis and so on. Our paper with Ruelle was maybe my first jump into disorder. It's also true that in lattice gauge theory we were doing a lot of work on phase transitions. At the time, there was really the problem of quantifying the statistical behavior of a lattice gauge theory system. Also, the confining transition, but the problem was relevant in more general terms.

FZ: So, the idea of Ruelle was to learn more generally about statistical physics and phase transitions?

EM: [0:23:11] Somehow, Ruelle had the idea of the behavior of the spectrum of the Sinai potential. We did a kind of numerical verification, and we discussed. We did together this argument about the Sinai potential. Also, Paul Windey was on this paper.

PC: As you were mentioning earlier, you got very much involved in numerical work from early on. Can you tell us a bit what that meant? And the evolution of the technology?

EM: [0:23:45] Also because it leads us to APE and JANUS[42] and what you want to discuss. It started with punched cards here, downstairs. There was a computer downstairs. Eventually, there were VAXes,[43] when things were improved. At the start, I think it was an IBM with punched cards.[44] We had to punch and send. When I was working on my first code… There were really different approaches to programming. Eventually the first VAX arrived in Frascati. VAX meant a friendly operating system and the fact you can program on the screen. That was completely new. So, we started to go Frascati with Giorgio.

I remember one night; Giorgio was very excited, and Giorgio was driving. So, we get in the car, and it was really raining, cats and dogs. So, we are in this car with Giorgio driving in the night. The glass of the car was completely fogged, and Giorgio was writing equations because we are trying to eliminate this determinant, to bring this determinant to the numerator, while driving. I remember me thinking very clearly: "I love so much physics, but do I want to die for that?" I remember that the answer to myself was "no", but still there was nothing I could do. I smiled and I continued. So, we would go to Frascati.

[42] See, *e.g.*, JANUS: http://www.janus-computer.com/ (Accessed March 3, 2024.)
[43] VAX: https://en.wikipedia.org/wiki/VAX
[44] Likely, it was a IBM System/360: https://en.wikipedia.org/wiki/IBM_System/360

This is interesting, because when I went to Saclay, the first Cray[45] had arrived there. It was in Cadarache[46] at the start, so we would work remotely. So, I had a Cray. It was a bit like being an experimentalist. We didn't have internet; very slow modems were not even coming. (I'm talking about '81-'83.) So, we would stay up to three or four in the morning, because the computer was empty in the night. (Guido Martinelli[47] had the same experience a bit later with the same Cray.) That was a bit of our attitude.

Then, at the same moment, when I came back, people started to talk about the APE project. It was a project for a dedicated supercomputer for originally lattice QCD, but eventually it also became a spin glass computer. So, we wanted to try and do that. There had been two factors. Nicola Cabibbo and Giorgio Parisi both felt in love with two different aspects of this story. Nicola loved hardware. In his basement, at home, he had built a cellular automaton machine. That was an idea of Toffoli,[48] at MIT. Toffoli had built a machine like that. Nicola thought: "I can do it." And he did. Now, it is in Giorgio's office. At the same time, Giorgio got in love with compilers. He started to buy books about compilers, optimization. You see why. The first software tool we built together was an optimizer for APE. It was something that was running a "shaker" (moving the order or operations in such a way to optimize the code), hopefully by preserving the meaning of what you wanted to do. So, Giorgio and Nicola started to talk. Nicola was important politically because he was the head of INFN, and then became the head of ENEA.[49] APE was a big Italian project that had a big success because of the incredible competence of Nicola and Giorgio and because it could get political support large enough to pass over barriers.

You see, I had already some experience with the Cray. That had started before. Even before my thesis, I had been in Edinburgh, where there was David Wallace[50] [and] there was Peter Higgs.[51] They had started to do things about parallel computing, both with this computer called MasPar,[52] that was a "stupid" (in the good sense of the word) parallel computer, meaning it would execute the same operation altogether for all its processors. APE is a very similar story. APE was an ape. The idea is that it

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[45] Cray: https://en.wikipedia.org/wiki/Cray
[46] Cadarache: https://en.wikipedia.org/wiki/Cadarache
[47] "Guido Marinelli," *Accademia Nazionale dei Lincei* (n.d.). https://www.lincei.it/it/content/martinelli-guido (Accessed February 7, 2024.)
[48] Tommaso Toffoli: https://en.wikipedia.org/wiki/Tommaso_Toffoli
[49] ENEA: https://en.wikipedia.org/wiki/ENEA_(Italy)
[50] David Wallace: https://en.wikipedia.org/wiki/David_Wallace_(physicist)
[51] Peter Higgs: https://en.wikipedia.org/wiki/Peter_Higgs
[52] MasPar: https://en.wikipedia.org/wiki/MasPar

is powerful, but not so clever. Why? Because we didn't want a computer that could do—the idea of GPU now—many different things at the same time. It was enough for us doing the same thing in parallel many times because at the end your goal is to multiply matrices. This is a bit less true for spin glasses, but still, you have a lattice structure, and this helps you so you can do things. So, there is this a kind of stupid parallelism, but then you can build a computer. Right then, I entered this program. (I created algorithms from the start. My thesis was a bit on algorithms because the idea of simulating QCD was to find algorithms.) Right then, I started to work with Nicola Cabibbo on these algorithms to update SU(3) matrices by using SU(2) embedding.[53] We got a powerful algorithm indeed, so I liked that. So, lattice QCD, for 10 years was a very happy place for me. But then, I was slowly starting to follow Giorgio on the disordered side. It is very difficult not to follow Giorgio. If you look in my curriculum, you see that disordered papers appear, and then more and more of them. Then, there is one year, maybe '93, I moved completely.

PC: Before we move to that, you mentioned your interest in computers, were you at all paying attention to other special purpose computers that people were developing, such as Ogielski's machine[54]?

EM: [0:30:46] Sure. We were both competitors and also colleagues, so we were learning. At the start, it was more Don Weingarten[55] at IBM, Norman Christ at Columbia,[56] and other people (I'm sorry, I am omitting some contributions.) Sure, we were following. It was really an interwoven thing with this fact of having computers and understanding and realizing what Wilson had in mind. Again, I insist on that. It is very clear that Wilson had a precise idea of numerical implementation, and this is one of the real axes of what he did. But then he didn't do the numerical integration, so this had to be done. Then, there was Ogielski and spin glasses. Then, we started with the Spanish friends. (I told you about postdoctoral people coming, we were good friend with many of these people.) They first built a computer that was called SUE.[57] Somehow, we decided to do something together. (Now, I'm jumping 15 years or something.) This was the JANUS project,

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[53] N. Cabibbo and E. Marinari, "A new method for updating SU (N) matrices in computer simulations of gauge theories," *Phys. Lett. B* **119**, 387-390 (1982). https://doi.org/10.1016/0370-2693(82)90696-7

[54] 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

[55] Don Weingarten: https://en.wikipedia.org/wiki/Donald_H._Weingarten

[56] Norman Christ: https://en.wikipedia.org/wiki/Norman_Christ

[57] See, *e.g.*, A. Cruz, J. Pech, A. Tarancón, P. Téllez, C. L. Ullod and C. Ungil, "SUE: A special purpose computer for spin glass models," *Comp. Phys. Comm.* **133**, 165-176 (2001). https://doi.org/10.1016/S0010-4655(00)00170-3

which was born to study spin glasses. The real problem of this computer has always been the software. It has always been easier to build a dedicated, very effective hardware, than to build a reasonably general-purpose software. Also, I must say we were able to do that because there was window of opportunity. Somehow, it was a moment in which very willful people like us could give a real contribution. Now, it would probably be very difficult to build something very specialized and competitive with commercial hardware.

PC: I want to take you back a bit to the late '70s-early '80s. As you said, you were very close to Giorgio at that time, which is when he first formulated the idea of replica symmetry breaking. Were you aware of that happening? Was it being discussed?

EM: [0:33:08] Not so much. At the start, really, not so much. I mean this place was particle physics. There was Carlo Di Castro, Giovanni Gallavotti, Gianni Jona,[58] and others, clearly, so there was an excellent statistical mechanics, but the bulk was particle physics. Giorgio was one of these particle physicists, and even very young one of the most prominent among them. At the same time, he was doing that, but it took 10 years. I can tell you, for example, when David Gross[59] visited Paris, when he did the random energy model with Marc,[60] I was in Saclay. He was working at École normale on the random energy model with Marc, and then in Saclay with me and with Alain Billoire on random surfaces.[61] That was a moment when people wanted to understand Polyakov's ideas. What about the discretization of random surfaces? We didn't understand the right thing. The right thing, the important thing was that you need to also integrate over triangulations. This is crucial because of invariance under diffeomorfisms that you want to preserve in your discretization. If you don't do that, things work but don't do what you wanted to do. Sasha Migdal,[62] Jan Ambjørn,[63] and François David[64] understood that. It was again, on the side—string theory in this case—of particle physics. I didn't know what he was doing with Marc. I didn't even know he was working with Marc. This is a typical example. I started very slowly to approach the field.

[58] Giovanni Jona-Lasinio: https://it.wikipedia.org/wiki/Giovanni_Jona-Lasinio
[59] P. Charbonneau, *History of RSB Interview: David Gross*, transcript of an oral history conducted 2022 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 16 p. https://doi.org/10.34847/nkl.dd4f3kf4
[60] D. J. Gross and M. Mézard, "The simplest spin glass," *Nucl. Phys. B* **240**, 431-452 (1984). https://doi.org/10.1016/0550-3213(84)90237-2
[61] A. Billoire, D. J. Gross and E. Marinari, "Simulating random surfaces," *Phys. Lett. B* **139**, 75-80 (1984). https://doi.org/10.1016/0370-2693(84)90038-8
[62] Alexander Migdal: https://en.wikipedia.org/wiki/Alexander_Arkadyevich_Migdal
[63] Jan Ambjørn: https://en.wikipedia.org/wiki/Jan_Ambj%C3%B8rn
[64] François David: https://fr.wikipedia.org/wiki/Fran%C3%A7ois_David_(physicien)

PC: So, there was no seminar, no presentation where you would have heard?

EM: [0:35:09] At the start, not much. Clearly, when I went to Paris, then yes, so I started to know about it. But if you ask when I was doing my thesis here, no. In Paris, yes, I was exposed.

FZ: When talking to Roberto Benzi the other day, I also understood that the action in disordered systems was mostly taking place in Paris at the time. Rome was really particle physics. Giorgio, if I understand well, was not talking much about disordered systems here; he was talking to people in Paris about it.

EM: [0:35:48] Exactly. It took a little time. Eventually, clearly, things changed.

FZ: But why? In the group of Di Castro, there was interest in disorder. They were working on localization, yet there were no interactions. There was also a workshop in '81 where Di Castro was an organizer,[65] and Giorgio presented.[66]

EM: [0:36:09] There was not much interaction. You should also remember that Giorgio has done crucial work in particle physics. At the time, Altarelli-Parisi,[67] all the unification results, the work with Brézin,[68] Itzykson-Zuber[69] with fixed dimension and regularization and so on. Di Castro indeed already had a statistical mechanics group in some sense, because fast he went to strongly correlated systems. Giorgio, no. Giorgio was the *enfant prodige* of particle physics here. You see, his position was peculiar in some sense, so it took him a little more to start talking about these ideas. He was very good friend of Carlo Di Castro, but it was a very different world.

With Roberto Benzi, Angelo Vulpiani and so on, there were relations. Giorgio was discussing a number of things, for example Fermi-Pasta-Ulam-

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[65] *Disordered Systems and Localization: Proceedings of the Conference Held in Rome, May 1981*, C. Castellani, C. Di Castro, and L. Peliti, eds. (Berlin: Springer-Verlag, 1981).
[66] G. Parisi, "Mean Field Theory for Spin Glasses," In: *Disordered Systems and Localization*, C. Castellani, C. Di Castro and L. Peliti, eds., (Berlin: Springer-Verlag, 1981).
[67] DGLAP evolution equations: https://en.wikipedia.org/wiki/DGLAP_evolution_equations
[68] E. Brézin, C. Itzykson, G. Parisi and J.-B. Zuber, "Planar diagrams," *Comm. Math. Phys.* **59**, 35-51 (1978). https://doi.org/10.1007/BF01614153
[69] 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

Tsingou.[70] We wrote a paper about equipartition in these systems.[71] Again, it was a quite complex idea about it. On that, for example, people were coming from Florence already. Then, Angelo and Roberto were very present.

PC: Fast-forwarding to disorder, from what we could tell it's in the early '90s, with a graduate student of yours, Giulia Iori that you started to work on disordered systems.[72]

EM: [0:38:17] A little bit before, but you're right. It was indeed something like '93, the moment at which you start to have a complete transition. This is also when we proposed the tempering approach with Giorgio.[73] We had this feeling that for numerics in disordered systems you needed something more. We tried. You get a normal Monte Carlo algorithm, you get an Edwards-Anderson model, and you just go nowhere. It was not clear before we tried.

Giulia was our PhD student indeed. Giulia was even something more. She was my entrance in disordered systems, but it was also Giorgio and my entrance in biologically relevant problems. We studied polymers, proteins and so on.

PC: What brought you to these problems?

EM: [0:39:27] Giorgio! Clearly, I was interested in that. I started doing numerics more and more about this kind of projects. That was, I think, more or less the period when Giorgio got his paper out about the immune system.[74] It was too early, as Giorgio would say. There is something that Giorgio used to repeat frequently: you discover something if you are the last person that discovers it, not if you are the first one. After Columbus discovered America no one could discover it anymore because the name was there. So, it was a bit early for a real discovery. We studied biological systems with Giulia. Giulia is now a full professor of economics and finance in

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[70] Fermi-Pasta-Ulam-Tsingou Problem: https://en.wikipedia.org/wiki/Fermi%E2%80%93Pasta%E2%80%93Ulam%E2%80%93Tsingou_problem

[71] F. Fucito, F. Marchesoni, E. Marinari, G. Parisi, L. Peliti, S. Ruffo and A. Vulpiani, "Approach to equilibrium in a chain of nonlinear oscillators," *J. Physique* **43**, 707-713 (1982). https://doi.org/10.1051/jphys:01982004305070700

[72] PhD, La Sapienza (1993). See, *e.g.*, G. Iori, E. Marinari and G. Parisi, "Random self-interacting chains: a mechanism for protein folding," *J. Phys. A* **24**, 5349 (1991). https://doi.org/10.1088/0305-4470/24/22/019

[73] 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

[74] G. Parisi, "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

Venice, on leave from London where she is also full professor. She had a very brilliant career.

APE was already quite advanced at that moment. Also, at the time, the other thing I was still doing a lot, which is a part of my career, was about random surfaces and quantum gravity. We did many simulations again after the paper with David Gross. I worked with Mark Bowick,[75] for example, and other people. That was also approaching more and more a full immersion in the disordered system stuff.

FZ: In the early '90s, when you started this work on simulated tempering, what was the situation of the community on the debate on finite-dimensional spin glasses? There is a point that we don't understand. There were simulations of spin glasses throughout the '80s. Most simulations were done in the US. David Huse[76] said that, from what he remembers, up until the late-'80s, there was no numerical attempt at finite-dimensional spin glasses using a RSB perspective. He remembers that most simulations did droplet-like analysis. Do you remember what was the situation before you arrived in the field?

EM: [0:42:06] We probably started that, but you see the situation was tense.

FZ: This is my question. Was the situation tense because of computational results or of theoretical reasons?

EM: [0:42:26] There was a tension about those two very different ideas that were supposed to be not very amenable one with the other. It was very difficult for us to publish in *Physical Review Letters*. It was a very honest, bona fide, interesting, funny scientific battle, in the best sense of the word.

FZ: But the tension was based on competing theoretical pictures?

EM: [0:42:56] The tension was based on competing theoretical pictures indeed. From when we started numerical simulations,[77] you needed to have tempering, because you could not do anything without an optimized Monte Carlo method. Let say we consider zero magnetic field. I don't want

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[75] See, *e.g.*, M. Bowick, P. Coddington, L. Han, G. Harris and E. Marinari, "The phase diagram of fluid random surfaces with extrinsic curvature," *Nucl. Phys. B* **394**, 791-821 (1993). https://doi.org/10.1016/0550-3213(93)90230-M

[76] P. Charbonneau, *History of RSB Interview: David Huse*, transcript of an oral history conducted 2023 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2023, 28 p. https://doi.org/10.34847/nkl.8717c159

[77] See, *e.g.*, E. Marinari, G. Parisi and F. Ritort, "On the 3D Ising spin glass," *J. Phys. A* **27**, 2687 (1994). https://doi.org/10.1088/0305-4470/27/8/008

to go now to finite magnetic field because I don't know. But in zero magnetic field it was clear from the moment we were able to do these simulations, that the situation was replica symmetry breaking like. There is no doubt because $P(q)$ has a number of peaks, that scale in the right way woth the volume size, because it can be easily established that this is not due to interfaces that appear and disappear, and so on. Again, I think it could be different in finite magnetic field. I'm not completely convinced of what happens as soon as you put a small finite magnetic field. But in zero magnetic field, numerically it always looked clear. Then, you can say: "Fine! This is not the asymptotic regime." Why not? We can discuss all that. Also, when you look at experiments, you find that in experimental conditions you never have more than one million spins that are really interacting. That means that in three dimensions, the correlation length is of order 100. So, you could say: "Fine, numerics say that, but when you go to large lattices something will change. Only a one million cube side lattice works this way… But this is what we really measure in experiments, so this is what is important." But from what we could see from the start, this was very clear.

FZ: So, this was in your simulation in the early '90s. But before that, what were people saying? Do you remember what was the status of the debate?

EM: [0:45:07] I don't remember. Nobody never said that from numerics you would see something droplet-like. I mean from bona fide numerics of the model itself. If you get the Migdal-Kadanoff approximation, you get droplets, as you should.[78] My remembering of the story is that before we started there was not much on numerics. Maybe more dynamical stuff, for example, computations of dynamical correlation functions. Surely, nothing that would tell you that Edwards-Anderson behaves like droplets. I really don't think so. After we started, basically everybody found what we found. You can do different analysis and use it to try to show that it's not really true, that numerics could give something different. Sure! There are numerical problems, but…

FZ: So, the heated debate of the '80s was mostly based on theory arguments. That's what you remember.

EM: [0:46:26] Yes, but we were never talking to each other.

PC: Were there no meetings where people discussed? Was this all through the literature, only papers?

[78] See, *e.g.*, M. A. Moore, H. Bokil and B. Drossel, "Evidence for the droplet picture of spin glasses," *Phys. Rev. Lett.* **81**, 4252 (1998). https://doi.org/10.1103/PhysRevLett.81.4252

EM: [0:46:41] Yes, preprints and referee reports, obviously. There were meetings. People would talk, but I think it has been very... I don't know, because [it was] two groups in different area. We had been talking a lot, from the start to many experimentalists, [Éric] Vincent, [Miguel] Ocio, for example.[79] All people doing very interesting things. Also now, we work a lot with experimental groups (with Raymond Orbach[80] first). I think this is one of the very important parts. Various people, Mike Moore,[81] David Huse, and Daniel Fisher,[82] were doing excellent theoretical work. The problem is theoretical, and it is a theory that is very difficult. The renormalization group computations were done by people on the two sides. So, Mike Moore and co-worker on one side; Cirano de Dominicis, Imre Kondor,[83] Tamás Temesvári and others later "on the side of Giorgio". These people would talk for sure, so there has been common ground.

FZ: Why did you decide in the early '90s was the time to attack the problem numerically and not before? Giorgio was following this debate before, so why then?

EM: [0:48:28] You needed to have a Monte Carlo method that worked. We found that in '92. Also, computers were just not powerful enough. One should never forget the dramatic, exponential increase of computational power with time. Things that now we can do in a cell phone in a few minutes were impossible in months of the best supercomputer center of the world.

FZ: Were you already looking for it?

EM: [0:48:38] Not really. Numerics was less common as a tool of choice. For particle physics, we had introduced it with lattice gauge theories. People were doing molecular dynamics, so it was more like a phenomenological [tool] that reproduced experiments in some sense. This idea that numerics would be the basis for theory and would be able to have strong interaction

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[79] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Éric Vincent*, transcript of an oral history conducted 2023 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2023, 26 p. https://doi.org/10.34847/nkl.04a4bo4n

[80] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Raymond Orbach*, transcript of an oral history conducted 2022 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 23 p. https://doi.org/10.34847/nkl.cfddyh9y

[81] 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

[82] Daniel S. Fisher: https://en.wikipedia.org/wiki/Daniel_S._Fisher

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

with theory was completely new. We started to think about it, probably in the '90s. The computers were very small. You could do very little things, so probably having the APE computer helped, because after working a lot on QCD we thought we could use it also for spin glasses and indeed we did. Then, we started to think about optimized Monte Carlo. There, the presence of Giovanni Ciccotti was very important. I met many of the people—because I was a good friend of Giovanni—that could come visiting and give seminars. There was this idea of parallel tempering and of tempering, but it was not revolutionary because it was brought somehow from another field. But in our field, it was revolutionary. Talking with people in other areas, now maybe it has become a bit easier and bit more common, but it is not trivial at all—because different fields develop different languages—to recognize that some things called with different names in two different fields are the same thing. So, bringing all of this body of work from molecular dynamics—something very similar—to our statistical mechanics of disordered systems was not easy at all. After tempering, as we went fast, to say the truth—we can maybe later come back to it—now we're a bit blocked again probably because of temperature chaos.

PC: We have brought up tempering many times already, but I would now like to dive more specifically into it. Was the motivation for looking at simulated tempering to study spin glasses explicitly?

EM: [0:51:16] Yes, for us. We tried the random field Ising model because it was easier, but it was for spin glasses from the start. It was clear there were high free energy barriers. It was clear that Monte Carlo would not pass those barriers. How can we be smart and pass these free energy barriers? It was completely clear that this was the question. Other groups were doing the same. For example, we were interacting sometimes with Hukushima.[84] All groups really were looking into these other fields and try to bring it to us.

PC: How was the work received? You said there were many other people were working on similar ideas at about the same time.

EM: [0:52:06] Very soon, it was clear that there was an effective way to do this with parallel tempering. There has been just a little bit [of arguments] about the priority and on the name.[85] It's still on, but in very friendly terms,

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[84] 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
[85] Parallel tempering: https://en.wikipedia.org/wiki/Parallel_tempering

I must say. Also, people in statistics were working on that, so there are contributions from them.

PC: How did the realization that all these ideas were very similar take place? Was there a meeting where you all met?

EM: [0:52:50] Just the papers. I never met probably many of the other people. I had a bit of correspondences. So, papers. Then, very soon people started to see this could also be a way to do optimization in an effective way. *Spin glass theory and beyond*[86] was there, so it was known that optimization is important.

PC: At about the same time as you worked on these ideas, you started studying models that had self-induced disorder.[87] What drew your interest in these models and the possibility that quenched disorder was not needed to have spin glass-like behavior?

EM: [0:53:56] The Bernasconi model[88] looked very interesting because it was this clean statistical model, but it was so difficult to go down in the free energy landscape. Then, there was somehow this intuition from Giorgio that this could be a way to apply RSB to situations in which disorder was not quenched but was dynamically generated. Clearly, glasses came to mind right ahead. Also, you remember those random matrix models, Giorgio was a real expert [on], because Giorgio had worked in the French environment about that. Very soon, he started to connect about all that he knew about that, and this is what came out.

PC: Were you aware of the work that Mézard and Bouchaud[89] were doing very similar ideas and about the same time? Was there a discussion?

EM: [0:55:11] Probably, yes. I never discussed that with Marc. Very probably Giorgio would have discussed it with Marc, but maybe later on. These things were going fast sometimes. It was competitive in a good sense. For me, I remember this period as an intense few months' work on the precise issue and its clarification. But I'm pretty sure Giorgio probably would have discussed it with Marc when things were done.

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[86] M. Mézard, G. Parisi and M. A. Virasoro, *Spin Glass Theory and Beyond* (Singapore: World Scientific, 1987).

[87] E. Marinari, G. Parisi and F. Ritort, "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

[88] J. Bernasconi, "Low Autocorrelation Binary Sequences: Statistical Mechanics and Configuration Space Analysis," *J. Physique* **48** 559 (1987). https://doi.org/10.1051/jphys:01987004804055900

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

FZ: We're asking this because this is another thing that we don't understand. There was the work of Kirkpatrick,[90] Thirumalai and Wolynes[91] in the late '80s, from '87 to '89 in the US.[92] From what we understand, they were not talking much to other people, so it went largely unnoticed. Then, in the mid-'90s, there is a rush in Europe where it seems that everyone wanted to do models without quenched disorder to do glasses. There was your work, there was the work Bouchaud and Mézard, there was also the work of Kurchan and Cugliandolo.[93] What happened?

EM: [0:56:42] All these people were in touch with one another.

FZ: Why did it suddenly become interesting to do this type of models?

EM: [0:57:01] There was a bit this idea that for getting a clear recognition (and maybe even a Nobel prize) spin glasses were not ideal and that glasses would be far better. This was present. I remember talking to Nicola Cabibbo. Nicola didn't appreciate Giorgio working on glasses. Nicola had the wonderful taste in physics and, in general, in life. He had this idea: "You know. The glass community is too large. It's too close to engineers. They will never really recognize him. Giorgio is throwing his time away." That turned out to be somehow wrong, I believe, but I understand very well why he meant by that. Somehow, the main idea was as follows. Spin glasses are a wonderful technical accident. This technical accident maybe will be precious. For what can it be precious? By the way, Giorgio never loved so much "real" applications. He loved optimization, for example. Also, biology was always a bit… He entered because we did have an idea**.** He loves better a fundamental problem. So, glasses, in this sense, were the obvious choice, because glasses would bring you toward interest for many and would be a very relevant contribution.

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

[91] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Peter G. Wolynes*, transcript of an oral history conducted 2023 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2024, 37 p. https://doi.org/10.34847/nkl.3df5b08z

[92] See, *e.g.*, T. D. Kirkpatrick and D. Thirumalai, "Random solutions from a regular density functional Hamiltonian: a static and dynamical theory for the structural glass transition," *J. Phys. A* **22**, L149 (1989). https://doi.org/10.1088/0305-4470/22/5/003

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

FZ: So, the idea was to find an application of RSB within physics to a fundamental problem in physics.

EM: [0:59:01] If that could work.

FZ: But did you know, at the time, the work of Kirkpatrick, Thirumalai, and Wolynes?

EM: [0:59:15] I didn't know till later. I cannot tell you exactly. But when we wrote these three papers, no. This is not [what] we were looking for.

FZ: And they were not in touch. Did they ever write to you?

EM: [0:59:30] No. Not to me. Maybe to Giorgio.

FZ: It seems that Peter Wolynes was visiting Europe at some point.

PC: He went to Paris in '93-'94.

FZ: So, this was already after. Were you already on that?

EM: [0:59:50] Yes. This was '94-'95. But in the end, Giorgio was right. You are right, somehow. The emergence of numerics in this field was at a very clear point, then it lasted a bit, then we are to see what will happen. I will repeat myself, but there were two main ingredients: the powerful computers that you needed and the algorithms.

PC: We were talking about the computers just before. Can you tell us a bit about how APE transitioned to doing simulations of spin glasses?

EM: [1:01:04] APE was doing two things. APE was a project that was very close to being an industrial project because Nicola cared. There were four generations. So, there has been this line that was mainly Nicola. Then, there was this line of programming on APE. What happened is that we did lattice QCD as a statistical mechanics problem from the start. But I think Giorgio's interest was in having to really do phenomenology. There was a moment in which Guido Martinelli basically took the lead for analyzing lattice QCD problems: real phenomenological tools, weak interaction physics, computing physics you could not access only with perturbation theory. At this point, for us there was a bit less to do because the fundamental questions had been in some sense clarified. Also, it started to be clear that you could implement interesting numerics with spin glasses. So, at this point, we started. We evaluated the fact that it was possible, meaning that APE could be a very reasonable spin glass computer, because the number of data you would have to process would be enough to keep the computing processors busy, and it would work. I think we were losing a factor of two or four in performance compared to lattice QCD, but in any case, it was such a fantastic tool for that. So, we decided to go ahead. We wrote the program. Very soon, we started to design JANUS. The Spanish friends got funding. That was completely different because it was an FPGA[94]-based computer.

PC: Relative to the simulated tempering ideas, did it happen at the same time as APE moved to spin glasses?

EM: [1:03:09] Yeah. It's exactly the same idea. The same time.

PC: The same idea and the same drive.

EM: [1:03:21] Yes.

PC: Getting back to the finite-dimensional spin glasses. When you wrote these first papers in the mid '90s, what was the response? How did that fuel or interact with the debate on from a theoretical standpoint? Was there immediately some pushback or was it accepted?

EM: [1:03:55] Again, the droplet versus replica symmetry breaking dispute has never been very open-minded, I would say, on both sides. These results were clear; they were correct. But then there is an interpretation. It was always something like: "Yes. Numerics look like Sherrington-Kirkpatrick but maybe there are interfaces." It was always a bit defensive and a bit attacking. So, you would do new simulations, for example, on the idea of analyzing small observable quantities in small cubes that grow. So, not global observables, but things that are small and grow. It was fun. It was interesting, because you would use theory to think about new numerics you could do to answer theoretical questions of people that didn't think about it like that. That was more than challenging. But it's never led to contradiction, I believe. At the end, we'll see. Again, we are at this point blocked probably by temperature chaos because after so many years we cannot do something larger than 32^3,[95] because you cannot really thermalize. It looks quite clear it's because of the physical properties of the system. But from what we can see at the moment, the situation is quite clear.

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[94] Field-programmable gate array (FPGA): https://en.wikipedia.org/wiki/Field-programmable_gate_array

[95] See, *e.g.*, R. Alvarez Baños et al. "Nature of the spin-glass phase at experimental length scales," *J. Stat. Mech.* P06026 (2010). https://doi.org/10.1088/1742-5468/2010/06/P06026

PC: In a review in 2000,[96] you mentioned the importance from a theoretical standpoint of addressing the difficulties highlighted by Newman and Stein[97] through your simulation work. How did the conversation with Newman and Stein, in particular, take place? Was this again only through the paper trail? Or were there meetings, visits, or conferences?

EM: [1:05:59] Mainly papers, again. There would have been some meetings, but really it was mainly papers. It is really something that has gone through papers. We would read, we would study, we would think, we would study we would think. We would try to find things. This is what I just told you before. We would try to find new numerics to answer to doubts. Before Nicolas Read,[98] it looked like Newman and Stein were a kind of RSB killing. Then, it's not like that, on the contrary. Newman and Stein have been doing very important mathematical work. In the start we would somehow try to understand how could a given point of view not be true and how to come with clear and simple answers. At the same time, we were talking to mathematicians who were trying to prove things. Francesco Guerra,[99] Michel Talagrand,[100] Dmitry Panchenko[101] and many others. With Francesco, we would always be talking a lot. Francesco has always been very present, close to us, discussing, trying to prove that the Parisi solution was right. It was not clear. Even Giorgio, I believe, before the mathematical proof was not completely convinced that he had the correct solution—even eventually waiting for Panchenko about ultrametricity[102]-- [that] every detail of the Giorgio solution was the right thing. It was clear that it was sensible and accurate, but being the exact was another story. There, rigorous mathematics has been…

PC: As you mentioned several times already, the numerical simulation of finite-dimensional spin glasses became its own research program, through

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[96] E. Marinari, G. Parisi, F. Ricci-Tersenghi, J. J. Ruiz-Lorenzo, 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

[97] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Charles M. Newman and Daniel L. Stein*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 35 p. https://doi.org/10.34847/nkl.3dbc3ja3

[98] Nicholas Read: https://en.wikipedia.org/wiki/Nicholas_Read

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

[100] 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

[101] See, *e.g.*, "Dmitry Panchenko," *Mathematics Genealogy Project* (n.d.). https://www.genealogy.math.ndsu.nodak.edu/id.php?id=99699 (Accessed March 10, 2024.)

[102] D. Panchenko, "The Parisi ultrametricity conjecture," *Ann. Math.* **177**, 383-393(2013). https://www.jstor.org/stable/23350562

JANUS and JANUS-II, which persists to this day. How did this come to develop? What led this to become a big enterprise?

EM: [1:08:24] It was something you could do. It was something that was giving results. [As] you know very well, the renormalization group is very difficult in these systems. (Very good and smart people, among whom Cirano De Dominics,[103] spent even all their life doing that.) But being completely sure about what is happening it's still an open question. What would you do? Ok, you get droplet. Yes, there is a droplet theory, we know, we understand. We got replica symmetry breaking, the Parisi solution. Then, what do you do? It's clear that in this scenario, you need very good ideas to do well the renormalization group, from theoretical point of view. You need new approaches, such as for example M-layers[104], where for example Maria Chiara Angelini has contributed a lot. Clearly, Giorgio is putting energies on that, introducing new ideas all the time. And, as we are discussing, numerics is there to help.

PC: What enabled such a large program to exist? It's a lot of money, for one thing.

EM: [1:09:37] There has been some Spanish funding, that has been very important. Giorgio's personality has also been crucial. It goes in many directions, because for example all these Spanish young researchers came to Italy, eventually got back to Spain: Victor Martin-Mayer, Juan Ruiz-Lorenzo, Luis Antonio Fernandez, Alfonso Tarancón. They eventually came back to Spain, became full professor, and became relevant figures. Giorgio's ideas, Giorgio's capabilities of international relations, then these people started… I'm telling that because, for example, JANUS came from dedicated Spanish funds. In Italy, it was more difficult. In Italy we did APE because there was Nicola, but then it was not clear that it would end up this way. There are computer scientists that will not always agree, that sometimes do not have very open minds (and foresee reasonable risks in such an enterprise). Because you asked specifically about a powerful research program that needs money. For example, these Spanish colleagues, they had become important in their universities. They believed in this project, and it has been a success. There was BIFI.[105] BIFI is a research center in Zaragoza. I've been director for physics of BIFI for five years. From the start, BIFI was a very interesting experience of something

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

[104] See, *e.g.*, A. Altieri, M. C. Angelini, C. Lucibello, G. Parisi, F. Ricci-Tersenghi and T. Rizzo, "Loop expansion around the Bethe approximation through the M-layer construction," *J. Stat. Mech.* 113303 (2017). https://doi.org/10.1088/1742-5468/aa8c3c

[105] Instituto de Biocomputación y Física de Sistemas Complejos (BIFI): https://en.wikipedia.org/wiki/Institute_for_Biocomputation_and_Physics_of_Complex_Systems

with complexity, network, and quantitative biology. Alfonso Tarancón has been instrumental in creating it. They put a lot of energy on that. This is somehow the kind of construction that come up.

PC: So, EU-wide funding was never the drive, and it was more a Spanish effort?

EM: [1:11:40] No. JANUS has been more Spanish. Somehow, APE has been Italian, fully Italian. INFN has been very important in the APE project. This activity was not really based around computer scientists, and it was in this sense an anomaly. In order to get the money for this anomaly, you need a strong presence of the field. So, the local field is a bit more adapted. If I go to a funding in Europe to a computer science call for building a computer, I don't have any chance.

PC: We know that you taught the disordered system class at La Sapienza for many years. Can you tell us about the genesis of this class? How did it come about?

EM: [1:12:41] I've been teaching it for close to 20 years. Now, Federico Ricci-Tersenghi is teaching it.

PC: You created this class, right?

EM: [1:12:53] Kind of. Let me finish one phrase. Now, I'm teaching what Giorgio was teaching before retiring. I'm teaching critical phenomena. Giorgio had the idea of teaching this class about disordered systems. He had introduced it—but very different from what I eventually did—for maybe two years. He was teaching already critical phenomena, so he could not teach too much. He had a bit this idea, but he did not really had the possibility to put a strong effort in it. When I came here, when I became full professor, I was given that in my hands, and asked to do something about it. It was still a moment in which people in Italy many times would have to move to become full professor. It was better, I believe, because you could become full professor earlier, and there would be a real interchange of people and ideas. So, I went to Cagliari as a full professor, where I spent a few years, (For example, Angelo Cacciuto was my student there, and he has now tenure at Columbia.[106]) Before that I was in Tor Vergata as an associate professor. I went to Cagliari as a full professor, and then Giorgio in the meantime had moved back to Sapienza from Tor Vergata. I came here, and I was thinking about what to teach. Giorgio had proposed something, but he didn't have the time to develop it. I said: "I

[106] See, *e.g.*, Angelo Cacciuto, *Statistical mechanics of self -avoiding crystalline membranes and topological defect formation*, PhD Thesis, Syracuse University (2002).

could introduce that." Then, Francesco Zamponi, has been one among the first students, a very wonderful student.

FZ: When did you start the class?

EM: [1:14:37] I came back in '99. Maybe 2000-2001.

FZ: So, before then, disordered systems were not part of the curriculum?

EM: [1:14:48] No. Again, Giorgio had started maybe for two years to do a course.

FZ: Was Di Castro, in his class, not doing any disorder at all?

EM: [1:15:02] Not in the Parisi way. Di Castro was more field theory and strongly correlated systems.

FZ: The students who eventually worked with Giorgio, where did they get their training before you started your class?

EM: [1:15:20] They would get a course from Giorgio and then they would just learn. I introduced something quite organized: the diluted systems, random field Ising model, spin glasses, replicas. For example, I would discuss supersymmetry and discuss Parisi-Sourlas.[107] That was always very nice to the students, because clearly here supersymmetry used to be something meant as particle physics. Students would see it is not an easy computation but more or less I was going through, and they were at ease with it.

FZ: What was the reaction of the department when you proposed that?

EM: [1:16:11] There was no problem. They never wanted it to be a compulsory course, but I never wanted my courses to be compulsory courses. I like that a student comes if he likes it. I can be even more advanced in this way. If it's mandatory, you have to keep it simple. So, they were friendly. Not enough to say that every student in statistical mechanics should take it, but I have always had many students, because students were interested. Indeed, I think all young people who are now professors here have taken it.[108] (Federico Ricci-Tersenghi is not young enough.)

[107] Parisi-Sourlas: https://en.wikipedia.org/wiki/Supersymmetric_theory_of_stochastic_dynamics
[108] For instance, Chiara Cammarota and Francesco Zamponi.

PC: Had you taught about spin glasses before teaching this class, or was this the first time?

EM: [1:17:02] Giving a course about spin glasses, it was the first time. The course really, I conceived it for that, putting together all the material. It was non-trivial, because I had to select something that I could present to students in a reasonable way. I wanted it still to be quite advanced. For example, writing the Parisi-Sourlas in a way that I felt was both rigorous enough and accessible to students, that was not completely trivial. Remember that the Parisi-Sourlas paper is one page and a half.[109] It's a PRL with one full page and one half on the back. It's nothing you could approach a student with. There were some things done later on, but still… These, for example, I could probably do. I would not do, for example detailed replica symmetry breaking computations. I would do, in detail, a replica symmetric computation, and then I would discuss the physics of replica symmetry, but there was no time in 60 hours of teaching for going out there. So, these I would not do in details. But I would do things I had a feeling the students should be able to get completed.

FZ: Another subject that you could tell us about is how computational physics emerged as a subject in the teaching here.

EM: [1:18:48] This is indeed very interesting. We wrote a book, and this was very much related to all the history of this department, to Cabibbo and Parisi and their passion for hardware and software. They had given us an imprinting. They gave us a way to do things. Giorgio himself had invented a course of C++ that he called *Programmazione++*, *i.e.*, *Programming++*. He has given it a few years, and it was very enjoyed by many students. Now, we are starting it again with a new teacher, Cristiano De Michele.

FZ: When? In the '90s or later?

EM: [1:19:44] In part of the period going from 2005 to 2015.

FZ: When I was a student,[110] he was not teaching that. He was teaching probability at the time. I think it's after that.

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[109] 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

[110] Francesco Zamponi was a student at La Sapienza from 1997 to 2005. Francesco Zamponi, *Un metodo per la misurazione delle temperature interne negli stati stazionari fuori dall'equilibrio: relazione illustrativa, Laurea thesis*, Università di Roma "La Sapienza" (2001); *Some applications of recent theories of disordered systems*, PhD Thesis Università di Roma "La Sapienza" (2005).

EM: [1:19:55] Maybe just after. He invented this course. But let me go back to my answer. This thing that had been built here, this peculiar approach to physics and computers deserved some dedicated teaching. So, I got in touch with Federico Ricci-Tersenghi and with two colleagues that are in particle physics, Luciano Barone and Giovanni Organtini, and we started to think about a book that would really teach students by telling them how to program on the problems they are studying in physics and mathematics at the same time[111] (we were trying to be quite general in such a way that it could also be useful out of physics). We wanted to understand everything about the computer. So, learning how to program, but learning how a chip is done, how a register is done. What is the speed of the communication bus compared to the speed of a central processor? We were looking at it as the antiparticle of an Excel course. Something in which you really learn how this tool—the computer—is built, and that helps a lot in order to use it for your work. That's why we wrote a book. Then, we go very close to statistical mechanics, and even to disordered systems. There are the Sinai potentials; there are random walks and optimization and percolation and Monte Carlo. I think this has been something good in this department. Was this course already on when you were a student?

FZ: No.

PC: Did you teach the class a few times and then he wrote a book, or did you write a book and then decided to teach it?

EM: [1:22:21] We wrote it at the start. It’s more like we wrote a book, and then we taught it.

PC: Had there been no tradition of teaching computational methods in physics before that?

EM: [1:22:37] No. There was really nothing. Also, computers were far less important. For me, I had two hours of Fortran when I was a student in the laboratory course. (I am talking about 1978-1979.) Nothing was there. It's true the world changed. We tried to really have programming as the core of an approach to physics. I think this has been something good. It works. We have now had this for the more than 15 years, close to 20, probably. Two courses—one first year, one second year—that are completely connected. The students can program a language. It is not the most important part, but they understand what a computer is.

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[111] L. M. Barone, E. Marinari, G. Organtini, F. Ricci-Tersenghi, *Programmazione scientifica. Linguaggio C, algoritmi e modelli nella scienza* (Milano: Pearson, 2006); *Scientific programming: C-language, algorithms and models in science* (Hackensack, New Jersey: World Scientific, 2014).

PC: We're approaching the end of our discussion, so is there anything else you would like to share with us about this era that we may have missed or overlooked?

EM: [1:23:38] Let me see. There are a number of contributions about two-dimensional spin glasses that may be of interest.[112] Also, maybe one thing we should quote are the biological application of all that. Because eventually I started to work more on these. Both neural networks and Hopfield models, both applications of statistical mechanics but not really replica symmetry breaking. It's really the path that Giorgio designed. I've done a lot on metabolic networks. I've been working with Terry Hwa,[113] for example, in two different phases. We analyzed the problem of alignment many years ago.[114] More recently, we've modified flux balance analysis using constraints that make it far more physiological and allow you to understand far more,[115] and complex systems based on micro-RNA interactions in biological systems.[116] Somehow, this is worth remembering. Again, it's not strictly a replica symmetry breaking, but it is the application of these ideas of statistical mechanics in disordered environments and complexity to a world where they can be very important. On these, I have done many [works] recently. Now, again more on Hopfield models.[117]

PC: I had never heard you describe yourself as a biophysicist. Where does your connection to biology come from?

EM: [1:25:26] It has been intense for a period. Talking to biologists is not easy. I have a feeling that computer scientists are a bit easier. Talking to them is easier, for a number of reasons. In the end, I've been working mainly with

[112] See, *e.g.*, T. Jörg, J. Lukic, E. Marinari and O. C. Martin, "Strong universality and algebraic scaling in two-dimensional Ising spin glasses," *Phys. Rev. Lett.* **96**, 237205 (2006). https://doi.org/10.1103/PhysRevLett.96.237205; L. A Fernández, E. Marinari, V. Martín-Mayor, G. Parisi and J. J. Ruiz-Lorenzo, "An experiment-oriented analysis of 2D spin-glass dynamics: A twelve time-decades scaling study," *J. Phys. A* **52**, 224002 (2019). https://doi.org/10.1088/1751-8121/ab1364
[113] Terry Hwa: https://de.wikipedia.org/wiki/Terry_Hwa
[114] T. Hwa, E. Marinari, K. Sneppen and L. H. Tang, "Localization of denaturation bubbles in random DNA sequences," *Proc. Nat. Acad. Sci. U.S.A.* **100**, 4411-4416 (2003). https://doi.org/10.1073/pnas.0736291100
[115] M. Mori, T. Hwa, O. C. Martin, A. De Martino and E. Marinari, "Constrained allocation flux balance analysis," *PLoS Comput. Bio.* **12**, e1004913 (2016). https://doi.org/10.1371/journal.pcbi.1004913
[116] A. Martirosyan, M. Figliuzzi, E. Marinari and A. De Martino, "Probing the limits to microRNA-mediated control of gene expression, *PLoS Comput. Bio.* 12(1), e1004715 (2016). https://doi.org/10.1371/journal.pcbi.1004715
[117] See, *e.g.*, M. Benedetti, E. Ventura, E. Marinari, G. Ruocco and F. Zamponi, "Supervised perceptron learning vs unsupervised Hebbian unlearning: Approaching optimal memory retrieval in Hopfield-like networks," *J. Chem. Phys.* **156**, 104107 (2022). https://doi.org/10.1063/5.0084219

physicists: Andrea De Martino has been a crucial relation when he was here in Sapienza, in Rome, and now he is in in Torino; Olivier Martin in Paris, until he really moved to a plant laboratory. I've been talking to biologists that are delightful people, but it is not easy to build a real interaction. I don't know. Maybe I didn't study enough to describe myself even as a tentative biophysicist. I tried hard, I must say. But eventually, I see [myself] as someone who does physics. But, yes, I have applied these ideas much to biology. Now, the way we are trying to look at neural networks is a bit of the same kind of approach. I have moved a bit so this is my best summary: flux balance analysis and metabolism, micro-RNA interactions and now I do a bit more a Hopfield-model like stuff.

PC: In closing, do you still have notes, papers, or correspondence from that epoch? If yes, do you intend to deposit them in the physics department archives at some point?[118]

EM: [1:27:32] Yeah, I have some things. Probably not so much because email came quite soon. I have, for example, things from the APE period. Some initial logbooks, they're interesting. So, yes, everything I have. I don't think it will be much to store, but everything worth to store I will make sure to deposit there. I can deposit it. I'm very happy about it.

PC: Professor Marinari, thank you very much for this discussion.

EM: [1:28:06] Thank you very much to both of you.

[118] "Archivi personali dei fisici del dipartimento di fisica e di altri scienziati," *Dipartimento di Fisica di Sapienza-Università di Roma* (n.d.). https://archivisapienzasmfn.archiui.com/oggetti/5782-dipartimento-di-fisica-di-sapienza-universita-di-roma (Accessed March 3, 2024.)