History of RSB Interview: Francesco Guerra

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PC: Hello, Professor Guerra. Thank you very much for joining us. As we've discussed, today we will be mostly discussing the history of replica symmetry breaking in a broad sense. Before we get there, however, we wanted to ask you a few questions about your background and interests. In particular, could you tell us what led you to be interested in physics and to pursue a Laurea degree in theoretical physics?

FG: Like many people in Italy, in the first two years of university I was enrolled in engineering, like Ettore Majorana[1] for example. Then I found that I was more interested in basic scientific questions, so I decided to shift to physics. It was the start in Naples at the time—it was '62—of new, big enterprises in physics. In Naples, there was the old institute of physics and they were involved mostly in classical physics, spectroscopy, solid state physics, and so on. Then, in the middle of the '50s, modern physics underwent an impressive development. My big boss at the time was Eduardo Caianiello[2]. (Ed**u**ardo, not Ed**o**ardo. In Italian, it's normally Ed**o**ardo, but in Neapolitan spelling it's Ed**u**ardo. You should also know that Eduardo is a famous novel writer and actor in the Neapolitan theater[3].) He was in Princeton, and he won the competition for a full professorship in Italy. At the end, he was called in Naples in 1955. He started to develop modern physics—nuclear physics and theoretical physics—in Naples, starting from zero, essentially.

[1] See, for instance, Francesco Guerra and Nadia Robotti, *Ettore Majorana: Aspects of His Scientific and Academic Activity* (Pisa: Edizioni della Normale, 2008).
[2] Eduardo Renato Caianiello: https://en.wikipedia.org/wiki/Eduardo_R._Caianiello .
[3] Eduardo De Filippo: https://en.wikipedia.org/wiki/Eduardo_De_Filippo

In particular, an advanced school of physics was founded, because it was necessary also to train people who had already graduated but did not have any exposition to modern physics. So he started some advanced school in physics. The first lecture—I was a student in high school, so I was not there—was given by Werner Heisenberg in '59. It was a big success. They had a lot of money, so they could call on many good people. I was still interested in engineering, but in '62 I knew that there was this big development in Naples. The central quarter was quite near to my house, adjacent to the Zoo and an exhibition park, so there was no problem with moving by bus. I decided to go to physics, and in particular, I was interested in theoretical physics. Not in mathematics, but in theoretical physics with mathematical methods.

There was very good teaching. I can tell you that, for example, Gianfausto Dell'Antonio[4] was our teacher in quantum mechanics, and he gave beautiful lectures. I could tell you, word by word, all lectures on quantum mechanics: the discrete spectrum, the continuous spectrum, wave packets, observables and so on. It was very, very good. There were other people in experimental physics also very good, who essentially formed a good school. Giulio Cortini[5] and Ettore Pancini[6], two important experimental physicist at the time, were called to join the faculty in Naples. Ettore Pancini did a very important experiment with Marcello Conversi[7] and Oreste Piccioni[8]. They made a very important experiment just after the war[9], such that they discovered the true nature of the mesotron[10], this quite heavy particle in the cosmic rays. At the beginning, the mesotron was believed to be the particle associated by Yukawa to the nuclear interaction[11], and so called also the Yukon, but they discovered that this mesotron did not interact with the nuclei. Essentially, it was the beginning of elementary particle physics, because the study of many particles began. It is well known that only after some years the true Yukon particle imagined by Yukawa was

[4] Gianfausto Dell'Antonio: https://de.wikipedia.org/wiki/Gianfausto_Dell%E2%80%99Antonio
[5] Giulio Cortini: https://it.wikipedia.org/wiki/Giulio_Cortini
[6] Ettore Pancini: https://it.wikipedia.org/wiki/Ettore_Pancini
[7] Marcello Conversi: https://en.wikipedia.org/wiki/Marcello_Conversi
[8] Oreste Piccioni: https://en.wikipedia.org/wiki/Oreste_Piccioni
[9] See, *e.g.*, W. A. Wenzel, R. A. Swanson and W. A. W. Mehlhop, "Oreste Piccioni," *Phys. Today* **56**(4), 80 (2003). https://doi.org/10.1063/1.4729372
[10] Mesotron is the name originally given to mesons: https://en.wikipedia.org/wiki/Meson
[11] See, *e.g.*, J. L. Spradley, "Particle Physics in Prewar Japan: A rapid assimilation of Western science culminated in Yukawa's prediction of the meson fifty years ago," *American Scientist* **73**(6), 563-569 (1985). https://www.jstor.org/stable/27853487

discovered in the cosmic rays by Giuseppe Occhialini[12], Cecil Frank Powell[13] and others. Pancini was professor in Naples, so we had professors who were at the forefront of research. Moreover, Cortini and Pancini participated to the Resistance during the German occupation of Italy during the last years of WWII. There was a kind of heroic aura surrounding them.

So I completed my study. I wrote a research thesis on liquid helium[14]. A quite good work. After almost 60 years, it is still essentially correct. It's not completely rigorous from a mathematical point of view, but it is essentially correct. I began the usual career. I got a fellowship from the National Council of Research[15]. Then, I became a researcher in the Naples section of the National Institute for Nuclear physics[16], which was a very important institution in Italy. The university did not have so much money for research at that time, but there was this National Institute for Nuclear Physics and we could get some money [there]. We could participate in conferences and so on. It was quite good. Then, I got a temporary professorship, so that I entered an academic career. I did some work on renormalization theory[17], and I decided, at the end, to go abroad.

My idea was Princeton, because in Princeton there were Eugene Wigner[18], Arthur Wightman[19], and many other good people. I wrote to Arthur Wightman that I would like to go there. After some time—there was some kind of negotiations—I got a position in Princeton. Surely, I got great support from Eduardo Caianiello, but also Gianfausto Dell'Antonio, who was visiting in Princeton at that time.

I arrived in Princeton in September 1970. At that time I was interested in quantum field theory, especially constructive quantum field theory[20], which was the program of Arthur Jaffe[21] and James Glimm[22]. Very difficult

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[12] Giuseppe Occhialini: https://en.wikipedia.org/wiki/Giuseppe_Occhialini
[13] Cecil Frank Powell: https://en.wikipedia.org/wiki/C._F._Powell
[14] Unpublished work.
[15] The Consiglio Nazionale delle Ricerche: https://en.wikipedia.org/wiki/National_Research_Council_(Italy)
[16] Istituto Nazionale di Fisica Nucleare: https://en.wikipedia.org/wiki/Istituto_Nazionale_di_Fisica_Nucleare
[17] See, *e.g.*, F. Guerra and M. Marinaro, "Divergence of renormalized vs convergence of regularized perturbative expansions in a field-theoretical model," *Il Nuovo Cimento A* **42**, 285-305 (1966). https://doi.org/10.1007/BF02717920; E. R. Caianiello, M. Marinaro and F. Guerra, "Form-invariant renormalization," *Il Nuovo Cimento A* **60**, 713-755 (1969). https://doi.org/10.1007/BF02757301
[18] Eugene Wigner: https://en.wikipedia.org/wiki/Eugene_Wigner
[19] Arthur Wightman: https://en.wikipedia.org/wiki/Arthur_Wightman
[20] Constructive quantum field theory: https://en.wikipedia.org/wiki/Constructive_quantum_field_theory
[21] Arthur M. Jaffe: https://en.wikipedia.org/wiki/Arthur_Jaffe
[22] James G. Glimm: https://en.wikipedia.org/wiki/James_Glimm

program, with very few people involved, but I was able to give some essential contributions, so Princeton was a very good place. I solved some important problems. Just to give a joking representation, I recall that during the tea break at the Department of Physics in Princeton, Arthur Wightman explained me what the problems were, and Edward Nelson[23] explained to me one method—the so-called Euclidean quantum field theory he had developed—without knowing that it could be applied there. So I took Nelson's method and applied it to Wightman's problems[24]. It was just to put together two things. These are miracles in Princeton. If you stay at tea time and you hear what people are talking about—and if you are not completely idiot—you produce good work. So I was able to do good work there.

I then came back to Italy. In the first national competition for full professorship in theoretical physics [to follow], I was able to win. This was a terrible thing, because there were many very good applicants. Consider that at that time, Luciano Maiani[25]—one of the most important scientists in Italy and in the World, who invented flavor physics and anticipated the standard model of elementary particles—was also candidate. We were young, of course. Even Luciano Maiani at some point was young. We participated in this competition. There were enough positions so that I could succeed. Then, I had to stay as full professor in Salerno, because the rules were complicated and stringent. In the end, one should go somewhere where there was a position participating to the competition, and I arrived in Salerno, a small city south of Naples. Then I was called as full professor in Rome "La Sapienza" in 1979, in the frame of an ambitious program to relaunch modern mathematical physics, started by Sergio Doplicher[26] and Giovanni Gallavotti[27].

PC: Before we go there, if you don't mind, you said you were working in quantum field theory at that point, as you did for long stretches of your career as well. How did you select research questions? Was there a program? How were you influenced?

[23] Edward Nelson: https://en.wikipedia.org/wiki/Edward_Nelson

[24] F. Guerra, "Uniqueness of the Vacuum Energy Density and van Hove Phenomenon in the Infinite-Volume Limit for Two-Dimensional Self-Coupled Bose Fields," *Phys. Rev. Lett.* **28**, 1213 (1972). https://doi.org/10.1103/PhysRevLett.28.1213
F. Guerra, L. Rosen and B. Simon, "Nelson's symmetry and the infinite volume behavior of the vacuum in $P(\varphi)_2$," *Comm. Math. Phys.* **27**, 10–22 (1972). https://doi.org/10.1007/BF01649655

[25] Luciano Maiani: https://en.wikipedia.org/wiki/Luciano_Maiani

[26] Sergio Doplicher: https://en.wikipedia.org/wiki/Sergio_Doplicher

[27] Giovanni Gallavotti: https://en.wikipedia.org/wiki/Giovanni_Gallavotti

FG: [0:12:21] I mean, quantum field theory in the frame of the constructive program. Let's go back a little bit to the '60s. In the '60s, quantum field theory was not very popular[28]. Matrix theory, duality, sum rules, bootstrap were at the basis of the general frame to describe elementary particles. Why was it not very popular, quantum field theory? Quantum field theory had great success in explaining quantum electrodynamics, but starting already at the beginning of the '50s, it was clear that there were big difficulties with the strong interaction. There was not any good result on which to build a perturbative expansion, for example, for meson theory and so on. So most people left quantum field theory in favor of matrix theory, duality and so on. There were very few people who were strongly in favor of quantum field theory, yet at one point one should find the right way to go forward. For instance, Arthur Wightman had first exploited the road to axiomatic field theory, exploiting only the general properties based on quantum mechanics, locality and relativistic covariance, trying to build from this axiomatic point of view a field theory good for the entire particle physics. Consider that quantum field theory was a tradition very strong starting from Heisenberg, Dirac, and Yukawa in the '20s and '30s.

Very few people in the '60s were still interested in quantum field theory. Arthur Wightman in Princeton may have been the most important one. But also, in Naples, there was Eduardo Caianiello who had worked on it at the beginning of his career. He kept investigating the renormalization methods in quantum field theory, trying to build some non-perturbative scheme for strong interaction. I was, of course, his co-worker; he was 20 years older than me, so I consider him one of my important mentors. I must say that Gianfausto Dell'Antonio, who is only 10 years older than me and who was my teacher in Naples, was also involved in quantum field theory. A small number still believed in quantum field theory. Arthur Wightman shifted from axiomatic field theory to the so-called constructive field theory. The project was to build up firstly the theory in a simplified formulation by putting cutoffs on relativistic quantum field theory, with well-defined approximate correlation functions. The last, and most difficult step, was to remove the cutoffs. Cutoffs typically are infrared, so the system is in a finite volume, and also ultraviolet, so that only quite low energy modes interact. Then you remove the cutoffs and find the right correlation functions, and the final theory.

Princeton was surely the good place to pursue research in quantum field theory, and this is the reason I decided to go there. I was lucky because I was supported by Eduardo Caianiello and Gianfausto Dell'Antonio, and at

[28] See, *e.g.*, T. Y. Cao and S. S. Schweber, "The conceptual foundations and the philosophical aspects of renormalization theory," *Synthese* **97**, 33-108 (1993). https://doi.org/10.1007/BF01255832

the end Arthur Wightman gave me a job, so that I could reach the faculty in Princeton. Marvelous! At that time—it was September 1970—I had already three young boys and we were there. My boys went to school, so they began to speak English much better than I was able to do. I stayed there, in total, almost three years with some intervals. First at the department of physics in Princeton University, and then at the Institute of Advanced Studies[29]. It was a very productive time. Also, I initiated a collaboration with Barry Simon[30] and Lon Rosen[31], who were marvelous, because they were so smart and they were able to do anything, especially on the mathematical side[32]. Then, I was back. At the end, after some years, I was in Rome as full professor.

PC: At RSB40[33], a couple of years ago, you mentioned in your presentation that while you were at Salerno, you invited Giorgio Parisi to give a talk at the Institute of Physics, around 1978. How did you know that important developments were at the horizon?

FG: [0:18:26] At this time, I was at this small university, Salerno, south of Naples. There were very few professors, so automatically, by default, Caianiello was the dean of the faculty, and I, by default, was the director of the institute of physics. I had some money, and I was interested to build a good research program.

In particular, I knew that Giorgio Parisi was very smart. The first time I met him was in 1975, in a conference in Lecce, in the south of Italy[34]. But I knew him in a very good way from what Kurt Symanzik[35] told us in a conference in Capri in 1973[36]. Kurt Symanzik, a legendary hero in theoretical physics, was very fond of Parisi, then very, very young. From what he said—in Capri 1973— I was not surprised at all, when Giorgio Parisi made a marvelous

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[29] Francesco Guerra was at the Institute of Advanced Studies from 9/1975 - 6/1976, https://www.ias.edu/scholars/francesco-guerra (Last consulted April 4, 2021).

[30] Barry Simon: https://en.wikipedia.org/wiki/Barry_Simon

[31] Lon Rosen, https://www.researchgate.net/profile/Lon-Rosen

[32] See, *e.g.*, F. Guerra, L. Rosen and B. Simon, "The P(ϕ)2 Euclidean Quantum Field Theory as Classical Statistical Mechanics," *Ann. Math.* **101**, 111-189 (1975). https://doi.org/10.2307/1970988

[33] 40 years of Replica Symmetry Breaking: A conference about systems with many states, Sapienza University of Rome, Italy, September 10-13, 2019. https://sites.google.com/view/rsb40/home (Last consulted April 4, 2021)

[34] Convegno aspetti strutturali e ideologici nel rapporto tra scienze fisiche e matematiche, Lecce, Italy, 1-5 July, 1975. Proceedings: F. Guerra, "Sviluppi Recenti della Teoria Quantistica dei Campi. Linee di Tendenza e Considerazioni Generali," in E. Donini, A. Rossi and T. Tonietti, eds., *Matematica e fisica: struttura e ideologia* (Bari: De Donato, 1977).

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

[36] School of the NATO Advanced Study Institute, Capri, Italy, July 1973. Proceedings: *Renormalization and invariance in quantum field theory*, ed. E. R. Caianiello (New York: Plenum Press, 1974).

career, had important results, important prizes, appointments, the academies and so on. Symanzik had told me he was very smart, and Symanzik could not be wrong.

So I invited Parisi to Salerno. He was involved with spin glasses. Spin glasses were, and are, a terrible problem, for both experiments and theory. It is very difficult to work on them from the experimental side. From a physical point of view, they admit excitations with a very long life time. So it is difficult even to have a spin glass at thermal equilibrium. What is more important is that the models of spin glasses which were built were extremely interesting from a theoretical point of view. They were essentially frustrated models. They were similar to ferromagnetic systems. But in a ferromagnet different domains try to stay together, so that you can get spontaneous magnetization and so on. In the famous Sherrington-Kirkpatrick spin glass model—a mean-field model, but that is terribly complicated, each spin interacts with every other spin with a properly rescaled interactions—the interactions can be at random, either ferromagnetic or antiferromagnetic. So there is competition between the sites. This makes the problem terribly complicated. It's very simple to write, but very difficult to handle. In any case, I know well that, for example, David Sherrington is very proud to have invented this model, because so many papers have been written over the course of almost 50 years. Parisi was very well-known for being able to solve important problems, like Altarelli-Parisi[37], so clearly he was involved also in this complex system.

PC: Did you know of this problem, and of spin glasses, before he gave a talk? Is this a problem you were aware of?

FG: [0:22:25] Of course, no. We were involved in constructive quantum field theory. If you take enough cutoffs in the Euclidean frame—we worked in the Euclidean frame, a kind of world where time is imaginary, very beautiful, very useful—quantum field theory is a ferromagnet. So it has spontaneous magnetization, for example, even in two or three dimensions. I was strongly influenced by the idea that at very small scale quantum field theory is essentially ferromagnetic. There can be no frustration; there can be none of this kind of complexity. In fact, one of the important tasks of theoretical physics is to move away from simple non-frustrated problems. With the spin glass you move toward complex problems, where complexity has a meaning quite deeper with respect to what is understood in mathematics, in information science and so on. This is quite deep.

[37] 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. See also DGLAP Equation: https://en.wikipedia.org/wiki/DGLAP

If you consider the humanities, history, or let's say the art of government, politics and so on, there, complexity is everywhere. Even the basic behavior of the animal: fight of flight shows important aspects of complexity. When you are in a difficult situation, either you fight another animal or you flight, you run away. This is based on very complicated, complex notions. If the animal does not fight, but he always flies—runs away—then he will never eat anything. If he is willing to fight and does not pay attention, in the cases where it would be better to run away, then he is bound to make terrible mistakes. My friend, Miguel Virasoro, tells it in the following way. If you make the mistake of confounding a tiger with a cat—you believe it is a cat, but as a matter of fact it is a tiger—this is a mistake you will do only once in your lifetime. Fight or flight is the first complex system, and this works in the brain of any animal. So we have to learn how simple animals make their reasoning in order to, at the end, understand the spin glass. Still, we do not do so completely, but there has been a lot of progress.

So I invited Giorgio Parisi for a talk to Salerno. I knew him from the conference in Lecce 1975. We had also some kind of discussions on political aspects. (Take into account that in Italy politics is very much discussed by people involved in scientific research, at least at that time). In Salerno he talked about spin glasses, what is the problem and so on. He still did not have the solution, but he was very near. After a few months, suddenly the solution came to his mind, and so we learned a lot of very important things. In the usual case of a statistical mechanics system—like in a standard ferromagnet—you have a very small number of order parameters, typically magnetization, for example. But in the spin glass, you have a big number of order parameters. Essentially, the order parameter in the Parisi theory is of infinite dimensions. It looks likes Le Châtellier's principle is violated, but it is not so, of course. In any case, the order parameter is of infinite dimension. So, again, Parisi was able to give the solution.

Let's talk about the free energy. In spin glasses it is given through a variational principle, involving the order parameter, which is in the opposite direction with the usual variational principle in statistical mechanics, where free energy is lowest among all possible values. The second principle of thermodynamics will drive a system to the lowest possible free energy, which is the equilibrium. But the Parisi variational principle is the opposite. The free energy is supremum, so to speak, with respect to the order parameter. There was a rumor that the referee report for this paper by Parisi was negative[38], because the guy who was a very good physicist, of course, said: "No! In statistical mechanics—in physics, thermodynamics if you like, in a very general way—the free energy must be the infimum over all possible configurations of the system, taking into account the entropy and the energy. Now, Parisi tells us that the free energy in spin glasses is the supremum. This cannot be true. So the paper was not very well appreciated, at the beginning. Many years after, I asked Parisi: "Is it true that you got a negative report?" He said essentially that this was true. So it all began with people having difficulties to understand it. I must say that, first of all, I have been always sure that it was true. But to understand it and to prove it, this was very difficult.

PC: This brings us to the question: How closely you were following this work? He came and gave a talk in Salerno and…

FG: [0:31:59] At that time, I was working—my field was quantum field theory—on constructive quantum field theory. Problems connected with gauge invariance, in particular, were important in trying to understand confinement on quarks and so on. I must say that I began to be interested in a productive way in complex systems and spin glasses, because inside the spin glasses there is a gauge symmetry. So I said to myself: "We know so much about gauge theory in quantum field theory, maybe this will be useful also for these spin glasses." This is in fact the story of many people, like Jorge Kurchan[39], who studied these gauge structures in spin glasses in a very deep way.

So we're at the beginning of the '80s, when Parisi had a good idea. He wrote marvelous papers. I always say that it is very important to study complex systems and, in particular, spin glasses, which are a kind of a paradigm, a kind of basic example. From a scientific point of view—also from a historical, philosophical and so on point of view—it is interesting to understand how Parisi was able to solve the basic problem of this field with this so-called replica method, ultrametricity and so on. How could he? In fact, I must say—we speak among friends—that I always associated the idea of giving some contribution to spin glasses also with the important task to try to understand how Parisi did reach some results, which is very important. Because if one understands this, then any kind of problem… If one knows the method, so to speak…

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[38] G. Parisi, "Infinite Number of Order Parameters for Spin-Glasses," *Phys. Rev. Lett.* **43**, 1754 (1979). https://doi.org/10.1103/PhysRevLett.43.1754

[39] Jorge Kurchan: https://fr.wikipedia.org/wiki/Jorge_Kurchan

I think that the method is very important. The method, in general, is very important. This is science like, of course, Galileo Galilei proves, but we can find a lot of cases… The method of Enrico Fermi, for example. Also interesting is the method of Ettore Majorana. I'm interested not only in the results, but also in the method. I believe… (This is of course my opinion. You know that I'm not only a friend of Giorgio Parisi, but an admirer.) I think that he really left an imprint on many fields. Not only for the results, but also for the method. So we should study the results and the method. I don't know if you would like that I talk about the replica trick, and Parisi's understanding and use of the replica trick.

PC: If you have insight into the method that got Giorgio to the replica trick, that is important. However we understand the physics of the replica trick, so it is not necessary for this interview.

FG: [0:36:30] The Parisi method is very tricky. It's based on physical intuition. There is a very nice sentence of Schiller, who said that: "The genius stays constantly in contact with nature, what the one will promise, the other—nature—will allow."[40] So intuition is relevant. You see how things happen. Galilei Galileo and all physicists think about what happens. We can imagine Maxwell's equations because the change of the electric field produces a magnetic field and vice versa, and at the end you have electromagnetic waves. By contrast, our intuition about complex systems is very much reduced.

For example, as an aside, consider how difficult it is to have a theoretical understanding of medicine. Of course, physicians are scientists. In a broad sense, they are scientists. They know that if you have some disease, then you can cure it using some chemicals, so that you face the effects of the disease. But since the system is complex, of course, the cure will have other consequences. There is the whole problem of medicine, which is the science that studies the medical countereffects on mechanisms and so on. The iatrogenic studies investigate the effects of the cure on the disease. There are people who are extreme, they think that all diseases have a iatrogenic origin. You can read in the old books, for example, when there was this Spanish disease in 1918. This was terrible, of course, the Spanish flu. It was terrible because it hit very strongly very healthy young people: the soldiers at the end of the First World War. But surely there was help from a new medicine, which had been evolved and which was considered mi-

[40] "Mit dem Genius steht die Natur in ewigem Bunde//Was der eine verspricht, leistet die andre gewiß." From: "Kolumbus" in *Friedrich Schiller: Sämtliche Werke* (Münich: 1962), vol. 1, S. 163. http://www.zeno.org/nid/20005595509 (Consulted May 10, 2021)

raculous. This is aspirin. Aspirin was considered miraculous, and some people used big amounts of aspirin to fight this Spanish flu. In some cases, it was a disaster. There is complexity there. Using aspirin is very useful in some cases, but in some other cases can be very dangerous.

Parisi, first of all, has the merit—maybe more his mother or his father have the merit—to have received a brain which has a kind of intuitive understanding of complexity. Of course, I speak in modern terms. How this happens, I do not know. Then, at that time and even later, Parisi had other very good ideas and methods: numerical simulations. You know that he also, with Nicola Cabibbo[41], was involved in the APE project to build a big computer for elementary particle physics, statistical mechanics, and so on[42]. When you use computer simulation, you get some results, but he was able to see inside the numerical simulation for some deep understanding, so that he could build by intuition the elements of a solid scientific thinking about some important qualitative and quantitative results on the behavior of complex systems. I'm sure, for example, that ultrametricity came from this line. If you make numerical simulation, you never find, of course, ultrametricity, because these systems evolve very slowly. But you can find something which will be the beginning, so to speak, of ultrametric behavior, which in the end you find through mathematical analysis of this system. So the Parisi method is based on mixing physical intuition and the proper understanding of numerical simulation.

Since I am not very good in numerical simulations and physical intuition is what it is, far from Galileo or Fermi or Majorana, so to speak, then I was obliged to resort to something which is also strong, namely mathematical rigor. I tried to attack interesting physical problems with mathematical rigor. Not mathematics for the sake of mathematics. This is not very interesting. But mathematics to understand the structure of systems, in particular complex systems.

FZ: Can I ask you a question about this? In the '80s in Rome and in Italy in general, there was a quite strong mathematical physics community. Do you know how the mathematical physics community responded to the solution of Giorgio? It seems that you were the only one at the time who considered working on this problem. Why did other people not think about…?

[41] Nicola Cabibbo: https://en.wikipedia.org/wiki/Nicola_Cabibbo
[42] APE100: https://en.wikipedia.org/wiki/APE100

FG: [0:45:51] This is a very interesting question, because there is a very interesting history of mathematical physics in Italy. I shall recall a few aspects of this, because it is interesting.

Mathematical physics was a very serious enterprise. Consider the '20s, for example. There were very good people, like Vito Volterra[43], Levi-Civita[44], and others. If you look at the papers of Vito Volterra, they are extremely interesting. Also, he graduated in physics. He made also very important contributions to what we now call theoretical physics. But then, there were many chairs in mathematical physics because rational mechanics[45] and mathematical physics were important courses in the university. It was necessary to have many people, and of course you cannot always find people like Vito Volterra, or Levi-Civita, Federigo Enriques[46]**,** and so on. So the field, on average, was quite depressed in the sense that there were some old problems in the 19th century that were studied and studied again, such as fluids, elastic bodies, and so on. Most of the research in mathematical physics in the '20s and the '30s was not of a very high level, if you forget Volterra, Levi-Civita and a few others. Then, there was an important event. I think it has not been studied in a careful way, yet it is very important.

In 1926, there was in Italy a national competition for a chair at the University of Cagliari in mathematical physics[47]. The rules at the time were the following. There were the applicants, and there was a committee. The competition was for one chair, but the committee produced a list of three people who were good to have the chair, and they gave the order: first, second and third. It was perfect. The committee was made of five people and three would get to this, by forming a majority. Among the applicants, there was Enrico Fermi[48]. Enrico Fermi was very smart and very good. At that time, he already had a very good production in mathematical physics. He had juvenile papers written at quite a young age, very good mathematical physics. For example, he gave important contributions to general relativity, to electromagnetism, to dynamical systems. So he was a very good candidate. The committee surely was favorable, because Vito Volterra and Tullio Levi-Civita were there. They were the strongest persons in mathematical physics. Everybody would have done what they would have asked.

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[43] Vito Volterra: https://en.wikipedia.org/wiki/Vito_Volterra

[44] Tullio Levi-Civita: https://en.wikipedia.org/wiki/Tullio_Levi-Civita

[45] Analytical mechanics. Rational mechanics is a calque from the Italian: https://it.wikipedia.org/wiki/Meccanica_classica#Discipline_della_meccanica_razionale

[46] Federigo Enriques: https://en.wikipedia.org/wiki/Federigo_Enriques

[47] See, *e.g.*, F. Guerra and N. Robotti, *The Lost Notebook of Enrico Fermi* (Berlin: Springer, 2018), 6. https://doi.org/10.1007/978-3-319-69254-8

[48] Enrico Fermi: https://en.wikipedia.org/wiki/Enrico_Fermi

And there were the other people, who were quite good, but were part of the old mathematical physics. When they arrived to a decision, they would immediately agree on who the three people would be on the winning triplet. The problem then was to choose the first, then the second, then the third. Then, there was a split. I have analyzed this. The very deep fact is that there was among the three a physicist, Giovanni Giorgi[49], who was the inventor of the MKS system of units which is still exploited. He was very good, and he was quite connected to all the world. Consider that Giorgi took the Laurea degree in physics when Fermi was born, so he was more than 20 years older. At the end the committee, with a vote of three against two–the two were Volterra and Levi-Civita—voted that the first would be this Giovanni Giorgio, the second Enrico Fermi, and the third Rocco Serini, who was good but was the third. The chair was in Cagliari. So it looks like Volterra and Levi-Civita were defeated, essentially, because they were two against three. So Giovanni Giorgi went to Cagliari. Cagliari, you know, is on the island. You have to make a long trip by boat to go to Sardinia. For some months, it looked like—the other two people could be called in other universities if there was a position—there was no position. So at the end, Orso Mario Corbino[50], the director in Rome said: "We make a new competition for full professor, but this time it will not be in mathematical physics, it will be in theoretical physics." Here, of course, Fermi won. This was the end of '26. At the beginning of '27, Enrico Fermi was the first chair in theoretical physics in Italy. This also is told, but the problem for academicians, especially very powerful academicians like Orso Mario Corbino, is to have more positions in order to have more people on the positions. With this trick of making a new competition, they lost one position in mathematical physics, because Fermi was the second, but they could earn three more positions in total, so they could let other people win, so it was very good. But as a matter of fact, Fermi could have been easily called into mathematical physics. It was necessary to have a new chair in mathematical physics, but for Corbino—Levi-Civita and Volterra were in the faculty in Rome—it would be very easy, and Fermi would have gone to mathematical physics.

What was the consequence? The consequence was that theoretical physics was enhanced**.** You can see, even now, what is theoretical physics in Italy. Surely, it was the merit of this big *exploit*[51] at the beginning. But mathematical physics lost Fermi, and so it was left without the only person who could have produced a big advance toward modernity, which is still

[49] Giovanni Giorgi: https://en.wikipedia.org/wiki/Giovanni_Giorgi
[50] Orso Mario Corbino: https://en.wikipedia.org/wiki/Orso_Mario_Corbino
[51] Feat, in French.

felt right now. One should think about that. Mathematical physics was scientifically weaker because Fermi could not get a chair.

FZ: Despite that, in physics there were quite a lot of researchers that were interested in mathematical aspects, like Gallavotti, Giovanni Jona-Lasinio[52] and others.

FG: [0:56:20] I was talking about the end of '26. The point is the following. Fermi was not professor in mathematical physics, so he could not properly develop the field from an academic point of view: to participate in the commissions, in the committees for professorships and so on. Then, many people were involved in the racial laws[53], even Volterra and Levi-Civita. Volterra, during the racial laws, was not hit because he was already put outside. He did not take the oath in favor of the fascist regime in '31, so he was put on a pension already in 1931. But Levi-Civita, who was victim of the racial laws, was also put in pension. In the end, the field took an enormous hit, and the mathematicians wanted to have the chairs lost because of the racial laws go to mathematics. In particular, in mathematical physics there was a lowering of the level. Even during the '60s—we are talking about this—consider that Francesco Calogero[54], one of the main theoretical and mathematical physicists in Italy since a long time, in a competition for full professorship in mathematical physics did not go into the winning triplet. I mean the '60s! Francesco Calogero! Next competition, in the '70s, Francesco Calogero still did not win. I must say that Sergio Doplicher did not win, and—I'm not being very modest—also I did not win in mathematical physics. I won in theoretical physics, as Calogero and Doplicher did. So the field in mathematical physics was depressed.

You mentioned, of course, Giovanni Gallavotti, Sergio Doplicher and so on. I tell you, Sergio Doplicher, who had written the famous papers with Haag, Roberts and so on[55], and was an authority in quantum field theory, in '74, he participated in two competitions: in mathematical physics and in theoretical physics. He lost in mathematical physics, and won in theoretical physics. There is a connection. So Doplicher still is a (now emeritus) professor of theoretical physics, not mathematical physics. This was essentially a disaster.

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[52] Giovanni Jona-Lasinio: https://en.wikipedia.org/wiki/Giovanni_Jona-Lasinio
[53] Italian Racial Laws: https://en.wikipedia.org/wiki/Italian_racial_laws
[54] Francesco Calogero: https://en.wikipedia.org/wiki/Francesco_Calogero
[55] See, *e.g.*, S. Doplicher, R. Haag and J. E. Roberts, "Local observables and particle statistics I," *Comm. Math. Phys.* **23**, 199-230 (1971). https://doi.org/10.1007/BF01877742

In the competition for Cagliari (in Sardinia) in ’71, Giovanni Gallavotti was among the applicants. It was clear that the traditional mathematical physicists would not have considered him. Fortunately, Gianfausto Dell’Antonio was able to explain informally to the members of the Committee that Giovanni Gallavotti was very good. In the Committee there was also Luigi Salvadori[56] from Trento, an open-minded mathematical physicist formed in Naples, who helped to convince the others. As a matter of fact, Giovanni Gallavotti, even if very young, had already done important research in statistical mechanics. He solved important problems on the Ising model at the end of the ‘60s[57]. He was a co-worker of Salvador Miracle-Sole in Marseille. So, in the end, Giovanni Gallavotti succeeded in the big enterprise, in which Fermi was essentially unable to succeed: he won a full professorship in mathematical physics. At the end, Gallavotti was called in Rome, and founded an impressive school in modern mathematical physics. Why was he not interested in spin glasses? Because at that time he was interested in other problems of statistical mechanics and dynamical systems, so he was in completely different fields. He also worked on quantum field theory with the renormalization group methods.

The position of the mathematicians toward the theory of Parisi was extremely cold. They did not understand the problems and the depth of Parisi’s methods. Most of them, did not know anything. They did not understand. The replica trick is so deep, so profound. The replica trick is not run according to traditional mathematics, so mathematicians did not follow the reasoning. If you read even the book by Michel Talagrand[58] on spin glasses… Michel Talagrand is a great mathematician, with great contributions to the understanding of the Parisi solution, to the understanding of ultrametricity and so on. But he said, for example, that the replica trick has no real meaning, except for the end result, which is the Ansatz written by Parisi. So there is no good understanding of the mathematicians toward the Parisi theory. While the good modern mathematical physicists, as for example Giovanni Gallavotti, are interested on important different problems on statistical mechanics, dynamical systems or ergodic theory and so on. It is also important to say that the scientific focus of Giovanni Gallavotti is in physics, not in mathematical physics. There are papers by Gallavotti on fluctuations for systems toward equilibrium which have thousands of

[56] Luigi Salvadori (1925-2019). Marco Rossi, “SALVADORI, Luigi," in *Enciclopedia Italiana - V Appendice* (Roma: Istituto della Enciclopedia italiana, 1995). https://www.treccani.it/enciclopedia/luigi-salvadori_%28Enciclopedia-Italiana%29/

[57] See, *e.g.*, G. Gallavotti and S. Miracle-Sole, "Statistical mechanics of lattice systems," *Comm. Math. Phys.* **5**, 317-323 (1967). https://doi.org/10.1007/BF01646445; "Correlation functions of a lattice system," *Comm. Math. Phys.* **7**, 274-288 (1968). https://doi.org/10.1007/BF01646661

[58] Michel Talagrand: https://en.wikipedia.org/wiki/Michel_Talagrand

citations[59], a very unusual number in mathematical physics. The ideas in these papers stimulated also an intense experimental activity.

In the '60s, there was an important school between Florence and Rome, with Raoul Gatto[60] and Nicola Cabibbo, who were involved in elementary particle physics, without using quantum field theory. At the beginning Gallavotti was one of them, together with other good people of the Rome-Florence school of theoretical physics. Very few people know that one of first jobs in the life of Giovanni Gallavotti was to teach nuclear physics. During the '60s, many people beyond Giovanni Gallavotti, as for example Brunello Tirozzi, Mario Pulvirenti[61], Carlo Boldrighini and others, began their scientific career in connection with this important Gatto-Cabibbo school, whose members were called the gattini, which means "small cats"[62] (Gatto-gattini). Among the gattini we can count physicists of the stature of Luciano Maiani, Guido Altarelli[63], Giuliano Preparata[64], Gabriele Veneziano[65], Franco Buccella, and many others, including Giorgio Parisi, at the start of his research career. At the end of the '60s, some of the gattini left elementary particle physics and moved toward modern mathematical physics by changing objectives and methods, but keeping their elan toward scientific excellency and international recognition. This process would be worth to analyze from the point of view of the history of physics.

But other people were interested in spin glasses. In particular, I was very interested. I began to think about, and tried to give some results from a rigorous mathematical point of view. At one point in the '80s, I began to think that there was nothing to do. Of course there were rigorous results in the high temperature region, as for example in the work of Michael Aizenman, Joel Lebowitz, David Ruelle[66], and others, but in the low temperature regime, the whole business was only based on physical intuition and numerical simulations. But Leonid Pastur[67], an important mathematician in the Soviet Union of the '80s, came to Rome—invited by Giovanni Jona-Lasinio—and talked about spin glasses at the beginning of the '90s.

[59] See, *e.g.*, G. Gallavotti and E. G. D. Cohen, "Dynamical ensembles in nonequilibrium statistical mechanics," *Phys. Rev. Lett.* **74**, 2694 (1995). https://doi.org/10.1103/PhysRevLett.74.2694

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

[61] Mario Pulvirenti: https://en.wikipedia.org/wiki/Mario_Pulvirenti

[62] R. Casalbuoni and D. Dominici, "Il maestro dei gattini/The teacher of the gattini (kittens)," *Il Colle di Galileo* **7**, 47-69 (2018). https://doi.org/10.13128/Colle_Galileo-24234

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

[64] Giuliano Preparata: https://en.wikipedia.org/wiki/Giuliano_Preparata

[65] Gabriele Veneziano: https://en.wikipedia.org/wiki/Gabriele_Veneziano

[66] M. Aizenman, J. L. Lebowitz and D. Ruelle, "Some rigorous results on the Sherrington-Kirkpatrick spin glass model," *Comm. Math. Phys.* **112**, 3-20 (1987). https://doi.org/10.1007/BF01217677

[67] Leonid Pastur: https://en.wikipedia.org/wiki/Leonid_Pastur

In particular, he proved on the blackboard an important theorem, that if the overlap order parameter is self-averaging, then necessarily the solution of the spin glass should be the Sherrington-Kirkpatrick replica symmetric solution[68], which, of course, we know is wrong. Not completely wrong, but it is wrong. It was possible to be rigorous! You start from assumptions—the order parameter is self-averaging—then you end up with a free energy given by Sherrington-Kirkpatrick. Rigorous mathematics. Then, I understood that one could study spin glasses and this particular solution in rigorous mathematical tools, and so I was involved. I will not tell in detail my involvement, which was very complicated and very painful. To give results was not so easy. Even though, at the end, my results were extremely useful and important, but it was difficult to put them together.

PC: That's exactly what I wanted to be pivoting to. So was it toward the late '80s that Pastur came and gave a talk in Rome?

FG: [1:09:52] I do not remember exactly the date, but there was a series of lectures. It was before 1991.

PC: If we look at your publication record: In 1991, you were organizing a workshop at which no one from the spin glass community was present[69], and within a few months of that you submit your first preprint on spin glasses. You have mentioned a couple of elements that made you realize that maybe there is something to provide to the field: Pastur, …

FG: [1:10:27] The workshop I organized was in Pontignano, in [1991]. This was a strongly Rome-centered mathematical physics event. Many people from Rome were invited, and it was really mathematical physics in Rome.

I was invited, around the same time, to a conference in Switzerland by Sergio Albeverio[70], and there I gave a talk about spin glasses[71]. In particular, I gave what I believe is the simplest proof of the fact that the Parisi formula should be involved in the solution for the free energy. I still did not know how to find the order parameter, but the Parisi formula was involved. This

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[68] L. A. Pastur and M. V. Shcherbina, "Absence of self-averaging of the order parameter in the Sherrington-Kirkpatrick model," *J. Stat. Phys.* **62**, 1-19(1991). https://doi.org/10.1007/BF01020856

[69] International Workshop Probabilistic Methods in Mathematical Physics, Certosa di Pontignano, Siena, Italy, May 6-11, 1991. See: *Proceedings of the International Workshop Probabilistic Methods in Mathematical Physics*, eds. F. Guerra, M. I. Loffredo and C. Marchioro (Singapore: World Scientific, 1992).

[70] Sergio Albeverio: https://en.wikipedia.org/wiki/Sergio_Albeverio

[71] Stochastic processes, physics and geometry II, Locarno, Switzerland, June 24-29, 1991. Proceedings: *Stochastic processes, physics and geometry II*, eds. S. Albeverio, U. Cattaneo and D. Merlini (Singapore: World Scientific, 1995). The preprint of the chapter on spin glasses included in this book is dated April 1992: Francesco Guerra, "Fluctuations and thermodynamic variables in mean field spin glass models," arXiv:1212.2905.

is quite a rigorous proof, lacking the fact that it would have been necessary to prove it really. But the general structure of the free energy for a spin glass was of this Parisi kind. Of course, it was a preliminary result, because at that time I could not give a variational principle. I could not grow any property of the variational principle. In any case, it gave great satisfaction to me, because I had the idea that I was on the good track of what the Parisi order parameter was really, from a general point of view, without using any physical intuition, which I could not provide in a very strong way, and without using any computer simulation, which I am completely unable to do. Just look at the shape of the marginal free energy, which is the change in free energy when you go from a system to a slightly larger system, so from N sites to N+1 sites. This marginal necessarily has the structure invented by Parisi. I would say this is an important result. Even though I did not give too much emphasis on it in this conference in Lugano, I gave an account.

FZ: I wanted to understand. Who else was working on providing rigorous results on spin glasses for that period? You mentioned Pastur, and then there was Aizenman[72].

FG: [1:14:10] Maria Shcherbina[73] worked with Leonid Pastur in the Soviet Union. I say Soviet Union, but they were in Ukraine in Kharkov. In Italy also there were people. For example, Brunello Tirozzi was working actively on neural networks at the time[74], and Enzo Scacciatelli and other people from the Rome mathematical physics group gave some results[75]. Even an important mathematicians like Claudio Procesi[76], who is now emeritus, of course, in Rome, gave a nice result on neural networks[77]. There's also Daniel Amit[78], who was in Rome at the time. Michael Aizenman gave important results on the high-temperature region at that time as well.

FZ: I was thinking more specifically to the Sherrington-Kirkpatrick model, to spin glass models. It seems that not many people were trying seriously to prove something about these.

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[72] Michael Aizenman: https://en.wikipedia.org/wiki/Michael_Aizenman
[73] Mariya Shcherbina: https://en.wikipedia.org/wiki/Mariya_Shcherbina
[74] See, *e.g.*, V. Dotsenko and B. Tirozzi, "Structured Hierarchical Neural Network," *Intl. J. Mod. Phys. B* **3**, 1561-1571 (1989). https://doi.org/10.1142/S0217979289001007
[75] See, *e.g.*, E. Scacciatelli and B. Tirozzi, "Fluctuation of the free energy in the Hopfield model," *J. Stat. Phys.* **67**, 981-1008 (1992). https://doi.org/10.1007/BF01049007
[76] Claudio Procesi: https://en.wikipedia.org/wiki/Claudio_Procesi
[77] C. Procesi and B. Tirozzi, "Metastable states in the Hopfield model," *Intl. J. Mod. Phys. B* **4**, 143-150 (1990). https://doi.org/10.1142/S0217979290000085
[78] Daniel Amit: https://en.wikipedia.org/wiki/Daniel_Amit

FG: [1:15:47] It was considered too difficult a problem. And many people, especially on the mathematical side, did not grasp the replica ansatz and the complex character of the model. If you understand the Sherrington-Kirkpatrick, then by going along you will understand neural networks, the *k*-SAT problem[79], hard optimizations. Essentially, it is a problem of hard optimization which is a main problem now in science and in applied sciences. There are very strong people, like Andrea Montanari[80] and Riccardo Zecchina[81], who work in hard optimization. They study extremely interesting NP-complete problems. Very difficult! Sherrington-Kirkpatrick is inside this. It is a paradigm, like the Ising model for ferromagnetism. The Sherrington-Kirkpatrick model is a kind of Ising model in the spin glass context.

PC: Following the fact that you could start demonstrating rigorous results on the SK model, what was the response from the community? Both the theoretical physics and the mathematical physics, and even the pure mathematics communities. Was there some interactions?

FG: [1:17:34] I received some invitations in many important meetings by mathematicians and physicists. But to have a response in the direction of studying those problems in rigorous mathematical terms, this was not. The fact is that people, like Giorgio Parisi, Miguel Virasoro, Marc Mézard, Daniel Amit and so on, were happy with their methods. While most mathematicians did not grasp the real essence of the problems. I nevertheless received invitations by physicists and by mathematicians, but was quite isolated in the scientific community. The history is complicated.

PC: That's why we're asking you!

FG: [1:18:35] It's complicated.

I still do not completely understand the consequences that Enrico Fermi was not called a professor of mathematical physics, but was called a professor of theoretical physics. There is a big difference from the academic point of view. Mathematical physics is a course in the university, which asks for many positions. Theoretical physics became a big enterprise only in modern times since it was strictly connected with big accelerators, as for

[79] Boolean satisfiability probem: https://en.wikipedia.org/wiki/Boolean_satisfiability_problem
[80] Andrea Montanari: https://pt.wikipedia.org/wiki/Andrea_Montanari
[81] Riccardo Zecchina: https://it.wikipedia.org/wiki/Riccardo_Zecchina

example at the CERN[82]. The power of theoretical physics at CERN is impressive. The connection with the big accelerators is important. The LHC[83] is where most of theoretical physicists work to understand new results.

But in teaching, in the '30s, the positions of theoretical physics were of a very small number. The competition of Fermi was in '26, so he became a professor on January 1, 1927 in Rome. There were three positions in the winning triplet, so there were two other winners: Enrico Persico[84], who went to Turin and then Rome, and the other winner was Aldo Pontremoli[85], who was a very good physicist with initial formation in Rome, then emigrated to Milan. You should know that he liked adventure, so he participated in the airship *Italia* expedition to the North Pole, organized by Umberto Nobile[86] in 1928. The expedition was very successful from some point of view, but they crashed on the ice in their way back from the North Pole. Pontremoli was very unlucky, because when the airship went down, most people were thrown away and left of the ice pack. In the end, after one or two months, these people were rescued. Pontremoli was left on the airship at the moment of the crash, so the wind took them away and it is not known what happened [to him]. In 1928, it was a big disaster. The expedition was successful, but the material was lost and many people died. It was also a big problem with the fascist regime and so on.

It is interesting historically, because it is strongly connected with scientific research. This enterprise to the North Pole was also a scientific enterprise studying cosmic rays, the magnetic field and the electric field in the atmosphere. Pontremoli was in charge of measuring the electric field in the atmosphere. Maybe we now are able to find some of the scientific instruments that were there and survived the crash because they went down to the ice pack. This is interesting history, the *Italia* history to the North Pole by Umberto Nobile. Aldo Pontremoli was involved and he was one of the three winners with Enrico Fermi in the national competition of two years before. So he was lost for theoretical physics.

PC: If you allow, we will move back to the 1990s. Should I understand that the main impact of you getting demonstrably correct results about the SK was to motivate you to keep working on it? Is that what convinced you?

FG: [1:23:34] Of course, even in the '80s I knew this was a very important problem. Because, first of all, the model is so beautiful and apparently simple.

[82] CERN: https://en.wikipedia.org/wiki/CERN
[83] Large Hadron Collider (LHC): https://en.wikipedia.org/wiki/Large_Hadron_Collider
[84] Enrico Persico: https://en.wikipedia.org/wiki/Enrico_Persico
[85] Aldo Pontremoli: https://en.wikipedia.org/wiki/Aldo_Pontremoli
[86] Umberto Nobile: https://en.wikipedia.org/wiki/Umberto_Nobile

If one writes the few formulae for the Sherrington-Kirkpatrick, at the beginning, one thinks: “How stupid they were? I could have solved it in a few hours.” Then, you begin to work and you see how difficult it is. There are such strong fluctuations everywhere that are controlled only because of thermodynamics. It is a connection between probability theory and thermodynamics in a very deep sense. What is involved here is disorder, but not Boltzmann’s view, so to speak. The system works in a highly disordered landscape, which gives strong fluctuations to everything. These fluctuations are to be controlled. There must be some self-averaging, like some law of large numbers, but non-linearly related. It is very complicated.

In the ‘80s, surely I knew that it was an important problem, but it was difficult. Between the ‘80s and the beginning of the ‘90s there was this visit of Leonid Pastur in Rome, where I could see that it was possible to give rigorous results. So I said: “Ok, then I must really do something.” In fact, then it was very nice, because I worked along the track of the Parisi representation. I worked on the track of ultrametricity with the so-called Ghirlanda-Guerra relations[87]. Then I found the bound so that the Parisi solution, at least, is a bound[88]. Then we saw that other problems also look simple. To prove that the infinite-volume limit, when the system becomes very large, does really exist. Many physicists think: “I don’t know that I have to prove it.” Of course, if the model is interesting, it must have an infinite volume limit, because there must be a free energy. When the system is very large, the free energy per volume should not change when I change the volume a little bit. Now, I’m strongly involved in the replica trick. Many mathematicians think that replica trick, in the usual formulation, is not rigorous, but I’m trying to show that it is as rigorous as possible, in the frame of the proper interpretation. Parisi found it differently, not through rigorous mathematical proofs, but through physical intuition. But in any case either the result is true or the result is not true. Physical intuition and rigorous mathematics must go together.

PC: Was your goal, from the start, to prove the validity of the replica trick, or did this develop along the way?

FG: [1:28:00] The replica trick is very complicated, because it is a paradigm. If you like, I’ll say a few things about the replica trick.

PC: Sure.

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[87] 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

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

FG: So we take this spin glass system. From the physical point of view, there are two sets of variables to describe it. First, the spin variables, which fluctuate according to Boltzmann in a kind or random environment. The variables of the environment describe the coupling among the spins. We call them the quenched noise. From the point of view of statistical mechanics they evolve with a very long life time. To a first approximation—like the adiabatic approximation or the chemical bond—you say they are fixed. They are random but fixed. You study the thermodynamics of the spin variables, trying the usual things of statistical mechanics. First, the intensive quantities, like the free energy per volume, the internal energy, entropy and so on. Then, you study the fluctuations, the spin correlations and so on. The noise is quenched, so to speak. It does not participate in the thermodynamic equilibrium. Firstly you take the Boltzmann averages, then you average with respect to the external noise. Of course, you can make the approximation that the quenched variables will participate in equilibrium. Then you have annealed averages. The metallurgical terminology fits there for good physical reasons.

Then, there is this idea of replica. What is a replica? A replica of a system is not a physical copy. If you have one system and you make a physical copy—if the system is disordered—then the physical copy will be disordered with a different disorder. There are two different systems. Instead, we talk about replicas. It is a system, which has different spins—because it is a different system—which evolve, but the environmental noise is the same. From a physical point of view, of course, nothing will change. If you take the log, for copies of the system the log of the product will be the sum of the logs. You have the same thing. But when you take the annealed average of replicas—you take Z^s with s integer—the annealed average has nothing to do with the product of the annealed averages. For the quenched average it's the same, of course, but the annealed averages are very different. Annealed averages of replicas are quite easy to study. You can study, even in the infinite-volume limit, the average of $1/N \log \langle Z^s \rangle$. The average of Z^2 is not the square of the average of Z, of course. The annealed averages are all different. They can be studied. There is some work to do here, but it can be done. What is the replica trick? It's a very deep idea. There is the idea—the illusion, if you like—that if I know the quenched averages in the infinite-volume limit for any number of replicas—s=1, 2,… —then I know everything. In particular, I can consider the average to zero replica. If you have $1/N \log \langle Z^s \rangle$ when $s \to 0$ this is the same as the $1/N \langle \log Z \rangle$. (I talk in general, not for you, of course.) So you have that knowing the annealed averages, when $s \to 0$ you find the quenched average, which is the physical object, of course. So, essentially, knowing the annealed averages for integer number of replicas, you build the analogous expression for when the number of replicas goes to zero.

If only you consider this problem, the mathematicians will be lost. In fact, it is perfectly alright. If you know Z, then Z^s has a meaning not only when s is integer, but in general. When s is integer, you can calculate it explicitly. In general, you have the expression, you can find the limit, and you can find also the solution. So the replica trick is the pretension, the idea to study the case of zero replicas, knowing only integer replicas. This is a well-defined problem. It also appears in elementary particle physics. If you take, for example, some analytic continuation if you know the function on the integers, of course there are many possible analytic continuations in general. But in the Parisi replica trick, there is another ingredient, which younger people in general undervalue. To find this limit, so to speak, toward zero replica, one has to make an ansatz, which is the ultrametric ansatz by Parisi. Then, when you make this ansatz, you have the Parisi formula.

My attempt is completely different. I do not make an analytic continuation, instead I work on the explicit expression for any real number of replicas. I try to find, with respect to the number of replicas, a kind of phase transition, such that when the number of replicas—real not integer—becomes less than such a number there is a phase transition. I hope to give an interpretation, so to speak, of the replica trick, not based on an analytic continuation, but based on what I call interpolation in the sense that you consider Z^s with s integer, you can consider Z^s with s any real number and study it. It is a real challenge, because if you do this you can apply it to many other cases. For example, for the case of neural networks or multispecies models, where there are many different kinds of site variables interacting, like the bipartite spin model. It is not completely clear what is then the Parisi formula, but one can easily write the expression, in general, using this interpolation.

PC: During your time at La Sapienza or elsewhere, did you ever get to teach a class on replica symmetry breaking or the replica trick? If yes, in what context?

FG: [1:38:45] I was called to Rome in '79. At that time, I was professor of theoretical physics, of course. I was called to Rome, and Giovanni Gallavotti and Sergio Doplicher were so efficient to convince people to call me in the mathematics institute—at the time it was the mathematics institute, not department—because the physics institute was entirely working on elementary particle physics and solid state physics. I was obliged to take a chair in theoretical physics, not in mathematical physics. So I was professor of statistical mechanics, a subject which in Italy academically is a part of theoretical physics. For 33 years—from my call to Rome to my retirement—I gave a course in statistical mechanics, always changing the program year by year, so that I could speak about problems of Euclidean field theory, of statistical mechanics in the field theoretical frame, and so on. In the last years, I also considered the problem of complex systems and the structure of replica symmetry breaking. I had many good students, because I was lucky that my course was not compulsory. There were about 10 students per year, but all well-motivated research students. Consider that I had as student Pierluigi Contucci[89], who is now a professor in Bologna, Stefano Ghirlanda[90], who is now a professor in New York, Adriano Barra[91], who is now professor at the University of Salento. Many good people, even with good recognition, participated in this course. In particular, I brought up the infinite-order limit of the Parisi solution, and how to deal with this object, essentially from a rigorous point of view. But I speak rigorous mathematics only if it is simple mathematics. Each step must be a simple single step. The complexity comes from the fact that all steps are put together. In the last years of my teaching—I was near retirement—I was very upset because they made my course compulsory in some study plans. I had some additional 30 students of mathematics who did not know anything about physics and statistical mechanics. Also, there was a change in the spirit of the courses, as a consequence of a bizarre national reform, informally called "three plus two". At the end, I was told that I was making too difficult of a course, and I should make it much simpler. I was told that I was teaching too much, and that I should teach less.

PC: Should I understand that you taught the mathematics of the replica trick roughly from the mid-'90s up to 2005? Are these roughly good bounds?

FG: [1:43:09] I would say… no. I taught this material until the end. I left in 2013, but I did begin during the '90s, yes. I think I also had some impact, because many people in the field attended some of my course, even people not involved in mathematical methods and so on.

PC: Is there anything else you'd like to tell us about this era, that we might have skipped over, and you think is important?

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[89] See, *e.g.*, P. Contucci and C. Giardinà, *Perspectives on Spin Glasses* (Cambridge: Cambridge University Press, 2012). https://doi.org/10.1017/CBO9781139049306

[90] See, *e.g.*, M. Enquist and S. Ghirlanda, *Neural Networks and Animal Behavior* (Princeton: Princeton University Press, 2006).

[91] See, *e.g.*, A. Barra, "Irreducible free energy expansion and overlaps locking in mean field spin glasses," *J. Stat. Phys.* **123**, 601-614 (2006). https://doi.org/10.1007/s10955-005-9006-6

FG: [1:44:01] No. I think I told everything that I wanted to say. I apologize that I put some things connected with the '20s and the '30s. But I think it's important, because all that happens now, especially in Italy and in Rome, has its source in Fermi and Majorana, so the '30s. The '30s are the essential part. If you look at the papers, especially by Majorana, they are really good papers in mathematical physics in the modern sense, strictly connected with the phenomenology. The Majorana method is really interesting. He wrote a lot of things on the method, which are not very well known, because, of course, there are other things about Majorana. He was interested in the method, especially in connection with important scientists in his family, who knew that the method was important. Of course, there were scientists in sociology, in economy, in law and so on, but they were in a sense high-level experts in complex systems. So I think that history is important, especially for mathematics and physics in Italy and in Rome, because the present is a direct development of these far away times. For example I am daring to say that the "gattini" school is a continuation of the Fermi work on beta decay in 1933-1934.

PC: I'm grateful that you brought up those connections. It's a singular perspective that you have, so it's extremely helpful for us to hear them as well. In the spirit of history, have you kept notes, papers, correspondence from those years? If yes, do you have a plan to deposit them in an academic archive at some point?

FG: [1:46:33] This is an important question. I will tell you one example. You know there is a simplified model of spin glasses—the random energy model—developed by Bernard Derrida. It's a very important model, because it is a kind of laboratory. If you have an idea about something, and you would like to know whether it works, you do not try immediately the Sherrington-Kirkpatrick model. You first try it on Derrida's random energy model. It's an important simplified model. There are thousands of papers dedicated to it. Even I wrote a replica trick with my interpretation in the random energy model.

I was told by Parisi that the random energy model was introduced by Cabibbo—Nicola Cabibbo was an important theoretical physicists we have spoken about—in the '70s, independently from Sherrington-Kirkpatrick and Derrida. Cabibbo did not publish it, because he was a very ambitious theoretical physicist. He thought it was too simple and so he did not publish. It would be very interesting to find among the Cabibbo papers—there are a lot of papers, notes, correspondence and so on—whether there is something related to this. Of course, things are more difficult, because many years ago I asked Nicola Cabibbo—he was already very ill, but nevertheless we had a good relation—about this episode, but he could not remember exactly. Shall we find notes from Nicola Cabibbo about the random energy model, I would be very pleased, of course.

In any case, our department has the tradition of keeping all papers of people who go to retirement or, like Cabibbo, go "abroad". So we have the papers by Cabibbo. Still, there is no complete catalogue and no people studied them. But we have everything. At the end, we will also have notes by Giorgio Parisi. There is a tendency of keep the notes even for living people. For instance, Luciano Maiani, who did important contributions to particle physics, left all his notes in the archive of the department, so you can study how this flavor physics developed and the understanding of Luciano Maiani.

I think you must preserve the memory, and I think this is more or less the right time, January, to remember what is the meaning of preserving the memory. We must preserve the memory of the good and, of course, the tragedies. They made our own stamina.

PC: What about your own papers and correspondence? Are they being preserved?

FG: [1:50:48] I have everything. I moved office many times, and I kept everything. Many things I have at home. I have letters by Arthur Wightman, Tullio Regge[92], Hiroomi Umezawa[93], Eduardo Caianiello, Edward Nelson, and Gianfausto Dell'Antonio, obviously. Many people are there. I have a lot of material, and I kept all notes. When I read them after many years, in some cases I do not understand anything, but with some effort you understand the development of ideas. I kept everything. People in the archives told me: "Why do you not send something to us?" I'm Neapolitan, so I told them it's not a good idea, but in some years they will get everything. I have also had the opportunity to make a kind of "treasure", some boxes where the most important things are.

Recently, I found the work done in Princeton in 1970-71-72. It was very important in my life, because I got good results that made my presence known, so to speak, in the field. I have everything preserved.

Now I will tell something related to what is left from Fermi, Majorana, and others. For example, Majorana wrote with ink, so after so many years the ink is still perfect. But at the time, we used to write with ball-point pen, and there is a big problem that the ink of the ball-point pen, according to

[92] Tullio Regge: https://en.wikipedia.org/wiki/Tullio_Regge
[93] Hiroomi Umezawa: https://en.wikipedia.org/wiki/Hiroomi_Umezawa

statistical mechanics, has a kind of diffusion process within the paper. After fifty years, when now you read the notes, you have to make a deconvolution. It is a convolution of what you had before with a Gaussian distribution, which gives the movement of the ink within the paper. Now, you have to make a deconvolution, which is like solving an antiparabolic equation. It is known that it is very unstable, because you have to invert an exponential (to the minus) Gaussian. So in some cases it is very difficult to read in a proper way. If you write by pencil, then other disasters can happen. Everything disappears. This we find also in papers by Majorana. In some cases, he wrote by pencil and it is very difficult to read it.

PC: On these wise advices, I'd like to thank you very much for your time.

FG: Ok.