History of RSB Interview: Roberto Benzi

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PC: Thank you very much, Prof. Benzi, for sitting with us. As we've discussed ahead of this discussion, the theme of this series of interviews is the history of replica symmetry breaking in physics, which we roughly bound from 1975 to 1995, during which time you were very much involved with the Sapienza and Rome-based group. But before we dive into the heart of the topic, we have a few questions. First, can you tell us a bit ybout your family and your studies before starting university?

RB: [0:00:37] I was born in a middle-class family. I started to be interested in physics when I was extremely young. My first book was a rather simple one, but I bought it at eight years [old]. So, I was starting to learn a little bit about physics, and I was fascinated by that. Then, when I was in high school, I met an older guy who told me about the *Feynman Lectures on Physics*[1]. So, during high school, I was already reading the *Feynman Lectures on Physics*. From there, I never left physics as a main topic. My family was really disappointed by the fact that I was doing physics. "Oh, my poor guy! So clever and then you're going to waste your life in physics." But I went along. Then, when I arrived here, at La Sapienza. The second year I met the younger brother of Giorgio Parisi,[2] who introduced me to Giorgio. At the time, Giorgio was going to Columbia University. It was in '74, probably. Then, at the end of '74, when I was going to take my degree—at the time there was no master's degree or PhD—I went to Nicola Cabibbo[3] to ask for thesis. Nicola put me in the hands of Giorgio. That was really interesting, because Giorgio decided to do a high-temperature expansion in *d* dimension, where *d* is any number, by using a

[1] Feynman Letures on Physics : https://en.wikipedia.org/wiki/The_Feynman_Lectures_on_Physics
[2] Valerio Parisi (1954-)_was an undergraduate physics student at La Sapienza from 1972 to 1977.
[3] Nicola Cabibbo: https://en.wikipedia.org/wiki/Nicola_Cabibbo

very simple trick on the lattice.[4] By going to very long-wavelength so that the propagator was a Gaussian function, so you can do the expansion without caring about the topology of the different links on the lattice. At the very beginning, what he did was going to Geneva. We were two: myself and a friend of mine, Guido Martinelli[5], [whom] you may know. What he did—probably in this room—was to draw a vertical line and a horizontal line without any index, and said in the middle: "Our friends in France found a phase transition here. Now, I am going to leave for one month, but if you can read the last 15 year of *Physical Review* on the subject. Then, at the end of the month, we can start to do the computation." I looked at the face of my friend, and the face of my friend was really showing that we were lost in the middle of nowhere. That was the starting point, but Giorgio was fantastic. We did the work in three months. I remember that I did the computation of 1500 Feynman diagrams. For me, that was enough. I did not want to do any Feynman diagrams for the rest of my life. But Giorgio was very clever to introduce us to the problem.

PC: Can you tell us about the theoretical physics training at La Sapienza at the time? What courses were particularly memorable or interesting?

RB: It was a very strange time in '72-'74, because most people were considering theoretical physics—as they still do now—as high energy physics. In high energy physics, there was no clear statement about what to do; if you have to do a field theory; if you have to use the S-matrix[6]; or if you have to invent some other tool to understand particle theory. There was quite strong evidence of quarks' existence, but the framework was still unclear. In '74, there was the paper by Wilson on the renormalization group[7]. It was an exciting period, but it was too hard for the training that we were doing. So, we were essentially trained in statistical mechanics, but old-style. No theory of phase transition. The theory of phase transitions, I learned from Giorgio and from Kadanoff[8].

FZ: So, not even the Landau mean-field theory[9]?

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

[5] "Guido Marinelli," *Accademia Nazionale dei Lincei* (n.d.). https://www.lincei.it/it/content/martinelli-guido (Accessed February 7, 2024.)

[6] S-matrix theory: https://en.wikipedia.org/wiki/S-matrix_theory

[7] See, *e.g.*, K. G. Wilson, "Confinement of quarks," *Phys. Rev. D* **10**, 2445 (1974). https://doi.org/10.1103/PhysRevD.10.2445

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

[9] Landau theory of phase transitions: https://en.wikipedia.org/wiki/Landau_theory

RB: [0:05:36] No, nothing like that. The Landau-Ginsburg equation was something that I discovered after. Now, we give to the students a very clear understanding of what is statistical physics and what is the first step in non-equilibrium statistical physics. But at that time, there was no information about that. There was the Fermi-Dirac statistics, the Bose-Einstein statistics, there was a good course in solid-state physics, a wonderful course in nuclear physics, not on the theoretical part, Giorgio was in Columbia, Nicola Cabibbo was somewhere else (I don't remember where), Luciano Maiani[10] was [not yet] working the university. So, there was no really good training in that sense. We were learning everything that we could.

FZ: What about the statistical physics group, like Jona-Lasinio[11] and Di Castro[12]? Where they not teaching?

RB: [0:06:48] Jona came [back] from Padova in '72. I don't know what he was teaching when I was here in Roma. I didn't meet him during my courses.

FZ: So, they were not teaching here.

RB: [0:07:09] Not yet.

FZ: So, the only theorists in the department were Cabibbo and Maiani?

RB: [0:07:15] There were a lot of people working. Altarelli[13] was here, Giuliano Preparata[14] was here, Roberto Petronzio[15] was here, but they were already tuned in at a high level. Altarelli was my teacher in quantum mechanics. That was very funny. They used a book, which I don't remember, but I didn't like it. I had already read it, but I decided to read again the Feynman book. Then, I read the book. So, when I went to the examination, I said to Altarelli: "I was working on this book. Do you know it?" Then, he opened the front page, and it said translated by Guido Altarelli[16]. I thought: "That's the end of my examination." In fact, it was not true. I [got] the maximal [grade] for that examination. Also, there was no training on computer.

[10] Luciano Maiani: https://en.wikipedia.org/wiki/Luciano_Maiani
[11] Giovanni Jona-Lasinio: https://en.wikipedia.org/wiki/Giovanni_Jona-Lasinio
[12] "Carlo di Castro," *Accademia Nazionale dei Lincei* (n.d.). https://www.lincei.it/it/content/di-castro-carlo (Accessed February 7, 2024.)
[13] Guido Altarelli: https://it.wikipedia.org/wiki/Guido_Altarelli
[14] Giuliano Preparata: https://en.wikipedia.org/wiki/Giuliano_Preparata
[15] " Roberto Petronzio (1949-2016)," *Società Italiana di Fisica* (2016). https://www.sif.it/riviste/sif/sag/ricordo/petronzio (Accessed February 7, 2024.)
[16] Richard P. Feynman [trad. G. Altarelli, C. Chiuderi], *Meccanica quantistica* (London: Addison-Wesley, 1970).

Essentially, I started to learn computer together with Giorgio and Nicola Cabibbo. It was a nice time.

FZ: So, from the point of view of field theory, it was still undeveloped.

RB: [0:08:42] For us, it was essentially Maxwell's equations. There was, of course, the Dirac equation and the Schrödinger equation, but field theory was a challenge as a tool for high energy physics. In '75-'76, I read the review paper by Weinberg. There was a *Reviews of Modern Physics* on the standard model, where all the ideas were there[17]. I do remember very well that I said "What is left?", because more or less all the basic mechanisms were explained in that review. In '77-'78 years, three year after I took my degree, I was quite clear that the situation was going to move in this direction. During my degree, I was giving a course on statistical mechanics using the Feynman book.

FZ: You were teaching a course while you were a laurea student?

RB: [0:10:12] Yes. I was a student, and I gave a talk on this. I made a computation of I don't remember what.

FZ: What about condensed matter or solid-state physics?

RB: [0:10:24] There was a very strong group of condensed matter here: Careri,[18] [etc.] There was a very strong group, but they were mostly experimentalists. The theoretical focus point was solid state physics. At that time, there was a very good teacher of solid state physics. When I took my degree, I made all the possible spectrum of courses, not focusing alone on one subject, because I didn't know what to do. I took a class in general relativity and astrophysics, in solid state physics, in nuclear physics. Condensed matter was one we had to do anyway. And theoretical physics. Condensed matter was not so tied to theoretical physics and statistical physics. Now, it's completely different. At that time, molecular dynamics was still moving a few steps. I did the first simulation in molecular dynamics in '79 with 5000 particles. It was huge.

PC: You mentioned that you discovered computers and programming in computational physics with Giorgio and Nicola. Could you tell us more details?

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[17] S. Weinberg, "Recent progress in gauge theories of the weak, electromagnetic and strong interactions," *Reviews of Modern Physics* **46**, 255 (1974). https://doi.org/10.1103/RevModPhys.46.255
[18] Giorgio Careri: https://it.wikipedia.org/wiki/Giorgio_Careri

RB: [0:12:15] When I started the university courses, the most powerful machine that you could buy was a calculator that was able to take the square root. Then, after two years, they were able to do the logs. That was an incredible step. Then, there was the HP-25[19] that nobody remembers and that was able to do programming. You were able to do 50 lines of program. So, in order to repeat a set of instructions you were able to save time. My thesis was done on that. We were doing a lot of computations by hand and then the final computation was done on that. Then, Giorgio was able to convince us to move the first step in Fortran. I studied Fortran by myself. What we did was doing the computation in the national laboratory in Frascati[20] during the night. There was a computer free, and we went there for several days to do the computation. But they were very elementary. I remember that my first computation was done in the following way. The large floppy, you put inside the machine. The machine was reading part of the program, doing part of the computation. Then you take away the floppy and put another floppy for the other part of the computation. So, there was a lot of checking and constant checking this or that. So, numerical simulation as we think of it now was completely outside. But the things were moving very fast, because in '81-'82, we already started to do some numerical simulations. So, everything was done from scratch and by reading and learning by myself, as all of that generation was doing. You don't know, but at the time I'm talking about, you were not able to write on the screen. You had a piece of paper, a card, and you had to put holes in the card. One of the favorite jokes was to take the cards of someone else and to scramble them. Or, you could put an instruction like "you will never be able to do this computation", and to loop for 10,000 times. So, the results were a lot of useless paper. It was funny, but it was very tiring. You can't imagine how many hours we spent doing this kind of things.

PC: Giorgio was formerly based at Frascati at this time, and you were at La Sapienza.

RB: [0:15:24] We started to believe Giorgio was formed somewhere else because we didn't understand how he was getting all this information. He was formed here, then he got a position in Frascati, but was working essentially at La Sapienza, here, in the department. Then, from '72 to '75, I think, he was visiting Columbia. Then, he told us about the Wilson and

[19] HP-25: https://en.wikipedia.org/wiki/HP-25
[20] Laboratori Nazionali di Frascati: https://en.wikipedia.org/wiki/Laboratori_Nazionali_di_Frascati

Kogut report.[21] We studied it, and that is why we were introduced in the field.

PC: Were there group meetings with Giorgio or with Nicola Cabibbo as a community? Or was it only you interacting directly with Giorgio?

RB: [0:16:18] The community of theoretical physics was working around Cabibbo. That was the master of all of us, including Giorgio, as he said several times. Together with Nicola, there was an outstanding scientist that people don't know very well, Bruno Touschek[22]. Bruno Touschek was really a top-class physicist. He invented the electron-positron mechanism at Frascati. There was a group of theoretical physicists that recognized Nicola as setting the guideline for all of us. There was Altarelli, as I said, Roberto Petronzio, Massimo Testa, and Giuliano Preparata, of course. They were meetings every few days on different subjects.

PC: How did you organize these meetings?

RB: [0:17:30] You came, you arrived, and there was this meeting. That's it. There was no formal gathering. You entered in the room. You knocked on the door: "Can I speak?" Then, after a while there was another guy there to discuss. That was totally happening. There was no organization, and it was working perfectly well.

PC: After your time here, you moved to the CNR[23].

RB: [0:18:00] At first, I did the military service. That was a nightmare.

PC: Was it in any way related to science?

RB: [0:18:08] No. It was one year wasted. After that, I got a scholarship at the CNR. At the time, the only position available was in oceanography. That was interesting, because to do oceanography you have to know about fluid mechanics. Somebody told us that this was important. So, I started to study fluid mechanics, in particular turbulence. In '78, …

PC: To be clear, did you join that group because that was the only group that had positions? Were there no positions otherwise open? Or did you have a particular curiosity for the topic?

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[21] K. G. Wilson and J. Kogut, "The renormalization group and the ϵ expansion," *Phys. Rep.* **12**, 75-199 (1974). https://doi.org/10.1016/0370-1573(74)90023-4

[22] Bruno Touschek: https://en.wikipedia.org/wiki/Bruno_Touschek

[23] Consiglio Nazionale delle Ricerche (CNR): https://en.wikipedia.org/wiki/National_Research_Council_(Italy)

RB: [0:18:58] The problem was at the time strange, because in ’75 there were huge amounts of position to stabilize the people that were under contract for several years. Here, at the Institute of Physics[24], we had more positions that people. After that, the money was over. For three or four years, there were no other possibilities. In fact, in ’76, I asked Giorgio to go outside to take a PhD, and he told me: “Why? Do you want to work in the United States?” That was the status. There was no reason to take the PhD. The reason was that at the time, the laurea thesis degree was almost a PhD. There were people spending three years on the thesis. But that was a very old way of think about teaching. So, for many years there were no available positions. They started again in ’78, but the money that was put on the table was on geophysics or solid state physics. So, I started to do this fluid dynamical work. That was even more interesting because there was nobody in Roma that was working on turbulence. So, we started to do the first step, the thing on turbulence. I say we, because at that time there was also Angelo Vulpiani[25] that got a graduation. We went together for the same position. For three years, I was there. But I did everything except oceanography, really. Although I spent one month in the Indian Ocean taking measurements.

PC: How constrained or free were you to choose research ideas once hired in that unit?

RB: [0:21:12] That was always true; they left me completely free. Actually, that was a good choice for me, but it was a very dangerous choice for them. People as the CNR were looking for somebody that was working on turbulence, more specifically on oceanography. I could have spent my time sitting on the table playing card without any problems. So, it was really a fluctuation that I was able to do something all along these three years. I did a lot of things, by the way. At the time, we were also starting to work on dynamical system theory. The paper by Lorenz was in ‘63[26], but then the paper by Ruelle and Takens was ’71[27], so we started moving in this direction. Giorgio was pushing us in this direction as well.

PC: So, you were still in touch with Giorgio even though you were at the CNR?

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[24] Istituto Nazionale di Fisica Nucleare (INFN): https://en.wikipedia.org/wiki/Istituto_Nazionale_di_Fisica_Nucleare

[25] Angelo Vulpiani: https://it.wikipedia.org/wiki/Angelo_Vulpiani

[26] Lorenz system: https://en.wikipedia.org/wiki/Lorenz_system

[27] D. Ruelle and F. Takens, “On the nature of turbulence,” *Les rencontres physiciens-mathématiciens de Strasbourg-RCP25* **12**, 1-44 (1971). https://doi.org/10.1007/BF01646553

RB: [0:22:25] I was going there every now and then to explain what was going on. That was the reason why we worked together on stochastic resonance, by the way.

FZ: Where was the CNR office?

RB: [0:22:44] The CNR, as an institute, was in the EUR part of Roma,[28] so far away from here. But we were not supposed to go there every single day, like in the university. What happened is that we were working here and sometimes we were going there.

FZ: So, you were spending a big part of your time here.

RB: [0:23:12] Yeah. That was a good way to increase your knowledge. If you gave to a young student the possibility to do whatever he wants, of course you take a risk. But if you support him, then the student is able to do a good job, and it's a fantastic opportunity. It was so. They were very clever. So, this is what I did.

PC: In a recent talk you gave about the non-linear dynamics work,[29] you mentioned that you were discussing with Angelo Vulpiani, Alfonso Sutera,[30] and Giorgio Parisi the nearly periodic alternance between glaciation interglacial periods. How did this problem come to your attention?

RB: [0:24:05] I explain to you. We were working at listening, first of all, to a series of talks given by Jona-Lasinio on stochastic differential equations. At the time, the knowledge of stochastic differential equation was completely absent here. There was nobody working on this field. Jona-Lasinio was introducing us to the theory of Wentzell and Freidlin,[31] which is not exactly the first thing you should learn in order to understand stochastic differential equations. But then we started to work on this, and we were able to understand how to take, for instance, $x(t)$ from a potential well. We discovered later that that was Kramer's computation, but that was a different story. Then, we also studied the book of Gihman and Skorohod on stochastic differential equations.[32] That is a nice book.

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[28] EUR, Roma: https://en.wikipedia.org/wiki/EUR,_Rome

[29] R. Benzi, "The physics of noise," *The interdisciplinary contribution of Giorgio Parisi to theoretical physics*, La Sapienza, April 13, 2023. https://www.youtube.com/watch?v=UdmHKAYe4Bs (Accessed February 8, 2024.)

[30] Alfonso Sutera: https://it.wikipedia.org/wiki/Alfonso_Sutera

[31] A. D. Wentzell and M. I. Freidlin, "On small random perturbations of dynamical systems," *Russian Math. Surveys* **25**, 1-55 (1970). https://doi.org/10.1070/RM1970v025n01ABEH001254

[32] I. I. Gihman and A. V. Skorohod, *Stochastic Differential Equations* (Berlin: Springer, 1972).

One of us, Alfonso, was in the States working at Yale university, and he got interested in climate change because of an extraordinary person that was working in Yale, Barry Saltzman[33]. Barry was a fantastic scientist. He was behind the story of the Lorenz attractor. He was the first guy that took the Rayleigh–Bénard problem[34], made the expansion in order to go for orthogonal modes in series of sines and cosines[35]. Then, he got 26 equations. He started to made simulations of these 26 equations and at a certain point, he got a signal. A signal means that you have to look at the numbers. (At that time, you had to put on the paper, the information.) The signal was not periodic or quasi-periodic. It was clearly something else. He didn't understand what was going on, because it wasn't even clear that the numerical simulations were correct. There was the problem that the truncation was giving you spurious physical properties in the system. Then, he went to Lorenz. (In fact, in the paper, he makes an acknowledgment of Lorenz.) Lorenz was able to extract from this the Lorenz model.[36]

Barry was introducing Alfonso to climate change. At that time, there was a huge debate about the Milankovitch cycles[37]. The problem was that the forcing due to the change in eccentricity was too small to explain the glacial-interglacial period. So, Alfonso was starting to play with stochastic differential equations that were introduced to climate theory by us. When I visited Alfonso in '79, we started working together on these ideas, trying to understand this property of stochastic effects. What is the non-perturbative effect of stochastic different equation on climate models?

PC: Were you visiting him Yale?

RB: [0:27:52] I was visiting in Yale. I stayed there for three months. We were working together; we wrote a paper on that.[38] When I came back, I discussed with Giorgio and Angelo about this. In fact, we wrote two papers with Angelo not on climate, but on turbulence, trying to understand whether the original Landau idea can be dressed by some fluctuation effect used by turbulence in order to explain non-trivial scaling properties

[33] " In Memoriam: Yale Pioneer in the Theory of Weather and Climate, Barry Saltzman," *Yale News* (February 5, 2001). https://news.yale.edu/2001/02/05/memoriam-yale-pioneer-theory-weather-and-climate-barry-saltzman (Accessed February 8, 2024.)

[34] Rayleigh-Bénard convection: https://en.wikipedia.org/wiki/Rayleigh%E2%80%93B%C3%A9nard_convection

[35] B. Saltzman, "Finite amplitude free convection as an initial value problem—I," *J. Atm. Sci.* **19**, 329-341 (1962). https://doi.org/10.1175/1520-0469(1962)019<0329:FAFCAA>2.0.CO;2

[36] E. N. Lorenz, "Deterministic nonperiodic flow," *J. Atm. Sci.* **20**, 130-141 (1963).

[37] Milankovitch cycles: https://en.wikipedia.org/wiki/Milankovitch_cycles

[38] R. Benzi, A. Sutera and A. Vulpiani, "The mechanism of stochastic resonance," *J. Phys. A* **14**, L453 (1981). https://doi.org/10.1088/0305-4470/14/11/006

like intermittency.[39] Then, when Alfonso arrived, he gave a talk about this property that we found. That was the initial start of the work with Giorgio. It's a long story. You don't wake up in the morning and say: "Let's work on the Milankovitch [cycles]." This is how.

PC: In that talk, you also mentioned that working with Giorgio at that time took place only through irregular meetings, while in town.

RB: [0:29:01] Even now. When you have to discuss with Giorgio, you discuss when you walk, when you take the elevator or go to the airplane, whatever. It is very difficult to fix Giorgio on the blackboard. He has no time, as usual. But he's always interested to speak with you. On the case of stochastic resonance, he was on the second floor of this building, going out from the elevator and I was going inside the elevator. We met together and he said: "I spoke with Alfonso, and I think that if you put the noise with a periodic forcing, you may find something interesting. You see, you have an exponential effect." Then, we started thinking about how to do it. I convinced Giorgio that we couldn't do it by hand. We had to do it numerically. So, we did. That's it.[40]

PC: How was the work received initially?

RB: [0:30:10] Very well. When I gave the talk in 1980 at Erice,[41] there was Hasselmann[42] there. Most of the people who were working in climate theory, they immediately understood that that was non trivial. Outside the Erice meeting, it was a nightmare. We sent it to *Science*, rejected, *Journal of Climate Science*, rejected. After a while, when I discussed with climatologist in England, I remember he told me: "If this is physics, I prefer sex." The most interesting answer was given to me by Kadanoff, when I told him the story is: "Maybe sex is better anyways." After a short while, Michael Ghil[43] convinced us to send the paper to *Tellus*.[44] Then, we sent the paper to *Tellus* and that's it. Then, it started again.

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[39] R. Benzi, and A. Vulpiani, "Small-scale intermittency of turbulent flows," *J. Phys. A* **13**, 3319-3324 (1980). https://doi.org/10.1088/0305-4470/13/10/026; R. Benzi, M. Vitaletti and A. Vulpiani, "Energy dissipation and Kolmogorov law in turbulent flows," *J. Phys. A* **13**, L339 (1980). https://doi.org/10.1088/0305-4470/13/9/011

[40] R. Benzi, G. Parisi, A. Sutera and A. Vulpiani, "Stochastic resonance in climatic change," *Tellus* **34**, 10-16 (1982). https://doi.org/10.1111/j.2153-3490.1982.tb01787.x

[41] *NATO Advanced Study Institute: First Course of the International School of Climatology*, A. Berger, Ettore Majorana Center for Scientific Culture, Erice, Italy, March 9–21, 1980. Proceedings: *Climatic Variations and Variability: Facts and Theories*, A. Berger, ed. (Dordrecht: D. Reidel, 1981). https://doi.org/10.1007/978-94-009-8514-8

[42] Klau Hasselmann: https://en.wikipedia.org/wiki/Klaus_Hasselmann

[43] Michael Ghil: https://en.wikipedia.org/wiki/Michael_Ghil

[44] Tellus: https://en.wikipedia.org/wiki/Tellus_A

FZ: What was the criticism of the paper?

RB: [0:31:42] Two main criticisms. The first was a cultural problem. It is there. You know Stéphan Fauve,[45] I guess. Stéphan Fauve did a fantastic experiment on dynamo problems. He observed the reversal in time of the magnetic field experimentally. Now, that dynamo was small, of course. Astrophysically and geophysically, you have two types of dynamos that is very well recorded. The first one is magnetic earth; the other one is the sun. They are on totally different timescales. (Actually, I learned that the two communities do not speak to each other, but that's another story.) The idea that you can reverse the full circulation can be an effect of the noise is something that is difficult to accept. The problem is what you think is the noise. In 2005, I wrote paper showing that if you take a model of Navier-Stokes equation, which has two possible states, by symmetry—of course, you have to put them by hand but that is part of the large scale effect—then the effect of the fluctuations give you the reversal in time exactly as if this is a noise.[46] But this is not noise; it is the fluctuations of the Navier-Stokes equation. In the case of the climate, things happen in the same way, but this was not accepted. That was the real problem. The second [criticism] was a little bit more funny. When you have a transition from one state to another and you look at the graphic, then you see a jump. But actually this is not true. If you plot everything every hundred timesteps, a rare fluctuation that goes in one direction—the probability is exponentially small—then the idea is that the noise will give you a large fluctuation, which is wrong. The distinction between large fluctuation and rare fluctuation is fundamental in understanding this effect. That is the point. These were the two main criticisms concerning climate. In climate science, at that time, nobody found any abrupt change in the record. Now, they found it. That's it.

PC: At about the same time as that work came together, Giorgio was working on spin glasses. Were you aware at all of that work?

RB: [0:34:56] He told me about it, but it was a very abstract meeting. I went there to discuss about turbulence, then he told me about the computation of the solution of the spin glass but explained me the technical tool. The physics behind, I was absolutely unable to understand what was really the problem. I discovered the problem afterwards.

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[45] Stéphan Fauve: https://en.wikipedia.org/wiki/St%C3%A9phan_Fauve

[46] R. Benzi, "Flow reversal in a simple dynamical model of turbulence," *Phys. Rev. Lett.* **95**, 024502 (2005). https://doi.org/10.1103/PhysRevLett.95.024502

PC: Had you ever heard of spin glasses before?

RB: [0:35:30] No. I didn't understand the problem at all, physically. Then, I was too much involved in turbulence to spend time in another direction.

PC: When would this have been that he explained the technical details to you?

RB: [0:35:52] I think it was about '80. I cannot remember.

FZ: While Giorgio was working this solution of the SK model, in Rome, did he give talks? Or would only talk about it in private conversations?

RB: [0:36:15] I never heard a talk about it. Only private conversations.

FZ: So, the community in Rome was not aware.

RB: [0:36:22] Giorgio, most the work he did while in Paris in '77 or '78. I don't remember exactly, but in '78 I'm sure because I was there visiting him for two weeks and he was working on that. For two years he was there, and when he came back to Roma in '80, I don't remember any talk about spin glasses. We were aware that he was working on spin glasses, but we were thinking of a rather exotic subject. Remember that before that, there was a huge work on instantons in quantum field theory by Giorgio.[47] That was very interesting work. And there was the work done with other people on the time scale for the ergodicity for the lambda ϕ^4 theory. There were a lot of things on the table, and to us the problem of spin glasses was not really the top one.

FZ: What about the physics of disorder more generally?

RB: [0:38:04] The most disordered thing at the time was turbulence.

FZ: But it was a different type of disorder.

RB: [0:38:12] Yes, but the idea was still the same. When I was a student, the interest in fluid mechanics is what happens before turbulence, that turbulence is like the high-energy phase of the Ising model. It's irrelevant. Then, we had to change our minds. That was the most disordered thing. The spin glass was not on the table. I understood it really well around '83-

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

’84, when the first few papers came out with the physical interpretation and the possible output.[48] At that point, it was clear that it was a hot topic.

FZ: The difference between turbulence and spin glasses is that in spin glasses you have quenched disorder, while in turbulence it’s self-generated.

RB: [0:39:14] Technically speaking, yes. However, a few months ago, Giorgio came to us and gave a talk for the memory of Roberto Petronzio and I asked him: “When you were introducing the multifractal, were you thinking of the complexity function in spin glasses?” Then, he said: “Well…” There was no answer. After a couple of hours, he sent me an email: “Yes! Thank you for the question. That was precisely what I had in mind.” So, the two things were not so far away. It’s true that they are quenched, but it’s also true that if you look at the way energy is going from large scale to small scale in turbulence—it is in time—during the time that the energy goes down, you have a kind of quenching of the possible scale. The two ideas have something in common, but you have to play with time in turbulence, which has no role in spin glasses.

PC: To dive a bit into what led to this problem. In a topical review that Angelo Vulpiani wrote,[49] he credited lectures that Giorgio gave on turbulence at a Varena summer school in 1983,[50] as the origin of your interest on the multi-fractal nature of turbulence. Can you tell us a bit more about you became interested in these ideas and in this particular viewpoint?

RB: [0:41:00] We were thinking a lot about turbulence and, in particular, about intermittency. Then, in 1983, I asked Giorgio to help me in organizing a Varenna school on geophysical fluid dynamics and predictability, where one of us, Alfonso, was giving a lecture on stochastic resonance.[51] So, Giorgio came. At the time, he was writing the book on statistical field

[48] See, *e.g.*, G. Parisi, "Order parameter for spin-glasses," *Phys. Rev. Lett.* **50**, 1946 (1983). https://doi.org/10.1103/PhysRevLett.50.1946; M. Mézard, G. Parisi, N. Sourlas, G. Toulouse, and M. Virasoro, "Nature of the spin-glass phase," *Phys. Rev. Lett.* **52**, 1156 (1984). https://doi.org/10.1103/PhysRevLett.52.1156

[49] G. Boffetta, A. Mazzino, and A. Vulpiani, “Twenty-five years of multifractals in fully developed turbulence: a tribute to Giovanni Paladin,” *J. Phys. A* **41**, 363001 (2008). https://doi.org/10.1088/1751-8113/41/36/363001

[50] *International School of Physics "Enrico Fermi" Course LXXXVIII: Turbulence and Predictability of Geophysical Fluid Dynamics*, M. Ghil, R. Benzi and G. Parisi, Varenna, Italy, June 1983. Proceedings: *Turbulence and predictability in geophysical fluid dynamics and climate dynamics* M. Ghil, R. Benzi and G. Parisi, eds. (Amsterdam: North-Holland, 1985).

[51] R. Benzi and A. Sutera, “The mechanism of stochastic resonance in climate theory,” In: *Turbulence and Predictability in Geophysical Fluid Dynamics and Climate Dynamics*, M. Ghil, R. Benzi, and G. Parisi, eds. (Amsterdam: North-Holland, 1985): 403-423.

theory.[52] He arrived with a typewriter and lots of papers, writing this book. That was the school where for the first time Uriel Frisch[53] showed the result about the anomalous scaling of all moments—at the time eight—of turbulence.[54] The idea that was behind intermittency was a stick on the fractal dimension introduced by Mandelbrot. The idea is that you have this vortex or you have sheets—those kinds of stuff—in the turbulence behavior, which have a fractal dimension statistically. You play with the idea of geometry that has some random property and then if you use those ideas, you are able to obtain two things: anomalous scaling for the few moments and the agreement with the Kolmogorov equation, which is an exact equation.[55] Then, of course, the question is that the results obtained by Uriel were not correct. They were so-to-speak against this interpretation, that this interpretation was too naïve. We were lucky for three reasons. First of all, Uriel did not put error bars on the numbers. If, at that time, you were putting the error bars they were so huge that there was no possible discussion. A straight line was still correct. Second, Giorgio was there. So, for two days we were discussing together about what is the possible generalization of this interpretation. Eventually, he came out with this simple idea of the multifractal. The multifractal is simple and abstract, which is always the taste of Giorgio. The problem is that simple is good, but abstract is a nightmare. We had to fight for, I guess, 20 years to convince people that the original idea was correct. Now, we have very good ground to say that the original idea of Giorgio was correct. We have the possibility of making predictions starting from there, which is non-trivial.

PC: Was the original reception of this proposal also challenging?

RB: [0:44:45] As usual, everybody was enthusiastic, because everybody was working on the fractal. The idea to have more room to write papers, as usual, makes people enthusiastic. There were some discussions, because one of us—Giovanni Paladin[56]—gave our paper to Kadanoff in Trieste in ’85, and after a while Kadanoff, Procaccia and other people wrote a paper about the multifractal without citing us.[57] So, Giorgio wrote a letter to

[52] G. Parisi, *Statistical field theory* (Redwood City, CA: Addison-Wesley, 1988).

[53] Uriel Frisch: https://en.wikipedia.org/wiki/Uriel_Frisch

[54] U. Frisch, “Fully developed turbulence and intermittency,” In: *Turbulence and Predictability in Geophysical Fluid Dynamics and Climate Dynamics*, M. Ghil, R. Benzi, and G. Parisi, eds. (Amsterdam: North-Holland, 1985): 71-84.

[55] Kolmogorove equations: https://en.wikipedia.org/wiki/Kolmogorov_equations

[56] See, *e.g.*, “Premio ‘Giovanni Paladin’,” *Società Italiana di Fisica Statistica* (n.d.) https://www.fisicastatistica.org/premio-giovanni-paladin (Accessed February 8, 2024.)

[57] M. H. Jensen, L. P. Kadanoff and I. Procaccia, "Scaling structure and thermodynamics of strange sets," *Phys. Rev. A* **36**, 1409 (1987). https://doi.org/10.1103/PhysRevA.36.1409

Kadanoff. Kadanoff said: "Yes. You are right." Kadanoff was a gentleman. Also, I am sure—because I worked with him[58]—that he was really forgetting who gave him the paper, or even reading the paper. Kadanoff wrote an article in *Physics Today* making the full story and acknowledged our results.[59] That was the point. But the idea was originally very well accepted in the statistical physics community. If you were speaking with turbulence people, they immediately rejected this point of view. This was the starting point of our fight. That was a lot of energy. If you do something that everybody agrees, then it's boring. If there is discussion, then you start to understand very well. Then, this is the most interesting part of the job.

FZ: I had another question. When Giorgio was working on this spin glass stuff, you said he was mostly working in Paris. Still, in this department, there was a group of people working on impurities in solid state physics, localization.

RB: [0:47:08] Probably. I don't know, as I said.

FZ: So, you don't remember if there was any discussion between them and Giorgio?

RB: [0:47:18] No. I don't remember anything. People like Giovanni Gallavotti[60] were really well introduced into this problem. Jona-Lasino as well. But remember that this was the time, the beginning of the '80s, that when Giorgio was here, most of the time was spent on the APE supercomputer.[61] That was really a strong effort with many people. The APE supercomputer was not done for the spin glass at the very beginning, not even for turbulence.

PC: Following your work on turbulence with Giorgio, your publication records started to be more distinct.

RB: [0:48:20] Up to the point when Giorgio was working at Tor Vergata, up to '92-'93. So, from time to time we were discussing. We wrote several papers together on the subject. I was discussing with him the problem of the lattice Boltzmann equation.[62] That was our breakthrough, to do simulations on a parallel computer. We were discussing a lot of the Shell

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[58] See, *e.g.*, L. P. Kadanoff, D. Lohse, J. Wang and R. Benzi, "Scaling and dissipation in the GOY shell model," *Phys. Fluids* **7**, 617-629 (1995). https://doi.org/10.1063/1.868775
[59] L. P. Kadanoff, "A Model of Turbulence," *Physics Today* **48**(9), 11 (1995). https://doi.org/10.1063/1.2808151
[60] Giovanni Gallavotti: https://en.wikipedia.org/wiki/Giovanni_Gallavotti
[61] APE100: https://en.wikipedia.org/wiki/APE100
[62] See, *e.g.*, R. Benzi, S. Succi and M. Vergassola, "The lattice Boltzmann equation: theory and applications," *Phys. Rep.* **222**, 145-197 (1992). https://doi.org/10.1016/0370-1573(92)90090-M

model, that is a simple model on which to test the multifractal idea.[63] Then, he moved here, in La Sapienza. Then, from ’95 to 2003, I was working in the government, essentially, in one of these authorities,[64] so I wasn’t really following what he was doing. But up to ’93-’94, we were very close together in working. Then, we were essentially working in direct numerical simulation of turbulence. That was our major effort. He participated also in the discussion when we did the extended self-similarity, in all of these steps.[65] We were not writing a lot of papers together, but a few of them were important and interesting. I started to understand better spin glasses, when I moved to soft matter, in the simulation of lattice Boltzmann.[66]

PC: Is there anything else you'd like to tell us about this era, from the mid ‘70s to the mid ‘90s, that we may have overlooked?

RB: [0:50:43] I don’t know if I understand correctly. Are you trying to understand the period of time when these two ideas were emerging together?

PC: We’re trying to understand your perspective on what was happening, and your experience working with Giorgio on different problems at that time.

RB: [0:51:02] The most funny experience I had was in ’92. We organized a school on turbulence—no longer geophysics—in Les Houches.[67] I invited Giorgio, and he gave a talk on the work we did together on the Shell model.[68] There were other people. Among them, Sasha Migdal.[69] (The son

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[63] See, *e.g.*, R. Benzi, L. Biferale, and G. Parisi, “On intermittency in a cascade model for turbulence,” *Physica D* **65**, 163-171 (1993). https://doi.org/10.1016/0167-2789(93)90012-P

[64] L'Autorità per l'informatica nella pubblica amministrazione : https://it.wikipedia.org/wiki/Autorit%C3%A0_per_l%27informatica_nella_pubblica_amministrazione

[65] See, *e.g.*, R. Benzi, S. Ciliberto, R. Tripiccione, C. Baudet, F. Massaioli and S. Succi, “Extended self-similarity in turbulent flows,” *Phys. Rev. E* **48**, R29 (1993). https://doi.org/10.1103/PhysRevE.48.R29; R, Benzi, S. Ciliberto, C. Baudet, G. R. Chavarria and R. Tripiccione, “Extended self-similarity in the dissipation range of fully developed turbulence,” *Europhys. Lett.* **24**, 275 (1993). https://doi.org/10.1209/0295-5075/24/4/007

[66] See, *e.g.*, R. Benzi, S. Chibbaro and S. Succi, "Mesoscopic lattice Boltzmann modeling of flowing soft systems," *Phys. Rev. Lett.* **102**, 026002 (2009). https://doi.org/10.1103/PhysRevLett.102.026002; R. Benzi, M. Sbragaglia, S. Succi, M. Bernaschi and S. Chibbaro, “Mesoscopic lattice Boltzmann modeling of soft-glassy systems: theory and simulations,” *J. Chem. Phys.* **131**, 104903 (2009). https://doi.org/10.1063/1.3216105

[67] *NATO Advanced Studies Institute: Turbulence in Spatially Extended Systems*, R. Benzi, C. Basdevant and S. Ciliberto, Les Houches, France, January 1992.

[68] G. Parisi, "Multifractal and intermittency in turbulence," In: *Turbulence in Spatially Extended Systems*, R. Benzi, C. Basdevant and S. Ciliberto, eds. (New York: Nova Science Publishers, 1993): 163-187.

[69] Alexander A. Migdal: https://en.wikipedia.org/wiki/Alexander_Arkadyevich_Migdal

of the famous Migdal of phase transitions,[70] but the son also is extremely clever.) He started to be interested in turbulence. He was giving a talk about circulation. One of the problems of turbulence is what is the correct way to characterize the statistical properties of turbulence. You cannot take one point, so you take the correlation function as you do in field theory. But this is not written in stone. You can take other things. For instance, you can take the circulation on a loop and say whether the statistical properties of the circulation are different or similar. You can say something and so on. He was making the claim that for the statistical property of circulation, there were no anomalous scaling. I remember a huge discussion in '92, together with 100 people, where Giorgio stood up, went to the blackboard, and gave a demonstration of why the argument is wrong. That was very nice. There was a talk by Giovanni Gallavotti on the existence of a new theorem about the existence and uniqueness of the solution of the Navier-Stokes equation that was somehow inspired from the spin glass theory.[71] So, there was a cross-fertilization between the two fields, but you have of course two different phenomena. It was clear that it was not a direct mixing.

Where there was strong mixing was in 1985, in the Hopfield model. That was a completely different story. I was, at that time, working at IBM Research Center, the IBM European Center for Scientific and Engineering Computing. I was able to convince IBM to have a group in computational physics. I don't know how I was able to do that, but they gave me a group in computational physics. So, we started to use the supercomputers—at that time, they were not really super—to do computational physics. One particular topic was neural networks. Giorgio was visiting there several times, together with Nicola. We were working together on understanding how to improve our understanding of neural networks, of the Hopfield model and so on. There was a very nice paper by Paternello and Carnevali,[72] which was inspired by Giorgio. Giorgio always cite this paper by Paternello. That was one of the things that was coming out of this collaboration.

FZ: Where was this center?

RB: [0:55:20] The center was in Roma. At a certain point, somebody suggested me to make a discussion with IBM. So, I went to a colloquium, and we discussed about the research center in IBM. Remember, at that time, IBM

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[70] Arkady Migdal: https://en.wikipedia.org/wiki/Arkady_Migdal

[71] G. Gallavotti, "Some rigorous results about 3D Navier-Stokes," In: *Turbulence in Spatially Extended Systems*, R. Benzi, C. Basdevant and S. Ciliberto, eds. (New York: Nova Science Publishers, 1993): 45-74.

[72] P. Carnevali and S. Patarnello, "Exhaustive thermodynamical analysis of Boolean learning networks," *Europhys. Lett.* **4**, 1199 (1987). https://doi.org/10.1209/0295-5075/4/10/020

was fighting to get a position in supercomputers. At that time, IBM was computer. There was no Apple, no Microsoft, nothing. There was only IBM and UNIVAC.[73] UNIVAC was more for the scientific computations, and IBM was having everything else. IBM was supposed to launch the new mainframe with parallel computing and vector computing to challenge Cray.[74] IBM was also thinking of launching the personal computer, which came out in ’82.[75] So, we discussed the possibility of doing some research. They were not really doing research. They were supporting, in some sense, IBM in showing that the mainframes were able to do something also for research. It was a kind of a showroom. They were changing the idea and tried to push this direction of research. They were establishing a European Center for Vector and Parallel Computing here in Roma. I was able convince them that computational physics was one of the topics, together with others like image processing and so on. They gave me five guys. One was Sauro Succi,[76] who is still working with me. The other ones were [Paolo] Santangelo, [Stefano] Paternello, and others. For seven years, I was working there. When I got a position at the university, I left IBM.

FZ: So, Giorgio and Nicola were visiting regularly. What did you discuss?

RB: [0:58:04] One of the things that we were discussing was… One of the computers in IBM was called the 3080.[77] The 3080 was very expensive. One of the ideas that they had at CERN was to build up an emulator of the 3080, which was a very simple machine. They made a lot of them in order to get the data from the collider and to make the analysis as fast as possible. Remember that Nicola was thinking about the APE, so they were basically discussing with us how to build up APE and how to collaborate. As IBM, I was part of the meeting in CERN, when Nicola discussed the APE with other people. This was a fantastic meeting.

FZ: So, the Hopfield model came…

RB: [0:59:28] The Hopfield model was one of the things that we were working on. I didn’t work on the Hopfield model, but I followed the work that they did, of course. I was responsible for that. They were working on that. [Stefano] Patarnello was working on spin glasses, on Potts models.[78]

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[73] UNIVAC: https://en.wikipedia.org/wiki/UNIVAC
[74] Cray: https://en.wikipedia.org/wiki/Cray
[75] IBM Personal Computer: https://en.wikipedia.org/wiki/IBM_Personal_Computer
[76] Sauro Succi: https://en.wikipedia.org/wiki/Sauro_Succi
[77] IBM 308X: https://en.wikipedia.org/wiki/IBM_308X
[78] See, *e.g.*, S. Caracciolo and S. Patarnello, "Effects of frustration on the orderings of multi-valued spin systems," *Phys. Lett. A* **126**, 233-238 (1988). https://doi.org/10.1016/0375-9601(88)90752-9

PC: Was the IBM Research Lab in Rome at all in touch with the US one,[79] with Scott Kirkpatrick,[80] for instance?

RB: [0:59:57] Yes. We were visiting regularly the Yorktown one and the other research centers both in Europe and the States. There was the idea of setting up a sort of Yorktown Lab in Europe. But then, for a reason I never understood, the stock of IBM was crashing by a factor of two,[81] so they cancelled this piece.

The other thing that I do remember sitting down in one of the meetings with the people that were responsible for the personal computer, that was moving the step to the personal computer. The guy, unfortunately, died after a few months together with all of the collaborators in air crash.[82] That is one of the reasons why all the development of the software went to Microsoft. Microsoft was one of the things that you were seeing on the screen when you started to work with this PC. That was all we knew about Microsoft. I must say that the software in IBM was not so good. So, there was this link.

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

RB: [1:01:47] I have pictures from then but not notes.

FZ: What about letters? For example, this letter that Giorgio wrote to Kadanoff to ask…

RB: [1:02:07] No. Maybe Angelo has it, but I don't have it. I had a lot of stuff, but in the last 20 years, everybody threw away all the papers, so it is difficult to find something.

PC: Prof. Benzi, thank you very much for this conversation.

RB: [1:02:47] It's my pleasure.

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[79] Thomas J. Watson Research Center: https://en.wikipedia.org/wiki/Thomas_J._Watson_Research_Center

[80] P. Charbonneau, *History of RSB Interview: Scott Kirkpatrick*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 24 p. https://doi.org/10.34847/nkl.cba615t7

[81] On Black Monday, October 19, 1987, the stock of IBM shares by 31 ¾ to 103 ¼.

[82] See, *e.g.*, David E. Sanger, "Philip Estridge Dies in Jet Crash; Guided IBM Personal Computer," *The New York Times* (August 5, 1985). https://www.nytimes.com/1985/08/05/us/philip-estridge-dies-in-jet-crash-guided-ibm-personal-computer.html (Accessed February 9, 2024.)