History of RSB Interview: Raymond Orbach

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PC: Good morning, Professor Orbach. Thank you very much for joining us. As we've discussed ahead of this interview we'll mostly be focusing on the theme of replica symmetry breaking and its formulation, a period which we bound roughly from 1975 to 1995, but we'll bleed a bit on both ends. First, we'll be asking a few questions on background. You were trained as a theoretical condensed matter physicist, and largely kept on that track once you became a faculty member[1]. But from what I can tell, around the late '60s-early '70s you seem to have gotten increasingly involved in actual experiments. It then became a larger share of your production. Is that correct? If yes, can you walk us a bit through what led to this transition in your production, in your research?

RO: [0:01:01] First of all, I remain a theoretical physicist. What started my experimental program was my work with Professor Kittel[2] at Berkeley when I was a graduate student[3]. I had a lot of ideas but I couldn't find people who would actually explore them in the laboratory. The same was true, by the way, of Kittel himself. His own issues and interests led to very strong collaborations with experimentalists. I guess it was from that experience that I developed my own trajectory. I went to Oxford as a

[1] For additional context, see D. Zierler, "Oral history interview with Raymond Orbach, 2020 May 5 and 2020 May 13," *Niels Bohr Library & Archives, American Institute of Physics* (2020). https://www.aip.org/history-programs/niels-bohr-library/oral-histories/44543
[2] Charles Kittel: https://en.wikipedia.org/wiki/Charles_Kittel
[3] Raymond Lee Orbach, *Some problems in spin wave theory in ferromagnets and antiferromagnets and in the generation and attenuation of microwave frequency phonons at low temperatures,* PhD Thesis, University of California, Berkeley (1960). https://berkeley.primo.exlibrisgroup.com/permalink/01UCS_BER/1thfj9n/alma991035638289706532

postdoc, working with a theory group, but in fact I went most often over into the experimental wing of the Clarendon Laboratory[4], and talked to people who were doing experiments. That began that close collaboration between theory and experiment which I think marks most of my career since the early 1960s. I went to Harvard as an assistant professor and I asked for a laboratory for that reason. That has been the case for my career. I've always tried to have an experimental program associated with the theoretical interests that I have. That was true then, and has remained true over the years. Today, I'm a retired physicist at the University of Texas at Austin, but I still maintain a laboratory and have a research program associated with theory, but also more and more with experiment. As we will discuss, the confluence of simulations, theory and experiment that Giorgio [Parisi] really pioneered has led to an extraordinary period of productivity on complex system dynamics.

PC: Would it be fair to say then that it's a misreading to think that you got more involved with experiments at some point? You always had both of them going in parallel, and there's not a time when you ramped up the experimental side.

RO: [0:03:56] That depends on the eyes of the beholder. P. W. Anderson[5] always referred to me as an experimentalist. It depends who is looking at it. What I try to do is to understand the subtleties of the theory and turn them into a practical experimental tests. That's indeed how I got involved into replica symmetry breaking.

PC: When did you first hear about spin glasses?

RO: [0:04:30] A long time ago. I was fascinated by the slow dynamics of complex systems. Even in my earliest years at UCLA—again I started a laboratory—we began to look at what was then known as spin glasses and their properties. But you have to remember [that] at that time we didn't understand very much at all about how they behaved. So almost on my arrival at UCLA, in 1963, I began work on that. One of my first graduate students, Ralph Chamberlain[6], was really the first to see what we call the waiting time effect in the experiments on the thermal remanent magnetization of spin glasses. This is back in the '60s and early '70s. In the period that the dynamics began to unfold, we were very much involved—

[4] Clarendon Laboratory: https://en.wikipedia.org/wiki/Clarendon_Laboratory

[5] Philip W. Anderson: https://en.wikipedia.org/wiki/Philip_W._Anderson

[6] Ralph Vary Chamberlin, *Magnetic measurements of spin glasses,* PhD Thesis, University of California, Los Angeles (1984).

along with the Swedes[7] at Uppsala University—in trying to figure out what this phenomena was.

PC: I think that the first couple of papers from your group that use the words spin glasses were published in 1978 with UCLA collaborators Paul Chaikin and Ivan Schuler, in particular[8]. Do you remember how these works came about or why did you start calling them spin glasses?

RO: [0:06:13] That followed on our earlier work, where we really got into the dynamics of it[9]. I don't always put my name on papers that my students publish, because if they do the work themselves I think they deserve to be the authors. So you won't see my name on some of those very early papers, mainly the one by Chamberlin, for example. But it was in my laboratory and he was my student at that time. So what you're referring to was a continuation of that research.

PC: In 1979, you had a collaboration with Jean Souletie at Grenoble[10]. How did that come about? How did that work come together?

RO: [0:07:05] Because of my interests and also the wonderful work that the magnet lab in Grenoble[11] had been doing[12], I visited the laboratory and we talked about experiments. Again, you have to remember this is before we really understood the nature of the dynamics. Those first two decades really were ones of exploration of what was going on. We really didn't understand it. The waiting time effect, for example, which is well-known in polymers and other materials, was a surprise because people had assumed that when you cooled a spin glass in a magnetic field, the magnetization stayed constant, so they said that was the equilibrium state.

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[7] See, *e.g.*, Olof Beckman: https://sv.wikipedia.org/wiki/Olof_Beckman; Per Nordblad: https://sv.wikipedia.org/wiki/Per_Nordblad

[8] For instance: I. Schuller, R. Orbach, P. M. Chaikin, "Spin-flip scattering time of a spin-glass," *Phys. Rev. Lett.* **41**, 1413 (1978). https://doi.org/10.1103/PhysRevLett.41.1413; I. N. Ibrahim, E. Chock, R. Orbach, I. Schuller, "Susceptibility of a thin-film spin glass," *Phys. Rev. B* **18**, 3559 (1978). https://doi.org/10.1103/PhysRevB.18.3559. **PC:** The first external funding related to spin glasses also appears at that time. See, *e.g.*, R. Orbach,"An Experimental Study of Spin Glasses and Dynamics of Superconductors", *National Science Foundation,* **DMR 7827129** (1979-1982). https://www.nsf.gov/awardsearch/showAward?AWD_ID=7827129 (Accessed July 18, 2022.)

[9] See, *e.g.*, O. Entin-Wohlman, G. Deutscher and R. Orbach, "Anomalous spin-flip lifetime near the Heisenberg- ferromagnet critical point," *Phys. Rev. B* **11**, 219 (1975). https://doi.org/10.1103/PhysRevB.11.219

[10] E. D. Dahlberg, M. Hardiman, R. Orbach, and J. Souletie, "High-frequency ac susceptibility and ESR of a spin-glass," *Phys. Rev. Lett.* **42**, 401 (1979). https://doi.org/10.1103/PhysRevLett.42.401

[11] Laboratoire National des Champs Magnétiques Intenses: https://en.wikipedia.org/wiki/Laboratoire_National_des_Champs_Magn%C3%A9tiques_Intenses

[12] For some context: J. Souletie, J. "Les Verres de spin," *J. Physique Coll.* **39**, C2-2—C2-16 (1978). https://doi.org/10.1051/jphyscol:1978202

That was the word that we all heard, but the problem was that it wasn't. What we found was that it depended on the amount of time you waited before you changed the magnetic field. So it couldn't possibly be the equilibrium state. There were a series [of works] during the early days of spin glass research, where there were just a lot of wrong turns. Not because people were ignorant, it was because there simply wasn't experimental evidence that indicated that the ideas were wrong. Of course, it's become more and more fruitful as time went on.

PC: Do you know what interested Jean Souletie himself to this problem? Did you bring this problem to him or was he already working on spin glasses?

RO: [0:08:53] He was already working on it. The Grenoble group, starting with Néel[13], had done a lot of work on dilute magnetic systems. They were really the pioneers in looking at what we call the thermal remanent magnetization. That's their words that have stayed in the literature. So if you wanted to explore magnetism and its dynamics you went to Grenoble.

PC: Was this part of a sabbatical leave, or you were just visiting through?

RO: [0:09:27] Every time I could get to France, I did. It was sabbaticals, it was travel, it was conferences. I just love the way they do physics. In fact, I spent a sabbatical year there in the early 1980s where I worked on fractals, of all things. The French environment for research, to me, is just extraordinary. They are wonderful people to interact with, and they love to argue, which is of course the basis of how you get progress in this field.

PC: As you were doing these experiments on the dynamics first replica symmetry breaking (RSB) results of Parisi appeared in 1979 and 1980[14]. Did you pay any attention to that work at that time?

RO: [0:10:23] Absolutely. We all—everyone—tried, first of all, to reproduce what he had done, which turned out to be very hard, for us anyway. We all understood the states that he was working with, but you have to remember that Parisi's solution to the mean-field [model] of Sherrington-Kirkpatrick was not a dynamic calculation. It was static. His so-called pure states, which are a very small subset of the total number of states, had infinite barriers between the states. There were no dynamics. So we all read it, and thought it was interesting but it didn't give us really a clue as

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

[14] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Giorgio Parisi*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, XX p. https://doi.org/10.34847/nkl.7fb7b5zw

to how to translate our dynamical experiments into the language of RSB. It was really experiments that we did with the French group at Saclay, in the late 1980s and the early 1990s with Michel Hammann's group and his colleagues that gave us the connection[15]. It was frustrating, in a way, that we had no guidance on the dynamics, because there were no dynamics in the mean-field solution by Parisi. But what we did was a series of temperature changes and magnetic field changes that surprisingly showed us the relationship between the statics and the dynamics. That was published[16] actually at a conference in 1991 in Trieste[17], at the theory Institute[18], where for the first time we were able to relate the laboratory dynamic to Parisi's—what I would call—structure of phase space. What we found was that the symmetries that were contained in the pure states applied to the dynamical states. That is, when we looked at the free energy manifold it was self-similar. It didn't change as a function of temperature. That was astonishing. We made measurements from close to the glass temperature to about half, and we kept seeing the same dynamics. Well, what other kind of systems would you find where you change the temperature by that much but the dynamics stayed the same? It slowed down but its properties remained the same. What we found, when we did an analysis of our results was that if we make a significantly large temperature drop, the barriers in between diverged. That was the first clue that in fact the pure states were nothing but the consequence of the dynamical states exploding in their barrier heights as you lowered the temperature. That is, the free energy barriers were not infinite, they were finite, but they all diverged as the temperature was lowered. So, what we found from experiment, which later turned out to be consistent with a number of theoretical pictures, was that the phase transition for spin glasses was a continuous set of phase transitions, starting at the glass temperature and going down to as low as we could measure. That is, at every temperature, you give me a barrier height and I will tell you the temperature at which it diverges. That means that I can tell you then how the dynamics fits into the statics that Parisi had calculated. To me, that was

[15] See, *e.g.*, M. Lederman, R. Orbach, J.-M. Hammann, M. Ocio, and E. Vincent, "Dynamics in spin glasses," *Phys. Rev. B* **44**, 7403 (1991). https://doi.org/10.1103/PhysRevB.44.7403; M. Lederman, R. Orbach, J.-M. Hammann, and M. Ocio, "Temperature dependence of barrier heights in spin glasses," *J. Appl. Phys.* **69**, 5234-5236 (1991). https://doi.org/10.1063/1.348089

[16] J.-M. Hammann, M. Lederman, M. Ocio, R. Orbach, and E. Vincent, "Spin-glass dynamics: relation between theory and experiment: a beginning," *Physica A* **185**, 278-294 (1992). https://doi.org/10.1016/0378-4371(92)90467-5

[17] *International Conference on Complex Systems: Fractals, Spin Glasses and Neural Networks*, Giorgio Parisi, Luciano Petronero, Miguel Virasoro, 2-6 July 1991, International Center for Theoretical Physics in Trieste, Italy. Proceedings: *Physica A* **185**(1-4). https://www.sciencedirect.com/journal/physica-a-statistical-mechanics-and-its-applications/vol/185/issue/1

[18] International Center for Theoretical Physics: https://en.wikipedia.org/wiki/International_Centre_for_Theoretical_Physics

the most important result we've ever produced. Namely, we were able to connect the beautiful phase space symmetry that Parisi had developed to laboratory time scales. That has driven us ever since 1990 in all of our experiments.

PC: I'd like to just step back a decade or so before getting to that. You mentioned earlier that you had a sabbatical at the ESPCI in 1981-1982. Is that when you got to dig in more seriously in the theoretical representation of spin glasses? In particular, is that when you got in touch with Bernard Derrida[19], Gérard Toulouse[20] and others in the neighborhood? Or did you know them from before?

RO: [0:15:46] No. This was a discovery for me. École normale was right around the corner and I would wander over almost daily. What was really important, in addition to my meetings at École normale, were the lunches. There was a little street called Pot-au-Fer[21], which had a series of little restaurants. The theorists and the experimentalists would go there almost every day for hours-long French lunches. But it was then that ideas got thrown around that I enjoyed so much, and why I made that comment right at the beginning of this talk. It was a fascinating opportunity for me to see how different people do physics differently. I did meet almost all of those who were involved in the early stages of spin glass theory, [and], in particular, the ultrametric symmetry, which was really invented—or discovered, if you like—at École normale by Toulouse, by the group that he worked with, a wonderful collection of theorists[22].

It turns out that my major work during that year was not on spin glasses. I had been fascinated—again because of symmetries of random systems—by fractal geometry. Again, a similar self-similar geometry, but this time in real space. I became fascinated by it, and a colleague of mine in Israel, Shlomo Alexander[23], who had worked with me closely over the years, and I began to think about how one would describe the dynamics of a fractal network[24]. Again, dynamics vs statics. Fractal geometry, of course, from

[19] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Bernard Derrida*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 23 p. https://doi.org/10.34847/nkl.3e183b0o

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

[21] Rue du Pot-de-Fer: https://fr.wikipedia.org/wiki/Rue_du_Pot-de-Fer

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

[23] Z. Luz, R. Bruinsma, Y. Rabin, and P.-G. De Gennes, "Shlomo Alexander," *Physics Today* **51**(12), 73 (1998). https://doi.org/10.1063/1.2805729

[24] S. Alexander and R. Orbach, "Density of states on fractals: «fractons»," *J. Physique Lett.* **43**, 625-631 (1982). https://doi.org/10.1051/jphyslet:019820043017062500

Mandelbrot[25] and on, was well known and used almost everywhere in a number of applications. But no one had actually done the dynamics. If you had a fractal network, how would it vibrate? How would particles diffuse? Shlomo Alexander and I began to explore that. And then we interacted with Derrida who had some wonderful pictures of dynamical systems that were random. Indeed, I published a paper with him during that period[26]. But what we discovered was to actually calculate the dynamics of fractal networks. I focused more on that than on spin glasses, but of course I was interested in and got involved with the theory that people were working on at École normale.

PC: In 1984-85, you published two PRLs on relaxation dynamics in spin glasses[27]. You found in one that the results were in agreement with an exponential distribution of states, and the other with theories that were not related to spin glasses. How closely were you following, or influenced by the theory work in your study of dynamics at that point?

RO: [0:19:28] Well, I had been following the theoretical developments in particular at the phase transition. There were some theories of Toulouse on the nature of the spin glass transition, and I had been working in that direction. We did a series of experiments to test whether the so-called [Gabay-Toulouse transition] was correct or not. One of those PRLs showed that in fact you did have the [Gabay-]Toulouse transition of the transverse moment and then you had the freezing of the longitudinal[28]. So you went from the Toulouse transition to the de Almeida-Thouless transition. That was one of the PRLs. Remember, in those days, again we didn't really understand the nature of the spin glass transition, at least as far as I'm concerned. So we were trying to probe it experimentally in any way we could. We didn't understand it until later that decade—I've already made reference to the Physica paper, where we showed the connection with Parisi and the nature of the continuous set of phase transitions. But in '85 that was unknown and so we were probing it as best we could.

[25] Benoît Mandelbrot: https://en.wikipedia.org/wiki/Benoit_Mandelbrot

[26] B. Derrida and R. Orbach, "Frequency dependence of the conductivity in presence of an electric field in one dimension: weak-disorder limit," *Phys. Rev. B* **27**, 4694 (1983). https://doi.org/10.1103/PhysRevB.27.4694

[27] R. V. Chamberlin, G. Mozurkewich, and R. Orbach, "Time decay of the remanent magnetization in spin-glasses," *Phys. Rev. Lett.* **52**, 867 (1984). https://doi.org/10.1103/PhysRevLett.52.867; R. Hoogerbeets, W. L. Luo, and R. Orbach, "Spin-glass response time in Ag: Mn: exponential temperature dependence," *Phys. Rev. Lett.* **55**, 111 (1985). https://doi.org/10.1103/PhysRevB.30.6514

[28] **PC:** It's actually a later paper: G. G. Kenning, D. Chu, and R. Orbach, "Irreversibility crossover in a Cu: Mn spin glass in high magnetic fields: Evidence for the Gabay-Toulouse transition," *Phys. Rev. Lett.* **66**, 2923 (1991). https://doi.org/10.1103/PhysRevLett.66.2923

PC: In 1982, you became Provost of the College of Letters and Science at UCLA. In that position, did you ever get to the chance to tilt the scale in favor of spin glasses and/or replica symmetry breaking at UCLA?

RO: [0:21:53] No. I kept it separate. The Provost position was a new position that Chancellor Young[29], at that time, had created. It was a very powerful position because I was in charge of the College of Letters and Science[30], which at that time was the largest academic unit in the entire University of California complex. So I had my hands full, trying to organize the college, really to bring it into the eminence that it deserved. I was a Provost with the so-called associate Deans of the various divisions of the College. I transitioned them into full deans. We developed the power of the college. But at the same time I continued my research. I also continued teaching freshman physics, which I enjoyed. I felt that I could set an example of somebody going into administration, but also understanding what it was like to teach and do research at the same time. Those were 10 years, [when] I really developed our experimental program while at the same time I was doing administration. I didn't sleep much during those years.

PC: Was there ever the opportunity to try to foster sub-areas of interest in that position?

RO: [0:23:23] I would never do that. Scientists, humanists and social scientists should be completely free to choose their fields of interest. No. I kept my laboratory… Actually, at the time it wasn't very popular, because spin glasses were somewhat of a loner experiment. There were only a few laboratories around the world who were involved in it. In fact, there was some feelings that I felt that my work was really tangential to the main course of physics. Who was interested in these spin glasses anyway? We had the very fortunate opportunity to grow our own samples. So here you see again, where I could actually test ideas about anisotropy, about coupling, about concentrations, about different hosts experimentally to see if I could tease out the theoretical dynamics that we were seeing.

PC: You were just saying that there was only a pretty small group of people worldwide who were interested at this point. But there was a certain concentration of interest in California, and in Los Angeles in particular. Was there ever an idea to propose a center, say a Materials Research Center[31], that would be focused on spin glasses at UCLA?

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[29] Charles E. Young: https://en.wikipedia.org/wiki/Charles_E._Young
[30] College of Letters and Science: https://en.wikipedia.org/wiki/UCLA_College_of_Letters_and_Science
[31] Materials Research Centers:
https://en.wikipedia.org/wiki/Materials_Research_Science_and_Engineering_Centers

RO: [0:25:10] We had ideas for major programs, but not for spin glasses. As I said, it was viewed as a relatively narrow area. We made a number of attempts to put together a large collaborative program on the campus but spin glasses never entered into that language.

PC: What was then the focus of these efforts?

RO: [0:25:43] It was very early on, but I was also interested in systems where you have strongly interacting electron gases. Now, of course, with high-T_C [superconductors] and everything else, they are common, but at that time, apart from the work at Los Alamos, people really didn't understand what happened—especially in oxide materials—to systems where you had very high susceptibilities and interacting electron gases that were certainly not Fermi-like. So, with George Grüner[32], who was doing spin density wave experiments, we were exploring the possibility of developing strongly-coherent electron systems as a laboratory. My theoretical interests were not limited to just spin glasses, as you'll notice from the many papers that you are referring to. I was also interested in paramagnetic resonance[33]; I was interested in lattice dynamics[34]. One of my students and I had done very early work on what we have called phonon breakdown[35]. [This] is now standard parametric work, but at the time it was a theoretical concept that—to be fair—was ahead of its time.

PC: We now move forward to the 1990s. You mentioned earlier a work that was an important piece for your understanding of the relationship between dynamics and statics[36]. What rekindled your interest in spin glasses throughout the '90s?

RO: [0:27:50] It was certainly my work with the Saclay group. When we did some work in the early '90s, it was a very close collaboration. I would send a graduate student at Saclay and we would exchange samples so that they

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[32] George Grüner: https://en.wikipedia.org/wiki/George_Gr%C3%BCner

[33] See, *e.g.*, S. Chakravarty and R. Orbach, "Electron and nuclear magnetic relaxation in La_2CuO_4 and related cuprates," *Phys. Rev. Lett.* **64**, 224 (1990). https://doi.org/10.1103/PhysRevLett.64.224

[34] See, *e.g.*, R. Orbach, "Dynamics of fractal structures," *J. Stat. Phys.* **36**, 735-748 (1984). https://doi.org/10.1007/BF01012935; "Dynamics of fractal networks," *Science* **231**, 814-819 (1986). https://doi.org/10.1126/science.231.4740.814

[35] R. Orbach and L. A. Vredevoe, "The Attenuation of high frequency phonons at low temperatures," *Physics Physique Fizika* **1**, 91 (1964). https://doi.org/10.1103/PhysicsPhysiqueFizika.1.91; Lawrence Arthur Vredevoe, *The anharmonic interactions of acoustic and infrared phonons and the effects of the phonon electric field on vibronic spectra and on the lattice-dipole interaction in KC1:OH⁻*, PhD Thesis, University of California, Los Angeles (1966). https://search.library.ucla.edu/permalink/01UCS_LAL/trta7g/alma9935532513606533

[36] See Ref. 15.

would do work on their apparatus and we'd do work on our apparatus and compare. The results were so bizarre that I wasn't sure if they were consequences of the apparatus or real. The paper that I already referred to was a result of that collaboration. I spent a lot of time at Saclay, because the theory group with Bouchaud[37] and the experimental group with Vincent[38] and Hammann[39] were, as far as I was concerned, the best in the world in spin glasses at that time. We worked very closely together as the number of publications shows. The 1991 paper with the Saclay group [of] one of my students[40], Marcos Lederman[41]… Really, at the time, I didn't appreciate its importance, but even today we're using its results to understand rejuvenation and memory. It's just a remarkable paper. It talked about the temperature dependence of the barrier heights. We had the idea—really Michel Hammann's idea—that the dynamics of spin glasses was controlled not by some average over all of the free energy states, but rather a specific set of free energy states associated with the very largest free energy barrier. This, of course, resulted from the ultrametricity symmetry, where the number of states increases exponentially as the overlap between states diminishes, or—as we said—as the barrier heights increased. So it was only the states at the very highest level that were giving you the dynamics. Again, a real surprise. But that meant that we could actually pinpoint a specific barrier height. And then by doing some tricks with temperature cycling, we could actually measure how that barrier height changed as a function of temperature. Now, the full paper didn't emerge until a couple of years later than that I've already referred to, but it was that paper that showed the relationship between the barrier heights and the overlap between states[42]. Remember that was the basis of Parisi's analysis. Namely, he looked at the overlap q as the operative for his calculation of the order parameter. So we adopted the same language—again not for pure states but for dynamical states—and we were able then to associate in that paper the way the barrier heights increased as the overlap diminished, or, as we said, as the Hamming distance—sorry to be technical here—increased. (The Hamming

[37] Jean-Philippe Bouchaud: https://en.wikipedia.org/wiki/Jean-Philippe_Bouchaud

[38] "Eric Vincent," Centre pour l'énergie atomique (undated). https://iramis.cea.fr/Pisp/eric.vincent/ (Accessed July 17, 2022.)

[39] Jacques-Michel Hammann. See, *e.g.*, "Jacques Hammann, 80 ans," *Les Dernières Nouvelles d'Alsace* (3 October 2020). https://www.dna.fr/culture-loisirs/2020/10/03/jacques-hammann-80-ans (Accessed July 17, 2022.)

[40] See Ref. 15.

[41] Marcos Lederman, *Dynamics of random magnetic systems : spin-glasses and random fields*, PhD Thesis, University of California, Los Angeles (1991). https://search.library.ucla.edu/permalink/01UCS_LAL/17p22dp/alma9920633463606533

[42] J.-M. Hammann, M. Ocio, E. Vincent, M. Lederman, and R. Orbach, "Barrier heights versus temperature in spin glasses," *J. Mag. Mag. Mater.* **104**, 1617-1618 (1992). https://doi.org/10.1016/0304-8853(92)91480-H

is nothing but a way of looking at the overlap between the initial state and the stage you're dealing with[43].) It was a remarkable paper because it showed that the barrier heights depended not linearly as the overlap diminished, but actually increased exponentially. That sounds a little technical, but what it has led to is our very recent work. [This] still hasn't been published but is in the final stages with the Janus collaboration[44], where we showed that it's the basis of the breakdown of the symmetry of the dynamics of spin glasses in a finite magnetic field. This is somewhat technical, but the point is that it's this very early work—[from] 1991—that laid the groundwork for the dynamics that we're now actually probing almost to the limit of laboratory ability today.

PC: If I'm not mistaken, I think you also met Giorgio Parisi at that time. Is that correct? If yes, can you tell us a bit more about how that interaction came about and what it led to?

RO: [0:33:14] I had seen Parisi at meetings. I think—to be honest—he put up with me because I kept asking him these questions about how you relate dynamics to pure states. He was very patient and, I think, somewhat incredulous really at our results in the early '90s. But I don't know. We listened to everything that Parisi said. I met him at that conference I've already referred to in Trieste[45]. We sat down and really probed the results that we were finding with what his thoughts were. I had met him previously on a number of occasions. Those days, in the '90s, he didn't travel much. I was so interested in what he was doing that I found there was a conference every three years in Andalo that was organized by the University of Trento[46]. Andalo is a little tiny ski village in the Dolomites, just in the north of Italy. Parisi would take his family skiing. It was always held in March and there was still enough snow that you could still ski there. The conference would go on for a couple of weeks, and I made sure that I was there at all those conferences because there was Parisi for [a whole] week in the same room as I. I was able to ask him questions and probe. I had one

[43] Hamming distance: https://en.wikipedia.org/wiki/Hamming_distance
[44] http://www.janus-computer.com/
[45] Ref. 17.
[46] *IV International Workshop on Disordered Systems*, A. Fontana and G. Viliani, Andalo (Trento), Italy, March 4-6, 1991. Proceedings: *Philo. Mag. B* **65**(2) (1992). https://www.tandfonline.com/toc/tphb20/65/2;
V International Workshop on Disordered Systems, A. Fontana and G. Viliani, Andalo (Trento), Italy, February 27-March 3, 1995. Proceedings: *Philo. Mag. B* **71**(4) (1995). https://www.tandfonline.com/toc/tphb20/71/4;
VI International Workshop on Disordered Systems, A. Fontana and G. Viliani, Andalo (Trento), Italy, March 3-6, 1997. Proceedings: *Philo. Mag. B* **77**(2) (1998). https://www.tandfonline.com/toc/tphb20/77/2. See also: http://ds.science.unitn.it/index.php/Andalo/Previous (Accessed July 17, 2022.)

of my students come[47], and we sat down and worked through things with him. So I had met Parisi under this very nice informal relationship. Whether he remembers that or not, whether he thought anything about it, I have no idea. But it was the only way that I could interact with Parisi on an extended period. I think I went to three or four of these Andalo meetings[48]. They were just a wonderful opportunity, first of all to listen to him, and trying to understand what he was saying, but also to interact with him in this very informal environment.

PC: Did he, by any chance, visit your lab at any point?

RO: [0:36:00] Well, that starts my RSB experience. Yes. Remember that I was Provost for 10 years at UCLA, from 1982 to 1992. I was asked to be Chancellor—that's in other language President of a university—at one of the University of California campuses, the Riverside campus. I became Chancellor there in 1992 and stayed as Chancellor for 10 years. Just as I did at UCLA, I asked for a laboratory.

In those days, the superconducting quantum interference devices, or SQUIDs[49], were very primitive but they existed, and they were the most sensitive measure of magnetic moments. So we had a SQUID, but what I asked for was a room at Riverside that was as underground as possible, and as away from equipment as possible. So we got the dirtiest, lowest, no window room in the physics building, and then we dug a hole in the floor and buried the SQUID in this hole. People thought we were completely nuts, but what we were doing, of course, was using the Earth [to] shield the SQUID from all the background radiation that you have in any building.

So I continued my research while I was Chancellor. I also continued teaching freshman physics while I was Chancellor. My argument was that nobody could tell me they were too busy to teach freshmen if the chancellor was teaching freshmen, and doing research, and being Chancellor.

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[47] Marcos Lederman, Dynamics of random magnetic systems : spin-glasses and random fields, PhD Thesis, University of California, Los Angeles (1991). https://search.library.ucla.edu/permalink/01UCS_LAL/trta7g/alma9920633463606533 (Consulted August 11, 2022.)

[48] We note three: R. Orbach, "Vibrational transport in disordered systems," *Philo. Mag. B* **65,** 289-301 (1992). https://doi.org/10.1080/13642819208217903; D. Chu, G. G. Kenning, and R. Orbach, "Effect of magnetic fields on the relaxation of the thermoremanent magnetization in spin glasses," *Philo. Mag. B* **71**, 479-488 (1995)**.** https://doi.org/10.1080/01418639508238539; Y. G. Joh, R. Orbach, and J.-M. Hammann, "Spin-glass dynamics and the barrier model: Extraction of the Parisi physical order parameter," *Philo. Mag. B* **77** 231-238 (1998). https://doi.org/10.1080/13642819808204948

[49] SQUID: https://en.wikipedia.org/wiki/SQUID

It was in 1998 that I had my most productive interaction with Giorgio. Remember [that] I had met with him a number of times at Andalo and at conferences. I don't know if he ever took me seriously—frankly—but he was very kind and very helpful. It turned out that because I knew him, and I knew that he was attending the March Meeting of the American Physical Society in Los Angeles in 1998[50]—Riverside is almost a suburb of Los Angeles. I asked him if he would come on his way to Los Angeles and visit my laboratory. Precisely, on the 14th of March, at 11 o'clock in the morning Parisi came and visited our laboratory at the University of California, Riverside.

The reason I'm being so specific is that we had done a number of temperature cycling experiments, and we had data that we simply did not understand. We had piles of data, looking at the dynamics and what we call the thermal remanent magnetization decay, and in particular at the magnetic field dependence, which had never really been explored before. Parisi looked at it, and said: "Well, it's in my paper." I said: "What do you mean?" He said: "It's in my paper. In the PRL, volume 76, page 843, 1996.[51]" I said: "I read your paper in PRL, and I didn't find any reference there." He said: "Yes, look at Equation 4." It turns out that there's one sentence in that paper—that I had read but obviously not understood—that solved everything. All of the data fell onto a single line. It was one of the most incredible discoveries I have ever experienced. This had to do with the correlation length in spin glasses.

Remember [that] up to that time people had looked at critical exponents in both theory and experiment. They had looked at the decay in the dynamics, and tried to figure out some way of understanding it. But obviously the fundamental problem is the correlation length. Are the spins correlated, or aren't they? It was in that paper of 1996 that Parisi invented—with his colleagues—the correlation length for spin glasses. To me, that's one of the most important papers ever written on the topic. If you do a standard correlation length, that is, a two-particle correlation length, when you do the thermal averaging and the ensemble averaging it's zero for a spin glass. That's because [there's] no *obvious* magnetic order. But if you do the four-spin correlation function, it turns out to be finite. That was something that was in that PRL in 1996, that I hadn't picked up. It just suddenly exploded into an understanding of the dynamics. Even

[50] *1998 APS March Meeting,* Los Angeles Convention Center, Los Angeles, California, USA, 16-20 March 1998. See, e.g., "APS 1998 March Meeting," *APS News* **6**(10), 1 (1997). https://www.aps.org/publications/apsnews/199711/march.cfm (Accessed July 17, 2022.)

[51] E. Marinari, G. Parisi, J. Ruiz-Lorenzo, and F. Ritort, "Numerical evidence for spontaneously broken replica symmetry in 3D spin glasses," *Phys. Rev. Lett.* **76**, 843 (1996). https://doi.org/10.1103/PhysRevLett.76.843

today, we use the correlation length as the principal probe of spin glass dynamics, because, if you should think about it, that's why spin glasses are interesting. There is a correlation in this completely random system, spatially, that has a length scale. It was extraordinary that he predicted it from the Greens function, as I said, in Equation 4. Typical Giorgio, he said: “It’s obvious.” We said: “Well, thanks a lot.” We just didn't appreciate the fact that the correlation length existed. Once you do that, then it’s possible to think of the volume of the correlations in a spin glass. That is, instead of something distributed over all length scales, there's actually a region, a true correlation length for this four-spin concept that describes the coupling between the spins within that length scale. That suddenly said to us: “If that's true, there must be an energy associated, in a magnetic field, with that volume.” We called it the Zeeman energy. That is a function of the strength of the magnetic field. But if you could somehow isolate it, you would then know the correlation length, because you can measure in some way the absolute value of the Zeeman energy. And because it's related to a volume, the subtended radius of that volume is nothing but the correlation length. So by doing that quantitatively, we could come up with a number for the number of correlated spins in a spin glass. That has turned out, in my view, to revolutionize our understanding of spin glass dynamics. It all started at 11 o’clock, on the 14th of March, in that laboratory when Parisi came through. RSB, replica symmetry breaking, in my world began at that moment.

It has turned out to be one of the most powerful tools. It also separated us from the rest of the world doing spin glass dynamics. Fortunately, we had interactions with the Saclay group. We were working on a metallic system, namely copper manganese and silver manganese; they were working on a spinel system, an insulating system. We said: “It's true in ours. Is it true in yours?” This became the first evidence that there was a universality associated with spin glasses, that it didn't depend on the nature or the length scale of the coupling. Because obviously in a metal it’s much longer than what it would be for super exchange in an insulator. They then did the same set of experiments on their sample. We then exchanged samples, because we weren't sure… Remember, this was all first stuff. We didn't know whether we were right or not. The Phys. Rev. Letter that we published in 1999[52], titled “Extraction of the spin glass correlation length”, was joint with the Saclay group. To me, that was the monumental achievement that Parisi had generated. It has served us very well ever since then. So RSB started in my laboratory on the 14th of March at 11 o’clock in 1998.

[52] J. G. Joh, R. Orbach, G. G. Wood, J.-M. Hammann, and E. Vincent, “Extraction of the spin glass correlation length,” *Phys. Rev. Lett.* **82**, 438 (1999). https://doi.org/10.1103/PhysRevLett.82.438

PC: As you mentioned before, in recent years, you've been collaborating quite closely with the Janus group, on a mix of simulations, experiments and theory. Can you tell us how that came about? Does it directly flow from these late '90s experiments?

RO: [0:47:19] The Janus Collaboration was unknown at that time. Parisi, in his Nobel lecture, refers to the importance of simulations, and how they interface with theory to give you a picture of things that you just simply can't calculate analytically[53]. Up to about the mid-teens, 2012-2013, the simulation group had been looking at statics: critical exponents and so on[54]. A beautiful set of experiments. It occurred to me that they had the power of actually looking at the correlation length. They had the microscopic configurations. They could actually write down the nature of the spins and from that determine—they now call it the micro correlation length—the actual correlation length as a function of temperature. Now, what happened in the Janus collaboration was that there was a Janus I and a Janus II. Janus II was much more powerful. They began to look at dynamics associated with a growth law of spin glasses[55].

It was roughly at that time that I sent an email, or somehow contacted, Victor Martin-Mayor[56], in Madrid, about their simulations, asking if they could do some simulations for some of the things we were measuring. He was very kind. It turned out that he was working with a colleague at the University of Southern California[57], so he came by on his way back to Madrid. (This is when I was at Austin, in the mid-teens.) We just hit it off, I think, wonderfully, and began our collaboration at that point. We began then to do experiments which would overlap with what they could simulate. In our first couple of papers, we looked again at the correlation length[58]. We began to work back and forth. We had Skype for visual

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[53] G. Parisi, "Nobel Prize lecture," *NobelPrize.org* (2021). https://www.nobelprize.org/prizes/physics/2021/parisi/lecture/ (Accessed July 17, 2022.)

[54] See, *e.g.*, M. Baity-Jesi, *et al*. "Critical parameters of the three-dimensional Ising spin glass." *Phys. Rev. B* **88**, 224416 (2013). https://doi.org/10.1103/PhysRevB.88.224416

[55] See, e.g., F. Belletti *et al.* "Nonequilibrium spin-glass dynamics from picoseconds to a tenth of a second," *Phys. Rev. Lett.* **101**, 157201 (2008). https://doi.org/10.1103/PhysRevLett.101.157201

[56] "Victor Martin-Mayor," Universidad Complutense de Madrid (undated). http://teorica.fis.ucm.es/victor/ (Accessed July 17, 2022.)

[57] See, *e.g.*, V. Martin-Mayor, Victor and I. Hen, "Unraveling quantum annealers using classical hardness," *Sci. Rep.* **5**, 1-9 (2015). https://doi.org/10.1038/srep15324

[58] Q. Zhai, V. Martin-Mayor, D. L. Schlagel, G. G. Kenning, and R. L. Orbach, "Slowing down of spin glass correlation length growth: Simulations meet experiments," *Phys. Rev. B* 100, 094202 (1999). https://doi.org/10.1103/PhysRevB.100.094202; Q. Zhai, *et al.* "Scaling law describes the spin-glass response in theory, experiments, and simulations," *Phys. Rev. Lett.* **125**, 237202 (2020). https://doi.org/10.1103/PhysRevLett.125.237202

communication, which was pretty primitive, but there it was, and emails and visits and so on. Just to give you an example, one of the things that they found when they actually looked at the correlation length was that as the correlation length increased the growth slowed down. That was a bizarre result. So the question was: Is that true? You do one thing in simulations, but what happens in the experiments? We had a very patient graduate student[59] (these experiments take weeks). When you talk about long times, spin glasses are very slow. We were able to get the longest correlation length ever seen in a spin glass. The reason was that we were able to collaborate with a wonderful crystal grower. Her name is Deborah Schlagel at Ames laboratory at Iowa State University. She was able to grow single crystals of spin glasses. Here, you see how the simulations drove the experiment, literally. Namely, if you use a polycrystalline, which everybody—literally everybody—had done up to that time, you had crystallite boundaries. So what happened to the correlation length as it grew? It stopped. It was interfered with by the crystallite boundaries. It's a subtle point, because copper is very good at transmitting electricity, but not very good at spin glass correlations, because the Ruderman-Kittel interaction[60] transitions from oscillatory to exponential when you have scattering. Of course, crystallite boundaries scatter. So even though the connectivity is high it doesn't mean that the correlation length can grow very large. Again, a very subtle but important point. We worked with Schlagel to see if she could grow single crystals, where we wouldn't have that problem, so we could test Victor's—or I should say the Janus collaboration's—results. She was able to do that. We now have a collection of single crystals, which is just a gold mine. We can do things that nobody else has ever been able to do. We were able to get correlation lengths as large as a tenth of a micron. Normally, people in glasses think of length scales of 20 angstroms-30 angstroms as big. Here, we were talking about a thousand angstroms. Sure enough, we found that the growth of the correlation length slowed down as the size of the correlated regions increased. That was an extraordinary example—to me anyway—of how the simulations drove experiments and led to an understanding of what was going on. Since then, we've done a number of experiments that have really used the correlation length as our microscopic probe. Victor and I are now in very close contact, along with the rest of the Janus Collaboration, to really probe, using the correlation length, the dynamics of spin glasses.

[59] Qiang Zhai, *Glassy behavior : chaos and other problems in spin glasses*, PhD Thesis, University of Texas at Austin (2020). http://dx.doi.org/10.26153/tsw/13643

[60] RKKY: https://en.wikipedia.org/wiki/RKKY_interaction

PC: In your interview at the AIP, you mentioned your vision for the importance of computational sciences, already in the early 2000s, but I don't think you've ever led computer simulations yourself. Where does your appreciation for the approach come from?

RO: [0:54:53] It all started actually in 2002. I had left the chancellorship at Riverside, and went to the Department of Energy (DOE) as the Director of the Office of Science[61]. The Office of Science supports more physical science research in the United States than any other agency, including the NSF. And really nearly the entirety of the physical sciences. I have to say this: It was the DOE that found that you could use computers to determine the [sequence] of DNA, not the NIH[62]. It's just a wonderful organization that uses the power of computation for many purposes. Obviously, [it uses it] for the development of nuclear weapons, which is the prime driver, but the Office of Science supports research that is not classified across the country. At that time, the Earth simulator[63], in Japan, had just released its results, which showed that, first of all, they could run circles around anything that we had. And they did calculations of climate change that were the most advanced in the world at that time. So I asked people at DOE: "Why aren't we doing that?" They said: "Well, we have faster computers." It turned out we did have faster computers, but only at LINPACK speed[64]**.** In particular, they were very inefficient at actually doing real calculations. The efficiency of the Earth Simulator was of the order of 25 to 40 [tflops]. Ours was less than 8 [tflops]. That was because we used commodity chips. Our whole approach was to cheapen the cost of the computers. The Japanese used vector chips; we used scalar chips. It was clear there was something wrong in the United States[65]. So I started at the Office of Science a major initiative in high-end computing. Out of that, it became clear that high-end computing had a huge role to play in basic science and also in industry. Because one can simulate, for example, the airfoils associated with airplanes, instead of building little models, which are very expensive, in wind tunnels. You can actually simulate them on a computer if you have enough speed. It's just very nice. Yesterday, Oak Ridge formally announced the exaflop scale of their computer, which I have to tell you in those day—I didn't even know what the word meant—[was] so far beyond anything we were… We were talking about teraflops as our goal. But that started high-end computing and simulations in my

[61] Office of Science: https://en.wikipedia.org/wiki/Office_of_Science

[62] Joint Genome Institute: https://en.wikipedia.org/wiki/Joint_Genome_Institute

[63] Earth Simulator: https://en.wikipedia.org/wiki/Earth_Simulator

[64] LINPACK: https://en.wikipedia.org/wiki/LINPACK

[65] "Frontier supercomputer debuts as world's fastest, breaking exascale barrier," *Oak Ridge National Laboratory* (May 30, 2022). https://www.ornl.gov/news/frontier-supercomputer-debuts-worlds-fastest-breaking-exascale-barrier (Accessed July 17, 2022.)

mind. The following year, we issued the 20-year facilities outlook for the Office of Science[66], and number two was high-end computing. Nobody had thought about computing as a facility. Okay, it was expensive and you did it, but a facility? The reason that's important is that it gives you a planning track. It gives you the ability over time to start investing in faster and faster machines. Towards the end of my stay at the Office of Science in 2008, we realized that an exaflop was feasible. We started—people thought we must have been out of our minds—workshops to figure out what could you do at an exaflop. Yesterday, it became a reality. They are testing it now, but it'll be on track for users in September of this year.

There's just such an exciting role for simulations that can now be undertaken both in industry and in research in the public sector. That was another thing that was very important when you talk about simulations. The computers at DOE when I got there were only available to those who had DOE grants. My first question was: “Why? Everybody pays taxes in the United States that funds these computers. Why are we limiting access?” “It's a tradition.” Well, I didn't think that tradition made any sense. So one of the first things we did was to open up the computers to the private sector. We said you will get free time, no charge, but you will have to compete for time on the basis of the quality of your research with those who are involved in universities and institutes and so on. One of the first successes was Procter & Gamble[67]. I use this [example] because most people have forgotten what their babies were like, but the first use was diapers. Now, you’re going to say: “Why are diapers in need of simulations?” Diapers are very sophisticated. They can't leak, but they also have to wick away the moisture from the baby. That's a non-trivial diffusion problem. It turned out that the people who were working on the subject understood that and had been working on laptops… Anyway, they succeeded. And I like to think that the quality of diapers today is due to high-end computing simulations. I’m not sure everybody recognizes that. And on and on. Simulations were the meat that you can get from these high-end computations.

Now, obviously the Janus collaboration is a special-built computer just for Ising spin glasses. But there it is. So even during the time I was at DOE, we were exploring the possibility of simulations, but it wasn't until I was actively involved with Victor, in the mid-teens that it really became serious.

[66] “Facilities for the Future of Science: A Twenty-Year Outlook,” *Office of Science, US Department of Energy*, **DOE/SC-0078** (December 2003). https://fribusers.org/frib/docs/2003-DOE20YearScienceFacilities.pdf (Accessed July 17, 2022.)

[67] See, e.g., M. Feldman, “Procter & Gamble’s Adventures in High-End Computing,” *HPCwire* (March 21, 2008). https://www.hpcwire.com/2008/03/21/procter_gambles_adventures_in_high-end_computing/ (Accessed July 17, 2022.)

Now, we have an interaction which is literally daily between ourselves in the laboratory and our theory and the simulators on spin glasses in Madrid and Rome.

PC: In that same oral history, you also expressed a certain interest for the physics of structural glasses[68]. Have you followed developments in that field over the years or was it just a comment in passing?

RO: [1:03:25] We've followed it very closely, and so by the way has Giorgio. He's had a number of papers about structural glasses and their dynamics[69]. The real breakthrough, as far as I'm concerned, was from a group in Orsay led by Ladieu[70], who looked at the higher-order dielectric constants—this is about four or five years ago and continues today—of structural glasses, in particular, of glass-forming liquids[71]. You have to think of a glass former as you lower the temperature. The dynamics slows down very rapidly, and you get what's called the glass transition, which simply means that the dynamics is longer than any laboratory time scale. What you want to do is to look at glass forming liquids as you lower their temperature. What he and his colleagues did in a series of beautiful experiments was to look at the third- and the fifth-order dielectric constants. The reason for that is that if you think of a correlation length in glasses, it may be there in the third-order dielectric constant which diverges, but the correlation length plays a different role—a different power—in the fifth order than it does iin the third-order. So by dividing the fifth-order by the third-order, you actually project out the correlation length in a glass forming liquid. Lo and behold, they found that a correlation length exists. This was a huge observation, because people had argued for years that it's nothing but a random first order phase transition—RFOT it's called. It wasn't clear whether there was a correlation length or not, whether it made any sense. Now, through Ladieu's analysis and experiments, we know that there is a connection. Beyond that, I don't know where the field stands right now, but to me it means there is a link between spin glasses and structural glasses, in the sense that they're both random, they're both slow, and they both exhibit a correlation length that diverges at some point. We know

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[68] Ref. 1: "What is the physics of glasses? You're expecting this thing to last for tens and hundreds of thousands of years. Is glass a liquid or a solid? Is there any chance that that would happen? These are fundamental issues where I thought the Office of Science could play a major role. So that's just an example of where I played a role."

[69] See, *e.g.*, R. Di Leonardo, L. Angelani, G. Parisi and G. Ruocco, "Off-equilibrium effective temperature in monatomic Lennard-Jones glass," *Phys. Rev. Lett.* **84**, 6054 (2000). https://doi.org/10.1103/PhysRevLett.84.6054

[70] "François Ladieu," *Centre pour l'énergie atomique* (undated). https://iramis.cea.fr/Pisp/francois.ladieu/ (Accessed July 17, 2022.)

[71] S. Albert *et al.* "Fifth-order susceptibility unveils growth of thermodynamic amorphous order in glass-formers." *Science* **352**, 1308-1311 (2016). https://doi.org/10.1126/science.aaf3182

ours diverges at the critical temperature. Where does glass transition correlation length diverge? Is it at the lower [Kauzmann] temperature[72]? Where is it? Is it at zero? Nobody knows, but they're probing it, and who knows? We'll see.

PC: Were you in contact with or aware of the work of Daniel Kivelson[73] at UCLA in the '80s, when they were working on structural glasses? Or was this too far afield?

RO: [1:07:19] I really wasn't aware. We had worked on a glassy-like materials at UCLA in those days. I had a student who was looking at superionic conductors[74], and we were doing work on diffusion in one dimension[75]. But we had not really… Remember, those were the '80s. We didn't know about correlation lengths. To me, it's very nice to do critical exponents and static numbers, but to me dynamics is everything. That's what I was interested in. It wasn't until we were really able to get the correlation length out, in the late '90s, that suddenly everything opened up for both systems.

PC: From your position of having bridged across the Atlantic, and of having held fairly high-level positions in administration of science in the US, what do you think are the differences between the US and European engagement with spin glass and replica symmetry breaking ideas?

RO: [1:08:41] First of all, RSB is not limited to spin glasses. There's a lot of work on optimization problems, even on quantum optimization. This is all based one way or another on spin glass dynamics. As the Nobel citation indicated, it's everywhere, all over physics. So there's a huge amount of interest. In terms of spin glasses themselves, there are some laboratories in the States working on this. I'm working, for example, with colleagues in Minnesota and in Pennsylvania—two different universities—on spin glass dynamics[76]. There are other programs, as I said, looking at the equivalent of spin glass dynamics in these other fields. There's even a sociological paper that uses

[72] Kauzmann's paradox: https://en.wikipedia.org/wiki/Glass_transition#Kauzmann's_paradox

[73] C. M. Knobler, A. J. Liu, and R. L. Scott, "Daniel Kivelson," *Physics Today* **56**(12), 83 (2003). https://doi.org/10.1063/1.1650247

[74] Marian Underweiser, *The electrical conductivity of mixed (Na, Ba²⁺)-[beta]"-alumina*, PhD Thesis, University of California, Los Angeles (1991). https://search.library.ucla.edu/permalink/01UCS_LAL/trta7g/alma9933297543606533 (Consulted August 11, 2022.)

[75] See, *e.g.*, S. Alexander, J. Bernasconi, W. R. Schneider and R. Orbach, "Excitation dynamics in random one-dimensional systems," *Rev. Mod. Phys.* 53, 175 (1981). https://doi.org/10.1103/RevModPhys.53.175

[76] See, *e.g.*, Q. Zhai, D. C. Harrison, D. Tennant, E. D. Dahlberg, G. G. ., Kenning, and R. L. Orbach, "Glassy dynamics in CuMn thin-film multilayers," *Phys. Rev. B* **95**, 054304 (2017). https://doi.org/10.1103/PhysRevB.95.054304

RSB. The beautiful thing Parisi has accomplished is its incredible importance across not only science, but even social science. Now, in Europe, the Saclay group has more or less moved on, and so has the Swedish group. I know that Vincent and Hammann are both retired. Europe has this unfortunate requirement that when one reaches 65 one has to retire. When I was a young kid at the university and they lifted the requirement age in the United States, I was furious. [I thought:] “Oh goodness! People can stay on forever.” Now that I'm well beyond 65, I think that’s the best thing that has ever happened. We get a number of our best faculty here, at the University of Texas, from Europe because they don't want to retire at 65. John Goodenough[77], who just got the Nobel Prize, is from Oxford and he was told he would have to retire in two years, and so came to the University of Texas. We profit greatly from the forced retirement in Europe. What that means is that the group in Uppsala, which had been so productive, and also the Saclay group have more or less finished. There isn't, to my knowledge, an active European experimental program currently in existence. That may be unfair. I've already given you an example of an experimental group on structural glasses. But in the literature anyway, in terms of our work with the simulators, there aren’t many experimental groups that are active in the field that I know of. There are spin glass papers that appear from time to time, from India, from Europe and from other sources. They're interesting and important, but they don't focus where we’re looking, mainly at this interaction between simulations and experiments that has proven so vital.

PC: At you UCLA, at Riverside or elsewhere, did you ever teach about spin glasses or replica symmetry breaking? If yes, can you detail?

RO: [1:13:17] I actually did give a series of lectures on spin glasses at various universities, but never a course or a book. I have in the back of my mind of writing a book on spin glasses, because I think frankly our laboratory has a unique perspective on [them]. The difficulty is that I don't understand [some things]. It's hard to write a book about something you don't understand. In particular, what has eluded almost half a century of research is the origin of rejuvenation and memory. These are not unknown in other systems, in polymers for example, but in spin glasses you’re able to actually get down to the microscopics. So what are the microscopics of rejuvenation? Just for the record, let me describe what they are. If you cool a spin glass to a temperature below the freezing temperature and you wait, the system works toward the equilibrium and the dynamic susceptibility diminishes. If you continue to lower the temperature, of all things, it goes back to what it would have been had you not waited. That's

[77] John Goodenough: https://en.wikipedia.org/wiki/John_B._Goodenough

called rejuvenation. We now believe it's due to so-called temperature chaos. Literally this year we published our first experimental paper on temperature chaos[78]. There are only two papers in the literature: one in [2002][79] and one in 2022. Ours is the latter. What happens is that when you then warm up to where you started, the system remembers the equilibrium state in which you left it. How can that be? You've gone chaotic, and yet you come back and it remembers its own memory. That has bedeviled scientists for at least 20 years, if not longer. We now think we understand it. Of course, it's based on the correlation length. If that understanding holds, then I'm finally able to write my book, because I think I will then finally understand the dynamics. It's a little bit like the Gilbert and Sullivan song: "When I know more of tactics than a novice in a nunnery"[80]. It's when I know enough about spin glasses that I can write a book about [them], and then I'll start. That's where I stand right now. Whether I'll live long enough to do that is another matter, but it looks promising.

PC: Is there anything else you would like to share with us about this era that we may have missed or overlooked?

RO: [1:16:37] I don't think you've overlooked anything. I would just like to reiterate the remarkable scientific importance of the interaction between experiments, theory and simulations. I've never seen anything like it. One will guide the other and vice versa and surprises pop up. We did some experiments on memory for example. They hadn't seen it in the simulations. They looked more closely and they saw it. It's driving both of us to a fundamental understanding. The importance of theory is [great], because this all takes place in a theoretical framework. Nevertheless, to be able to understand the physics behind the dynamics is what it's all about. To me, that's what physics is about. This wonderful interaction, I think, is an example beyond comparison of the fruits of such an interaction.

PC: Do you still have notes, papers or correspondence from that epoch? If yes, do you have a plan or an intention to deposit them in an academic archive at some point?

RO: [1:18:10]. I really don't. My archives are the published papers. I'm just leaving it at that. I never wanted to spend time putting together notes and

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[78] Q. Zhai, R. L. Orbach, and D. L. Schlagel, "Evidence for temperature chaos in spin glasses," *Phys. Rev. B* **105**, 014434 (2022). https://doi.org/10.1103/PhysRevB.105.014434

[79] P. E. Jonsson, H. Yoshino and P. Nordblad, "Symmetrical Temperature-Chaos Effect with Positive and Negative Temperature Shifts in a Spin Glass," *Phys. Rev. Lett.* **89**, 097201 (2002). https://doi.org/10.1103/PhysRevLett.89.097201

[80] Major-General's song : https://en.wikipedia.org/wiki/Major-General%27s_Song

so on, because the theory and experiment was so exciting I wanted to do that first. So I never had the patience to do that. I have a set of lecture notes on fractal dynamics that I gave when I was a Lorentz professor at the University of Leiden[81], which just begs to be a book. I'd be delighted to deposit it, but that was in the days when everything was hand written [with] viewgraphs, if you can remember those days. I wouldn't know where to deposit it either. The papers are what speak for themselves.

PC: No email or paper correspondence that you would've kept over the years?

RO: [1:19:15] No.

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

RO: Patrick, thank you for your patience. I hope this has been useful.

[81] In 1987. See, *e.g.*, "Lorentz Chair," *Instituut Lortenz, University of Leiden* ([2020]). https://www.lorentz.leidenuniv.nl/lorentzchair/ (Accessed July 17, 2022.)