

History of RSB Interview:

Joel L. Lebowitz

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Interviewers:

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Location:

Over Zoom, from Prof. Lebowitz's home in Princeton, New Jersey, USA.

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PC: Good morning, Professor Lebowitz. Thank you very much for joining us. As we discussed ahead of this interview, the theme of our discussion is the formulation and development of symmetry breaking ideas from roughly 1975 to 1995. We've already obtained some context for your early work in oral interviews you've done in the past¹, so I thought we would jump right into the meat of the matter. How did you first hear about spin glasses and replica symmetry breaking? In particular, did you follow the early developments from the Edwards-Anderson and Sherrington-Kirkpatrick papers to the Parisi scheme in the late '70s?

JL: [0:00:47] I guess the answer is no. I did not follow in general those things at all. Where did I first hear about it? I believe I heard Parisi give a talk in Paris, or rather in Bures-sur-Yvette. I was visiting the IHES². I was there from '76 to '77³. I spent a sabbatical at the IHES that year. I guess it must have been during that time that Parisi talked. The only thing I remember, vaguely, is about taking the limit of dimension going to zero and finding that rather strange. But I did not really follow up at that time. I was doing different things—I forget what—but definitely not spin glasses. I heard of

¹ J. L. Lebowitz and L. Bonolis, "A life in statistical mechanics—Part 1: From Chedar in Taceva to Yeshiva University in New York," *Eur. Phys. J. H* **42**, 1–21 (2017). <https://doi.org/10.1140/epjh/e2017-80006-9>; Marie-Laure Moinet, "Interview de Joël Lebowitz, lauréat 2014 de la Grande Médaille de l'Académie des sciences," *Institut de France—Académie des Sciences* (2014). https://www.academie-sciences.fr/archivage_site/activite/prix.htm (Consulted July 19, 2021)

² Institut des Hautes Études Scientifiques: https://en.wikipedia.org/wiki/Institut_des_Hautes_Études_Scientifiques

³ He visited IHES in a number of other occasions, including Fall 1980 and Fall 1984.

Edwards and Sherrington's thing, but just took it as general information, rather than something very specific at that time.

I think that the time, I took it more seriously was when—I can't remember the year—David Ruelle was visiting Rutgers and gave a course on his ideas of cascades. At some point, we ended up—Michael Aizenman⁴, David Ruelle and myself—writing a joint paper⁵, proving some things about the [Sherrington-Kirkpatrick] model at high temperature. I don't remember what year that was, but of course you can look up the date of the paper.

PC: 1987 is the date of the paper.

JL: [0:03:12] Oh. Was it? That's what I did. What's the period you said you were particularly concentrating on?

PC: That falls within it, yes. We've heard about the series of lectures that David Ruelle was giving. Was this at your invitation? Can you tell us a bit more about the context?

JL: [0:03:31] I had a general invitation out to David Ruelle to visit Rutgers. For some time he was coming every year, or every second year. He would come and spend several months at my invitation. He would generally give some course. This was one of the courses he gave.

PC: So you did not request a course on this topic. Did he volunteer this idea?

JL: [0:04:11] Yeah. I think it was really up to him what he wanted to talk about. I guess this was the topic he was interested in at that time. I remember that it was very complicated. His construction—and his talk—was a very complicated thing, and I'm not sure I understood—well, certainly not all of—it. I don't remember what fraction of it I did understand. Now that you mention it, I don't remember exactly what he proved, or what was the technique he used for proving it. I'd have to look back.

Just before talking to you, I was looking at my papers about randomness, but I was still in the 1960s. I had a paper with Bob Griffiths about the kind of things that might happen when not all lattice sites are occupied by spins, [but all] with a ferromagnetic interaction⁶. What we proved there is that if the concentration of spins is sufficiently high you'll get the kind of phase

⁴ Michael Aizenman: https://en.wikipedia.org/wiki/Michael_Aizenman

⁵ M. Aizenman, J. L. Lebowitz and D. Ruelle, "Some rigorous results on the Sherrington-Kirkpatrick spin glass model," *Commun. Math. Phys.* **112**, 3–20 (1987). <https://doi.org/10.1007/BF01217677>

⁶ R. B. Griffiths and J. L. Lebowitz, "Random spin systems: some rigorous results," *J. Math. Phys.* **9**, 1284–1292 (1968). <https://doi.org/10.1063/1.1664710>

transition that you get with the regular Ising model, where all sites are occupied, and then maybe also that some additional properties of the free energy depend on the concentration. Things like that.

I didn't look back at the paper with Aizenman and Ruelle, so I don't remember. You may have looked at it more recently.

PC: Do you recall why you didn't pursue these ideas further after this one paper?

JL: [0:06:19] Presumably, mostly because I did not have a good idea about it and HAD other things to do. There was one paper, I remember, that was aborted in some sense. It was a joint paper with a French probabilist, Francis Comets. We had some interesting results about spin glasses, but the referee caught a mistake. We made some assumptions about certain inequalities being true. We just somehow assumed that they were, and in the end we could not actually prove that they were true. We finally wrote a short paper with some results⁷, but that was a much later period. (I would have to look at my publication to remember what this was about.)

PC: You can do that at a later stage.

JL: [0:08:01] Yeah. Sorry. I was just busy these last few days and I did not get the chance to go back and look through what I did.

PC: Did you nevertheless keep abreast of the later developments in spin glass models and their many applications, especially neural networks?

JL: [0:08:30] Only peripherally. During that time I was in touch with Derrida⁸ and heard about his ideas, but just heard about them. I somehow didn't absorb them. There were many different things to work on, and this did not catch my attention directly.

Getting back again to some properties or inequalities for such systems. I did some work, maybe 10 years ago, with Pierluigi Contucci⁹. That was

⁷ F. Comets, G. Giacomin and J. L. Lebowitz, "The Sherrington-Kirkpatrick model with short range ferromagnetic interactions," *C. R. Acad. Sci. I* **328**, 57-62 (1999). [https://doi.org/10.1016/S0764-4442\(99\)80012-3](https://doi.org/10.1016/S0764-4442(99)80012-3)

⁸ Bernard Derrida: https://en.wikipedia.org/wiki/Bernard_Derrida

⁹ P. Contucci and J. L. Lebowitz, "Correlation Inequalities for Spin Glasses," *Ann. Henri Poincaré* **8**, 1461–1467 (2007). <https://doi.org/10.1007/s00023-007-0342-8>; "Correlation inequalities for quantum spin systems with quenched centered disorder," *J. Math. Phys.* **51**, 023302 (2010). <https://doi.org/10.1063/1.3293753>

much more recent. I never really got into the idea of letting the dimension go to zero or negative.

I remember Oliver Penrose¹⁰ was visiting, I think, Stanford, and he was giving the course on statistical mechanics. He was saying that the students thought that going to negative dimensions was really, really far out.

PC: As part of running your famous conference series at Rutgers, you've had the chance to enhance the visibility of various subfields of statistical mechanics, and to acquire a very deep knowledge of the community¹¹. In recent years, you've highlighted the work of scientists who've worked pretty extensively on spin glasses, such as Leonid Pastur¹², John Hopfield¹³, Giorgio Parisi, Michael Aizenman, Daniel Fisher, and David Huse. What was your impression of these conferences, and how do you think that the spin glass problem fit within the broad statistical mechanics field?

JL: [0:11:21] Very good question. I don't think I have a ready answer. I am still mostly an outsider to that field. Of course, with the conferences, the literature and other things, I'm aware [of it] in a general way, and of course I was aware of the breakthroughs with the work of Toninelli and Francesco Guerra proving some of the things rigorously¹⁴. And Talagrand¹⁵. Somehow, I never got into the nitty gritty of it. In fact, I do not know what the current state is. I'm sure that you know much better. I guess Francesco [Zamponi] is much more into that. Actually, Francesco, how did you get involved in this project?

FZ: In this project about the history of science? I have been collaborating with Patrick for quite a long time on problems related to glass physics. So when he proposed that I help with this project, I thought it was a good idea. We would like to understand better how the ideas behind replica symmetry breaking in glasses originated in the first place. That is my motivation.

JL: [0:13:25] As I said, I was not really ever part of that community in any direct way.

¹⁰ Oliver Penrose: https://en.wikipedia.org/wiki/Oliver_Penrose

¹¹ As part of the activities of the Center for Mathematical Sciences Research at Rutgers University, semiannual conferences in statistical mechanics are held. Recent Statistical Mechanics Conferences: <https://cmsr.rutgers.edu/news-events-cmsr/statistical-mechanics-conference/> (Consulted July 10, 2021)

¹² Leonid Pastur: https://en.wikipedia.org/wiki/Leonid_Pastur

¹³ John Hopfield: https://en.wikipedia.org/wiki/John_Hopfield

¹⁴ F. Guerra and F. L. Toninelli, "The Thermodynamic Limit in Mean Field Spin Glass Models," *Comm. Math. Phys.* **230**, 71–79 (2002). <https://doi.org/10.1007/s00220-002-0699-y>

¹⁵ M. Talagrand, *Spin Glasses: A Challenge for Mathematicians* (Berlin: Springer-Verlag, 2003).

- FZ:** I think it's useful for us to have also the view of an outsider. You have a very complete view of the statistical mechanics community, so it's very helpful.
- JL:** [0:14:04] I'm very happy to let you know of my ignorance. That's mostly what I can say. I think that field was pretty much a specialty. It didn't have so much overlap with some of the other parts going on in statistical mechanics. The connection with real glasses came a bit later, if I remember correctly. At that time, I just was not involved. I remember listening to talks, but that's much later. People peripherally maybe related... Random matrices came in there. My main interest were and are in other areas of statistical mechanics, not in this one.
- PC:** Do you nevertheless have any insight to offer about the difference between the US and the European statistical mechanics communities, with respect to the replica symmetry breaking ideas and spin glasses?
- JL:** [0:16:16] I guess I mostly thought of it as a European thing, with Giorgio Parisi, Marc Mézard¹⁶, Bernard Derrida and people like that. Obviously, there were people working in the United States, like Michael Aizenman as you mentioned. Who were the other names which you mentioned from the United States besides Aizenman?
- PC:** Daniel Fisher and David Huse.
- JL:** [0:16:58] I do remember the controversy, particularly, between Fisher and Huse¹⁷, on one side, and Parisi, on the other side. Chuck Newman¹⁸ and Daniel Stein¹⁹ were involved a little bit in that.
- FZ:** As an outsider, what was your impression of that controversy? We are trying to reconstruct it, because we were not there at the time. For people outside of the community, was it clear what was the object of the controversy and what was the interest of the discussion? Or did it look like a personal discussion between a few specialists?
- JL:** [0:17:54] I think that was more like [the latter]. If I remember correctly, the question was whether the [Edward-Anderson] system with a short-range interaction will have the same type of behavior as the Sherrington-[Kirkpatrick] model. Has this controversy ever been resolved?

¹⁶ Marc Mézard: https://en.wikipedia.org/wiki/Marc_M%C3%A9zard

¹⁷ D. S. Fisher and D. A. Huse, "Ordered Phase of Short-Range Ising Spin-Glasses," *Phys. Rev. Lett.* **56**, 1601 (1986). <https://doi.org/10.1103/PhysRevLett.56.1601>

¹⁸ Charles M. Newman: https://en.wikipedia.org/wiki/Charles_M._Newman

¹⁹ Daniel L. Stein: https://en.wikipedia.org/wiki/Daniel_L._Stein

FZ: Not really. No.

JL: [0:18:35] I see David Huse very frequently. I just had lunch with him yesterday. But I don't think we have talked about that for... I don't remember talking about it. But I remember in the old time about questions of personalities. When Giorgio became a foreign member of the National Academy of Sciences²⁰, I remember worrying about him having some encounters with Michael Fisher²¹, Daniel's father, worrying whether they might have some personal animosities. I don't think there was any real problem, but I can't remember. Usually, at these National Academy meetings, Michael Fisher would host some party—he lived in Maryland close to Washington, DC—but I can't remember whether Giorgio was invited or not. I remember worrying about it at some point.

As a general outsider understanding, the question was mainly how many minima were there in the landscape of the energy. This was what the argument was about. I remember, in particular, Chuck Newman and Daniel Stein writing some papers²² trying to prove that Fisher and Huse were right. I don't remember anymore and I certainly was not involved in any personal way about it.

PC: As a final question, is there anything that we may have missed that you'd like to tell us about this era?

JL: [0:21:05] Not that I know at this time, but if I think of something I'll write to you.

PC: Thank you.

²⁰ Giorgio Parisi was elected to the National Academy of Sciences of the U. S. A. in 2003. <http://www.nasonline.org/member-directory/members/20004881.html> (Consulted July 10, 2021)

²¹ Michael Fisher: https://en.wikipedia.org/wiki/Michael_Fisher

²² See, e.g., C. M. Newman and D. L. Stein, "Multiple states and thermodynamic limits in short-ranged Ising spin-glass models," *Phys. Rev B*, **46**, 973 (1992). <https://doi.org/10.1103/PhysRevB.46.973>; "Spin-glass model with dimension-dependent ground state multiplicity," *Phys. Rev. Lett.* **72**, 2286 (1994). <https://doi.org/10.1103/PhysRevLett.72.2286>; "Non-mean-field behavior of realistic spin glasses," *Phys. Rev. Lett.* **76**, 515 (1996). <https://doi.org/10.1103/PhysRevLett.76.515>