History of RSB Interview: Jennifer Chayes

April 19, 2023, 12:00 to 1:00pm (ET). Final revision: July 16, 2023

Interviewer:

Patrick Charbonneau, Duke University, patrick.charbonneau@duke.edu

Location:

Over Zoom, from Dean and Associate Provost Chayes's office in Berkeley, California, United States.

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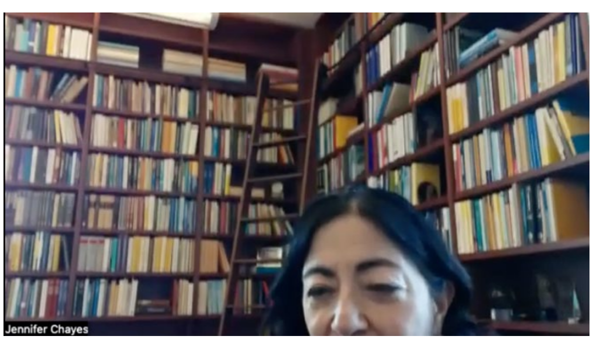
PC: Good morning, Professor Chayes. Thank you very much for joining us. As we've discussed ahead of this interview, our discussion will be centered on the history of Rebecca symmetry breaking in physics, which we roughly bound from 1975 to 1995. But before we dig into that topic, I'd like to ask you a few questions on background about your own career and life. In prior interviews[1], you've mentioned how you became interested in science and math, which led you to pursue a pretty STEM-heavy load in college. Then, what drew you to pursue a PhD in mathematical physics of all topics?

JC: [0:00:40] It's funny. I had always loved math, but I thought it was a hobby and not a profession. I was a pre-med when I started out [in college], so I had to take some physics to satisfy the pre-med requirement, which I did the summer after my freshman year. I went to Harvard and took a physics class, and absolutely fell in love with physics, and decided to do a physics major. Then, I was thinking I wanted to go to physics graduate school instead of going to med school, [because] being in physics as opposed to being a doctor you really could use math.



You can see behind me [left] that I have many, many books. I started to buy a lot of books as an undergraduate. So, even

[1] See, *e.g.*, Elizabeth Ghaffari, "Jennifer Tour Chayes," In: *Women Leaders at Work: Untold Tales of Women Achieving Their Ambitions* (New York: Apress, 2012): 181-198; A. Bonato, "Interview with Jennifer Chayes," *Notices Am. Math Soc.* **69**, 403-407 (2002). https://www.ams.org/journals/notices/202203/rnoti-p403.pdf (Accessed July 13, 2023.); Yu Kiang Leong, "Jennifer Tour Chayes," In: *Creative Minds, Charmed Lives: Interviews at Institute for Mathematical Sciences, National University of Singapore* (Singapore: World Scientific, 2010): 41-49.

though I was not in a place that was known for mathematical physics, or even known that much for physics—there was some good experimental physics, a little bit of theory—I bought these books on mathematical physics, and I started to fall in love with them. Some of them were by faculty at Princeton, so that was my dream: to go to Princeton and do mathematics.

PC: What led you, once you got to Princeton, to work with Elliot Lieb[2] in statistical physics?

JC: [0:02:24] I also had fallen in love with statistical physics. As an undergraduate, I spent a summer after my sophomore year taking thermodynamics at MIT. I worked very closely with an experimental chemist, Clark Stephenson[3], who was very good at thermodynamics. Although I was only officially taking one class with him, we went through about six classes[-worth] of thermodynamics, because he was retired. He had just lost his lab, which is what they did in 1977. They had taken away his lab, so he spent four to five hours a day with me working on thermodynamics. I would then go home and work for 12 hours doing homework and learning about it. So, that was thermodynamics. There was no statistical physics there, but then I really went… Statistical physics is even more mathematical, so I fell in love with that. When I got to Princeton, I knew that Elliott Lieb had worked on statistical physics in the past. At that point, he was doing more quantum mechanics and there wasn't anyone really doing mathematical statistical physics, but I went to him and told him that I'd like to do a thesis with him but only in mathematical statistical physics[4]. He had not been doing [it] for a while, but he said: "Okay!"

PC: By lack of other choices, in a sense, he was a natural match?

JC: [0:04:27] Yes, he was a natural match. I also ended up working with Michael Aizenman[5], who's just fantastic. He was there at Princeton when I was there, and then went to Rutgers when he didn't get tenure. [This] was huge mistake on Princeton's part, so much so that they then hired him

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[2] Elliott H. Lieb: https://en.wikipedia.org/wiki/Elliott_H._Lieb

[3] See, *e.g.*, "Clark C. Stephenson," *Chemistry Tree* (n.d.). https://academictree.org/chemistry/peopleinfo.php?pid=100359 (Accessed July 13, 2023.)

[4] Jennifer Tour Chayes, *The inverse problem, plaquette percolation and a generalized Potts model*, PhD Thesis, Princeton University (1983). https://catalog.princeton.edu/catalog/991737763506421 (Accessed July 13, 2023.)

[5] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Michael Aizenman*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 16 p. https://doi.org/10.34847/nkl.dfd42521

back to Princeton. So, I ended up working with both Elliott Lieb and Michael Aizenman.

PC: After your graduate studies, you went for a postdoc at Harvard and Cornell, but you were rather free to choose the problems you were working on. In general, what made you choose particular problems to pursue?

JC: [0:05:10] I had in my thesis worked on percolation, which I know is not quite a standard statistical mechanics model, but it really is. It's kind of pure quenched disorder. So, I was very taken with disordered systems. I was attracted to anything that involved disordered systems in some way. I think that that was a lot of the lens through which I saw the world, and what I wanted to do. I wanted to work on phase transitions in disordered systems. Although the one I worked on a lot in the beginning was percolation, which sometimes a physicist wouldn't think of as a disordered system, it's basically pure quenched disorder.

PC: The first published work of yours about models with quenched disorder—excluding percolation—was done in collaboration with Jürg Fröhlich[6], on the low-temperature behavior of disordered magnets[7]. How did this collaboration come about? And what drew you to this model amongst all you could be looking at?

JC: [0:06:34] Jürg was a collaborator of mine on a major paper that I had done as a graduate student on percolation. Jürg was also looking at spin systems, at magnetic models. So, every year for a number of years, I would visit Jürg. I and my ex-husband, Lincoln Chayes[8], would visit Jürg for some period over the summer. We'd visit him in Hönggerberg at ETH. (The physics was it Hönggerberg and math was in Zentrum, so we'd spend time in both places[9].) Jürg suggested this problem because many of the techniques and much of the intuition that we had developed in our previous work he thought could apply to this and indeed it did it. This was still a ferromagnetic system, which made it simpler because you didn't have the frustration that you have in something like a spin glass, but it had quenched disorder in a ferromagnetic system.

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[6] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Jürg Fröhlich*, transcript of an oral history conducted 2023 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2023, 13 p. https://doi.org/10.34847/nkl.6b2d7aqr

[7] J. T. Chayes, L. Chayes and J. Fröhlich, "The low-temperature behavior of disordered magnets," Communications in mathematical physics, 100(3), 399-437.

[8] Jennifer Tour married Lincoln Chayes in 1976. "Lincoln Chayes" *Math Tree* (n.d.) https://academictree.org/math/peopleinfo.php?pid=177367 (Accessed July 13, 2023.)

[9] ETH Zürich Campus: https://en.wikipedia.org/wiki/ETH_Zurich#Campus

PC: Would you go for a few weeks or a few months at a time? What was the context of these visits?

JC: [0:08:04] I don't remember exactly. Maybe it was a month or so over the summers for a few years. That was a great atmosphere for us. Some of the great Swiss mathematical physicists were there. In fact, over the summers at ETH, many people passed through. I was actually really lucky because at Princeton and in places like Harvard and Cornell and ETH, great people would pass through. So, there were not only be the great people who were there, but I met a lot of, and got to talk with, and ultimately worked with some of the great mathematical physicists and statistical physicists.

PC: One of those summers, you also went to Les Houches, in the Alps, in France[10]. How important or not was this experience in your exposure to disordered systems?

JC: [0:09:11] It was transformative for me, that Les Houches experience. It was long. It was six weeks. It was an amazing group. Almost every grad student and postdoc who was there went on to be a tenured faculty member. It was phenomenal in that sense. And, of course, all the greats of disordered systems—not all but many—were there. It's where I met a lot of people. For example, Giorgio Parisi[11] was there for the whole time with his wife and kids. So, not only was I meeting the mathematical physicist, but I was meeting the great statistical physicists as well. My ex-husband and I gave a number of lectures on percolation, and then wrote a hundred-plus page review of those lectures on percolation[12]. It was the time when I met many people who would be my colleagues for decades to come. I learned a great deal about disordered systems at that time, which was 1984, which is right in the midst of this period of spin glasses that you're talking about, from ‘75 to ’95. It's really in the center of that.

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[10] *Phénomènes critiques, systèmes aléatoires, théories de jauge/Critical phenomena, random systems, gauge theories*, École de physique des Houches Session 43, Konrad Osterwalder and Raymond Stora, 1 August-7 September 1984, Les Houches, France.

[11] See, *e.g.*, P. Charbonneau and F. Zamponi, *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, 80 p. https://doi.org/10.34847/nkl.7fb7b5zw

[12] J. T. Chayes and L. Chayes, "Seminar 19. Percolation and random media," In: *Phénomènes critiques, systèmes aléatoires, théories de jauge/Critical phenomena, random systems, gauge theories*, K. Osterwalder and R. Stora, eds. (Amsterdam: North Holland, 1986): 1001-1140.

PC: At about the same time, you also worked with Daniel Fisher and Thomas Spencer[13], who was then at the institute for Advanced Studies, on the finite-size effects of disordered systems. How did this other collaboration come about?

JC: [0:10:59] I had known Daniel[14] since I was a grad student at Princeton. He was at Bell Labs at that time, and he taught a renormalization group special topics class. There were probably seven or eight of us in the class, so I had gotten to know Daniel quite well during that time. I also knew Tom Spencer[15] quite well, although I hadn't worked with him yet. He was Jürg Fröhlich's primary collaborator. I talked to Tom all the time. So, it was very natural to work with them. I think that some of it… Again, in a sense, this was percolation because the Harris criterion[16], which had to do with disordered systems… People had been trying to see whether it was true in this particular system, or that particular system, or that particular system. Really, it was true because of disorder. It was not the particulars of the system. To try to establish it in a particular system became quite difficult. Our proof was, in a sense, a percolation-type of proof. It allowed us to step back and say: "What is it about quenched disorder that would lead to this phenomenon?" It was based on that. I talked a great deal with Tom and Daniel during that time, so it was very natural for us to work with them on that. It was very exciting, because for years it was my most highly cited paper. It was just so simple, because we could step back and say: "Is this happening because of what happens in a magnet, or of electronic or this or that? No, it's just happening because of the nature of quenched disorder and on what scales you see the disorder manifest itself."

PC: In parallel to this work, you started collaborating with David Thouless[17] on a proper spin glass model, on a Bethe lattice, or Cayley tree[18]. How did that yet other collaboration come through?

JC: [0:13:51] I was a postdoc at Cornell with Lincoln Chayes, my ex-husband. David Thouless came to LASST, the Laboratory of Atomic Solid State Physics, where we were both postdocs. [He] gave a talk, and said he'd like to do a spin glass on a Bethe lattice, or Cayley tree, but he wasn't sure

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[13] J. T. Chayes, L. Chayes, D. S. Fisher and T. Spencer, "Finite-size scaling and correlation lengths for disordered systems," *Phys. Rev. Lett.* **57**, 2999 (1986). https://doi.org/10.1103/PhysRevLett.57.2999

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

[15] Thomas C. Spencer: https://en.wikipedia.org/wiki/Thomas_Spencer_(mathematical_physicist)

[16] See, *e.g.*, A. Brooks Harris: https://en.wikipedia.org/wiki/A._Brooks_Harris

[17] David J. Thouless: https://en.wikipedia.org/wiki/David_J._Thouless

[18] J. T. Chayes, L. Chayes, J. P. Sethna and D. J. Thouless, "A mean field spin glass with short-range interactions," *Comm. Math. Phys.* **106**, 41-89 (1986). https://doi.org/10.1007/BF01210926

exactly how to start it. There were many aspects of it. We got very excited. We walked out of there and said: "Wow! Maybe we know how to do this a little bit." So, we walked out of that seminar, and we stayed up all night. We worked really hard for a couple of days, and then we sent him all this stuff. Maybe, he was even there and the next day we told him some of it. It was just… It was based on all kinds of precise calculations that we could do. A lot of David's intuition was based on precise calculations—by hand not on computers. This was an iterative way of treating it. He saw it immediately when we showed him, and we started working together on it.

In his office, what really impressed me was that there were just rows of notebooks with all of his calculations and everything that he had done. I wasn't as good about it. I would do it on pads of paper. I still have some of those pads, but I didn't have them in such an organized way as David had. Actually, I used to do them on big sheets of computer paper, where the backs were empty and so I wasn't wasting paper. When I saw David, at least started to do them on pads. He had these notebooks and he really…

Sometimes, he would make these incredible leaps in understanding. I think what people who didn't work with him might not have realized was they were backed up with probably hundreds of pages of calculations in which he tried various things, which informed his intuition. It was just really fun working with him. I liked him a great deal. He had offered jobs in the physics department at the University of Washington to me and Lincoln as we left our postdocs. We decided to go to UCLA. We wanted more of a connection with mathematics, but I loved working with David, and I learned so much from him. It's as if when something happens, you're ready to understand it because you prepared yourself so much and you've done all this work to ground your intuition in the background.

PC: You mentioned that as soon as you heard from him at that talk you jumped onto the problem. What particular preparation or insight you thought you had that was allowing you to jump right in?

JC: [0:17:27] Again, we had really been thinking about disordered systems. We loved the spin glass. Of course, we knew about it. We learned a lot about it from Giorgio Parisi that summer in Les Houches, but it just seemed intractable to us mathematically on the lattice on which it had been considered, whereas on a tree one could really start to take calculate things, and take expectations, and see how things were behaving.

PC: Given that Thouless was in Seattle, how did the collaboration function beyond that first initial visit?

JC: [0:18:19] It's too bad we didn't have Zoom, but we actually traveled. We would travel to UW and see David there. We would talk on the phone, but we would also travel to UW and see David. It was one of the things that was wonderful about having postdocs. A lot of theorists applied for jobs coming right out of grad school at that time. I intentionally did not apply. I did one postdoc, did not apply for a tenure track job, then did a second postdoc. I even was thinking of going beyond there and people said to me: "No! No! No! You really have to apply for a job at some point." But it was wonderful because we didn't have teaching obligations, we didn't have to apply for grants, we didn't have to serve on committees. We were free to travel, we traveled a great deal to meet with and work with collaborators wherever they were. So, it was an incredible period of my life.

The travels from Cornell were kind of interesting, because it was a small one-room airport with propeller planes at that time. I know I sound like I'm coming from the century before last—almost, not quite. They had these propeller planes, and so they were always getting delayed. The airport would just close all the time. They were always getting delayed, but it was wonderful because you would sit there and there would be these other wonderful people to interact with. I had all these conversations with Carl Sagan[19]. Then, of relevance to the spin glass, Hans Bethe[20] [and I], we were stuck together at the airport. So, I would tell Hans Bethe what we were doing with the spin glass on the Bethe lattice. It was really wonderful to be able to explain to him what these spin glasses were and how one might conceive of them on the Bethe lattice. Of course, he immediately understood how we were doing the calculations.

PC: Given that you've talked so much with David Thouless, do you know what interested him in the problem? Did you get any insight into his own motivations?

JC: [0:20:44] David is someone who was very interested in phase transitions, in disordered systems. He had done things on various kinds of disorder or different kinds of order even. The work he won the Nobel Prize for was not a disordered system, but it was a very different kind of order. So, he was interested in the states that were not conventionally ordered. Of course, he was interested in doing things on the Bethe lattice, because he was someone who tried to ground his intuition in whatever precise way he could.

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[19] Carl Sagan: https://en.wikipedia.org/wiki/Carl_Sagan

[20] Hans Bethe: https://en.wikipedia.org/wiki/Hans_Bethe

PC: What was the initial reception of that first collaborative work with David Thouless?

JC: [0:21:43] Well, some people said: "Oh! You're missing RSB and all of this." We had mapped out some phase diagrams. And other people just loved it because you could actually calculate something. You could say something, and you could prove things about certain of the phases. There were differing reactions on it. I think people were surprised that we could say so much rigorously about something like a spin glass, as a proof of concept.

PC: You had some contacts with the European research community through your travels. Did you exchange with them about this work? Did you have the opportunity to present or exchange?

JC: [0:22:32] Yeah. Absolutely! The European community was absolutely part of the community of the spin glass work. As we worked on it, absolutely. Conferences were taking place all the time, and they had Europeans. We would go to Europe fairly often, especially to ETH, but many other people would come to ETH.

PC: Do you remember talking about this work with Giorgio Parisi or Marc Mézard[21]?

JC: [0:23:09] Absolutely, yeah!

PC: Following that initial work, you pursued that direction for a few more papers with Jim Sethna and his first graduate student, Jean Carlson[22], but I think you had by then moved at UCLA or were in the process of moving. How closely were you involved in that project?

JC: [0:23:30] Very involved in that project. Again, I traveled a lot because I was just used to that. My first two years at UCLA, I managed to not have teaching. I had one year left of my NSF postdoc[23], and then I got a Sloan.

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[21] Marc Mézard: https://en.wikipedia.org/wiki/Marc_M%C3%A9zard

[22] Jean M. Carlson: https://en.wikipedia.org/wiki/Jean_M._Carlson; Jean Marie Carlson, *Critical Properties of the Bethe Lattice Spin Glass*, PhD Thesis, Cornell University (1988). https://newcatalog.library.cornell.edu/catalog/1592719 (Accessed October 19, 2022.); J. M. Carlson, J. T. Chayes, L. Chayes, J. P. Sethna and D. J. Thouless, "Critical behavior of the Bethe lattice spin glass," *Europhys. Lett.* **5**, 355 (1988). https://doi.org/10.1209/0295-5075/5/4/013; "Bethe lattice spin glass: the effects of a ferromagnetic bias and external fields. I. Bifurcation analysis," *J. Stat. Phys.* **61**, 987-1067 (1990). https://doi.org/10.1007/BF01014364

[23] J. Chayes, "Mathematical Sciences Postdoctoral Research Fellowship," National Science Foundation Award #PHY-8414087, 21 June 1984-31 December 1987. https://www.nsf.gov/awardsearch/showAward?AWD_ID=8414087 (Accessed July 13, 2023.)

So, basically my first two years were still like being a postdoc in a way. I was able to travel a lot and participate in these things. Jean had moved to UCSB, so she wasn't that far away anyway. She was much closer to me than to Jim.

PC: After that series of work, you largely left the field of spin glasses and systems with quenched disorder altogether.

JC: Not really!

PC: Okay, so what drew you away from spin glasses, or in what ways were your interests morphed?

JC: [0:24:39] I guess it was 1995 or so. A lot of people who had been doing spin glasses were beginning to think about disordered systems and computer science. I heard a talk by Scott Kirkpatrick[24]—of the Sherrington-Kirkpatrick model—talking about random 2-SAT. (2-SAT is a satisfiability [problem], and satisfiability is a fundamental problem in in computer science[25].) 3-SAT is one of the most famous NP-complete[26] problems. What some people started doing, was saying: "Let me randomly draw clauses and try to satisfy this system." It has things like frustration, [and] I was just following frustration wherever it went. I was at the Institute for Advanced Study in '94-'95 as a member[27]. Maybe [I went to] Rutgers, I'm not sure, [when I heard] a talk by Scott Kirkpatrick on random satisfiability[28]. He was looking at it because it reminded him of spin glasses. I got very excited about it, and I started talking a great deal with computer scientists about that, and about some other things having to do really with finite-size scaling at phase transitions, which they looked at in a totally different way.

I got so excited about this that I proposed to the Institute for Advanced Study that… Or they asked me because I was talking about it a lot. We had a little workshop and that caused a lot of excitement during that '94-'95 year. They proposed that I come back in '96-'97 and run a program on the

[24] 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

[25] Boolean satisfiability problem: https://en.wikipedia.org/wiki/Boolean_satisfiability_problem

[26] NP-completeness: https://en.wikipedia.org/wiki/NP-completeness

[27] Chayes was a member of the School of Mathematics at IAS for 9/1994—4/1995 as well as for 1/1997—4/1997. Kirkpatrick was not. See "Jennifer Chayes," *IAS* (n.d.). https://www.ias.edu/scholars/jennifer-chayes (Accessed July 13, 2023.)

[28] See, *e.g.*, S. Kirkpatrick and B. Selman, "Critical behavior in the satisfiability of random Boolean expressions," *Science* **264**, 1297-1301 (1994). https://doi.org/10.1126/science.264.5163.1297

intersection of computer science and mathematical physics. So, I did that that, that year '96-'97[29]. It was just a revelation because many of the problems in theoretical computer science were really close to many of the problems in mathematical statistical physics. It was just amazing. We had been doing these parallel developments without talking with each other and in very different languages. So, we brought those people together.

I started thinking a lot about random satisfiability but there were other people, some of the spin glass people, Giorgio Parisi, Marc Mézard, [as well as] the younger generation who came up after them and had been doing the cavity method and all of that were starting [such as] Riccardo Zecchina[30], who turned out to be a really close colleague of mine over the years. He's, I think, kind of the next generation after Giorgio, who does this kind of things, but Giorgio himself and Marc himself [were] very involved[31]. In fact, Marc Mézard has now moved to Riccardo's institute in Milan. What happened is that we began to realize that the phase space structure of disordered systems like spin glasses resemble the phase space structure of some problems that were being looked at in computer science. So, it wasn't just me. It was many of the people who had done spin glasses and the next generation of their students and collaborators moved into doing things more relevant to computer science.

Interestingly, I spoke to a friend of mine, Nathan Myhrvold[32], who had been a classmate of mine at Princeton in graduate school—he'd done quantum gravity[33]. But then right after graduate school, he took a year and he founded a company with a bunch of other guys from my class, and that company was bought by Microsoft. He was on the board of the IAS, and he asked me what I was doing. I told him I was working on theoretical computer science, satisfiability with disorder basically. He said: "Oh, you should come to Microsoft." He was by that point chief technology officer

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[29] *Statistical Physics Methods in Probability Theory, Combinatorics, and Theoretical Computer Science*, Jennifer Chayes and Dana Randall, Institute of Advanced Study, Princeton, New Jersey, USA, 23-27 March 1997. See, *e.g.*, *Institute of Advanced Study: Report for the Academic Year 1996-97* (Princeton: IAS, 1997). https://albert.ias.edu/entities/archivalmaterial/66c093f6-f246-458a-b6db-aa8a245dc580 (Accessed July 13, 2023.). Proceedings: *Random Structures & Algorithms* **15**(3-4), 209-466 (1999). https://onlinelibrary.wiley.com/toc/10982418/1999/15/3-4 (Accessed July 14, 2023.)

[30] Riccardo Zecchina: https://it.wikipedia.org/wiki/Riccardo_Zecchina

[31] See, *e.g.*, M. Mézard, G. Parisi and Riccardo Zecchina, "Analytic and algorithmic solution of random satisfiability problems," *Science* **297**, 812-815 (2002). https://doi.org/10.1126/science.1073287; M. Mézard and R. Zecchina, "Random *k*-satisfiability problem: From an analytic solution to an efficient algorithm," *Phys. Rev. E* **66**, 056126 (2002). https://doi.org/10.1103/PhysRevE.66.056126

[32] Nathan Myhrvold: https://en.wikipedia.org/wiki/Nathan_Myhrvold

[33] Nathan Paul Myhrvold, *Vistas in curved space-time quantum field theory*, PhD Thesis, Princeton University (1983). https://catalog.princeton.edu/catalog/991546303506421

of Microsoft. I said: "Are you crazy? I don't do anything that has to do with computer science. I took *one* coding class as a freshman. I don't even do computational physics. Why should I do this?" He said: "Well, you're working on satisfiability!" He was quite persistent, and they persisted. They got me to visit and was kind of excited. I wanted to do interdisciplinary work.

By then, I was for a good number of years a full professor already. I got tenure in '87, when I went to UCLA, so by this time it was like 10 years that I had been a tenured professor of mathematics. I really wanted to do interdisciplinary work, and in academia it was still pretty siloed. So, when I went to Microsoft for an interview—just so that Nathan would back off and not keep pushing me to do an interview—I said: "Well, if I came here could I hire physicists?" They said: "Yes!" I called my husband—my second husband, my current husband, Christian Borgs[34]—and I said: "I think I want to go to Microsoft." He said: "Are you out of your mind? Why would you do that?" I said: "I can build a group with physicists." Christian too became convinced. This was during my second year at the IAS, because Nathan was on the board of the IAS. I went and interviewed in December of '96. Christian interviewed in January '97. We ended up going there. I officially started in February '97, but I remained at the IAS for the rest of that academic year.

Some of our first visitors were physicists. Riccardo Zecchina and Rémi Monasson[35] got in touch with me and Christian and said: "We're also working on random *k*-SAT and we're using the cavity method. We just published this paper and *Phys. Rev. Letters*[36]," which I had seen and couldn't begin to understand. I thought: "This is just crazy. I don't understand this at all." Here were these two young physicists who had authored that paper getting in touch with us and saying: "We're doing what you're doing. Can we come visit you?" They came and visited us. We started talking with them and working with them[37]. Then, within a year they brought Marc Mézard there to talk with us about this. It was kind of

[34] Chayes married Christian Borgs in 1993. See, *e.g.*, https://en.wikipedia.org/wiki/Christian_Borgs

[35] "Rémi Monasson," *Physics Tree* (n.d.) https://academictree.org/physics/peopleinfo.php?pid=809218 (Accessed July 14, 2023.)

[36] R. Monasson and R. Zecchina, "Entropy of the *K*-satisfiability problem," *Phys. Rev. Lett.* **76**, 3881 (1996). https://doi.org/10.1103/PhysRevLett.76.3881

[37] One of Zecchina's publications acknowledges the Microsoft Theory Group for its hospitality: R. Monasson and R. Zecchina, "Tricritical points in random combinatorics: the-sat case," *J. Phys. A* **31**, 9209 (1998). https://doi.org/10.1088/0305-4470/31/46/011 Reciprocally, one of Chayes' publications (submitted in 1999) acknowledges conversations with Zecchina: B. Bollobás, C. Borgs, J. T. Chayes, J. H. Kim and D. B. Wilson, "The scaling window of the 2-SAT transition," *Random Struct. Alg.* **18**, 201-256 (2001). https://doi.org/10.1002/rsa.1006

the spin glass crew looking at these questions. In fact, it was the continuation of spin glasses, and it was that connection that was for me the connection that moved me towards computer science, interestingly.

Then, other things arose. The internet is a random system[38]. It's a random network. It's not a structured network and the web is a random network. So, all of these things that we had thought about on random networks… Here, the network itself became random. Of course, in a sense the network itself is becoming random in any of your disordered systems because you have [either] no connection or a connection, and it can be of one sign or another. So, a lot of this was random systems on the complete graph. The case that people had said: "Oh, we know how to solve this." Physicists would solve that, but everyone would say: "Well, that's infinite-dimensional so that's not really relevant to what we're doing in physics." In computer science, it was really relevant to what they were doing. They were doing infinite-dimensional random systems. That whole explosion was again these random networks and algorithms on them. Then, you had algorithms on these random networks and how did they behave. There was quenched disorder in the network itself, on which you were doing the algorithm.

Finally, Riccardo spent two years with us in our lab in Cambridge[39], and he brought bunches of physicists: lots of students and postdocs and other visitors. (I do remember that it was during the time that Trump was taking off. We were comparing Italian politics and American politics.) There are a lot of people now who are looking at the fact that deep learning and the effectiveness of a lot of the deep learning algorithms is because of the phase structure, which in many ways looks like a spin glass phase structure with lots of different equilibria. So, you can understand that. With Riccardo, we did a paper called the *Unreasonable effectiveness of neural networks*[40]. That was not mathematical; that was mostly theoretical. We did some calculations, but it was trying to explain some of the behavior

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[38] See, *e.g.*, B. Bollobás, C. Borgs, J. T. Chayes and O. Riordan, "Directed scale-free graphs," In: *SODA '03: Proceedings of the Fourteenth Annual ACM-SIAM Symposium on Discrete Algorithms* (Philadelphia: SIAM, 3): 132-139. ; N. Berger, C. Borgs, J. T. Chayes and A. Saberi, "On the Spread of Viruses on the Internet," In: *SODA '05: Proceedings of the 16th ACM-SIAM Symposium on Discrete Algorithm* (Philadelphia: SIAM, 2005): 301-310. https://www.microsoft.com/en-us/research/publication/spread-viruses-internet/ (Accessed July 13, 2023.)

[39] Microsoft Research New England opened in Cambridge, Massachusetts, USA, in 2008, under Dr. Chayes' direction. See, *e.g.*, Microsoft Research: https://en.wikipedia.org/wiki/Microsoft_Research

[40] C. Baldassi, C. Borgs, J. T. Chayes, A. Ingrosso, C. Lucibello, L. Saglietti and R. Zecchina, "Unreasonable effectiveness of learning neural networks: From accessible states and robust ensembles to basic algorithmic schemes," *Proc. Nat. Acad. Sci., U.S.A* **113**, E7655-E7662 (2016). https://doi.org/10.1073/pnas.1608103113

that had been observed in these deep neural nets, which nobody can understand on the basis of looking at the phase space, doing the cavity method. Now, too, people are looking at—even today, ChatGPT[41]—that as another kind of phase transition in a system. What happened was that ChatGPT got enough knowledge that it… ChatGPT-2 did not have long range order in that sense, and ChatGPT-3 and 4 have long-range order. They can do things they weren't exactly taught. They have enough knowledge, just like there's enough order in a system, that they can propagate information from one side of the system to the other. So, these complicated systems, which are now, as we see, changing every aspect of what we do as human beings, can in fact be understood to some extent—actually to more extent than with any other methods—by the statistical physics of disordered systems, which began in the 1970s.

Interestingly, if you talk to someone like Yann LeCun[42], he will tell you that in the beginning he was working with physicists at Bell Labs. (Sara Solla[43], a physicist, is one of the founders of the NeurIPS conference[44].) And we did other work with Yann and Riccardo[45]. He said for him it felt so natural to be working with statistical physicists. The people who did disordered systems in statistical physics were the kinds of people he was talking to in the very beginning when no one was believing him and the kinds of logic which he uses also in his intuition about these systems.

So, really this was not leaving. This was taking spin glasses into this much larger world. And Giorgio and Marc Mézard and all these people are working in this world now because it is the new frontier for the people who started disordered systems in physics.

PC: I'd like to take you back to that 1996-1997 special workshop you organized. You said you brought people from computer science and from physics. Who in particular was attending? What sort of topics were being discussed at the time? Do you remember?

JC: [0:41:09] It was an entire semester program. There were a lot of people there for the entire semester. I would have to go look at the list of who

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[41] ChatGPT: https://en.wikipedia.org/wiki/ChatGPT

[42] Yann LeCun: https://en.wikipedia.org/wiki/Yann_LeCun

[43] Sara Solla: https://en.wikipedia.org/wiki/Sara_Solla

[44] Conference on Neural Information Processing Systems: https://en.wikipedia.org/wiki/Conference_on_Neural_Information_Processing_Systems

[45] P. Chaudhari, Pratik, A. Choromanska, S. Soatto, Y. LeCun, C. Baldassi, C. Borgs, J. Chayes, L. Sagun and R. Zecchina. "Entropy-SGD: Biasing gradient descent into wide valleys." *J. Stat. Mech.* **2019**, 124018 (2019). https://doi.org/10.1088/1742-5468/ab39d9

was there, but it was truly a revelation. Everyone was saying it was a revelation. There were certainly discussions of 2-SAT and relationships to spin glasses[46]. There were discussions of mixing of systems, which they had been doing in computer science. I was trying to get to equilibrium and finite-size scaling[47], but we had been doing it with entirely different languages and yet we were solving the same problems. If you speak to many people who were there, they say it was the most amazing semester or series of workshops that they had attended, because it brought all these new methods and new questions to two people who had been pursuing paths independently for so many years.

PC: Is that when you met at Riccardo Zecchina, or did you know him already?

JC: [0:42:33] I did not that semester, and Riccardo was quite young at that time, so he wasn't on my radar yet. Then, right after that semester I went to Microsoft. I think that within about a year or so of my arriving at Microsoft this paper appeared in *Phys. Rev. Letters*. I don't know if it was ’97 or ’98. And Riccardo Zecchina and Rémi Monasson reached out and said: “We're doing the same things you are. Can we come visit you?”

PC: So, it was people who were slightly older that were at the workshop at the IAS. It was not so much for students.

JC: [0:43:33] Right.

PC: You mentioned moving to Microsoft. At that point you also became an Associate Professor in the Department of Physics at UW. Did you get to interact with David Thouless in the process?

JC: [0:43:48] It’s really interesting. David was so lovely to me. He really wanted us to start working together again. He gave me an office right next door to his office. The problem was that I became incredibly busy when I got to Microsoft, establishing this group and doing all of this. I just didn't have time to get over to UW as much as I would have liked. So, we didn't work together again, but David was thrilled that I was there. He gave me this office right next door to his. We didn't end up working together again, but the way he approached problems has continued to be a big part of the way

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[46] R. Monasson, R. Zecchina, S. Kirkpatrick, B. Selman and L. Troyansky, “2+*p*-SAT: Relation of typical-case complexity to the nature of the phase transition,” *Random Struct. Alg.* **15** 414-435 (1999). https://doi.org/10.1002/(SICI)1098-2418(199910/12)15:3/4<414::AID-RSA10>3.0.CO;2-G

[47] C. Borgs, J. T. Chayes, H. Kesten and J. Spencer, “Uniform boundedness of critical crossing probabilities implies hyperscaling,” *Random Struct. Alg.* **15**, 368-413 (1999). https://doi.org/10.1002/(SICI)1098-2418(199910/12)15:3/4<368::AID-RSA9>3.0.CO;2-B

I look at the world. I think he knew that. I think he knew that he profoundly… At first, he was surprised. "Why are you coming [to Microsoft]?" I was like: "It's disordered systems. These are disordered systems!" He was surprised that I was showing up at Microsoft as was almost everybody I knew. Riccardo was the person who understood why I was there. I hadn't met him, but it's like: "Here is a physicist who completely gets why we came here."

PC: Do you know if David Thouless had any interest in these new applications of spin glasses to computer science?

JC: [0:45:34] He expressed interest, but we never pursued it. It's one of the things I'm sorry about, that I never made the time to do that with him, because I'm sure his insights would have been incredible.

PC: During your time at UCLA or Microsoft, did you ever teach about spin glasses or replica symmetry breaking?

JC: [0:45:59] I did. I taught a graduate course on mathematical statistical physics, which went over a lot of these things. I can tell you the semester it was. I can tie it to something. There was a riot in Los Angeles when Rodney King was beaten up, in '92[48]. I was teaching this class to this full room of graduate students and the riot had started, unbeknownst to us. People were setting things on fire all over the city, and these fumes started coming in the open windows. We could see black smoke, and we could see these fumes. I guess some people told their classes to go home. We had too much to cover, so I just shut the windows and just kept them. I do remember that from the class. That was the one class. I think Steve Kivelson[49] and I were teaching a class together on some of this stuff. So, I would do some of this spin glass and RSB stuff. I had worked with Steve on resonating valence bonds[50], so we did some of that also.

[48] 1992 Los Angeles Riots: https://en.wikipedia.org/wiki/1992_Los_Angeles_riots

[49] Steve A. Kivelson: https://en.wikipedia.org/wiki/Steven_Kivelson

[50] J. T. Chayes, L. Chayes and S. A. Kivelson, "Valence bond ground states in a frustrated two-dimensional spin-1/2 Heisenberg antiferromagnet," *Comm. Math. Phys.* **123**, 53-58 (1989). https://doi.org/10.1007/BF01244017

PC: At the UCLA, there were a few other people who were interested in disordered systems, for instance, Joe Rudnick[51] and Ray Orbach[52].

JC: [0:48:07] I talked a lot to Ray Orbach. He was very busy being Provost, but we talked a great deal, because there was some Orbach conjecture which was about percolation[53]. I talked to Ray Orbach about disordered systems all the time actually. He was very supportive. I wanted to bring more physicists I could work with to UCLA. I ended up writing up cases for Steve Kivelson and Sudip Chakravarty[54], for the physics department even though I was in the math department. I helped to prepare those cases and I worked with Ray closely on that. I brought them and then I could discuss some of this stuff with them as they both ended up coming to UCLA. Steve stayed a long time, but eventually went to Stanford. Sudip is still at UCLA.

PC: So, you had a like a group of people with whom you could talk, but it did not seed a lot of collaborations within that group. Would you meet more informally, or was this just over lunch? Were there group meetings?

JC: [0:49:30] We would talk over coffee. We would go get coffee together frequently. I had been working with Steve Kivelson, but it was… In the end, I guess by 1994, I was focused on these other things. I went to the Institute for Advanced Study, and I started getting very interested in this interface with computer science. I never talked to any of the computer science people at UCLA interestingly, but I would talk to computer scientists at other places. And I would talk to physicists who were interested in computer science.

Some of the quantum computing also started in our lab at Microsoft. Alexei Kitaev[55] visited us for a couple of years when he was just leaving Russia. Mike Freedman[56] was getting together with physicists. So, we had various

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[51] See, e.g., P. Charbonneau, *History of RSB Interview: Joseph A. Rudnick*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 13 p. https://doi.org/10.34847/nkl.ed19y09o

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

[53] 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 See, *e.g.*, G. Kozma and A. Nachmias, "The Alexander-Orbach conjecture holds in high dimensions," *Invent. math.* **178**, 635-654 (2009). https://doi.org/10.1007/s00222-009-0208-4

[54] "Sudip Chakravarty," *Physics Tree* (n.d.) https://academictree.org/physics/peopleinfo.php?pid=176719 (Accessed July 14, 2023.)

[55] Alexei Kitaev: https://en.wikipedia.org/wiki/Alexei_Kitaev

[56] Michael Freedman: https://en.wikipedia.org/wiki/Michael_Freedman

groups of physicists coming together with mathematicians and seeding these interactions. (That was very early, like ’98.) That was really the beginning of quantum computing. [They were] seeding these interactions between physicists and computer scientists which I think are very rich by now.

I do believe that the kind of the lens of disordered systems and the lens of these complex landscapes of spin glasses is the right lens through which to understand much of what is happening in deep learning and large language models.

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

JC: [0:51:50] I don't think so. I think you asked a lot of questions and I've taken some detours along the way, so I think I've covered most of it.

PC: You mentioned that you still have some notes, but do you have a plan to deposit your papers and correspondence to an academic archive at some point.

JC: [0:52:14] It hadn't occurred to me. Perhaps you could send me the names of some academic archives. I do have some notes. Maybe it wouldn’t be a bad thing to do, so please do let me know where the kinds of places are where I could send these.

PC: The Bancroft library[57] is not a bad place if you're looking for one.

JC: [0:52:39] You mean here? Right here?

PC: Yes.

JC: [0:52:44] Wow! That's such a great idea. Do you use the Bancroft Library for anything?

PC: Yes.

JC: [0:52:53] You do? That is amazing.

PC: Professor Chayes, thank you very much for this conversation.

JC: [0:53:01] Thank you so much, Patrick. And do please call me Jennifer.

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[57] Bancroft Library: https://en.wikipedia.org/wiki/Bancroft_Library