History of RSB Interview: James P. Sethna

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Location:
Over Zoom, from Prof. Sethna's home in Ithaca, New York, USA.

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PC: Good morning, Professor Sethna. Thank you very much for joining us for this interview. As we've discussed ahead of time, the theme of this discussion will be the discovery of replica symmetry breaking and spin glasses, which we bound roughly from 1975 to 1995. Before we dive into the subject, we have of a couple of background questions to help us situate your work on the topic. First, can you tell us a bit about your family and your studies before joining university?

JS: [0:00:31] I grew up in an academic household. My father was a professor of engineering at the University of Minnesota, one of the first cadre—I now understand—of Indian immigrants to the United States joining academics[1]. He met my mother at Ann Arbor[2]. I was born there, and I grew up in Minnesota. I was a math major until my senior year in college, at Harvard, when I shifted to physics. I went to Princeton, and I became a student of P. W. Anderson[3].

PC: Can you say a few things about how you got interested in physics, and then in pursuing graduate studies in physics?

JS: [0:01:32] I read every science book in the elementary school library. All through college, I thought I was a math major, but it kind of got boring. Physics seemed to be much more fascinating. By the time I was applying to graduate schools, I applied to one math graduate school, two law schools and about eight physics grad schools. By the time I got admitted to

[1] See, *e.g.*, K. Spilman, "Pataraspi R. Sethna papers, 1943-1980," *University of Minnesota Libraries* (2005). https://archives.lib.umn.edu/repositories/14/resources/1058 (Consulted October 20, 2022.); A. K. Bajaj and S. W. Shaw, "Foreword," *Nonlinear Dyn.* **4**, 527-530 (1993). https://doi.org/10.1007/BF00162230

[2] Patarasp Rustomji Sethna (1923-1993) married Shirley Sue Smith (1921-1984) in 1954.

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

various things, I clearly focused on physics. I thought at the time I wanted to be a high energy physicist, and that turned out to be boring when I got to graduate school. It's interesting. Moving institutions gives you a new perspective. I remember taking Bert Halperin's[4] statistical mechanics course. I thought: "Wow! This guy is smart." I asked: "Who is this guy anyway?" It was explained to me that he had recently come from Bell Labs and he worked on disordered systems. I thought: "Dirt? Why would I want to do something with dirt?" I wanted to study quarks and grand unified theory. Later on, I recognized that dirt is really quite fun. It's really my focus.

PC: What brought you to work with P. W. Anderson, if that was not your original intent in going to Princeton?

JS: [0:03:20] I read Ashcroft & Mermin's book[5] in preparing for the exam. It was just fascinating. It was really grounded, interesting. P. W. Anderson was the only person at Princeton who was doing anything in that field. I can't say his lectures in the solid-state course were inspiring, but as I got into working with him, his written materials were really engaging and deep and thoughtful. I've always tried to emulate that.

PC: During your graduate studies you did *not* work on spin glasses, but did you hear about them? Were they part of the landscape?

JS: [0:04:16] Of course. Phil Anderson would constantly be sending us papers that he got in the mail, and they would circulate through the group. I would get inundated with all these mysterious concepts and thoughts. I remember a colleague of mine—another graduate student—who was given the task of studying the long-range spin glass using replica theory. [Actually,] he wasn't told to use replica theory; he was just told to look at the long-range spin glass problem. He got buried. Phil Anderson eventually said: "You should go be an experimentalist." He became a great experimentalist at Bell Labs. Another grad student got assigned the same problem, but he just ended up leaving physics. Then, a third graduate student came along, Gabi Kotliar, and he really nailed it[6].

[4] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Bertrand I. Halperin*, transcript of an oral history conducted 2021 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 14 p. https://doi.org/10.34847/nkl.7ac326ng

[5] N. W. Ashcroft and N. D. Mermin, *Solid State Physics* (Philadelphia: W. B. Saunders, 1976).

[6] G. Kotliar, P. W. Anderson and D. L. Stein, "One-dimensional spin-glass model with long-range random interactions," *Phys. Rev. B* **27**, 602 (1983). https://doi.org/10.1103/PhysRevB.27.602; Bernardo Gabriel Kotliar, *One Dimensional Random Systems with Long Range Interactions*, PhD Thesis, Princeton University (1983). https://catalog.princeton.edu/catalog/9917350335066421 (Consulted October 19, 2022.)

Phil Anderson wasn't a nurturing fellow. He would give really interesting problems and then you were expected to work on them yourself. I managed to dodge that bullet. I was also not so interested in electrons. I thought electrons had all these… At the time, the intellectual framework of strongly correlated electron systems was very obscure. All these Green's functions and quasi-particles and diagrams. My instincts seem to be better for atoms and things squiggling around.

PC: Can you recall what was the group's and Anderson's reaction to Parisi's replica symmetry breaking solution when it came out[7]? Was this noted?

JS: [0:06:53] When was that? Was I still in grad school?

PC: 1979-1980.

JS: [0:06:59] Oh! No. I haven't a clue. That's a really interesting question. This landmark result, do I remember any discussions about it? It's not that I doubt there was some, but it's not associated in my mind with being in grad school or who was talking about it. My impression was that [of] Edwards and Anderson[8], Anderson always said: "This was really Edwards, mostly." It's possible that the whole replica theory part… Except, yes, there was this replica theory in spin glasses. He was interested in long-range spin glasses. So, I'm puzzled.

PC: You did start working on spin glasses in the mid-1980s, as a junior faculty, looking at the Bethe lattice version of the model with David Thouless[9]. How did this collaboration and this interest come about[10]?

JS: [0:08:28] David Thouless came up with a Bethe lattice version of the spin glass, and Lincoln[11] and Jennifer Chayes[12], who at the time were married and are amazing mathematical physicists that have always been the only punk mathematical physicists I know of…

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[7] 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

[8] S. F. Edwards and P. W. Anderson, "Theory of spin glasses," *J. Phys. F* **5**, 965 (1975). https://doi.org/10.1088/0305-4608/5/5/017

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

[10] 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

[11] "Lincoln Chayes," *Mathematics Genealogy Project* (s.d.). https://www.genealogy.math.ndsu.nodak.edu/id.php?id=48553 (Consulted October 19, 2022.)

[12] Jennifer Chayes: https://en.wikipedia.org/wiki/Jennifer_Tour_Chayes

(They were really a walking tolerance test. We were all graduate students together at Princeton, and at one point I was walking into physics library at Princeton with Lincoln Chase. We were talking excitedly, and the librarian came over and said: "Can I help you?" with a clear indication that obviously Lincoln looked like a dangerous person. Leather jackets and chains, torn leather jacket with safety pins holding them together. They were colorful, but anyway.)

They had just heard Thouless giving this talk[13], and they said: "We think we can do this." They started working on it. My contribution, I think, was mostly to write the introduction and to guide them into the center manifold theorem[14], which turned out to be quite useful in guiding the mathematical proofs.

PC: Were they postdocs at Cornell at the time?

JS: [0:10:21] They were postdocs at Cornell when I was a young assistant professor. Then went from being postdocs at Cornell to being tenured at UCLA. We tried to hire them, but we weren't willing to offer them tenure right off the bat, and UCLA [did].

PC: So, they had heard about the problem from Thouless. Did you interact with Thouless yourself directly?

JS: [0:10:49] He came to visit and gave a talk and this just happened. We negotiated a little bit, and he was an author, but it was mostly Lincoln and Jennifer's work, with me contributing the introduction and a little bit of other things. Later on, Jean Carlson[15] did the same problem in a field[16]. There, I was much more engaged in the details of the calculations.

PC: Do you know what brought Thouless to the problem, or how he got interested? Do you know anything about his personal motivation?

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[13] Most probably about D. J. Thouless, "Spin-glass on a Bethe lattice," *Phys. Rev. Lett.* **56**, 1082 (1986). https://doi.org/10.1103/PhysRevLett.56.1082

[14] Center manifold theorem: https://en.wikipedia.org/wiki/Center_manifold

[15] Jean M. Carlson: https://en.wikipedia.org/wiki/Jean_M._Carlson

[16] Jean Marie Carlson, *Critical Properties of the Bethe Lattice Spin Glass*, PhD Thesis, Cornell University (1988). https://newcatalog.library.cornell.edu/catalog/1592719 (Consulted 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

JS: [0:11:31] Two things. First, we were all interested in spin glasses, and he finally found a way that you could do it without replica theory. Then, it turned out that our solution suffered from many of the same diseases as the replica symmetric solution. It's not that it wasn't a rigorous solution. It has to be rigorous, because that's what Lincoln and Jennifer do, but the rigor wasn't enough to make it physical. Any of these branching trees has this property that you have to distinguish between what's happening inside and what's happening at the boundary, or something like. Later on, the replica theory people figured out that the proper limit to take to avoid negative entropy and things like that is to instead of having random boundary conditions at the edge, do something which rewire things at the boundary in random ways. Then, replica theory was the way to treat the Bethe lattice as well. I found that revealing. I found that interesting, and making us to yet another Sherrington-Kirkpatrick, replica symmetric answer.

PC: Did you pay attention to the work of Mézard and Parisi, in particular, on this problem, albeit much later[17]? Were you still paying attention to what was being done on the topic?

JS: [0:13:31] Which work are you talking about? The Bethe lattice work?

PC: Yes, the Bethe lattice 1RSB cavity formulation.

JS: [0:13:43] Ok. Two things. My style is to get fascinated by a problem and then adopt the tools, and yet to be admiring of tools that other people [are] developing without actually developing any real insight in them. So, the replica theory was always something that I thought I had a broad familiarity with and a broad interest in. I was of course interested in the fact that we had gotten the wrong answer, but I didn't exactly scrutinize each step of their calculation and try to follow the details, because it is one of those—at the time—very obscure [and] specialized… The cavity improved that and message passing later [further] improved that. I feel like at this stage I might be able to enter it if I had a problem that I wanted answered. Boy! They've done a wonderful job of answering a lot of different central problems in physics, in computer science, in neurology, in all kinds of subjects. It's amazing!

PC: Let's get back to the work you did with your graduate student, Jean Carlson. How did that collaboration come about? Did she overlap with the Chayes?

[17] M. Mézard and G. Parisi, "The Bethe lattice spin glass revisited," *Euro. Phys. J. B* **20**, 217-233 (2001). https://doi.org/10.1007/PL00011099

JS: [0:15:30] Lincoln and Jennifer, yes. This was definitely a collaboration. (I don't know if you're interested in funny stories or not.) When you add a field, instead of having an elaborate central limit theorem that gives you the answer, you get a much more nuanced and interesting mean-field solution. Adding a field to the spin glass problem had long been a big deal. [There had been] big arguments between different groups as to whether there is a transition at all in a field, whether there's a glass transition, with Fisher and Huse[18] disagreeing with the replica theory people in various ways. The opportunity to add a field to our calculation made a lot of sense. Again, in retrospect it was probably replica symmetry. I don't know actually if anyone has ever done the replica symmetry breaking [analysis] in a field on the Bethe lattice[19], but we got all kinds of interesting presumably new functions that describe the Bethe lattice solution. Jean and I worked a long time to find efficient and effective numerical means for generating these functions, which are analytic in the whole plane—I think they proved it—but still have all these wrinkles and things. In the process, Lincoln and Jennifer had sketched out what they thought it would look like. The paper was being written and had all their diagrams and not any of Jean Carlson's final plots, because Jean Carlson's final plots came at the last minute. At some stage, by some error, the sketches which they drew, which were really quite realistic, but were just sketches, got published instead. I remember talking to Lincoln and Jennifer. Lincoln was—they were punk but they really embraced that—"Oh yeah! Our figures were put in there by mistake." He was very smug about it. I said: "You know, you're the postdoc. The graduate student spent time doing a really good job really calculating these things. It's pretty sad that it didn't end up in the paper." He looked a little taken aback. They're good friends of mine, all three of them.

PC: In these works, you acknowledge discussions with many people, including Fisher, father and son, and Mézard and Peter Mottishaw. How did these conversations about the works come about? Were these visitors?

JS: [0:19:28] That's a really interesting question. I'm not sure whether I talked to them, or whether one of my coauthors talked to them. Let me think. Who were the people we acknowledged again?

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

[19] T. Jörg, H. G. Katzgraber and F. Krząkała, "Behavior of Ising spin glasses in a magnetic field," *Phys. Rev. Lett.* **100**, 197202 (2008). https://doi.org/10.1103/PhysRevLett.100.197202

PC: Daniel Fisher[20], Michael Fisher[21], David Huse[22], Marc Mézard[23], Peter Mottishaw, Chuck Newman[24].

JS: [0:20:05] I'm pretty sure Chuck Newman was someone Lincoln and Jennifer regularly corresponded with. Michael Fischer, of course, was at Cornell and I talked to him quite a bit. Daniel Fisher is somebody I talked to a lot. It's a small community. I'm not sure what we talked about. In either the first paper or the second paper I remember a very straight line being plowed by my collaborators to the answer. I'm not sure we got any deep insights from anybody else. It would have been nice if we found out from Mézard that we were up the wrong tree, but I don't think he knew that yet.

PC: What was the reception to these works?

JS: [0:21:12] I learned something from this paper. What you don't want to do is have a really new exciting result, but in the second chapter start with a really hard to read derivation of the basic equation. People would read our paper, they would read the introduction, they would get all excited, and then they would go into the nuts [and bolts].

The foundation of the calculation was the [Fortuin and Kasteleyn random cluster representation[25]], and it was just obscure. It was perfectly rigorous and completely obscure. Later on, Eric Grannan, who worked with Lincoln and Jennifer as a postdoc, came up with the much more intuitive and heuristic, but rigorous, calculation of the same relation[26]. The later sections were this beautiful center manifold theory. That one chapter kept everybody from getting to the later ones unless they were really diligent.

Since then, [I learned that] when you have a very complicated analysis, you stick it in an appendix or something. You say: "This is rigorous. You can go look for it, and then we're going to get you the stuff that's fun."

I think a lot of people read it and a lot of people found it interesting, and a lot of people found it challenging and figured out what was wrong with it.

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[20] Daniel Fisher: https://en.wikipedia.org/wiki/Daniel_S._Fisher
[21] Michael Fisher: https://en.wikipedia.org/wiki/Michael_Fisher
[22] David Huse: https://en.wikipedia.org/wiki/David_A._Huse
[23] Marc Mézard: https://en.wikipedia.org/wiki/Marc_M%C3%A9zard
[24] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Charles M. Newman and Daniel L. Stein*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 35 p. https://doi.org/10.34847/nkl.3dbc3ja3
[25] Random cluster model: https://en.wikipedia.org/wiki/Random_cluster_model
[26] **JS:** This work was not published.

Or, at least a few people did, and then they realized they didn't have to read that bad section.

PC: You seem to have largely left the work related to Bethe lattice calculations and spin glasses after that. Was there no obvious follow up in your mind?

JS: [0:24:09] There was no obvious follow up and I didn't know replica theory. I believe in working on problems that not everyone else is doing. I also end up being dragged into things by my colleagues. Lincoln and Jennifer thought: "Here [is] an opportunity to do a rigorous calculation on a problem of current interest." In fact, it got a lot of attention in the mathematical physics community. Barry Simon[27] was very insistent that we published this paper in his journal. It's hard to do rigorous things, though, if you're doing spin glasses and things. The physicists really have an unfair advantage. We found a rigorous approach to do the wrong calculation, but I'm not sure anything comparable happened for getting the right answer.

PC: At about the same time, you started paying attention to structural glasses. In 1991, for instance, you published "Scaling theory for the glass transition,"[28] in which you mentioned that the ideas proposing an underlying phase transition are appealing. In your sense, what was appealing about these proposals for an underlying phase transition?

JS: [0:25:49] [About] regular glasses, I had this wonderful discussion with Daniel Fisher. I remember we were in Santa Barbara[29]. We were swimming in a pool in some place, and he was telling me about how there was no particular reason why energy barriers in a disordered system should have all energies that scale with temperature. Temperature could be an irrelevant variable under the renormalization group, and you would have a diverging barrier. I wrote two papers. One rather obscure paper, in which I made it very clear that a lot of the ideas here were coming from Daniel[30]. I was focused very much on fragile and strong glasses, some of which had very strong Vogel-Fulcher laws[31] and some of them looked almost

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[27] Barry Simon: https://en.wikipedia.org/wiki/Barry_Simon; Simon was editor of *Communications in Mathematical Physics* 1979-2014.

[28] J. P. Sethna, J. D. Shore and M. Huang, "Scaling theory for the glass transition," *Phys. Rev. B* **44**, 4943 (1991). https://doi.org/10.1103/PhysRevB.44.4943

[29] *Relaxation in Complex Systems*, Prof. Anderson and Prof. E. Abrahams, Institute of Theoretical Physics, Santa Barbara, California, USA (c. 1986).

[30] J. P. Sethna, "Speculations on the glass transition," *Europhys. Lett.* **6**, 529 (1988). https://doi.org/10.1209/0295-5075/6/6/010

[31] Vogel-Fulcher-Tammann equation: https://en.wikipedia.org/wiki/Vogel%E2%80%93Fulcher%E2%80%93Tammann_equation

Arrhenius in the divergence of the viscosity. They can all be systematized by this idea that you have a diverging barrier, and non-universal prefactors or something could explain the whole range. It seemed like a beautiful simple explanation and I'm not sure whether it's dead yet. I don't know. It's sort of entropic barriers versus energy barriers. I do still like it.

It definitely applies to other random systems. The random field Ising model definitely has a zero-temperature fixed point. Temperature is an irrelevant variable; it's a dangerous irrelevant variable. Later on, we looked at the three-dimensional Ising model with antiferromagnetic next-nearest neighbor interactions, which also has a diverging barrier as you coarsen[32]. That's not at the transition; it's below the transition.

The moral of the story, though, is [that in] the second paper, where we wrote it all down, I forgot to acknowledge Daniel. Boy, was that embarrassing! At the time, I was… Anyway, that was embarrassing.

PC: In that context, did you pay much attention to the spin glass-based work of Kirkpatrick, Thirumalai, and Wolynes that came out at about the same time[33]? If yes, in what sense?

JS: [0:29:42] I remember a previous paper of Wolynes that I had read, and I found kind of sloppy. I don't remember what I was complaining about. I think at the time I was a little young and still intolerant of people who don't really nail everything they talked about. And I didn't pay much attention to it. I thought: "Another Wolynes paper!" He's obviously a very creative and inventive guy. The fact that not all the *t*'s are crossed, and *i*'s are dotted is not a reason to not pay attention to it. Over the years, people kept telling me: "Ah! This thing with Wolynes and Thirumalai." That turns out to be key. It's also a pretty technical replica theory calculation, which then they boldly asserted has something to do with configurational [entropy in] glasses. I was also still in the opinion that Phil Anderson kind of plugged into my head when, as a former member of this group, I would ask him: "So, what is the difference in glasses and spin glasses?" He would say: "Spin glasses have disorder that is part of the Hamiltonian and glasses have an emergent disorder that happens as they freeze." The idea that the glass transition could be studied by something that had disorder in the Hamiltonian seemed to be… That was the distinction! Those are two

[32] J. D. Shore, M. Holzer and J. P. Sethna, "Logarithmically slow domain growth in nonrandomly frustrated systems: Ising models with competing interactions," *Phys. Rev. B* **46**, 11376 (1992). https://doi.org/10.1103/PhysRevB.46.11376

[33] T. R. Kirkpatrick, D. Thirumalai and P. G. Wolynes, "Scaling concepts for the dynamics of viscous liquids near an ideal glassy state," *Phys. Rev. A* **40**, 1045 (1989). https://doi.org/10.1103/PhysRevA.40.1045

different problems. But the phenomenon that we're talking about turned out to be very valuable and I missed that.

I've learned something else over the years. There are things that I thought were just dead ends that turned out to be very important. Mode-coupling theory I really was the enemy of when it first came out, and now we're doing calculations that are very much like mode coupling[34]. I remember thinking that random matrix theory and scars and things were all kind of probably useless, and they turned out to be wonderfully important. But everybody has to choose what they work on, and I don't feel like… On the one hand, I do feel like all those other problems were really important and turned out to be very useful, but lots of people were working on them. Why not do something different that is not so crowded? Also, why try to steal everyone else's thunder? When they know what the interesting questions are, I don't have to work on them. They'll do it. I can work on something only I think is important.

PC: By curiosity, what was it about mode-coupling theory and its formulation that you didn't think was correct?

JS: [0:33:33] Götze[35] and Sjögren[36] were working really hard to connect their mode-coupling theory to glasses. They had a giant number of parameters in their theory that they could fiddle with. There was this wonderful seminar that John Toner[37], who I hope I'm not embarrassing by mentioning this. John Toner is very creative. He's very clear when he explains things, and he's able to take situations where he will give wrong answer and say so. I remember at a very early age, he wrote an abstract (or I saw a talk, I forget which) for the American Physical Society, in which he said: "Last year, I talked to you about the cubatic phase, and this year I'll tell you that we haven't really found the cubatic phase."[38] He came and gave a similar talk at Cornell. He was talking about this really nice mode-coupling theory that he had developed for a one-dimensional degree of freedom with short-range interactions[39]. I said: "You're claiming that this one-

[34] See, e.g., W. Götze, *Complex Dynamics of glass forming liquids. A mode-coupling theory* (Oxford: Oxford University Press, 2009).

[35] Wolfgang Götze: https://en.wikipedia.org/wiki/Wolfgang_G%C3%B6tze

[36] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Lennard Sjögren*, transcript of an oral history conducted 2021 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 19 p. https://doi.org/10.34847/nkl.382d6bmv

[37] John Toner: https://de.wikipedia.org/wiki/John_Toner_(Physiker)

[38] See, *e.g.*, David Zierler, *Interview of Paul Steinhardt on June 4, June 18, June 30, and July 8, 2020*, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA (2022). www.aip.org/history-programs/niels-bohr-library/oral-histories/46757

[39] **JS:** No corresponding manuscript could be found. Perhaps Toner didn't publish it once he found out it was wrong.

dimensional degree of freedom connected to a heat bath is going to freeze in one of the two wells? That can't happen." He sort of was taken aback. He gave the rest of his seminar and then he pretty quickly said: "Yep! You're right." The idea that mode-coupling could have a transition in a system where you rigorously could prove it couldn't, made me think that the whole thing was ridiculous. It turns out that it doesn't have a transition. It describes the caging. The idea that you could have a self-consistent theory that describes something that doesn't happen, but is good far away from where it happens, this was all too subtle for me. I'm still not… I should go and examine the current status of all these things. I think in infinite dimensions there is a sharp transition in [structural] glasses that is described by the same kind of mode-coupling theory[40]. That's one of several transmissions that happens. I know the Gardner transition, but there was a second dynamical transition at a different temperature in spin glasses[41]. Again, I remember browsing the papers and collecting tidbits from them, but never really tunneling in and figuring out what is going on.

PC: After a sabbatical at NORDITA[42] in 1991-1992, you moved on to the study of hysteresis and avalanches. Was there in your mind a connection between these topics and the work you'd done on glasses and spin glasses? If not, what drew you in this other direction?

JS: [0:37:23] What drew me in this direction was a wonderful conversation I had with scientists at the University of Barcelona. I'd been working on tweed in martensites[43], and they were experts on martensites. One of them—it might have been [Antoni] Planes but I think it was one of the other people there—showed me this experiment where he had taken this martensite and he had stretched it and let it contract again and stretched it and let it contract again. It would form exactly the same hysteresis loop each time. So, that was really the primary focus of the original paper[44]. It was a random field Ising model, of course, that was part of disordered systems. I was happy with that, but it had avalanches instead of thermal

[40] See, *e.g.*, G. Parisi, P. Urbani and F. Zamponi, *Theory of simple glasses: exact solutions in infinite dimensions* (Cambridge: Cambridge University Press, 2020).
[41] **JS:** I think I heard about it from Jean-Philippe Bouchaud.
[42] Nordic Institute for Theoretical Physics: https://en.wikipedia.org/wiki/Nordic_Institute_for_Theoretical_Physics
[43] See, *e.g.*, S. Kartha, T. Castán, J. A. Krumhansl and J. P. Sethna, "Spin-glass nature of tweed precursors in martensitic transformations," *Phys. Rev. Lett.* **67**, 3630 (1992). https://doi.org/10.1103/PhysRevLett.67.3630; J. P. Sethna, S. Kartha, T. Castán and J. A. Krumhansl"Tweed in martensites: a potential new spin glass," *Phys. Scripta* **1992**, 214 (1992). https://doi.org/10.1088/0031-8949/1992/T42/034
[44] J. P. Sethna, K. Dahmen, S. Kartha, J. A. Krumhansl, B. W. Roberts and J. D. Shore, "Hysteresis and hierarchies: Dynamics of disorder-driven first-order phase transformations," *Phys. Rev. Lett.* **70**, 3347 (1993). https://doi.org/10.1103/PhysRevLett.70.3347

behavior. I'm still a little vague but Mark Robbins[45] had been using the same random field Ising model[46] to describe water entering oil bearing rock in his work with one of the oil companies that he was a consultant for or used to be a postdoc or a member of. He had already seen jerky motion of the front and different… That's the depinning version of the random field Ising model. We were studying the other version. I still think in some ways the memory effects that the hysteresis loop retracing, return-point memory and Alan Middleton's no-passing rules[47] may be the deeper thing, but the avalanches and the renormalization group[48] were the place where the light was good[49], and it's been great fun ever since.

Another thing. I think it might have been around that time that Michael Fisher and Ken Wilson[50] left Cornell, so I didn't feel like it was somebody else's job to do all that renormalization group stuff. I was the local expert now, so it made sense for me to spend more time thinking about it.

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

JS: [0:40:55] I've never thought about replica symmetry breaking. I gave a special topics course once. I did teach a whole course based on Mézard and Montanari's book[51], but the parts that I paid attention to were the parts that didn't involve any replica symmetry breaking. In fact, I don't think have much replica symmetry breaking in there. They really talk about algorithms and things that aren't as mysterious as replica symmetry breaking.

PC: When would that have been?

JS: [0:42:55] Probably spring 2006.

[45] Mark Robbins: https://en.wikipedia.org/wiki/Mark_O._Robbins

[46] J. P. Stokes, A. P. Kushnick and M. O. Robbins, "Interface dynamics in porous media: A random-field description," *Phys. Rev. Lett.* **60**, 1386 (1988). https://doi.org/10.1103/PhysRevLett.60.1386

[47] A. A. Middleton, "Asymptotic uniqueness of the sliding state for charge-density waves," *Phys. Rev. Lett.* **68**, 670 (1992). https://doi.org/10.1103/PhysRevLett.68.670

[48] See, *e.g.*, O. Perković, K. Dahmen and J. P. Sethna, "Avalanches, Barkhausen noise, and plain old criticality," *Phys. Rev. Lett.* **75**, 4528 (1995). https://doi.org/10.1103/PhysRevLett.75.4528; K. Dahmen and J. P. Sethna, "Hysteresis, avalanches, and disorder-induced critical scaling: A renormalization-group approach," *Phys. Rev. B* **53**, 14872 (1996). https://doi.org/10.1103/PhysRevB.53.14872

[49] Streetlight effect: https://en.wikipedia.org/wiki/Streetlight_effect

[50] Kenneth G. Wilson: https://en.wikipedia.org/wiki/Kenneth_G._Wilson; Wilson left Cornell in 1988 and Fisher in 1987.

[51] M. Mézard and A. Montanari, *Information, physics, and computation* (Oxford: Oxford University Press, 2009).

PC: What was the context? Were you just curious about the book? What brought you to teach that?

JS: [0:43:11] I thought it was important. I thought there were all these advances, and the book was just wonderful. I wanted to talk about… I still have an exercise on the random energy model[52] that I stole from their description. My technique of generating exercises is that I find something really interesting in the literature, and then I see if I can distill it into a form that I can assign to graduate students and have them work for a few hours and get the answer. That was a really fun [experience].

PC: In your own statistical mechanics textbook[53], you briefly mention spin glasses, replica symmetry breaking and the cavity method, but you do not provide any technical details on any of these systems or methods. Why that choice?

JS: [0:44:25] Well, the book is aimed at entering graduate students, who need to know a broad variety of subjects. There is one chapter on linear response. Linear response is the bread-and-butter for experimentalists everywhere and has all kinds of interesting relations that weren't invented in the 21st century: fluctuation-dissipation theorem[54] and things like that. You have to hear about that. There's one chapter on all of the renormalization group and scaling and things like that. So, it's not primarily because I didn't know enough about the subject to say something simple about it. I think I would have plunged into it and found something that I could say that's clear and obvious. It's also, you know, the market. (I was a postdoc with Eric Siggia[55].) The Martin-Siggia-Rose transformation[56] is really beautiful and interesting, but it's a little bit advanced for people who just have [learnt] what a Green's function[57] is, at the beginning of the course, to trace over all the possible trajectories and have a delta function at the right spot. I wanted to emphasize the conceptual aspects, and not the techniques. I'm still kind of looking for that. I'm still kind of looking for a way of pulling out the important concepts about disordered systems. I'd like to be able to talk about dangerous irrelevant temperature, and how it

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[52] Random energy model: https://en.wikipedia.org/wiki/Random_energy_model

[53] J. P. Sethna, *Statistical Mechanics: Entropy, Order Parameters and Complexity* (Oxford: Oxford University Press, 2006); *Statistical Mechanics: Entropy, Order Parameters and Complexity*, 2nd edition (Oxford: Oxford University Press, 2021).

[54] Fluctuation-dissipation theorem: https://en.wikipedia.org/wiki/Fluctuation-dissipation_theorem

[55] F. Ahmed, "Profile of Eric D. Siggia," *Proc. Nat. Acad. Sci. U.S.A.* **109**, 5551-5552 (2012). https://doi.org/10.1073/pnas.1204149109

[56] P. C. Martin, E. D. Siggia and H. A. Rose, "Statistical dynamics of classical systems," *Phys. Rev. A* **8**, 423 (1973). https://doi.org/10.1103/PhysRevA.8.423

[57] Green's function: https://en.wikipedia.org/wiki/Green%27s_function

slows things down. That makes me feel like I taught them something about the glass, and not about how we prove something about glasses. If that came out naturally from cavity method version seven, that would be just fantastic.

PC: In that book, you do describe glasses a bit more than spin glasses. You don't really mention anything related to Wolynes-Kirkpatrick-Thirumalai or RSB ideas, although you do mention Nelson's geometrical frustration ideas[58]. Why that choice?

JS: That was the statement about homotopy theory[59]. I have a chapter about homotopy theory. Homotopy theory is a wonderful way of understanding defects in solids and defects in liquids and liquid crystals. Yet it's a slightly empty theory. You can figure out what the defects are in a liquid or a liquid crystal pretty straightforwardly without ever having homotopy theory. It just gives you a nice framework to hang it on, so that you're not always reinventing basic concepts. You have a certain machinery. I wanted to tell them something that was deep and interesting that came out of it. The one thing is that if you have non-Abelian defects they don't cross. There are only two examples: there's biaxial nematics[60] and then there's metallic glasses. So, you end up talking a little bit about metallic glasses even though the Nelson's never did… I think we say that. I think this was a theory for metallic glasses, and maybe the metallic glass gets stuck because the defects can't cross, but that's never been turned into a real calculation. I think I say that. I'm not trying to say that Nelson's theory was more important than the recent stuff, starting with Wolynes and so on. When I pull it in references, I tried to put in things that if they look them up, they can get. Nelson's papers were pretty physical, pretty grounded. You can put the atoms this way and they packed this way, and then you get icosahedra and then something, something. The connection between Wolynes' work and real glasses, you need to really be engaged to make that connection, to understand what the connection might be, to appreciate it. [It's] so far that I didn't get it. So, if I say "Take a look at this paper," they'll get lost.

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

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[58] See, **e.g.**, D. R. Nelson and F. Spaepen, "Polytetrahedral order in condensed matter," *Solid State Phys.* **42**, 1-90 (1989). https://doi.org/10.1016/S0081-1947(08)60079-X

[59] Sec. 9.4 of Sethna (2021); homotopy Theory: https://en.wikipedia.org/wiki/Homotopy_theory

[60] See, *e.g.*, N. D. Mermin, "The topological theory of defects in ordered media," *Rev. Mod. Phys.* **51**, 591 (1979). https://doi.org/10.1103/RevModPhys.51.591; Biaxial nematic: https://en.wikipedia.org/wiki/Biaxial_nematic

JS: [0:50:54] It's not about replica theory, but one of the morality tales I tell people has to do with the random field Ising model. You may remember that in that era they were two competing theories for the random field Ising model: there was the supersymmetry theory[61], which claimed that random field Ising model has the same critical properties as the Ising model in two lower dimensions, and then there was the bonehead theory that Geoff Grinstein promoted[62], which looked at clusters and things, and said that there had to be a transition in three dimensions, and that it was borderline. Geoff Grinstein was on both sides. The theorists, everybody appreciated the supersymmetry arguments, and everybody agreed that it might be wrong, but they did not understand why it might be wrong. The experimentalists were much… There were two groups of experimentalists: Jaccarino's[63] group and Birgeneau's[64] group, on opposite coasts. They fought horribly, I understand, from Steve Kivelson[65]. (I hope Steve doesn't mind my mentioning this.) I always thought that that was sad. They would disparage one another's work, and reject one another's grant proposals. (Maybe I'm libeling somebody and I'll get sued for libel here.) They were both befuddled by the diverging barrier heights. Nobody knew at the time that the system was glassy, that you couldn't reach equilibrium by cooling. If one of them got the right answer and the other one didn't, it was a little bit of an accident because neither one of them could guarantee that they were really exploring the equilibrium state. That turned into a really interesting project, but in the meantime, there was a bunch of a rather unfortunate fights. It's a natural thing to ignore or belittle people who disagree with you, and you should be cautious about that. You should be respectful, you should be broad-minded, and you should be thinking hard about how both of you can be right, rather than being so sure of your own worth that you can't recognize the possibility that somebody else has something useful to say. It's tricky.

Sometimes people abandon fields when they're proven wrong about something.[66] I didn't do that for spin glasses. I was happy to find out that spin glasses on a Bethe lattice are more complicated than I thought. I just didn't have any new ideas about it.

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[61] See, *e.g.*, G. Parisi and N. Sourlas, "Random Magnetic Fields, Supersymmetry, and Negative Dimensions," *Phys. Rev. Lett.* **43**, 744 (1979). https://doi.org/10.1103/PhysRevLett.43.744

[62] See, e.g., G. Grinstein and S.-k. Ma, "Roughening and lower critical dimension in the random-field Ising model," *Phys. Rev. Lett.* **49**, 685 (1982). https://doi.org/10.1103/PhysRevLett.49.685

[63] Vincent Jaccarino (1924—2019); See, *e.g.*, https://chancellor.ucsb.edu/memos/2019-09-03-sad-news-professor-emeritus-vincent-jaccarino (Consulted January 13, 2021.)

[64] Robert J. Birgeneau: https://en.wikipedia.org/wiki/Robert_J._Birgeneau

[65] Steven Kivelson: https://en.wikipedia.org/wiki/Steven_Kivelson

[66] **JS:** Some text was omitted from the oral transcript, because I got the example wrong somehow. The paper I was quoting apparently does not exist.

But I was talking about the random field Ising model and how you should be tolerant of other people and welcome the fact that you might be wrong. I think that's a very valuable lesson. It's always better to be right. It's always better to be the first person, or an early person, to recognize when you're wrong and say so. It takes the courage that John Toner has—in later life, I've seen a few other people—to be able to recognize an error, and correct it, and not feel so embarrassed that you can't continue in the field.

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

JS: [0:57:08] I don't believe I have anything. I think I have all my emails since the dawn of time, but I'm not even sure we had emails in those days. No, I don't have any correspondence.

PC: With Thouless or the Chayes once they left, were you corresponding over the phone?

JS: [0:57:43] What kind of correspondence do people have that they mention when you ask them this?

PC: Some of them have paper letters in folders that they have saved.

JS: [0:58:08] I guess I usually collaborated—in those days at least—with people that I was in the same building with. So, Lincoln and Jennifer and I didn't have long letters to one another because we were all sitting in the same place. We would just talk. I don't remember any long-distance collaborations. There was a brief two or three emails back and forth with the Deepak Dhar[67] on the Bethe lattice random field Ising model[68] that I could probably dig up, but that's not replica theory and it's not even spin glasses. No, I don't think I have anything.

PC: Thank you very much for this conversation. It's been quite interesting.

JS: [0:59:15] This is great. Thank you very much. Thank you, Francesco, as well.

[67] Deepak Dhar: https://en.wikipedia.org/wiki/Deepak_Dhar

[68] See, *e.g.*, D. Dhar, P. Shukla and J. P. Sethna, "Zero-temperature hysteresis in the random-field Ising model on a Bethe lattice," *J. Phys. A* **30**, 5259 (1997). https://doi.org/10.1088/0305-4470/30/15/013