History of RSB Interview: David Huse

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PC: Good afternoon, Professor Huse. Thank you very much for joining us. As we've discussed ahead of this conversation, the theme of our series is the history of replica symmetry breaking in physics, which we roughly bound between 1975 and 1995. But before we get into the meat of the subject, we have a couple questions on background to help situate your own contributions. First, can you tell us a bit about your family and your studies before starting university?

DH: [0:00:35] I grew up in suburban Boston. I just went to the public high school there, Lincoln-Sudbury high school[1], a very good school. I was sort of born into this. Both of my parents have degrees in physics. My father worked as an electrical engineer at Raytheon[2], which was a defense contractor, and my mother was a computer programmer[3]. She actually programmed the ILLIAC[4] in the 1950s. My father was at Illinois for a master's degree, and my mother had that as a job. My mother was smarter than my father. It should have been the other way around. My mother should have been going for a PhD and my father working to support her, but it was the 1950s, so…

PC: What then led you to pursue a degree in physics at UMass Amherst?

[1] Lincoln-Sudbury Regional High School: https://en.wikipedia.org/wiki/Lincoln-Sudbury_Regional_High_School
[2] Raytheon: https://en.wikipedia.org/wiki/Raytheon
[3] Mason W. Huse (1928-2003) married Marjorie Burt Carroll Huse (1929-2022) in 1953. She graduated with High Honors in Physics from Wellesley in 1950. See, *e.g.*, "Wellesley College – Seventy-Second Annual Commencement," *Wellesley College Digital Repository* (1950). https://repository.wellesley.edu/islandora/object/wellesley%3A31469/datastream/OBJ/download (Accessed November 22, 2023.)
[4] ILLIAC: https://en.wikipedia.org/wiki/ILLIAC

DH: [0:01:34] We lived in Massachusetts, and the deal with my parents was if I went to an expensive school, I had to pay part of the tuition, whereas if I went to UMass, it was fine. It was a good school.

I sort of knew I liked physics from studying in high school, but I tried a couple other things, chemistry, engineering, math, but I liked solving problems and I didn't like doing proofs, and I like math, so theoretical physics was the natural place where I was comfortable.

PC: What drew you then to pursue graduate studies at Cornell, with Michael Fisher[5] in statistical mechanics, in particular?

DH: [0:02:28] At U Mass, there was a professor named Bob Guyer[6], who I did a project with my senior year. He wanted to learn about renormalization group and percolation, so he got me going on that. He didn't know much about it either, so we had a lot of fun learning that kind of stuff. He had been a student at Cornell[7]. There was another professor, who was very good teacher, Gene Golowich[8], who had also been at Cornell. So, they knew people at Cornell, so I ended up at Cornell. I was lucky Michael Fisher [was taking students]. Michael Fisher sort of had a cycle. He would have some students, then he'd finish them up and take a sabbatical. He had been on sabbatical at Caltech, and he came back, so he was at the point of taking on students. There was good timing there for me just by chance. So, I started working with him, which was a wonderful decision.

PC: How did the thesis topic come about[9]? Was it your idea, or was it Prof. Fisher's?

DH: [0:03:46] This was about commensurate-incommensurate transitions and that kind of related stuff[10]. He was working on those things already. There was this guy named Walter Selke[11], who he had been working with on

[5] Michael Fisher: https://en.wikipedia.org/wiki/Michael_Fisher

[6] Robert A. Guyer (1936-). See, *e.g.*, "Our History: 1963 to 1985 -- Faculty Growth and Evolution", *Duke Department of Physics* (n.d.). https://physics.duke.edu/about/history/1963-1985

[7] Robert Alan Guyer, *Solution of the linearized phonon Boltzmann equation*, PhD Thesis, Cornell University (1966). https://catalog.library.cornell.edu/catalog/6677 (Accessed November 16, 2023.)

[8] Eugene Golowich. See, e.g., "Gene Golowich Retires," *Fundamental Interactions Theory Group at UMass* (August 2, 2011). https://blogs.umass.edu/het/2011/08/02/gene-golowich-retires/ (Accessed November 19, 2023.)

[9] David Alan Huse, *Domain walls and the melting of commensurate phases*, PhD Thesis, Cornell University (1983). https://catalog.library.cornell.edu/catalog/70953 (Accessed November 19, 2023.)

[10] See, *e.g.*, W. Selke and M. E. Fisher, "Monte Carlo study of the spatially modulated phase in an Ising model," *Phys. Rev. B* **20**, 257 (1979). https://doi.org/10.1103/PhysRevB.20.257

[11] Walter Selke: https://en.wikipedia.org/wiki/Walter_Selke

these problems, who had been a postdoc before I got to Cornell. They had some work, and we started working on those problems. Julia Yeomans[12] was there. I worked with her a little bit[13].

PC: Can you give us a general feel of what was the statistical physics community at Cornell at that time for Prof. Fisher's group and the other groups?

DH: [0:04:33] In physics, there was lot of theoretical activity. Fisher and Ben Widom[14] sat in the Chemistry Department. I don't think Ben Widom was even a professor of physics. Michael Fisher, as he loved to say, was a professor of chemistry, mathematics, and physics, but he sat in the chemistry Department, which he had been chair of. We had this little suite of offices, which was Widom and Fisher together. A lot going on there. A lot of good postdocs coming through. Widom actually had an experimental lab as well. They were doing multiple fluid phase separation, kind of multicritical and stuff. Phase diagrams involving multi-component fluids and phase transitions, wetting and interface tensions, and all that kind of stuff. It was very stimulating. So, we interacted a lot with Ben Widom's group. Then, there was David Mermin[15], Neil Ashcroft[16], Vinay Ambegaolar[17], John Wilkins[18], Eric Siggia[19], Ken Wilson[20], Jim Krumhansl[21], and I'm not even remembering everyone. It was just an amazing time to be at Cornell. The low temperature physics group was doing… Experimental activity was really strong. I was just so lucky. In some sense, it's sort of a shame that Cornell isn't what it was then. It's one of the places which hasn't been able to keep that up, unfortunately.

PC: Were there joint group meetings, were there seminar series? How did this play out day to day?

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[12] Julia Yeomans: https://en.wikipedia.org/wiki/Julia_Yeomans

[13] D. A. Huse, M. E. Fisher and J. M. Yeomans, "Multiphase behavior and modulated ordering in soluble Ising models," *Phys. Rev. B* **23**, 180 (1981). https://doi.org/10.1103/PhysRevB.23.180; D. A. Huse, J. M. Yeomans and M. E. Fisher, "Exactly soluble Ising models exhibiting multiphase points," *J. Appl. Phys.* **52**, 2028-2030 (1981). https://doi.org/10.1063/1.329601

[14] Benjamin Widom: https://en.wikipedia.org/wiki/Benjamin_Widom

[15] David Mermin: https://en.wikipedia.org/wiki/N._David_Mermin

[16] Neil Ashcroft: https://en.wikipedia.org/wiki/Neil_Ashcroft

[17] Vinay Ambegaokar: https://de.wikipedia.org/wiki/Vinay_Ambegaokar

[18] "John Wilkins," *Physics Today* (2020). https://doi.org/10.1063/PT.6.4o.20200110a

[19] See, *e.g.*, "Eric D. Sigggia," *National Academy of Sciences* (n.d). https://www.nasonline.org/member-directory/members/2518670.html (Accessed November 19, 2023.)

[20] Kenneth G. Wilson: https://en.wikipedia.org/wiki/Kenneth_G._Wilson

[21] James A. Krumhansl: https://en.wikipedia.org/wiki/James_A._Krumhansl

DH: [0:06:37] Yeah, lots of [them]. Michael had a weekly seminar, and Widom would have [them as well, but] not so often. Lots of people, I remember. All these people, the first time I saw them is when they came: John Cardy[22], Jon Machta[23], Steve Shenker[24]. Those are who just come to mind right away. The first time I met them was when they came. It had this seminar room up on... (You've been there, presumably, although that's changed now.) The building, Clark Hall[25], on the seventh floor, had these windows out over. You just had these gorgeous views across the valley and the lake. So, we had this seminar room when Michael seminar would meet, and we'd have lunch. It was just this amazingly beautiful view over Ithaca and Cayuga Lake[26]. Of course, there was lots of what is now called condensed matter stuff from the department as well.

PC: Was the relationship between physics and chemistry fairly transparent, or were there any obvious barriers?

DH: [0:08:01] No. We had three grad students in the office where I was sitting. Two of us were physics students, one was a chemistry grad student, but it really didn't matter. Of course, details matter, but there was a lot of back and forth. I was really in the Physics Department, and I didn't do much in the Chemistry Department other than Widom and Fisher. I sat in the Chemistry Department, but it was just a short walk down the corridor into the physics building.

PC: Upon graduating, you went to work at Bell Labs. What drew you to leave a more academic setting and go to Bell Labs?

DH: [0:0854] Bell Labs was an academic setting. This was a long time ago. Bell Labs, at that time, was *the* top condensed matter physics laboratory in the world by, I would say, a substantial margin. So, in terms of doing condensed matter and being in contact with all the activity, I think it was much more active than any university department. Of course, that changed. I sort of saw just the tail end of it. It changed while I was there. You look at all these people. That was a standard career path back then. If you can go to Bell Labs, go to Bell Labs, stay a few years, and then go back into academia. It was very standard. Academia is full of people my age and a bit older who followed that career path.

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[22] John Cardy: https://en.wikipedia.org/wiki/John_Cardy
[23] "Jon[athan] Machta," *U Mass Physics* (2009) https://people.umass.edu/machta/ (Accessed November 19, 2023.)
[24] Stephen Shenker: https://en.wikipedia.org/wiki/Stephen_Shenker
[25] K. C. Zirkal, "Clark Hall of Science", *Wikipedia Commons* (2018). https://en.m.wikipedia.org/wiki/File:Clark_Hall_of_Science,_Cornell_University.jpg
[26] Cayuga Lake: https://en.wikipedia.org/wiki/Cayuga_Lake

PC: What was the group you were part of? Who were you interacting with in general when you started Bell labs?

DH: [0:10:11] There was the Theory Department, and the Theory Department was in the Physical Sciences Laboratory. Bell Labs was different than a university in that the ratio of experimentalists to theorists was more appropriate, given that physics is an experimental science. The place was 80 or 90% experimentalists, and I interacted a lot with experimentalists while I was there. There was an area that was sort of more material science, chemistry, but there were some physicists in there: John Weeks[27], Wim van Saarloos[28], and Pierre Hohenberg[29]. Eventually, Pierre Hohenberg became my department head. There was this other section of theorists, that were sort of more physical chemists: John Tully[30], Frank Stillinger[31]. So, there was a lot of really good theorists who were in the sort of more physical chemistry laboratory. I forget what it was called. A bunch of good polymer chemists. In fact, the guy who just won the Nobel Prize was there, Louis Brus[32]. There were some polymer theorists. (Names aren't coming back to me.) There were a lot of experimentalists, but there were 30 or 40 serious theorists, also doing research in physics and physical chemistry. Also, we interacted quite a bit with some of the mathematicians. There were some probabilists. I actually went to Peter Shor[33]'s first talk about [his algorithm]. I had no clue what I was seeing, but there was this probability group [and] they were somewhat interested in statistical physics problems. I was sometimes interested in what they were doing, so I used to go to their seminars. I happened to go to Peter Shor's seminar. I had no clue what I was seeing.

PC: At that point, were you mostly free to choose your own research problems?

DH: [0:12:37] Yeah, if you're performing well. Bell Labs was very strong feedback, very much a meritocracy. If you're performing well, you had complete freedom if you're a theorist. If you're an experimentalist, of course, you had to get approval for buying things, because people didn't really have… We weren't supposed to apply for outside funding. Funding would just come through the company. The experimentalists, of course,

[27] "John David Weeks," *Physics History Network* (n.d.). https://history.aip.org/phn/11507006.html
[28] Wim van Saarloos: https://en.wikipedia.org/wiki/Wim_van_Saarloos
[29] Pierre Hohenberg: https://en.wikipedia.org/wiki/Pierre_Hohenberg
[30] John C. Tully: https://en.wikipedia.org/wiki/John_C._Tully
[31] Frank Stillinger: https://en.wikipedia.org/wiki/Frank_Stillinger
[32] Louis Brus: https://en.wikipedia.org/wiki/Louis_E._Brus
[33] Peter Shor: https://en.wikipedia.org/wiki/Peter_Shor

had to ask for money when they needed something. But the management, you had to go about five layers up in management before it wasn't a PhD physicist. Management really understood what we were doing, and you'd really get feedback. People would get fired if they were underperforming. They'd be told to leave. (It's not that harsh.) They wouldn't be told to leave next week, but they'd get fired. When you're young, you could get a raise that was 5%; you could get a raise that's 25%, depending upon… The department heads would get together every year. (I forget what they called it.) They would evaluate the people and they'd really make distinctions. That was a very different environment.

The other thing is there were very few postdocs. There were a few postdocs. It was a real privilege to have a postdoc or a technician. A lot of the experimentalists, they would work with each other. Often, they'd have to just work by themselves, because they didn't have a postdoc or a technician. That was the kind of thing. They wouldn't give out a lot of [these positions]. They would have young people, and they wanted them to work with each other. If a postdoc or a technician would help, they would give it, but an awful lot of experimentalists didn't have a student, they didn't have a postdoc, they didn't have a technician. But they got a lot of good stuff done by just working with other people. If they wanted a bigger group, they would leave. That was the culture. Stay for 5, 10, 15 years, and if you're really just a researcher, go to the university. They would say: "Go to the university and send us your best students." The idea is when the people leave it's an improvement because you lose them, but you get all their best students. That was the culture. I was fortunate to see the end of it. I was there for 13 years, and it was pretty much over by the time I left.

PC: How did the first topics of research come to you then? How did you initially manage to prove your worth and to stay?

DH: [0:15:47] I found things to work on. I worked a bit with Daniel Fisher[34]. He was the person I talked with the most, but initially—I guess that again was part of it as they were evaluating me—he was not actively collaborating with me and having me show what I could do on my own. But we talked a bit. I was fortunate at the time. (I don't know if you know this.) This was just the beginning of conformal field theory. They did this thing where they found an infinite sequence of models whose exponents they knew. (I forget what the precise name of this is.) It's from Friedan, Qiu, and

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

Shanker[35], and then also Russians, Belavin, Zamolodchikov and Polyakov[36]. I knew about that probably because Daniel told me about it, but it wasn't something he was working on.

Then, I was also reading these exact solution papers from Baxter[37] and company. I had a couple of things as a graduate student, where I was sort of mining Baxter[38], [as] I called this. Rodney Baxter could do all these exact solutions, but he didn't really have a feel for the scaling theory and the renormalization group. He would sort of just put the formulas out there, but he wouldn't really take them apart and think about them from the Wilsonian RG point of view. I had done that a couple of times, where I went into some of their papers and just got out some good stuff that they hadn't been able to notice because they just weren't looking at it from that point of view. It wasn't something which was obvious from the mathematical approach they were using, but when you bring in physics view, you see: "Oh, it's got to have this scaling!"

There was this amusing [episode], where I wrote this… Andrews, Baxter and Forrester had this class of models—the hard hexagons—and they had four different critical points[39]. One thing I discovered is [that] one of them was a multi-critical point in a way that they didn't understand. They just saw it as a critical point because they only went through it in one direction. For three of these points which they all got from the same mathematics, I was able to pull out corrections to scaling and described everything in terms of scaling theory. But the fourth one, I couldn't see it. So, I wrote up this paper and I sent it to Baxter[40], and I made a conjecture: "This fourth one should also have some structure like this, but I can't see how to get it out from hypergeometric functions." (I wasn't good at hypergeometric functions.) He had this thing where he had some very early version of something like Mathematica[41], where it would just do these power series and get all these patterns out. He just found it. He could expand out these

[35] D. Friedan, Z. Qiu and S. Shenker, "Conformal invariance, unitarity, and critical exponents in two dimensions," *Phys. Rev. Lett.* **52**, 1575 (1984). https://doi.org/10.1103/PhysRevLett.52.1575
[36] A. A. Belavin, A. M. Polyakov and A. B. Zamolodchikov, "Infinite conformal symmetry of critical fluctuations in two dimensions," *J. Stat. Phys.* **34**, 763-774 (1984). https://doi.org/10.1007/BF01009438
[37] Rodney Baxter: https://en.wikipedia.org/wiki/Rodney_Baxter
[38] See, *e.g.*, D. A. Huse, "Tricriticality of interacting hard squares: some exact results," *Phys. Rev. Lett.* **49**, 1121 (1982). https://doi.org/10.1103/PhysRevLett.49.1121; D. A. Huse and M. E. Fisher, "Commensurate melting, domain walls, and dislocations," *Phys. Rev. B* **29**, 239 (1984). https://doi.org/10.1103/PhysRevB.29.239
[39] G. E. Andrews, R. J. Baxter and P., J. Forrester, "Eight-vertex SOS model and generalized Rogers-Ramanujan-type identities," *J. Stat. Phys.* **35**, 193-266 (1984). https://doi.org/10.1007/BF01014383
[40] D. A. Huse, "Exact exponents for infinitely many new multicritical points," *Phys. Rev. B* **30**, 3908 (1984). https://doi.org/10.1103/PhysRevB.30.3908
[41] Computer algebra systems: https://en.wikipedia.org/wiki/Computer_algebra_system

functions in a power series and just see the patterns and make conjectures, and then find the formula in Ramanujan. He sent me this thing back showing me how he found the formula which he'd fit with my conjecture.

Anyways, I had been doing that kind of stuff, so I knew these exact solutions. I was probably the only one in the world who knew Andrews, Baxter, and Forester in a fair amount of detail, and they had an infinite sequence of multi-critical points. These conformal field theories, who had an infinite sequence of multi-critical points, and it was just numerology. That just matched beautifully. It's just that nobody else had looked at them both at the same time. But then I was able to put that [together]. I think that was probably the highlight of my sort of "non-tenure". For the first year and a half when I was there, I didn't have "tenure". I was a postdoc basically. That was probably the highlight, but there were a few other things[42].

PC: You mentioned Daniel Fisher. From what I understand, you already knew him from your thesis days.

DH: [0:20:25] Not much.

PC: Didn't you thank him in your thesis as someone with whom you had conversations?

DH: [0:20:32] Yeah! I guess I had… I admit. We used to go to the Rutgers meetings, Joel Lebowitz's meetings[43]. That was a standard thing for Fisher's group. We'd all pile in a station wagon twice a year to go down to right near here, to Rutgers. So, I would see Daniel there. Then, what happened is the summer before I graduated—I graduated in '83, so the summer of '82—(I had met Daniel and of course there was a family connection) I was invited or it was suggested—I don't know if it was Michael's idea or whatever—[that] I spend the summer before the summer when I graduated, at Bell Labs. So, I certainly got to know Daniel then. I knew him pretty well by the time of my last year of graduate school because I had spent the summer at Bell Labs.

[42] See, *e.g.*, D. A. Huse, "Incomplete wetting by adsorbed solid films," *Phys. Rev. B* **29**, 6985 (1984). https://doi.org/10.1103/PhysRevB.29.6985; W. D. Selke, D. A. Huse and D. M. Kroll, "Interfacial adsorption in the two-dimensional Blume-Capel model," *J. Phys. A* **17**, 3019 (1984). https://doi.org/10.1088/0305-4470/17/15/019

[43] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Joel L. Lebowitz*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 6 p. https://doi.org/10.34847/nkl.ad7a1tmg

PC: Is he the one who recruited you to Bell Labs? Is that how it worked? Or was this a completely separate process?

DH: [0:21:56] Basically. I remember there was a phone call from Bill Brinkman[44] to my wife[45], which I think was pretty important to convince her that it's not so bad moving to New Jersey. We're still here! Bill Brinkman was two levels up in the management. I guess they called that Laboratory Director.

PC: Your first work actually on spin glasses was a paper with the late Ingo Morgenstern[46], who was then I think a postdoc at Bell Labs, if I understand the roles properly. I think he was mostly working with Andy Ogielski[47].

DH: [0:22:50] Yeah. Ogielski had this project. He built a special purpose computer, and he invited Ingo. (I think Ingo was Binder's student[48]. I'm not positive where Ingo came from.) I used to talk with him quite a bit. We did this transfer matrix calculation[49]. (I'd forgotten about that.) I had probably already done the paper with Chris Henley[50] about domain walls and random ferromagnets[51]. In the works before that, I had never done anything with quenched disorder. None of my thesis work or my early work at Bell Labs [had quenched disorder], but then we did this first project on the domain wall in the two-dimensional Ising random bond ferromagnet where we were able to get out that scaling, which now we know is KPZ[52]. It was one of the early pre-KPZ versions of that scaling, which also had been known before from hydrodynamics from the first Burgers' equation[53].

[44] William F. Brinkman: https://en.wikipedia.org/wiki/William_F._Brinkman

[45] David Alan Huse married Julia Smith (1956-) in 1982.

[46] See, *e.g.*, "In Memory," *Universität Regensburg* (2021). https://www.uni-regensburg.de/physik/grifoni/in-memory-of/index.html (Accessed November 21, 2023.)

[47] P. Charbonneau, *History of RSB Interview: Andrew T. Ogielski*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École Normale Supérieure, Paris, 2021, 23 p. https://doi.org/10.34847/nkl.86f6z55x

[48] Ingo Morgenstern, *Ising-Systeme mit eingefrorener Unordnung in zwei Dimensionen*, Dissertation, Universität Saarbrücken (1980). https://katalog.ub.rwth-aachen.de/permalink/49HBZ_UBA/kloccf/alma991024916119706448 See also: Patrick Charbonneau, *History of RSB Interview: Kurt Binder*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 20 p. https://doi.org/10.34847/nkl.5f2b685y

[49] D. A. Huse and I. Morgenstern, "Finite-size scaling study of the two-dimensional Ising spin glass," *Phys. Rev. B* **32**, 3032 (1985). https://doi.org/10.1103/PhysRevB.32.3032

[50] V. Elser and N. D. Mermin, "Christopher L. Henley," *Physics Today* (2015). https://doi.org/10.1063/PT.5.6160

[51] D. A. Huse and C. L. Henley, "Pinning and roughening of domain walls in Ising systems due to random impurities," *Phys. Rev. Lett.* **54**, 2708 (1985). https://doi.org/10.1103/PhysRevLett.54.2708

[52] Kardar-Parisi-Zhang equation: https://en.wikipedia.org/wiki/Kardar%E2%80%93Parisi%E2%80%93Zhang_equation

[53] Burgers' equation: https://en.wikipedia.org/wiki/Burgers%27_equation

PC: Is it through Henley or through Morgenstern that you first learnt about spin glasses? Or did you know about these systems already because they were in the air at Bell Labs or elsewhere?

DH: [0:24:35] It's a good question how I first learned about it. It was probably something that there would be seminars on. Henley had worked on spin glasses with Sompolinsky and Halperin[54]. I might have learned about it first from Chris. Sompolinsky[55] used to visit Bell Labs a lot, so I might have learned about it from Sompolinski. And he was working with Daniel. Actually, I might have learned it from Daniel, because Daniel had a paper with Sompolinsky[56]. (I'm not sure when that was.) So, it was around.

PC: Could it have been through Anderson[57], who was also around and who could have been the source?

DH: [0:25:30] Anderson was checking out of Bell Labs then. He was sort of moving. Daniel had been there a few more years; he went there a few years before I did. I guess him and Phil used to talk a lot, but by the time I go there I never really talked with Phil. (When I came to Princeton, I used to talk with him all the time.) He was around but I hardly interacted with him at Bell Labs. I think he may have already moved down here, and so he wasn't coming to Bell Labs very often. Maybe he was just focused on other things. He may not have thought I was a person worth spending his valuable time at Bell Labs talking with. I don't know. It actually surprises me how little interaction I had with Phil. Eventually, spin glasses became such a big activity at Bell Labs both experimentally and theoretically. [Yet] Phil just didn't seem to be particularly involved.

PC: How influential was the numerical work of Andy Ogielsky in you pursuing ideas of spin glasses? Is this what ticked you first?

DH: [0:27:05] I think that he didn't really get results till afterwards. It was really about interfaces in random media. I did this thing with Chris Henley, and then Daniel was doing spin charge density waves[58], and then there was

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[54] C. L. Henley and H. Sompolinsky and B. I. Halperin, "Spin-resonance frequencies in spin-glasses with random anisotropies," *Phys. Rev. B* **25**, 5849 (1982). https://doi.org/10.1103/PhysRevB.25.5849
[55] Haim Sompolinsky: https://en.wikipedia.org/wiki/Haim_Sompolinsky
[56] D. S. Fisher and H. Sompolinsky, "Scaling in spin-glasses," *Phys. Rev. Lett.* **54**, 1063 (1985). https://doi.org/10.1103/PhysRevLett.54.1063
[57] Philip W. Anderson: https://en.wikipedia.org/wiki/Philip_W._Anderson
[58] See, *e.g.*, D. S. Fisher, "Threshold behavior of charge-density waves pinned by impurities," *Phys. Rev. Lett.* **50**, 1486 (1983). https://doi.org/10.1103/PhysRevLett.50.1486; "Sliding charge-density waves as a dynamic critical phenomenon," *Phys. Rev. B* **31**, 1396 (1985). https://doi.org/10.1103/PhysRevB.31.1396

Imry-Ma and random fields. Then, we did a bunch of papers about various problems [about] interfaces in magnets and whatever[59]. I view it as it came out of that.

PC: We've already talked a bit about how you got started on spin glasses. You mentioned how the idea of low-energy excitations in disordered systems came about, but in your papers, you also mentioned the works of Bray and Moore[60] and of McMillan[61] on domain walls as motivation.

DH: [0:28:19] Definitely McMillan[62]. McMillan was the first one. McMillan actually was building a special computer too[63]. (I don't know if you knew that.) He had some very nice papers about the two-dimensional spin glass and the Nishimori line in that phase diagram. Those were very important.

PC: Were you following that literature?

DH: [0:28:48] When I got into it, certainly Macmillan's papers were something [I read]. Then, he passed away right around that time. He had an accident. He was bicycling and he got hit by a car or something. So, we sort of viewed what we were doing as carrying on. McMillan had actually been at Bell Labs. He had only been at Illinois for a few years. He also spent the first decade or so of his career at Bell Labs. I think we were very much building on what he had done, and of course Bray and Moore were doing roughly the same thing at the same time.

PC: Were you in touch with McMillan and/or Bray and Moore, of was this all through the paper trail that these ideas came through?

DH: [0:29:45] I'm pretty sure by the time I got started on it, McMillan was gone. I don't remember ever interacting [with him]. One or two of those domain

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[59] See, *e.g.*, D. A. Huse and M. E. Fisher, "Commensurate melting, domain walls, and dislocations," *Phys. Rev. B* **29**, 239 (1984). https://doi.org/10.1103/PhysRevB.29.239; D. S. Fisher and D. A. Huse, "Wetting transitions: a functional renormalization-group approach," *Phys. Rev. B* **32**, 247 (1985). https://doi.org/10.1103/PhysRevB.32.247; D. A. Huse and D. S. Fisher, "Residual energies after slow cooling of disordered systems," *Phys. Rev. Lett.* **57**, 2203 (1986). https://doi.org/10.1103/PhysRevLett.57.2203

[60] A. J. Bray and M. A. Moore, "Critical behavior of the three-dimensional Ising spin glass," *Phys. Rev. B* **31**, 631 (1985). https://doi.org/10.1103/PhysRevB.31.631

[61] W. L. McMillan, "Domain-wall renormalization-group study of the three-dimensional random Ising model." *Phys. Rev. B* **30**, 476 (1984). https://doi.org/10.1103/PhysRevB.30.476

[62] William L. McMillan: https://en.wikipedia.org/wiki/William_L._McMillan

[63] See, *e.g.*, W. L. McMillan, "Monte Carlo simulation of the two-dimensional random (±J) Ising model," *Phys. Rev. B* **28**, 5216 (1983). https://doi.org/10.1103/PhysRevB.28.5216; P. W. Anderson, "William L. McMillan," *Biographical Memoirs* **81**, 198-213 (2002). https://doi.org/10.17226/10470

wall papers were posthumous[64]; the student who was working with him—or something—had it together and published it after he died. (I'm sure you can find out.) I had met McMillan much earlier when I was a student. I had been out to Illinois once as a grad student. But no: we were very much aware of the work, but we didn't have any interactions with him.

PC: Already in your first PRL[65], you pointed out that you found that the "behavior was very different from the infinite-range model." How familiar were you with the mean-field or infinite-range description at that point? How did you become acquainted with this material?

DH: [0:31:00] We knew about it. One of the questions we were asking ourselves is: "Can we come up with some rather concrete phenomenology which could instantiate that in a short-range model?" We were asking ourselves: "Can we come up with such a thing?" And we couldn't. In some sense, nobody really ever has, as far as I know, in terms of something really concrete. It can't be ruled out as a possibility, and people are trying to hem in what the possibilities are, but there's no real concrete description of how that would work. We were very much aware of that and did think about whether we could somehow tweak what we were doing to include something like that, but we couldn't see any way to make that work. Those ideas had been around for more than five [years]. The first Parisi[66] paper was '79, right? So, those ideas had definitely been around, and we were well aware of them.

PC: Was it through reading the literature? Or had you been in touch with people who were from that community? Or was there a particular set of lecture notes circulating that you found useful? How did they make it to your consciousness?

DH: [0:32:37] My guess is it went to Sompolinsky, and then Sompolinsky to Fisher, and Fisher to me. I've never been a good reader. There probably were seminars and journal clubs. We used to have this journal club where people would go over papers. It could have been something like that. But I was never much of a reader. I still am not much of a reader. I get

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[64] W. L. McMillan, "Domain-wall renormalization-group study of the three-dimensional random Ising model at finite temperature," *Phys. Rev. B* **31**, 340 (1985). https://doi.org/10.1103/PhysRevB.31.340; "Domain-wall renormalization-group study of the random Heisenberg model," *Phys. Rev. B* **31**, 342 (1985). https://doi.org/10.1103/PhysRevB.31.342

[65] 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

[66] 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

information mostly from talking with people, going to seminars. It's only once I really know something that I have the patience to actually read the papers. Because once you know what you're doing, then reading the papers can be a pleasure. But when you don't know what it's about, it's hard work.

PC: How was your first work on this topic received?

DH: [0:33:41] I would say well received.

PC: Did you give lots of seminars? Were you in correspondence with people?

DH: [0:33:49] Yeah. Certainly, we'd be giving talks. It was the main thing I was giving talks about until vortex glass[67]. So, between that point and when we started really doing statistical mechanics of vortices in high-temperature superconductors, that was my main thing I would give talks about.

PC: Can you tell us a bit about what the talk circuit was? Was it just going to Rutgers and give talks there? Or were you traveling nationally and internationally to give seminars on this topic? In other words, what sort of attention was it receiving?

DH: [0:34:45] I remember a colloquium at Stanford and probably a few other places. I would be surprised if there wasn't a March Meeting[68] invited talk, because that was a big thing at Bell Labs, nominating each other for March Meeting invited talks. It was considered something you should do, so I'm sure that happened, but I don't really remember. In the summer, there were Gordon conferences. There was a Condensed Matter Gordon conference[69], and I'm pretty certain this would have been a topic in those days, when a lot of people would be there. There'd be a session about spin glasses, because there was a lot of experimental activity at the same time.

PC: Were you still in touch with your PhD advisor? Do you know how Professor Fisher reacted to these ideas?

DH: [0:35:59] I think he was positive. I'm trying to remember. I don't remember anything strong, but he was always very supportive and positive of

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[67] See, *e.g.*, D. A. Huse and H. S. Seung, "Possible vortex-glass transition in a model random superconductor," *Phys. Rev. B* **42**, 1059 (1990). https://doi.org/10.1103/PhysRevB.42.1059

[68] American Physical Society March Meeting.

[69] *Gordon Research Conference: Condensed Matter Physics – Disordered Systems*, Daniel S. Fisher and Robert J. Birgeneau, Brewster Academy, Wolfeboro, NH, USA, June 16-20, 1986. See, *e.g.*, A. M. Cruickshank, "Gordon research conferences," *Science* **231**, 1163-1199 (1986). https://www.jstor.org/stable/1696806

everything I did. He could be aggressive sometimes, but he never sent that in my direction.

PC: These ideas were developed over a series of papers[70]. Can you walk us through how the program developed? How did these ideas become refined? Or in response to what were they pushed in one direction or another?

DH: [0:36:53] Are you talking particularly about my work?

PC: Yes, your work on finite-dimensional spin glasses. Was it in response to experimental results? Or were there computational results? Or were you getting pushback from theorists?

DH: [0:37:10] Certainly, the computational results were very important. We could do one dimension. You just do it; it's exactly solvable, so that we did, of course. When we first started, the two-dimensional numerical work was reasonably good, but still primitive compared to what it is now. So, people started really pushing on that. Part of that was my thing with Ingo Morgenstern. But people kept pushing on that, and in fact my most recent work on spin glasses was with Alan Middleton[71] just 12 or so years ago, where we really made some more progress on the two-dimensional case[72]. We actually figured out very much because of the really powerful numerics he was able to do with his student, Creighton Thomas[73], how the droplet theory for the $\pm J$ square lattice Ising model works. It's actually much more intricate than you would have naively thought. It was the kind of thing, where… It's something Daniel and I could have fantasized about, but we didn't even try to think about that carefully. Back then, Larry Saul and Mehran Kardar did some nice numerics on the $\pm J$ model[74], but they could

[70] See, *e.g.*, D. A. Huse and D. S. Fisher, "Residual energies after slow cooling of disordered systems," *Phys. Rev. Lett.* **57**, 2203 (1986). https://doi.org/10.1103/PhysRevLett.57.2203; D. S. Fisher and D. A. Huse, "Absence of many states in realistic spin glasses," *J. Phys. A* **20**, L1005 (1987). https://doi.org/10.1088/0305-4470/20/15/013; D. S. Fisher and D. A. Huse, "Equilibrium behavior of the spin-glass ordered phase," *Phys. Rev. B* **38**, 386 (1988). https://doi.org/10.1103/PhysRevB.38.386; D. S. Fisher and D. A. Huse, "Nonequilibrium dynamics of spin glasses," *Phys. Rev. B* **38**, 373 (1988). https://doi.org/10.1103/PhysRevB.38.373

[71] A. Alan Middleton: https://en.wikipedia.org/wiki/A._Alan_Middleton

[72] C. K. Thomas, D. A. Huse and A. A. Middleton, "Zero-and low-temperature behavior of the two-dimensional $\pm J$ ising spin glass," *Phys. Rev. Lett.* **107**, 047203 (2011). https://doi.org/10.1103/PhysRevLett.107.047203

[73] Creighton Kays Thomas, *Optimization and exact sampling algorithms for simulations of glassy materials*, PhD Thesis, Syracuse University (2009). https://go.exlibris.link/wlFKyVwx

[74] L. Saul and M. Kardar, "Exact integer algorithm for the two-dimensional $\pm J$ Ising spin glass," *Phys. Rev. E* **48**, R3221 (1993). https://doi.org/10.1103/PhysRevE.48.R3221. See, *e.g.*, P. Charbonneau, *History of RSB Interview: Mehran Kardar*, transcript of an oral history conducted 2023 by Patrick Charbonneau and

only do up to a certain size, and if you don't do a big enough size, it's consistent with a zero-temperature critical point. So, for 20 years, the conjecture was that the $\pm J$ square lattice Ising spin glass has zero temperature critical point, like a lot of other frustrated Ising models do. It was very comfortable, because there were all these exact solutions, which also had zero-temperature critical points. Basically, what happened is that Alan Middleton was pushing on that with numerics, going up to 1000 x 1000 and it just wasn't holding together. So, he got in touch with me in 2010 or something, and we worked that out. That's the numerics in 2D. First of all, for the continuous distribution in 2D, the original data has really held up. It was just a matter of getting the exponents a little more precisely: what's theta, what's the fractal dimension. But the picture that was already there in McMillan has held up even when people use these modern algorithms to do these enormous systems. That's really held up. Then, the $\pm J$, you had this thing where there was something fishy. Then, there was 10 years, around 2000 to 2010, where people were saying: "Oh something is fishy. What's going on there?" There are a number of papers, and then when Alan did the work, which I was involved in, that seemed to just stop it, because that seemed like that was the answer. Nobody really has followed up on that, which I assume means people accept that answer. So, 2D seems pretty much under control. Although there's probably other cases with $\pm J$ models on different lattices which might behave rather differently, nobody seems to have bothered to check that out.

Then, there was the 3D. Ravin Bhatt and Peter Young were doing serious numerics[75]. There was another bit of spin glass that was going on at Bell Labs. When I first got what became a permanent job, Ravin Bhatt was on sabbatical. He had taken a sabbatical in Paris and then in London with Peter Young, and he was getting into spin glass, which was kind of a new thing for him, as a serious stat. mech. thing. He had been doing other disordered spin system problems for other reasons. (He was away, and I actually sat in his office for while he was away.) He also brought back the spin glass problem from his sabbatical, and they had done some very nice numerics, as were a lot of people. A lot of people were doing it then. Rajiv Singh and Sudip Chakravarthi were doing the high-temperature expansion soon after that to try to see what you could learn there[76]. In some sense, at the critical point, the numerics is probably pretty good. When you get above 2D, the numerics is probably pretty good for doing the critical point

Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2023, 14 p. https://doi.org/10.34847/nkl.cdf05i34

[75] R. N. Bhatt and A. P. Young, "Search for a transition in the three-dimensional $\pm J$ Ising spin-glass." *Phys. Rev. Lett.* **54**, 924 (1985). https://doi.org/10.1103/PhysRevLett.54.924

[76] R. R. P. Singh and S. Chakravarty, "Critical behavior of an Ising spin-glass," *Phys. Rev. Lett.* **57**, 245 (1986). https://doi.org/10.1103/PhysRevLett.57.245

and the high-temperature phase, but doesn't seem to really help very much with resolving any issues in the spin glass phase itself, which is unfortunate.

PC: I'd like to take you back to the 80s—the '85 to '88 period. Was there any theoretical push back on these ideas? Or was this broadly accepted from everyone who heard them?

DH: [0:43:55] It's the same as it is now. The ordered phase of the spin glass, it's not clear what the situation is. There are people who like the infinite-range model scenario, and there's people who like the more low-dimension viewpoint of it—droplet theory—or things in between. That's a controversy which started then and still goes today.

PC: Were you in touch with the groups in Rome and in Paris, or was this with other people in the US? Who was this controversy with, in these early days?

DH: [0:44:48] For me, it's mostly in the papers. I don't remember ever being at a conference and having heated arguments or things like that. Probably that happened. Daniel is much more emotional about these things than I am, so it probably happens sometimes when he's there.

PC: So, was there a back-and-forth in the literature already in these early years?

DH: [0:45:41] Yeah, I think so. Although in some sense, some of the work done purporting to support infinite-range model behavior in short-range models tends to be written completely ignoring our point of view. In that sense, it's not a back-and-forth. A lot of times they have some data that's perfectly consistent with both scenarios, but then they'll just say: "It looks like the infinite-range model". They won't even talk about the other scenario. In that sense, sometimes there isn't the back-and-forth there really should be.

PC: My impression, and maybe I'm wrong, is that most of the literature about the numerical work looking at finite-range systems through infinite-range lens came in the '90s and 2000s[77]. But maybe I missed something. Were

[77] See, *e.g.*, S. Caracciolo, G. Parisi, S. Patarnello and N. Sourlas, "3d Ising spin-glasses in a magnetic field and mean-field theory," *Europhys. Lett.* **11**, 783 (1990). http://doi.org/10.1209/0295-5075/11/8/015; E. Marinari, G. Parisi, F. Ricci-Tersenghi and J. J. Ruiz-Lorenzo, "Violation of the fluctuation-dissipation theorem in finite-dimensional spin glasses," *J. Phys. A* **31**, 2611 (1998). https://doi.org/10.1088/0305-4470/31/11/011; E. Marinari, G. Parisi, F. Ricci-Tersenghi, J. J. Ruiz-Lorenzo and F Zuliani, "Replica

there a specific back-and-forth already between ‘85 and ‘88 in that respect?

DH: [0:46:58] There were a lot of different people doing numerics and interpreting them [in] a lot of different ways. I don't know. It really was a very active subject with a lot of numerical work of very mixed quality being published. I would be amazed if there wasn't already some papers being published with simulations of short-range models being interpreted in terms of infinite-range model in the ‘80s[78]. But I don't know that, because it wouldn't have been high-quality numerical work, so I wouldn't have really remembered it. Most of the numerical work on spin glasses is just not useful.

PC: So, you don't remember anything in particular from ‘85 to ‘88 of either numerics or theory or experiments that would be in direct debate. Would it then be fair to describe this core sequence of paper was you pursuing your own program without being challenged?

DH: [0:48:22] I think so. There was this other thing, which is the non-equilibrium stuff and the chaos. That was another thing we were doing, thinking about the non-equilibrium dynamics and the pinning of the walls, which you can think about in random bond, random field, spin glass and all the time dynamics. Once you start thinking about the spin glass problem from that point of view, you get this chaos both under changing the random bonds and also chaos under changing the temperature, because the free energy of the domain walls is an almost cancellation of a much bigger energy and entropy. That all was also worked out at that time. That aspect of it—the temperature chaos and the disorder chaos—you get that in all models of spin glasses, so that doesn't discriminate between the models. That's actually a subject where there was a lot of nice experiments, the aging phenomena. That was probably the part of it that produced immediate activity right afterwards, was bringing in that aspect of it and then realizing that you know most spin glass experimental data, unless you're working around the critical point or above the critical point,

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symmetry breaking in short-range spin glasses: Theoretical foundations and numerical evidences,” *J. Stat. Phys.* **98**, 973-1074 (2000). https://doi.org/10.1023/A:1018607809852

[78] J. D. Reger, R. N. Bhatt and A. P. Young, “Monte Carlo study of the order-parameter distribution in the four-dimensional Ising spin glass,” *Phys. Rev. Lett.* **64**, 1859 (1990). https://doi.org/10.1103/PhysRevLett.64.1859 **DH:** This work was submitted in the very late ‘80s. People were also aware that the presence or absence of a de Almeida-Thouless line for the Ising spin glass was an important indicator; I’m not sure when numerical attempts at that started. I had a paper about that with Rajiv Singh in 1991, but I doubt we were the first. R. R. Singh and D. A. Huse, “Contours of constant χ_{SG} in the H-T plane: Mean-field versus droplet theories of Ising spin glasses,” *J. Appl. Phys.* **69**, 5225-5227 (1991). https://doi.org/10.1063/1.348086

you should just be always thinking about it as out of equilibrium and really engaging with it being out of equilibrium and studying all these aging and chaos effects. In late ‘80s, early ‘90s, I think that was the main activity and not so much the issue of: “Is it droplets or is it infinite range?”, because there was a lot of nice physics to get out, which was sort of independent of that.

PC: In your papers, you often describe the infinite-range approach as being the results of the *Parisi* ansatz, the *Parisi* solution, the *Parisi* picture. You very much tie this representation with Parisi. Were you at all in touch with him about these matters? Was there correspondence?

DH: [0:51:02] No. At least, not me.

PC: Why did you associate it so strongly with him then?

DH: [0:51:12] Why wouldn't we? It's his work!

PC: Sure, but there are other papers. There were the early works in ‘79 and ’80, and then there was a collaboration with other authors in ‘84[79]. In your minds was it completely his?

DH: [0:51:36] It was him by himself that got it started. There's no ambiguity about that. It doesn't seem that way to me. It's his thing and then other people joined in. It was maybe a shorthand, but I don't think it was inappropriate.

PC: In the early ‘90s, you have a paper where you mention the “many states versus two states controversy”[80]. What was the controversy by then? As we have sort of figured out, there was nothing really happening between ‘85 and ‘88 in terms of controversy, so what changed maybe between ‘88 and ’91, so that you identified one?

DH: [0:52:23] I think we were sort of trying to make these things more precisely defined, which is something other people have followed up on, certainly

[79] M. Mézard, G. Parisi, N. Sourlas, G. Toulouse and M. Virasoro, "Nature of the spin-glass phase," *Phys. Rev. Lett.* **52**, 1156 (1984). https://doi.org/10.1103/PhysRevLett.52.1156; "Replica symmetry breaking and the nature of the spin glass phase," *J. Physique* **45**, 843-854 (1984).

[80] D. A. Huse, "Monte Carlo simulation study of domain growth in an Ising spin glass," *Phys. Rev. B* **43**, 8673 (1991). https://doi.org/10.1103/PhysRevB.43.8673

Newman and Stein[81] and Read[82]. At that time, it was really just these two scenarios. Now, we have some ones that are more intermediate. We were just trying to make them as well-defined as we knew how to make them—those distinctions—and say what we could say. There certainly was this big gap. We had this low-dimensional picture, short-range model picture, which clearly applied well in 2D, and then we have this infinite-range model, which is infinite-range. You might think of it as infinite-dimensional, though that wasn't entirely clear. That's an awful big gap between 2D short-range and infinite-range. The question is: "What do you do in between?" [We were] trying to make it precisely defined and outline what the possibilities were.

PC: I think I understand this. What I'm trying to get at is: "Was this playing out at meetings, where people were having discussions about this controversy that all the community knew? Was it just taking place in personal correspondence? It's not obvious in the paper trail where the controversy was brewing.

DH: [0:54:13] There were two papers that Daniel and I wrote along those lines at that time[83]. As far as I can see, that was really just Daniel trying to make these things precise as he could at the time and get it written down. I don't know to what extent anybody else was paying a lot of attention to it at that time. I think Daniel appreciated… There was the random field Ising model problem, which had a controversy also. Of a different sort, but somewhat similar flavor.

PC: I understand that actually did take place in public and had people as witness.

DH: [0:55:27] Yeah. That was very different. It was very different in that it was much more aggressive and in public. But that one, John Imbrie, the mathematical physicists, cleaned it up right and resolved it[84]. If the spin glass ever gets resolved, that's probably how it's going to be resolved and I think Daniel had been working very heavily in the random field Ising problem. I think the idea was to try to get that process started of having

[81] 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
[82] Nicholas Read: https://en.wikipedia.org/wiki/Nicholas_Read
[83] D. A. Huse and D. S. Fisher, "Pure states in spin glasses," *J. Phys. A* **20**, L997-1003 (1987). https://doi.org/10.1088/0305-4470/20/15/012 ; D. S. Fisher and D. A. Huse, "Absence of many states in realistic spin glasses," *J. Phys. A* **20**, L1005-10 (1987). https://doi.org/10.1088/0305-4470/20/15/013
[84] J. Z. Imbrie, "Lower critical dimension of the random-field Ising model," *Phys. Rev. Lett.* **53**, 1747 (1984). https://doi.org/10.1103/PhysRevLett.53.1747

serious mathematical physicists look at this problem and see if they can start untying some of these knots. I think that's why Daniel… Daniel was at that time much more motivated in those papers than I was. Of course, I was a co-author, but it was sort of his thing to get those done and out there. That's what I think was the purpose there, to try to begin that process of mathematical physics trying to come in here and make some progress. It's happened slowly over the years since then. Although not enough to resolve the issue, certainly [enough] to constrain the possible solutions. I'm pretty sure Newman and Stein were very much following on those works. I don't know how many other people are, but I certainly I talk occasionally to Dan Stein, in particular, and he definitely was influenced by those papers.

PC: I'll try to rephrase this. Please correct me if I'm wrong. Essentially, it was presented in these words in part to try to entice, or to hook another community to pick up these problems, not necessarily because there was really this burning controversy à la RFIM. That said, it could also have been taking place more quietly through referee reports.

DH: [0:58:03] We never had trouble publishing. I think it was a clear disagreement of two scenarios, which are clearly incompatible. So, this is interesting, and a lot of people were aware of it. Anybody working in spin glasses was aware of it, but the thing is you could do a lot of nice stuff in spin glasses without making any progress on that right because the critical point was interesting and the non-equilibrium dynamics was very interesting. So, not many people were really focused on trying to resolve this issue. I'm probably describing it very much with hindsight, because now it's pretty clear that if this is ever going to get resolved it's going to probably be more from mathematical physics than from numerics or the kind of theory that's been done so far.

PC: Was it plausibly already on your mind by that point? That's what you were presenting.

DH: [0:59:23] Yeah, and I think more so on Daniel's mind. He was working with mathematical physicists much [more]. He had this nice paper with the Chayes and Spencer about those inequalities[85]. So, he really knew the mathematical physics community, [and he] knew what their strengths and possibilities were a lot more than I did.

[85] 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; "Correlation length bounds for disordered Ising ferromagnets," *Comm. Math. Phys.* **120**, 501-523 (1989). https://doi.org/10.1007/BF01225510

PC: As you were mentioning a bit earlier, and shortly afterwards even in the paper trail, you started to disengage from the spin glass problem around the early '90s. Were you satisfied with what you had accomplished? Did you feel that you had gone around the problem? Otherwise, what drew you away at that point?

DH: [1:00:14] It's a hard problem. It wasn't clear what to do. Of course, I got drawn away by interesting things. High-T_c came along[86], and it was related to quantum magnetism. So, I started working a lot in quantum magnetism, with typically no disorder—frustrated or whatever—quantum fluctuation[87]. I started doing that kind of stuff. Then, we were doing the statistical mechanics of vortices in the high-temperature superconductors[88]. That's a different aspect. The quantum magnetism was inspired by high temperature superconductors, but it had a life of its own. It was really just quantum magnetism, which was a subject which needed working on, and high-T_c got it started again. I did work in that area for a long time after that. Then, we started doing all this vortex dynamic stuff, and there was a strong experimental program at Bell Labs doing that[89]. So, I was spending a lot of time doing that.

Then, there was a vortex glass, so there was a real connection to spin glasses. The vortex glass is a type of XY-spin glass sort of. So, there was a strong connection to spin glasses there, and that work was very directly inspired by thinking a lot about spin glasses. Those are two big subjects. We were able to really do some things there, whereas the spin glass problem most of the time when you try to do something you get nowhere. It's kind of…

I remember one day Chandra Varma[90], who was my colleague in the theory department, came. I was working on quantum magnetism and I had worked on spin glasses and he came to me one day and he said: "How come you don't do quantum spin glasses?" I remember that very vividly. I was thinking: "Yeah, that's a good idea." So, I started working on quantum

[86] High-temperature superconductivity: https://en.wikipedia.org/wiki/High-temperature_superconductivity

[87] See, *e.g.*, D. A. Huse, "Ground-state staggered magnetization of two-dimensional quantum Heisenberg antiferromagnets," *Phys. Rev. B* **37**, 2380 (1988). https://doi.org/10.1103/PhysRevB.37.2380

[88] See, *e.g.*, Ref. 63.

[89] See, *e.g.*, H. Safar, P. L. Gammel, D. A. Huse, D. J. Bishop, J. P. Rice and D. M. Ginsberg, "Experimental evidence for a first-order vortex-lattice-melting transition in untwinned, single crystal $YBa_2Cu_3O_7$," *Phys. Rev. Lett*. **69**, 824 (1992). https://doi.org/10.1103/PhysRevLett.69.824; D. J. Bishop, P. L. Gammel, D. A. Huse and C. A. Murray, "Magnetic flux-line lattices and vortices in the copper oxide superconductors," *Science* **255**, 165-172 (1992). https://doi.org/10.1126/science.255.5041.165

[90] Chandra M. Varma: https://de.wikipedia.org/wiki/Chandra_M._Varma

spin glasses for a while[91], which eventually led… Then, Daniel was doing these random spin chains and these strong disorder renormalization group and the float infinite randomness[92]. That's actually pretty related to the spin glass.

In some sense, a lot of the problems I've worked on since then, they're not spin glasses directly but if I hadn't worked on spin glasses I probably wouldn't have been able to [do so], like many-body localization[93]. If I had never worked on spin glasses, I wouldn't be able to work on the problem that way. It gives you a sort of a perspective on things that applies to a lot of problems, even though it's not explicitly doing spin glasses. It sort of gives you a way of thinking about things that has a wide application. I would say I didn't really totally move away from it. I worked on related problems that are [ones I] could make progress on.

Students always come to me and say: “Oh, I want to work on spin glass.” I always discourage them[94]. I say: “It's a horrible thing to work on, because so many people worked on that for so long and made no progress.” When Alan Middleton came to me, and we did this project where we really made [some progress], I was like: “Wow, I made progress on a spin glass problem that is really a spin glass problem for the first time in like 15 years.” I haven't since then.

PC: Did you ever teach about spin glasses and/or replica symmetry breaking at Princeton or elsewhere?

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[91] See, *e.g.*, M. Guo, R. N. Bhatt and D. A. Huse, "Quantum critical behavior of a three-dimensional Ising spin glass in a transverse magnetic field," *Phys. Rev. Lett.* **72**, 4137 (1994). https://doi.org/10.1103/PhysRevLett.72.4137; M. Guo, R. N. Bhatt and D. A. Huse, "Quantum critical behavior of a three-dimensional Ising spin glass in a transverse magnetic field," *Phys. Rev. Lett.* **72**, 4137 (1994). https://doi.org/10.1103/PhysRevLett.72.4137

[92] See, *e.g.*, D. S. Fisher, “Random transverse field Ising spin chains,” *Phys. Rev. Lett.* **69**, 534 (1992). https://doi.org/10.1103/PhysRevLett.69.534; D. S. Fisher, “Random antiferromagnetic quantum spin chains,” *Phys. Rev. B* **50**, 3799 (1994). https://doi.org/10.1103/PhysRevB.50.3799; D. S. Fisher, “Critical behavior of random transverse-field Ising spin chains,” *Phys. Rev. B* **51**, 6411 (1995). https://doi.org/10.1103/PhysRevB.51.6411; O. Motrunich, S. C. Mau, D. A. Huse and D. S. Fisher, “Infinite-randomness quantum Ising critical fixed points,” *Phys. Rev. B* **61**, 1160 (2000). https://doi.org/10.1103/PhysRevB.61.1160

[93] See, *e.g.*, A. Pal and D. A. Huse, “Many-body localization phase transition,” *Phys. Rev. B* **82**, 174411 (2010). https://doi.org/10.1103/PhysRevB.82.174411; R. Nandkishore and D. A. Huse, “Many-body localization and thermalization in quantum statistical mechanics,” *Annu. Rev. Condens. Matter Phys.* **6**, 15-38 (2015). https://doi.org/10.1146/annurev-conmatphys-031214-014726

[94] **DH:** I think beginning students should work on research where they have a good chance of making some progress. Spin glass has generally not been like that (with occasional exceptions). Now, I am applying the same to many-body localization, which has reached a similar status (lots of work with very little if any progress).

DH: [1:04:34] No.

PC: Even though these ideas you found helpful and carryover a very different range of topics, is it still not something that you find pedagogically useful? Or have you simply never taught a class where that would have been relevant material?

DH: [1:04:52] I never taught a special topics course on advanced stat mech kind of stuff. I've never taught that. When I teach those courses, I tend to do things that are more recent interests of mine, so they tend to be much more quantum stat mech emphasized stuff. So, no, it never happened.

I remember once before I came to Princeton, but not much before, Phil was teaching a course and he called me up and said he wanted me to come and give a lecture. I'm not positive, but I believe I did actually give a lecture about it at Princeton—before I came to Princeton—in Phil's course. I remember coming down from Bell Labs and giving a lecture. I'm pretty sure it was about spin glasses, but I'm not absolutely positive.

PC: Would this have been an advanced stat mech class?

DH: [1:06:14] It would have been some special topics advanced class that Phil was nominally teaching, but he was actually just bringing in guest lecturers. Maybe he was lecturing. I really don't know.

PC: Did he pay much attention to your work on spin glasses by then?

DH: [1:06:37] I think he paid attention but didn't really engage.

PC: You were mentioning conversations you had later on with him. Was it ever on spin glasses?

DH: [1:06:63] No, it wasn't. I've been working on this localization stuff for 15 years. Phil was around and well aware of this for the first ten years of it. He never seemed interested. We were following up directly on his original paper[95], but he was interested in high-temperature superconductivity. This is during the time while I was here. Then, he got very interested in this supersolid business[96], when that was going on. Actually, the one time I

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[95] P. W. Anderson, "Localized magnetic states in metals," *Phys. Rev.* **124**, 41 (1961). https://doi.org/10.1103/PhysRev.124.41

[96] See, *e.g.*, P. W. Anderson, W. F. Brinkman and D. A. Huse, "Thermodynamics of an incommensurate quantum crystal," *Science* **310**, 1164-1166 (2005). https://doi.org/10.1126/science.1118625; P. W. Anderson, "Bose Fluids Above T_c: Incompressible Vortex Fluids and "Supersolidity"," *Phys. Rev. Lett.* **100**,

wrote a paper with him was about supersolids, and he kept that interest up for a long time after the rest of the community moved on. We used to argue about that all the time. I think the problem is… Phil was an interesting person. If he didn't disagree with me about something, he didn't really want to talk to me about it. He liked topics where I would disagree with him about, because he liked that. Eventually, we got this real disagreement going about the supersolid problem, so he just loved to come in and talk to me about [that], whereas on the other things, spin glasses and localization, we didn't disagree. So, there was no point in us talking about it from his point of view. That's just the impression I had.

PC: From what we can tell, it seems like very few US-based physicists used ideas of replicas symmetry breaking in the ‘80s and ‘90s. Do you have any perspective as to why that might be?

DH: [1:09:01] There were some people. Goldschmidt was doing it seriously[97]. Kardar got some nice results from it. Not necessarily for spin glasses, but for other problems. He was very proficient. But you're right. There was sort of the British-American approach and then the continental approach. I guess the Germans were maybe more in the British-American camp, although that's not clear.

PC: Was this really a cultural issue? Was this a pedagogical issue? Do you have any opinion or perspective?

DH: [1:10:12] There is this real affection for nice mathematics in the training for it that you get in Paris that you don't get in the United States, and perhaps also not in the United Kingdom. There is a certain culture of real strong mathematical background and an affection for those kinds of things, which I do associate more with Paris.

PC: You were earlier mentioning the exact results of Baxter, for instance. Granted, he’s not an Anglo-American scientist; he was an Australian. Is this also part of the cultural difference?

215301 (2008). https://doi.org/10.1103/PhysRevLett.100.215301; "A Gross-Pitaevskii treatment for supersolid helium," *Science* **324**, 631-632 (2009). https://doi.org/10.1126/science.1169456

[97] See, *e.g.*, Y. Y. Goldschmidt and P. Y. Lai, “Ising spin glass in a transverse field: Replica-symmetry-breaking solution,” *Phys. Rev. Lett.* **64**, 2467 (1990). https://doi.org/10.1103/PhysRevLett.64.2467; C. De Dominicis and Y. Y. Goldschmidt, “Replica symmetry breaking in finite connectivity systems: a large connectivity expansion at finite and zero temperature,” *J. Phys. A* **22**, L775 (1989). https://doi.org/10.1088/0305-4470/22/16/003

DH: [1:11:11] Interesting. You know, Bernard Derrida[98] does beautiful exact results, very creative. I would say what he does is more on the side of what I'm calling the Anglo-American thing, rather than the Mézard-Parisi thing. There's an example of someone very important in Paris, who is really more exact results as opposed to field theory and creative. Who else is doing exact results? Of course, there was a strong culture of that then in Holland. Elliott Lieb[99]. In Japan, [there was] Michio Jimbo[100]. There was this very strong group in Japan doing it back in those days. Also, Russia. In some sense, all the conformal field theory and stuff, that's more in the spirit of… Well, I don't know. I guess the conformal field Theory stuff is in both camps.

PC: We're nearing the close of this interview. Is there anything else you would like to share with us about that era that we may have missed or overlooked?

DH: [1:12:58] We didn't talk about the experimental activities.

PC: Were you following those closely?

DH: [1:13:06] Yeah. At Bell Labs, there was this beautiful experiment that Laurent Lévy and Hélène Bouchiat did [on] the nonlinear susceptibility done with AC methods[101]. That was going on.

PC: Were you discussing with them?

DH: [1:13:33] Yeah, but they knew what to do. The framework of what needed doing had been set out, because they weren't trying to learn anything about the ordered phase. They were trying to learn about the critical point. That the nonlinear susceptibility was the thing to look at. I think Suzuki was

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

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

[100] Michio Jimbo: https://en.wikipedia.org/wiki/Michio_Jimbo; Tetsuji Miwa: https://en.wikipedia.org/wiki/Tetsuji_Miwa; Mikio Sato: https://en.wikipedia.org/wiki/Mikio_Sato

[101] See, *e.g.*, L. P. Lévy and A. T. Ogielski, "Nonlinear dynamic susceptibilities at the spin-glass transition of Ag: Mn," *Phys. Rev. Lett.* **57**, 3288 (1986). https://doi.org/10.1103/PhysRevLett.57.3288; L. P. Lévy, "Critical dynamics of metallic spin glasses," *Phys. Rev. B* **38**, 4963(1988). https://doi.org/10.1103/PhysRevB.38.4963 ; L.P. Lévy, G. Dolan, J. Dunsmuir and H.Bouchiat, "Magnetization of mesoscopic copper rings: Evidence for persistent currents," *Phys. Rev. Lett.* **64**, 2074 (1990). https://doi.org/10.1103/PhysRevLett.64.2074 **PC:** Bouchiat is also acknowledged is the second article. **DH:** I was remembering incorrectly. Bouchiat had done spin glass nonlinear susceptibility in France, while Lévy was also doing it at Bell Labs. When she came to be Lévy's postdoc at Bell Labs, they worked on other things.

the first person to really spell that out clearly. That was well formulated, what needed to be done. Exactly the details of the scaling, yes, we used to talk about that quite a bit.

PC: You were not an author on these papers because these were informal conversations?

DH: [1:14:18] Yeah. You look back at those papers…. I remember talking with them. Whether we were acknowledged, I'm not sure. I'd have to look back and see. There was also another experimentalist named Stan Geschwind[102], who was looking CdMnTe and doing nonlinear susceptibility and also doing AC susceptibility[103]. I ended up working with him quite a bit and being on the papers with him—that was experimental activity—again about the critical point: how to scale that data; what is the best way to process that kind of take and analyze that kind of data to constrain the critical behavior of spin glasses.

Then, there was this business about the remnent magnetization. People were doing experiments where they would look at the remnant magnetization. I was simulating that, and we realized it's actually… If you polarize a spin glass and let it relax to its critical point as the remnant magnetization decay, which is a power law decay, the ferromagnet analog of that is to randomly polarize a ferromagnet and then look at the overlap with the initial state and how it decays as it relaxes to the critical point. I did some simulations of this and some theory of this[104]. We found out there was this new non-equilibrium dynamic critical exponent, which people had [not found] at that time. Then, there was an RG person in Germany who worked out the epsilon expansion for that[105]. That was found. It's called the persistence exponent now. We thought: "Oh! Is this exponent related to some of the exponents that were already known?" But

[102] L. Walker and Y. Yafet, "Stanley Geschwind," *Physics Today* **53**(2), 72–73 (2000). https://doi.org/10.1063/1.882985

[103] See, *e.g.*, S. Geschwind, A. T. Ogielski, G. Devlin, J. Hegarty and P. Bridenbaugh, "Activated dynamic scaling and magnetic ordering in $Cd_{1-x}Mn_xTe$: Spin glass or random antiferromagnet?" *J. Appl. Phys.* **63**, 3291-3296 (1988). https://doi.org/10.1063/1.340815; S. Geschwind, D. A. Huse and G. E. Devlin, "Improved form of static scaling for the nonlinear magnetization in spin glasses," *Phys. Rev. B* **41**, 2650(R) (1990). https://doi.org/10.1103/PhysRevB.41.2650; S. Geschwind, D. A. Huse and G. E. Devlin, "New approach to critical dynamic scaling in random magnets," *Phys. Rev. B* **41**, 4854 (1990). https://doi.org/10.1103/PhysRevB.41.4854

[104] D. A. Huse, "Remanent magnetization decay at the spin-glass critical point: A new dynamic critical exponent for nonequilibrium autocorrelations," *Phys. Rev. B* **40**, 304 (1989). https://doi.org/10.1103/PhysRevB.40.304

[105] H. Rieger, "Nonequilibrium dynamics and aging in the three-dimensional Ising spin-glass model," *J. Phys. A* **26**, L615 (1993). https://doi.org/10.1088/0305-4470/26/15/001

no. It's a new non-equilibrium dynamic critical exponent. That was another bit of theory that I did sometime around 1990.

There was a bunch of activity, it was directly inspired by experiments. A reasonably straightforward experiment is to take a spin glass and if you think you know it's critical point, just look at the remnant magnetization decay at the critical point, which is just a power law. You can fit it over decades and really get good estimates of that. That's another other thing that went on. There's a whole bunch of stuff having to do with non-equilibrium physics, which has become now about all I do now. I hardly ever do equilibrium physics anymore. It's all non-equilibrium to first approximation these days. That was the beginnings of that back then.

PC: As you were just mentioning, you also then did numerical simulations? When did that become part of your toolkit?

DH: [1:17:46] With Henley. I was doing a little bit of it as a grad student, but the paper with Henley… In some sense, I wanted to do that project, but I didn't really know how to do the simulations and Chris did. I got him involved, so he could show me how to do that. I had been doing a little bit of numerics as a grad student, but not very much. I was still doing a little bit of numerics after I came to Princeton, doing some of them myself. Since then, of course, only my students and postdocs are doing them. I'm just supervising and making suggestions.

PC: In the context of spin glasses, your first simulations came, as you said, in around 1990. So, they were not at all involved in developing the droplet picture, or in trying to test or examine in it somehow. Is that correct?

DH: [1:18:59] In terms of me doing them, yes. I did that transfer matrix calculation with Ingo. Of course, he did the actual calculations, but I was very involved. I did a paper with Ogielski about the diluted antiferromagnet in a uniform field[106]. We did this simulation which was very close to what they do in the experiment. That, I was very involved with but of course Andrew did all the simulations himself. But you're right, probably the non-equilibrium stuff was the first time I, myself, was doing spin glass simulations.

PC: In closing, do you still 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?

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[106] A. T. Ogielski and D. A. Huse, "Critical behavior of the three-dimensional dilute Ising antiferromagnet in a field," *Phys. Rev. Lett.* **56**, 1298 (1986). https://doi.org/10.1103/PhysRevLett.56.1298

DH: [1:20:07] I noticed in looking at the transcripts that you asked a lot of people this stuff. I actually looked. I moved many times and my office got renovated. It turns out the only thing I have is the transparencies. It looks like I kept the transparencies from every talk I gave, but that's all I have. What will happen to those, I don't know.

PC: Including of talks about the spin glasses? Including these talks that you were giving around the US?

DH: [1:20:39] I assumed I have. It looks like I [do]. I've got this great big box, which it looks like it's transparencies from every talk I ever gave.

PC: If ever you get around scanning one or two of them, we would certainly appreciate getting a look. That said, thank you very much for your time it's been a real pleasure and discussing.

DH: [1:21:03] Maybe, I'll take a look at them and see if there's some from particularly the era you're focused on. Maybe there's ones that would be of interest to you, now that I see the focus of the particular time you want to ask about. I'll see if I have those.

PC: Thank you!