History of RSB Interview:
Bertrand I. Halperin

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PC: Good morning, Professor Halperin. Thank you very much for joining us. As we just discussed, the topic of this interview is the ideas surrounding replica symmetry breaking in spin glasses, from roughly 1975 to 1995. But before we get to that I'd like to ask a couple questions on background and on things that led up to do these advances. In particular, in a recent oral history you did with the American Institute of Physics[1], you mentioned that you did not interact much with Phil Anderson[2] while you were at Bell Labs. It's true that you only had one article together[3], but it's been quite an influential one. Can you tell us what led to the genesis of that work?

BH: [0:00:51] I don't recall saying that I did not interact with him. I interacted with him quite a bit, but it was discussions. I didn't collaborate with him except on this one paper, but we certainly did interact. He was influential in my general understanding of physics, for sure.

You asked how we came to this particular question. Spin glasses were an afterthought. He, of course, had been working on spin glasses and thinking about spin glasses before this paper. The paper was motivated not by spin glasses at all, but by structural glasses. There were experiments by Bobby

[1] "I think I wrote only one paper with Anderson. I didn't interact with him that closely, but he certainly set a standard and I certainly spoke with him about things and learned from him." From: Interview of Bertrand Halperin by David Zierler on April 21, 2020, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, www.aip.org/history-programs/niels-bohr-library/oral-histories/44555. See also B. I. Halperin, "A Career in Physics," *Annu. Rev. Cond. Matter Phys.* **12**, 15-28 (2021). https://doi.org/10.1146/annurev-conmatphys-060120-092219

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

[3] P. W. Anderson, B. I. Halperin and C. M. Varma, "Anomalous low-temperature thermal properties of glasses and spin glasses," *Philo. Mag.* **25**, 1-9 (1972). https://doi.org/10.1080/14786437208229210

Pohl[4] which showed anomalies[5], and other experiments as well in both glasses and other disordered systems at low temperatures, particularly in the excess specific heat at low temperatures and also anomalies in heat transport, which we tried to explain by introducing this idea… Not introducing the idea because we didn't really originate it, but we explored in some detail what the effect of these two-level systems would be in glasses. The two-level systems that we had in mind were perhaps something where let's say an oxygen atom could sit in two different positions separated by some kind of a barrier, but not such a high barrier that it was absolutely forever separated in one or the other. It could [thus] quantum mechanically tunnel from one side to the other. That's what we called the two-level systems. Of course there would be many other levels, excited levels, but the two levels that would be almost exactly the same energy, with a very small transition rate between them, then that could lead to… The fact that they weren't exactly the same energy meant you could store energy in them in the higher energy state. At low but finite temperatures, the higher energy state could become populated and that could give you a specific heat. The two-level systems could interact with phonons and act as scattering centers that would control heat transport at low temperatures. That's what we were mainly interested in.

Really, Phil pointed out that this could also apply not to structural glasses but to spin glasses that he assumed could exist. He wasn't necessarily concerned about whether they were thermodynamic states, whether they would last forever or just last for a long time. It's just that he felt that there would be glassy-like states of spins, and there could be also tunnelling barriers between one state and another, and that might cause contributions to low-temperature properties there. That was, I would say, put in as an afterthought. I certainly was not motivated in any way by spin glasses, but he included it.

Historically, this paper has been cited quite a bit. It's had a lot of influence, but I would say almost entirely because of its possible relevance to real structural glasses. I don't know that it had any particular influence on spin glasses whatsoever. I've never seen particularly a reference to it in the spin glass context.

[4] See, *e.g.*, Robert O. Pohl papers, 1973-1998. #14-22-3253. Division of Rare and Manuscript Collections, Cornell University Library. https://rmc.library.cornell.edu/EAD/htmldocs/RMA03253.html (Consulted September 25, 2021.)

[5] See, *e.g.*, R. C. Zeller and R. O. Pohl, "Thermal conductivity and specific heat of noncrystalline solids," *Phys. Rev. B* **4**, 2029 (1971). https://doi.org/10.1103/PhysRevB.4.2029

You could say that that was how I learned about spin glasses in the first place, but it's not a contribution that I would particularly associate myself with.

PC: Had you ever heard of spin glasses before?

BH: [0:05:31] Well, you know, I maybe had. I can't remember back whether that was so, but I certainly had never thought about them. I may have heard of them, but they weren't something that occupied my interests. I was interested in a lot of things, and that was not one of the experimental situations that I was particularly focused on. I probably had heard of them, but…

PC: As you mentioned, this paper has been and remains very well cited. But what was the immediate reaction to it? Was there a lot of enthusiasm? Was this a signature work at that time?

BH: [0:06:12] Well, there were other people who had similar ideas. I don't think it was wildly controversial, and some of these ideas had really been—in a qualitative way—suggested by Bobby Pohl. But then there were a lot of attempts to try to see if it was right. And other experiments were done to look at relaxation properties and so forth. I don't remember it being wildly controversial at the time. There was a question of what these two-level systems really were, and that's still a question.

Later, there were arguments that the quantitative description would be modified quite substantially because of interactions between the two-level systems, which we didn't think were very important. I don't think that's been really resolved. Also, it may not be universal. In certain cases, the interactions may be so strong that you really have to concentrate on those random interactions, much more than we would have done. I haven't really kept up on it, but as far as I can see it remains unclear really what's going on at a quantitative level.

PC: Did that work get you to be interested in spin glasses in its aftermath? In particular, were you paying attention to the work that Edwards and Anderson, and Sherrington and Kirkpatrick later did?

BH: [0:08:27] Yes. So I was aware of that. Of course, that was slightly later. Certainly, I was aware of it. I never got that interested in it to really try to work on it until later. There were a couple of papers that I wrote that at least had spin glass in the title, or that were somehow involved in it, but none of it was really related to this phase transition business.

There was a paper I wrote with Wayne Saslow, in 1977, which was called the "Hydrodynamic theory of spin waves in spin glasses and other systems with noncollinear spin orientations"[6]. We were looking at… We were sort of assuming that there were no spin-orbit interactions. We were assuming that there was an overall SU(2) invariance. That is to say that there'd be Heisenberg-like interactions, but you had random positive and negative ones, and that it could form a spin glass.

Our picture of a spin glass… It was at low temperatures. We were looking really at what was happening at relatively low temperatures, but not zero temperature. Finite temperatures, but below the spin glass transition temperature, assuming there was a spin glass transition temperature. If there wasn't a sharp transition temperature, temperatures low enough that for large periods of time you could say this was frozen into some kind of staggered array, with the spins not aligned in any single plane but would be non-coplanar, actually just non-collinear, that's all that was necessary.

Then, you would have something like antiferromagnetic spin waves—Goldstone modes—but instead of two there would be three of them, because now it would be meaningful. For an ordinary antiferromagnet, rotations around the axis of staggered polarization don't do anything to you, so you don't get a new state. There's no broken symmetry associated with it. But here you've broken symmetry about every rotational axis. We pointed out that this would lead to three Goldstone modes. Furthermore, if you go to finite temperatures, these Goldstone modes would have renormalized velocities, and there would be damping at short wavelengths, but long wavelength [modes] should stay undamped. That was another venture into spin glasses.

PC: If I understand correctly, the genesis of this work took place at the Aspen Center for Physics, during the same meeting where Anderson, Thouless and Palmer were formulating their mean-field theory[7]. Is that correct? In any case, what can you tell us about the genesis of that work?

BH: [0:12:00] It's correct that it took place at Aspen. I met Wayne Saslow at Aspen and that's where it got started. And you may be right about… Let's say at that time I was certainly not aware of the collaboration between

[6] B. I. Halperin and W. M. Saslow, "Hydrodynamic theory of spin waves in spin glasses and other systems with noncollinear spin orientations," *Phys. Rev. B* **16**, 2154 (1977). https://doi.org/10.1103/PhysRevB.16.2154

[7] D. J. Thouless, P. W. Anderson and R. G. Palmer, "Solution of'solvable model of a spin glass'," *Philo. Mag.* **35**, 593-601 (1977). See also P. Charbonneau, *History of RSB Interview: Scott Kirkpatrick*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 24 p. https://doi.org/10.34847/nkl.cba615t7

Anderson, Thouless and Palmer, so that might have been true, but we didn't talk about it there. Was their paper also ‘77? Yes. That was also ‘77. It could have been. It never occurred to me, but you’re right that they were more or less simultaneous. But unrelated, really quite unrelated, I think. I seem to remember Anderson being there but we didn't talk to him about spin glasses as I recall. But of course this was quite a few years ago, so I may not remember that exactly. But it’s true that it began in Aspen.

PC: Were you a co-organizer of that meeting or was it a different one that you organized on the theme of disordered materials?

BH: [0:13:30] Let’s see. I organized a Gordon conference on disordered materials, I think in [1972] or something[8]. That was shortly after… I organized a conference on disordered materials with Bobby Pohl, which was a Gordon conference, I think in [1972]. That was following our work. That did certainly… So we were interested in that. And I did play a role in attracting condensed matter physicists in Aspen in 1973, but that was a different meeting. I don't remember having a very active role in this one, but it's possible. Again, it's a long time ago.

PC: One of your PhD students, just a couple of years later, Robert Pelcovitz, worked on spin glasses during an internship at IBM[9]. Were you following his work, and did you encourage him or others to work in or pursue this direction?

BH: [0:14:48] I don’t recall particularly any interaction with Bob about spin glasses. (This was after I had moved to Harvard, of course, and Bob was one of my first students[10].)

My interactions with people who were really interested in spin glasses took place later. Haim Sompolinsky[11], in particular, was a postdoc who worked with me to some extent. I was his sponsor, but he was very independent,

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[8] Gordon Research Conference on Dynamics of Quantum Solids and Fluids, Bertrand I. Halperin, chairman; R. O. Pohl, vice chairman, 17-21 July 1972, Wayland Academy, Beaver Dam, Wisconsin. “Excitations in disordered systems. Electronic states and properties of metallic and semiconducting alloys, liquids and glasses. Vibrational and magnetic excitations in alloys and glasses. P. W. Anderson, F. C. Brown, W. J. L. Buyers, J. S. Faulkner, W. A. Phillips, R. O. Pohl, M. Pollak, and D. Weaire.” See: A. M. Cruickshank, “Gordon Research Conferences,” *Science* **175**, 1145-1163+1165-1166 (1972). https://www.jstor.org/stable/1732827

[9] R. A. Pelcovits, E. Pytte and J. Rudnick, “Spin-Glass and Ferromagnetic Behavior Induced by Random Uniaxial Anisotropy,” *Phys. Rev. Lett.* **40**, 476 (1978). https://doi.org/10.1103/PhysRevLett.40.476

[10] Robert Alan Pelcovits, *Phase Transitions in Two-dimensional Systems and Disordered Magnets*, PhD Thesis, Harvard University (1978). http://id.lib.harvard.edu/alma/990038989480203941/catalog

[11] Haim Sompolinsky: https://en.wikipedia.org/wiki/Haim_Sompolinsky

though[12]. We did write a few papers together, but mostly he worked a lot on spin glasses, and I talked to him a lot.

Annette Zippelius[13] was another postdoc there at the time, who worked with Haim, and they wrote a lot of papers together[14]. They were definitely interested in more fundamental questions about spin glasses, about how you can understand them.

I don't recall them being much involved with the replica formulation that much. They may have been, but the main thing that I talked to them about—and that they worked on—was on trying to understand spin glasses simultaneously, or alternatively, from a dynamic point of view of being frozen into fractured states. What did that really mean in terms of time, putting in some kind of time? (You have to think about what kind of time-dependence you want to put in.) And how that compared with the various thermodynamic points of view. In particular, this was after the Thouless-Anderson-Palmer [work], where they really said: "Look, there has to be some kind of real—at least in the Edwards-Anderson model—a real thermodynamic transition. What happened below that is another matter but there really was something sharp happening. What did it mean in terms of limits of $N \to \infty$, and all of that? On the other hand, Haim was, I think, at the beginning more thinking in terms of dynamics. For me, I thought about dynamics also, which made sense regardless of… But then if you have the states… [What do] the TAP states mean? Are they really long lived? How long lived? They wrote a number of papers connected with those things and I spoke to them about them. They acknowledged some discussions with me, and I made some suggestions, I suppose. So I was interested in it, but I didn't have anything creative to contribute. And I certainly wasn't a co-author on any of those papers.

I think we wrote one paper, as I looked at my [publications], with Haim and Chris Henley[15], who was a student of mine[16], again on dynamic response

[12] See, *e.g.*, H. Sompolinsky, "Staggered-magnetization approach to spin-glasses," *Phys. Rev. B* **23**, 1371 (1981). https://doi.org/10.1103/PhysRevB.23.1371; "Time-dependent order parameters in spin-glasses," *Phys. Rev. Lett.* **47**, 935 (1981). https://doi.org/10.1103/PhysRevLett.47.935

[13] Annette Zippelius: https://en.wikipedia.org/wiki/Annette_Zippelius

[14] See, *e.g.*, H. Sompolinsky and A. Zippelius, "Dynamic theory of the spin-glass phase," *Phys. Rev. Lett.* **47**, 359 (1981). https://doi.org/10.1103/PhysRevLett.47.359; "Relaxational dynamics of the Edwards-Anderson model and the mean-field theory of spin-glasses," *Phys. Rev. B* **25**, 6860 (1982). https://doi.org/10.1103/PhysRevB.25.6860

[15] C. L. Henley, 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

[16] Christopher Lee Henley, *Studies of Vector Spin Glasses at Low Temperatures*, PhD Thesis, Harvard University (1983). http://id.lib.harvard.edu/alma/990010884870203941/catalog See also: Veit Elser and N. David Mermin, "Christopher L. Henley," *Physics Today* (2015). https://doi.org/10.1063/PT.5.6160 and

in spin glass with random spin-orbit anisotropy. I had some interest in it, but again that had nothing to do—that was the dynamics… It was more closely related to the stuff I had done with Saslow than it was [or] would have been with anything having to do with replicas or the [spin glass] phase transition.

It's also true that in the fall 1982 I spent a sabbatical, I was four months in France, partly at École Normale, partly at Saclay. At that time—I don't remember in complete detail—I think Parisi was actually there, or was visiting from time to time. De Dominicis[17] was, I think, already quite interested in spin glasses. I heard lectures on it, but I never really got a good intuitive feel for the replica symmetry breaking stuff. So I never had anything really to contribute to it. They were way ahead of me. They had done all sorts of complicated calculations, and I could see they were doing complicated calculations, but what it really meant, I didn't have much feeling for. It was a subject I was interested in as a spectator. At that time, I got very interested in the quantum hall effect[18], other aspects of disordered systems[19], and so forth. So I didn't…

PC: Getting back to the contributions of Haim Sompolinsky and Annette Zippelius. Was it in the context of your group or the department that spin glasses were being discussed? Or were these ideas or interests that they were themselves bringing from their prior studies? Or would have seminar speakers come through, or reading clubs, or anything else that could have encouraged their interest in this topic?

BH: [0:21:25] I don't think so. Haim brought this. I don't think the interest came from me, I don't think anybody else at Harvard… None of the other faculty members that I can think of were particularly interested in spin glasses. We might have had talks on it by Haim, but the driving force for that, I think, was Haim. Annette, I don't… She worked a lot with David Nelson on two-dimensional phase transitions[20], and that was more I think what she came to Harvard for. I think probably it was Haim that got her interested

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"Harvard PhD Theses in Physics: 1971-2000," Harvard University Department of Physics, https://www.physics.harvard.edu/academics/phds1971-2000 (Consulted September 25, 2021.)

[17] Cirano De Dominicis: https://de.wikipedia.org/wiki/Cyrano_de_Dominicis

[18] See, *e.g.*, B. I. Halperin, "Quantized Hall conductance, current-carrying edge states, and the existence of extended states in a two-dimensional disordered potential," *Phys. Rev. B* **25**, 2185 (1982). https://doi.org/10.1103/PhysRevB.25.2185

[19] See, *e.g.*, A. Weinrib and B. I. Halperin, "Critical phenomena in systems with long-range-correlated quenched disorder." *Phys. Rev. B* **27**, 413 (1983). https://doi.org/10.1103/PhysRevB.27.413

[20] A. Zippelius, B. I. Halperin and D. R. Nelson, "Dynamics of two-dimensional melting," *Phys. Rev. B* **22**, 2514 (1980). https://doi.org/10.1103/PhysRevB.22.2514; A. Zippelius, "Large-distance and long-time properties of two-dimensional solids and hexatic liquid crystals," *Phys. Rev. A* **22**, 732 (1980). https://doi.org/10.1103/PhysRevA.22.732

in spin glass problems. Again, that sort of happened in their offices. It certainly wasn't from me. I claim no credit.

PC: You mentioned your sabbatical in Paris. You had also been a postdoc in Paris, in the '60s, when I think you first met Cirano De Dominicis. Had you kept in touch with the Paris physics group throughout that time? Were you in regular exchanges or visits?

BH: [0:29:59] I'm not sure what the Paris physics group means.

PC: Cirano, for instance, Gérard Toulouse[21], and the LPS[22] group.

BH: [0:23:12] Ok. So it was different… I was there in '65-'66, that was before 1968[23], and a lot of things were different. Cirano, at that time, I believe was at Saclay. He gave a series of lectures on many-body diagrammatic perturbation theory at École Normale when I was there. So yes, I certainly met him then, I got to know him then, and I heard his lectures; they had nothing to do with spin glasses though. They were on many-body electron problems. That's how I got to know him then. I probably saw him at meetings, once or twice, between then and 1982, but I certainly wasn't… And I think he visited Harvard perhaps for a time also. I don't remember. He was close to Paul Martin[24], who was sort of my mentor originally at Harvard. I think my contacts with Cirano were maybe more through Paul, when he came and spent some time. I can't remember that.

Toulouse, I don't even think was on the scene in 1965. I think he's even younger, so I certainly don't remember him from then. But in 1982, then I certainly did interact with Gérard and we wrote a paper together[25]. That was a quantum hall-type paper. But again I don't remember talking to him about spin glasses particularly.

When I was there I spent, I think, two days a week at Saclay, and three days a week at ENS. Brézin[26] was my real host at Saclay, I would drive out with him. He was a really good friend, and we wrote papers together on

[21] Gérard Toulouse: https://en.wikipedia.org/wiki/G%C3%A9rard_Toulouse
[22] Laboratoire de Physique des Solides (LPS): https://en.wikipedia.org/wiki/Laboratoire_de_Physique_des_Solides
[23] May '68: https://en.wikipedia.org/wiki/May_68
[24] Paul C. Martin: https://de.wikipedia.org/wiki/Paul_C._Martin_(Physiker)
[25] R. Rammal, G. Toulouse, M. T. Jaekel and B. I. Halperin, "Quantized Hall conductance and edge states: Two-dimensional strips with a periodic potential," *Phys. Rev. B* **27**, 5142 (1983). https://doi.org/10.1103/PhysRevB.27.5142
[26] Édouard Brézin: https://en.wikipedia.org/wiki/%C3%89douard_Br%C3%A9zin

wetting[27], actually. I certainly spoke to Cirano then. I also spent three days a week at ENS, except I also gave some lectures at the Collège de France. But basically, these were the two places that I was at. As I say, I think Parisi was maybe visiting a lot at one or both of these places. So I was aware of what was going on, but I wasn't contributing.

PC: Your student Chris Henley, whose work you've mentioned before, is probably the only one of your students who has a dedicated thesis to spin glass problems. Can you tell us a bit how you decided to jump in more fully at that point by having a student on the topic?

BH: [0:26:53] Well, gee! Again it was looking at the dynamics. That's a good question. I don't remember exactly what was in Chris's thesis now. I'd have to refresh my memory on that one.

PC: There was one paper with you and Haim, then he had two papers on his own, in which he thanks you profusely for your guidance. That's why I presumed you were involved somehow.

BH: [0:27:37] Do you mind reminding me the themes of the papers?

PC: The work with Haim was "Spin-resonance frequencies in spin-glasses with random anisotropies,"[28] and the two papers on his own were "Computer search for defects in a three-dimensional Heisenberg spin glass" and "Defect concepts for vector spin glasses" [29].

BH: [0:28:05] He was certainly interested in topological defects, and that could well have been partly my influence. At that time, I had recently done stuff on two-dimensional melting with David Nelson and also others[30]. I was interested more generally in defects. David was certainly interested in defects in three-dimensional materials. Defects included, for us, disclinations and dislocations. That certainly could have been part of it. David was also thinking about metallic glasses at the time in terms of

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[27] E. Brézin, B. I. Halperin and S. Leibler, "Critical wetting in three dimensions," *Phys. Rev. Lett.* **50**, 1387 (1983). https://doi.org/10.1103/PhysRevLett.50.1387; "Critical wetting: the domain of validity of mean field theory," *J. Phys.* **44**, 775-783 (1983). https://doi.org/10.1051/jphys:01983004407077500

[28] See Ref. 15.

[29] C. L. Henley, "Computer search for defects in a *D*=3 Heisenberg spin glass," *Ann. Phys.* **156**, 324-367 (1984). https://doi.org/10.1016/0003-4916(84)90037-X; "Defect concepts for vector spin glasses," *Ann. Phys.* **156**, 368-411 (1984). https://doi.org/10.1016/0003-4916(84)90038-1

[30] See, *e.g.*, B. I. Halperin and D. R. Nelson, "Theory of two-dimensional melting," *Phys. Rev. Lett.* **41**, 121 (1978). https://doi.org/10.1103/PhysRevLett.41.121; D. R. Nelson and B. I. Halperin, "Dislocation-mediated melting in two dimensions," *Phys. Rev. B* **19**, 2457 (1979). https://doi.org/10.1103/PhysRevB.19.2457

different kinds of local orders[31]. That would have been around a lot. To what extent I helped him, I don't remember exactly now. I was his adviser, so I assume he had spoken to me about what he was doing and I helped guide him in some of that. But I don't recall specifically addressing these questions. Again, addressing: is there a phase transition? What do replicas mean? There was no replica in that. Again, same period when Haim was around.

PC: In 1985-1986, Harvard physics invited Giorgio Parisi and Gérard Toulouse to give two series of Loeb lectures[32]. I presume you were a key driver behind these invitations. Is that correct? And if yes, what motivated your interest in bringing them at that point?

BH: [0:30:38] Let's see. Yeah. They were friends in some sense. I don't remember… Was it the Loeb lecture they were giving? There's a departmental vote on the Loeb lectures. I was probably on the committee at the time. Whether I was the one who first suggested them, I don't recall. I would have certainly supported them; they'd be in competition with other people from other fields. It could be astrophysics or what not. Of course, at that time it was sort of, as you say, the middle of the period where [spin-glass theory] was the most active. Being considered… I was not myself doing that much work on phase transitions and in critical phenomena at that point, but I certainly was still aware of it. I would have certainly been interested in what are the effects of disorder in general on phase transitions. So I would have been certainly aware of it and interested in it. I think David Nelson[33] would have also been perhaps influential in that. Perhaps Paul Martin also played a role in that particular instance. He was quite familiar with Cirano's work and so forth. Again, it's too far back for me to remember exactly what went on at that point.

PC: You mentioned you following conversations about the nature of the phase transition in the low-temperature phase. In particular, were you following the work of your former student[34], Daniel Fisher[35], in these discussions?

BH: [0:33:15] Yes! I was aware that there was controversy as it were. Although I think the controversy had more to do with what happened with short-

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[31] See, *e.g.*, P. J. Steinhardt, D. R. Nelson and M. Ronchetti, "Icosahedral bond orientational order in supercooled liquids," *Phys. Rev. Lett.* **47**, 1297 (1981). https://doi.org/10.1103/PhysRevLett.47.1297

[32] "Loeb and Lee Lectures Archive: 1953 – 1990," Harvard University Department of Physics, https://www.physics.harvard.edu/loeblee3. (Consulted September 29, 2021.)

[33] David R. Nelson: https://en.wikipedia.org/wiki/David_Robert_Nelson

[34] Daniel Sebastian Fisher, *Fluctuations in Low Dimensional Systems*, PhD Thesis, Harvard University (1979). http://id.lib.harvard.edu/alma/990038383520203941/catalog

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

range interactions than… Some of the questions one could address within the context of the Edwards-Anderson model. What is going on there? There wasn't a question of whether there's a phase transition [in an infinite-dimensional system], but the question of whether a three-dimensional system would really have a phase transition that you could understand in terms of replica symmetry breaking or not was a controversy. I was certainly aware that Daniel had the different view. I was aware of it, but he was a former student and we weren't that close. I read about it more[36]. I might have heard him talk about it, but I certainly didn't contribute. I would say that my inclinations were to believe him in some sense, but that I didn't have a firm view on it. I certainly wasn't prepared to try to jump into the battle.

PC: So this was mostly you following the literature. Not attending workshops or…

BH: [0:34:58] I'm sure it came up at… I certainly didn't attend any workshop on spin glasses. But of course talks on spin glasses might well have come up in more general workshops or conferences that I attended, like statistical mechanics conferences and so forth. But I would have been more likely to have read or heard a lecture in a seminar series or something. I think Daniel was probably at Bell Labs at the time, if I remember correctly. I did consult at Bell Labs during summers. I visited Bell Labs relatively frequently in that period. I might have heard about it from him there.

PC: During your time at Harvard or elsewhere, did you ever teach anything about spin glasses or replica symmetry breaking? If yes, can you detail?

BH: [0:36:12] I think the answer is no. I haven't taught anything about spin glasses. I can't think of any course that I would have taught… I tended to teach more introductory graduate courses rather than specialized courses. Occasionally, I taught more specialized course but probably disorder was not something I really taught, even though I was interested in it.

PC: And you worked on in many different context.

BH: Yeah.

[36] See, *e.g.*, D. S. Fisher and H. Sompolinsky, "Scaling in spin-glasses," *Phys. Rev. Lett.* **54**, 1063 (1985). https://doi.org/10.1103/PhysRevLett.54.1063; 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

PC: We're about to complete the interview, so is there anything that you would like to share with us about this era that we may have skipped over, that we might have neglected?

BH: [0:37:05] No, I think I've covered my role, or my lack of role in it. Again, part of the reason that I didn't get that much involved is that there wasn't that much in the way of experiments that really gets at these problems. Simulations… You can't really simulate glasses very well. I had a long experience with real glasses of how hard it is to try to answer questions about whether there is a phase transition or isn't a phase transition. I guess that kind of made me… That's something that influenced my own particular attitude in trying to look at other problems, but I recognized the intellectual interest of it, and the fact that it was in a sense a really deep problem, which like many deep problems we may never know the answer to. That was my view at the time. I can't really say that much about how it fits into the world [now].

PC: Reading your AIP interview, it seems that your choice of problems, as you just alluded to, were more closely tied to experimental problems. Is that a general attitude that American physicists tended to have more than European physicists, or was it just your own personal choice?

BH: [0:39:21] No. Well, maybe. The European physicists that I interacted with in general were people who shared this kind of attitude. When I first went to Paris, for example, I was in the group of Nozières[37]. I had a lot of interactions with de Gennes[38] at the time. He was very influential in Orsay, so there was certainly no question that they were attuned to experiments very much. I remember visiting Peter Fulde[39], from the past, in Germany. These were people who were certainly interested in experiments, so I would not have made that distinction.

PC: But Cirano, for instance was much more…

BH: [0:40:28] Yeah, Cirano. There were also more mathematical… I could point to people in the US who do mathematical physics as well. I certainly wasn't aware of a difference…

PC: Of a cultural difference?

BH: Yeah.

[37] Philippe Nozières: https://en.wikipedia.org/wiki/Philippe_Nozi%C3%A8res
[38] Pierre-Gilles de Gennes: https://en.wikipedia.org/wiki/Pierre-Gilles_de_Gennes
[39] Peter Fulde: https://en.wikipedia.org/wiki/Peter_Fulde

PC: Fair enough. A last question. Do you still have notes, papers, correspondence from that epoch? If yes, do you intend to deposit them in an academic archive at some point?

BH: [0:40:56] I do. I have large files with the thousands of carbon copies of papers. In those days, there was no email, so for everything there would have been carbon copies. I have these files. I haven't looked. Most of them are completely worthless. I don't know if any of them have to do with spin glasses.

My expectation is that eventually they will… Harvard is prepared to put all these things in boxes and lock them away for 50 years, and then make available. That's probably what I would do with them, but then of course they wouldn't be available for 50 years. That’s the way they would do it. I don't think I have the strength to go through and look through and see if any of them are interesting myself. Would I put them…? I'm not sure under what conditions other people can look at them in the next 50 years or not. I haven't really negotiated that with them. But of course 99.9% of it is of zero interest. If you have suggestions… I don’t know.

PC: It's wonderful that you do have them. You'd be surprised of how few of these collections of papers still exist at this point. You're right that an archivist would need to index it and would need to decide what is confidential and what is not. That is part of the discussion that you would have with Harvard, if you prepare this ahead of time. If you engage in a conversation with them. I don’t insist on you going through it, but you should at least make them aware of it.

BH: [0:43:08] I've had one discussion with them. But it seems that at least their standard policy is just to lock it all away for 50 years and not worry about it. They’re certainly not going to index them, unless somebody actually… They certainly do not routinely index them. Harvard is rich, but it's not that rich. I don't know whether in 50 years anyone would be… I suspect no one is going to be interested, but there’ll be there. If there are specific questions…

PC: If you’ve had letter exchanges with Cirano, for instance, on the theme or around spin glasses.

BH: [0:44:03] It’s very unlikely. I don’t recall, and it would be unlikely that I would have done it.

PC: Or with Phil Anderson.

BH: No. You know, it's painful to write. Even those days I could pick up a phone. I could have even called Europe if I wanted. Discussion would have been much more likely to take place, undocumented, in front of a blackboard. That's where all discussions really took place, in front of blackboard. Not in front of a typewriter. Anything that involved equations I certainly never have tried to do by typewrite, because that would have been far too painful. There would have been very little scientific correspondence. I have some of that separated, actually. I might look, but I think maybe only more recently. Quickly, 90% of the correspondence was about travel arrangements or letters of recommendation for some student they wanted to send me, or I wanted to send them, that sort of thing. Very little of it would have been scientific.

I do have in my office, I believe, some separate files. At some point, I started separating letters that had in some way to do with science research, but I suspect that doesn't… Well, I'd have to look—I'm not in my office now, obviously—and see if any of it had anything to do with spin glasses. I'll try to look and see if any of it [does]. A lot of it had to do with quantum hall effect, because that was at the point when I started this collection. In fact, I think I separated out those that had to do with the quantum Hall effect, and those that had to do with all other things. So this is before computers, before email. That sort of has lapsed. After email, I started printing out certain things to put in that file, but now more and more I wouldn't. I would just leave it in my computer file, and so I could search for it if I needed it.

PC: Like we all do.

BH: [0:47:15] Well, I'm still a little behind. I'm still in the 20th century, rather than the 21st century. I'm somewhat shifting away from paper.

PC: Thank you very much. It's been a pleasure.

BH: Ok, Patrick. Good to speak to you, and good luck with your archive.