History of RSB Interview: John A. Hertz

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Interviewers:
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Over Zoom, from Prof. Hertz's home in Copenhagen, Denmark.

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PC: Good morning, Prof. Hertz. Thank you very much for joining us. As we've discussed ahead of this interview, we will be covering the period during which ideas behind replica symmetry breaking and spin glasses were developed, which we roughly bound from 1975 to 1995. Before we get to that we have a few questions on background, if you don't mind. In particular, could you tell us a bit about your family and your studies before starting university?

JH: [0:00:33] I grew up in an academic family in Pennsylvania[1]. I went to high school in Connecticut[2]. It was a private school. From there, I went to Harvard, where I studied physics as an undergraduate and then on to Pennsylvania for graduate school.

PC: What first got you interested in physics, and then to pursue graduate studies in physics?

JH: [0:01:09] When I started university I didn't know whether I wanted to do physics or math, and then I found out what real math was. It was a little bit of an abstract challenge to me. I just felt more on native turf in physics. I remember, in particular, my first year in the physics library discovering Feynman's Lectures on Physics[3], which read like it was a novel. I just couldn't put it down. That's how it started.

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[1] John Atlee Hertz was then faculty at Lehigh University. See, *e.g.*, "JOHN ATLEE HERTZ," Daily Press, January 22, 2004. https://www.dailypress.com/news/dp-xpm-20040122-2004-01-22-0401220143-story.html (Consulted December 4, 2021.)
[2] Choate-Rosemary Hall School: https://en.wikipedia.org/wiki/Choate_Rosemary_Hall
[3] The Feynman Lectures on Physics: https://en.wikipedia.org/wiki/The_Feynman_Lectures_on_Physics

PC: What then drew to you to U Penn to work with Bob Schrieffer[4] in theoretical physics?

JH: [0:02:12] In my first undergraduate year I met my wife to be. We got married the summer after we graduated[5]. We planned on looking for graduate schools together. She was in psychology. We had a number of options, but Penn seemed to be the joint optimum. Maybe not even at the top one for either of us, but best jointly. So we went to Penn[6]. I remember talking to my in-laws, just before we went there, and already then I thought I wanted to work on superconductivity. I knew about Bob Schrieffer, although I had never met him. Initially, I thought I'd be an experimentalist. I tried that for half a year and proved to be all thumbs. It seemed that to be an experimentalist you had to – at least in the lab I was working in – be one of these guys who played around electronics as a kid. I had never done that. I was really more mathematically oriented. Realizing that an experimental career probably wasn't for me, I explored theoretical possibilities at Penn where there were a number of people. I was lucky on the qualifying exam to score highly, so I could basically choose who I wanted to work with. There were a number of other people I could have worked with just as well, but I chose Bob. Certainly, I got a lot out of that. It started the initial direction of my career and the flavor of the physics I wanted to do.

PC: What then took you to work with Phil Anderson[7] in Cambridge?

JH: [0:04:50] I had… My dad was a professor of English literature, an anglophile of some kind. Throughout my childhood I sort of came to believe that the luckiest thing that could ever happen to me would be to go to Oxford or Cambridge. That set me looking at who was working in Cambridge and Oxford. Naturally, Phil's name caught attention, because I knew a lot about what he had done from my graduate studies. I spent two very happy years at the Cavendish[8]. I would have liked to stay in England, I think. Then I got an offer from Chicago, which was really too good to turn down. I enjoyed

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[4] John Robert Schrieffer: https://en.wikipedia.org/wiki/John_Robert_Schrieffer

[5] See, *e.g.*, "Barbara Brown Engaged to Wed John A. Hertz Jr.; Radcliffe and Harvard Seniors Planning to Marry in August," New York Times, May 9, 1968. https://nyti.ms/3rC41PM

[6] John Atlee Hertz Jr., *Dynamical scale invariance in itinerant electron ferromagnets*, PhD Thesis, University of Pennsylvania (1970). https://franklin.library.upenn.edu/catalog/FRANKLIN_9970853335O3681; Barbara Gevene Hertz, *Frequency-adaptation in the human visual system: effects on perceived contrast of supra-threshold gratings*, PhD Thesis, University of Pennsylvania (1973). https://franklin.library.upenn.edu/catalog/FRANKLIN_9994599535O3681

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

[8] Cavendish Laboratory: https://en.wikipedia.org/wiki/Cavendish_Laboratory

my time there as well. We had a group of young theorists. We all talked together at the time.

PC: Can you tell us about how the Cambridge group was functioning? How was it to interact in the theoretical physics part of the Cavendish?

JH: [0:06:23] It was called the theory of condensed matter group. Volker Heine[9] was actually the leader. He was there year round, whereas Phil was only there for half the year. There were other junior faculty as well. Lots of postdocs. People had college fellowships. It was a pretty big group. Impressions that I want to mention are… Twice a day we met for coffee time in the morning and tea time in the afternoon. This was an old building in the center of town. Things were pretty crowded. In the middle of the two years that I was there we moved to the new building, which was outside Cambridge. Well, it’s in Cambridge but it's outside of the center. It was very close to… You knew everybody in the group from these conversations at coffee twice a day. One thing that struck me was that it was much more informal than things at Penn. At Penn, you never knew how you should address a professor. In Cambridge, Phil was just Phil to everybody in the group whatever age they were. I enjoyed that, and I especially got a lot out of talking to Phil, as I guess you can guess that directed me. That’s where I first learned about spin glasses, although I didn't work on them until after I got to Chicago.

PC: Were you roughly free to choose your research directions, or would Phil assign you a problem?

JH: [0:08:45] In order to have money to pay me I had talked with him a little. He suggested something, and he wrote an application to the Research Council[10] to fund a postdoc, which was me. Basically, I solved that problem in the first six months. That sounds too strong. I got the basic idea and I wrote a Phys. Rev. Letter[11]. That led to further work, especially with David Edwards at Imperial College[12]. He was a coauthor with me on the paper. Phil was kind of pleased that essentially what he’d promised to do on his grant application he could say it was done.

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[9] Volker Heine: https://en.wikipedia.org/wiki/Volker_Heine

[10] Science Research Council: https://en.wikipedia.org/wiki/Science_and_Engineering_Research_Council

[11] J. A. Hertz and D. M. Edwards, "Intermediate-coupling theory for itinerant ferromagnetism," *Phys. Rev. Lett.* **28**, 1334 (1972). https://doi.org/10.1103/PhysRevLett.28.1334

[12] J. A. Hertz and D. M. Edwards, “Electron-magnon interactions in itinerant ferromagnetism. I. Formal theory,” *J. Phys. F* **3**, 2174 (1973). https://doi.org/10.1088/0305-4608/3/12/018; D. M. Edwards and J. A. Hertz, “Electron-magnon interactions in itinerant ferromagnetism. II. Strong ferromagnetism,” *J. Phys. F* **3**, 2191 (1973). https://doi.org/10.1088/0305-4608/3/12/019

I spent naturally more time developing that, but a lot of time just taking advantage of what I could learn from people there. For a while there was something about which I had a lot of conversations with Erio Tosatti[13] and Richard Palmer[14], who were working on an idea about neutron stars at the time[15]. Naturally, I learned about spin glasses from Phil. I remember especially his "More Is Different" lecture. It was published in Science around that time[16], but I guess I saw a copy of it and thought: "This is the way I like to think about physics."

PC: You just mentioned that you've heard about spin glasses at that point. Can you tell us how that came about? Was it through seminars? Or was it just Phil talking about spin glasses?

JH: [0:10:54] It was just conversations with Phil. I would go in his office and sometimes there'd be a paper lying on the table there, and I would ask about it. He had definitely ideas about it. It was a different field, of course, at that time. It was just very phenomenological, trying to characterize more than understand the slow dynamics. It wasn't really until the Edwards and Anderson paper[17], which came out after I left Cambridge that it got to be a real theoretical physics problem.

PC: Did you follow the development of those ideas associated with the Edwards and Anderson paper? Or did you learn about it like everyone else from the published literature?

JH: [0:11:59] By the time I really started thinking about working on it, as opposed to "it's good for me to learn about another idea", then I had been in Chicago for a year or something. I used to go back to the Cavendish in summers sometimes. I remember talking to Sam Edwards then[18].

Now we're getting into replicas. I was skeptical about replicas in the beginning. Partly because in Chicago I interacted, talked a lot with Shang-

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[13] Erio Tosatti: https://en.wikipedia.org/wiki/Erio_Tosatti

[14] Richard G. Palmer: https://en.wikipedia.org/wiki/Richard_G._Palmer

[15] See, *e.g.*, R. G. Palmer, E. Tosatti and P. W. Anderson, "Are Neutron Star Cores Pion Condensates or Quantum Crystals?" *Nat. Phys. Sci.* **245**, 119-120 (1973). https://doi.org/10.1038/physci245119a0

[16] P. W. Anderson, "More Is Different," *Science* **177**, 393-396 (1972). https://www.jstor.org/stable/1734697

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

[18] Sam F. Edwards: https://en.wikipedia.org/wiki/Sam_Edwards_(physicist)

keng Ma[19]. (He was in San Diego, but he was working with Gene Mazenko[20] in the office next to me.) I would see him at meetings. (Before that, when I went to Stanford, he was there.) He always had such a nice direct approach to stuff. He and Gene had this way of doing dynamical renormalization group calculations without replicas for random systems, just by formally averaging the graphs over the disorder. I thought: "I'm much happier with this." Throughout my years of working on spin glasses, I don't think I ever published a replica calculation. It was something I had to learn about to see what other people were doing, but aside from the initial lack of transparency about what it meant—that was cleared up a lot of course by Parisi and Mézard—some kind of skepticism remained about why you would want to do an equilibrium calculation about something [that] was never going to get to equilibrium. I did long papers on the dynamics of spin glasses. Of course, it was interesting to eventually learn how the natural dynamics calculation led to something which is related to, but not identical to, although strongly connected to one-step RSB.

PC: We're going to get to your work on dynamics a bit later. If you don't mind I'd like to keep talking about this early period. You said you were discussing with Sam Edwards over the summer, so you heard about the calculation that he and Phil were doing before it got published. Did they send you a preprint of that work as well? Were you part of the inner group on the topic?

JH: [0:15:47] I'm not quite sure I saw it. In those days, everybody got preprints all over the place. Nothing to do with arXiv, but you'd have preprint libraries.

PC: In your first paper on spin glasses, which is "Gauge model for spin glasses"[21] you mentioned that you started thinking about that particular topic during a visit to the LPS, in Orsay. Can you detail that interaction? Who were you interacting with Orsay? In particular, did you interact there with Blandin, and hear about his work on replica symmetry breaking[22]?

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[19] J. C. Y. Chen, J. Prentis and S. Schultz, "Shang-keng Ma," *Phys. Today* **37**(4), 102 (1984). https://doi.org/10.1063/1.2916178

[20] S.-k. Ma and G. F. Mazenko, "Critical Dynamics of Ferromagnets in 6−ε Dimensions," *Phys. Rev. Lett.* **33**, 1383 (1974). https://doi.org/10.1103/PhysRevLett.33.1383; "Critical dynamics of ferromagnets in 6−ε dimensions: general discussion and detailed calculation," *Phys. Rev. B* **11**, 4077 (1975). https://doi.org/10.1103/PhysRevB.11.4077; G. Grinstein, S.-k. Ma and G. F. Mazenko, "Dynamics of spins interacting with quenched random impurities," *Phys. Rev. B* **15**, 258 (1977). https://doi.org/10.1103/PhysRevB.15.258

[21] J. A. Hertz, "Gauge models for spin-glasses," *Phys. Rev. B* **18**, 4875 (1978). https://doi.org/10.1103/PhysRevB.18.4875

[22] A. Blandin, "Theories versus experiments in the spin glass systems," *J. Phys. Coll.* **39**, C6-1499 (1978). https://doi.org/10.1051/jphyscol:19786593

JH: [0:16:34] I can remember talking to him then, but I can't remember whether it was about replica symmetry breaking at that point or not. I went to Orsay to collaborate with Zazie Béal-Monod[23], but spin glasses had gotten on my mind. I think she was kind of disappointed that I didn't spend as much time talking with her. I also talked to Philippe Monod[24], who was interested in experiments in spin glasses. I was seduced by gauge theory, and the exciting things in particle physics at the time. I just enjoyed the thing with the ideas from one remote part of physics being used for insight in another part. When I wrote that paper, I didn't really understand that what I was talking about was a glass of things with Dzyaloshinskii-Moriya interactions. At least, that got me into the field in an active way.

PC: You started working also with Richard Klemm[25] following an Aspen meeting, the 1977 US-USSR meeting on condensed matter[26]. How did that collaboration come about in that context? What were you then trying to achieve with this work?

JH: [0:18:46] I think we were just trying to see what we could learn about spin glass states, the transition and so on using dynamical methods. We explored a few different models. In retrospect, it wasn't so profound. We were just feeling our way through this. Nobody else in Chicago was working on spin glasses, and Richard was the nearest person who was interested on working with me on this.

PC: Did you meet him at Aspen, or did you know him before?

JH: [0:19:45] I'm pretty sure I met him before, but probably that interaction started in Aspen.

PC: Afterwards, with your PhD student Anil Khurana[27], you worked on the instability of the Edwards-Anderson order parameter—or the RS

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[23] J. A. Hertz, K. Levin and M.-T. Béal-Monod, "Absence of a Migdal theorem for paramagnons and its implications for superfluid He^3," *Sol. State Comm.* **18**, 803-806 (1976). https://doi.org/10.1016/0038-1098(76)90209-X

[24] Philippe Monod: https://fr.wikipedia.org/wiki/Philippe_Monod_(physicien)

[25] See, *e.g.*, J. A. Hertz and R. A. Klemm, "Critical dynamics of a Heisenberg spin-glass," *Phys. Rev. Lett.* **40**, 1397 (1978). https://doi.org/10.1103/PhysRevLett.40.1397; "Dynamics near the spin-glass transition," *Phys. Rev. B* **20**, 316 (1979). https://doi.org/10.1103/PhysRevB.20.316

[26] R. N. Bhatt, "Condensed Matter Physics at the Aspen Center for Physics during the First Fifty Years," *Aspen Center for Physics* (2011). https://www.aspenphys.org/science/sciencehistory/cm.html (Consulted December 4, 2021.)

[27] Anil Khurana, *Spin-Dynamics and Sound Propagation Near the Spin Glass Transition*, PhD Thesis, University of Chicago (1981). https://www.proquest.com/docview/303041217 (Accessed May 30, 2022.)

solution—without replicas[28], roughly in parallel to Parisi's development of the RSB scheme[29]. How closely were you following his efforts, and what was your impressions of the work at the time?

JH: [0:20:20] My reaction to Giorgio's work?

PC: Yes, and the interaction it had with your own research?

JH: [0:20:34] It seemed like he really had something there, although I don't think any of us really fully understood initially why this replica symmetry breaking—and in particular infinite replica symmetry breaking—should be the way you handle this. I thought maybe there's somewhere to get to this through dynamics[30]. First, we were just trying to see what we could do with simple and fairly naive dynamics, to see if we could begin to see some connection, which I don't think we did. But maybe now we understand why. That was our motivation.

PC: In the middle of that effort, you moved from Chicago to Nordita[31], but you kept working on spin glasses through that move. Were you then interacting more with the Paris, Rome, and UK communities on spin glasses? How connected or isolated were you at Nordita compared to at Chicago?

JH: [0:22:10] At the beginning, I was equally isolated. After a few years—maybe two years—I had some conversations with David Sherrington[32], probably on a visit to Oxford. He, Giorgio, and I guess Heinz Horner, were involved in a little network that was supported by the EU. He just invited me to join the meetings, nothing official about it. That's how I kind of got connected. I remember, certainly as early as '83—probably also '82 but I'm not sure about it—going to some meetings in Paris and maybe somewhere else. I can't remember.

Dr. Khurana later worked for *Physics Today*. See, in particular, D. S. Fisher, G. M. Grinstein and A. Khurana, "Theory of random magnets," *Phys. Today* **41**(12), 56-67 (1988). https://doi.org/10.1063/1.881141

[28] A. Khurana and J. A. Hertz, "Instability of the Edwards-Anderson order parameter for an Ising spin glass," *J. Phys. C* **13**, 2715 (1980). https://doi.org/10.1088/0022-3719/13/14/013;

[29] See, *e.g.*, G. Parisi, "Infinite number of order parameters for spin-glasses," *Phys. Rev. Lett.* **43**, 1754 (1979). https://doi.org/10.1103/PhysRevLett.43.1754

[30] See also J. A. Hertz, A. Khurana and M. Puoskari, "Dynamics as a treatment of the Almeida-Thouless instability," *Phys. Rev. B* **25**, 2065 (1983). https://doi.org/10.1103/PhysRevB.25.2065

[31] Nordic Institute for Theoretical Physics: https://en.wikipedia.org/wiki/Nordic_Institute_for_Theoretical_Physics

[32] P. Charbonneau, *History of RSB Interview: David Sherrington*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 39 p. https://doi.org/10.34847/nkl.072dc5a6

PC: You wrote another work on the dynamics of spin glasses, a series of papers called “Dynamical mean-field theory for spin glasses”[33]. How did this idea of looking at dynamics again—from this different angle—come about? Did you interact or get inspired by the work of Sompolinsky and Zippelius which had come out a little before[34]?

JH: [0:24:10] I was trying to understand what Haim[35] had done. It seemed like he didn't take it to its conclusion--or what I hoped was its conclusion--which is that you could really see a direct correspondence with Parisi’s replica calculations. About that calculation that I was doing: in some sense I was trying to do a poor man’s version of Haim’s, or making stuff more explicit, so that I understood it better myself. I was wondering was there some simple kind of ansatz that got you to complete it. I was never sure that I had one, but I thought: “Let me write the paper, and see if anybody reacts to it. Maybe somebody uses it to develop an idea and takes further.” I had been sort of sitting on it for a long time. I felt like: “Let’s get it out and see what anybody thinks of it.” I think it was largely neglected, that is I don’t remember any useful conversation with people that came out of that. That was my thinking at the time.

PC: Can you provide us some perspective on what was the relationship between people who were thinking about dynamics and the replica symmetry breaking results? Were there a lot of ideas floating in the air? Was this something that was confusing people?

JH: [0:26:37] I had the feeling at the time you were either doing replicas or you were doing dynamics. In Haim’s work I saw the possibility of a connection. I didn't seem to me that he actually really solved it. I think, in retrospect, if I had been in Jerusalem then and got to talk about stuff and argue with Haim, maybe something more would have come out of it. Really there was nobody else in Scandinavia working on these things. Yes, I was connected through this network that David and Giorgio started, and there were lots of interesting conversations, but I never achieved what I hoped that might lead to.

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[33] J. A. Hertz, "Dynamical mean-field theory for spin glasses. I. Short-time properties in zero field," *J. Phys. C* **16**, 1219 (1983). https://doi.org/10.1088/0022-3719/16/7/009; "Dynamical mean-field theory for spin glasses. II. Approach to equilibrium in a small field," *J. Phys. C* **16**, 1233 (1983). https://doi.org/10.1088/0022-3719/16/7/010

[34] 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

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

PC: There were some efforts in the group of Alf Sjölander and collaborators to think about dynamics[36]. What was your interaction or your perspective on these dynamical efforts that came from a completely different viewpoint?

JH: [0:28:34] Of course, these are the ones which are connected to one-step RSB. This was a real connection when it became clear. But I think I was really focused on canonical SK spin glasses. I didn't see how to make a connection between Sjölander's and also Götze's work. They were looking at the problem in the same way I was looking at the problem. We were looking at different problems, but [also asking] what the methods of statistical dynamics can [say about] this problem. I was surprised they found something so neat in that problem that I couldn't really drag out of the spin glass problem.

FZ: You mentioned that at the time people were either doing replicas or dynamics. There was an early paper by De Dominicis, in 1978, entitled "Dynamics as a substitute for replicas in random systems"[37]. I think it was one of the first instances where the connection between dynamics and replicas was formulated. Are you saying that this paper was not very well known or understood at that time yet?

JH: [0:30:20] No. I was certainly well aware of that paper. I studied it a lot. I think everybody in the community knew about it. Of all the people in this field back in 1980 or something, I probably talked with Cirano[38] more than anybody else. The connection between replicas and dynamics was quite clear for the random-*J* ferromagnetic Ising model, but it wasn't such a straightforward thing when you tried to look at spin glasses. That paper was kind of my inspiration for trying to study dynamics the way I did.

PC: A couple of years later, around 1986, you switched gear and started working on neural networks. You had a series of works in collaboration with Sara Solla[39] and Geoffrey Grinstein[40]. How did that transition and that collaboration take place?

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

[37] C. De Dominicis, "Dynamics as a substitute for replicas in systems with quenched random impurities," *Phys. Rev B* **18**, 4913 (1978). https://doi.org/10.1103/PhysRevB.18.4913

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

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

[40] J. A. Hertz, G. Grinstein and S. A. Solla, "Memory networks with asymmetric bonds," *AIP Conf. Proc.* **151** 212-218, (1986). https://doi.org/10.1063/1.36259; "Irreversible spin glasses and neural networks," in *Heidelberg colloquium on glassy dynamics and optimization*, J. L. van Hemmen and I. Morgenstern I., eds. *Lecture Notes in Physics* **275**, 538-546 (1987). https://doi.org/10.1007/BFb0057533538-546

JH: [0:32:00] I got interested in the problem a couple of years before, when I spent half a year at the Institute[41] in Santa Barbara. Richard Palmer was there, and he had worked with John Hopfield[42], so I learned about Hopfield model at that point. I started going to some meetings.

PC: When was this?

JH: [0:32:35] It started in 1983, because that was the year I was in Santa Barbara. In 1984 is when I was trying to learn about that stuff. I was visiting IBM in Yorktown[43] really frequently in those years. I think it was ’85. I was there invited by Geoff, and Sara was a postdoc there. At the time, when the Hopfield model was new, the immediate questions were: “How can I make this more realistic?” The obvious thing, once you learn about real neurons, is that the connection between them is not symmetric. When you have say, an Ising model with asymmetric connections, you can't do equilibrium calculations. You can look for steady states, but there are no Gibbs equilibria. But you can use all the tools that I was using on spin glasses to study that. It was a fairly simple thing that Geoff and Sara and I did. We just took a simple soft-spin spin glass, but the matrix was asymmetric instead of symmetric. We showed what that did to the spin glass transition, that [then] you drive away the spin glass transition, and that you can still do the Hopfield calculation for stability of the memory states. We wrote up something which was for a conference proceedings on neural networks that I went to, in Utah[44]. Then, I talked about it at a meeting in Heidelberg, which Heinz Horner organized. Leo van Hemmen said[45]: “You really need to write this up more fully.” And we did. Of course, by this time Haim had already done a similar calculation[46].

[41] Institute for Theoretical Physics: https://en.wikipedia.org/wiki/Kavli_Institute_for_Theoretical_Physics

[42] J. J. Hopfield, D. I. Feinstein and R. G. Palmer, “‘Unlearning’ has a stabilizing effect in collective memories,” *Nature* **304**, 158-159 (1983). https://doi.org/10.1038/304158a0

[43] Thomas J. Watson Research Center: https://en.wikipedia.org/wiki/Thomas_J._Watson_Research_Center

[44] Neural Networks for Computing, John S. Denker, Snowbird, UT, USA, 13-16 April 1986. Proceedings: *AIP Conference Proceedings* **151**, J. S. Decker, Ed. (New York: American Institute of Physics, 1986).

[45] See, *e.g.*, P. Charbonneau, *History of RSB Interview: J. Leo van Hemmen*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 22 p. https://doi.org/10.34847/nkl.16e5m0oj

[46] See, *e.g.*, H. Sompolinsky and I. Kanter, “Temporal association in asymmetric neural networks,” *Phys. Rev. Lett.* **57**, 2861 (1986). https://doi.org/10.1103/PhysRevLett.57.2861

PC: Were you then at the ITP meeting in Santa Barbara in '86[47], and at the Institute of Advanced Studies meeting in Jerusalem on neural networks[48]. If yes, how important were these gatherings from your perspective?

JH: [0:36:15] I don't think I was at either of those. At this point, my mind was still on spin glasses and I was learning a lot about neural networks and learning about the calculations other people were doing.

There were some replica calculations, now that I think of it. On statistical mechanics of learning. I had a very good student, Anders Krogh[49], who did his thesis on that[50]. Later (in the early '90s) there were a couple papers on things like statistical mechanics of committee machines and so on. That's where I actually did some replica calculations, one step[51], with my PhD student Holm Schwarze[52].

At the same time, I had an old friend from college who worked at the NIH, the neurophysiologist Barry Richmond[53]. He was telling me: "Can you tell me if maybe we can use some neural networks in analyzing my data?" He was especially concerned in measuring information transmission in the visual system. It ended up, a few years later, after a lot of visits one way and the other—I spent a year at the NIH; they actually hired me on a permanent position, but I never intended to leave Nordita—we developing ways of using conventional layered neural networks just as a statistical tool in measuring information transmission in Barry's experiments[54].

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[47] Spin Glasses, Computation, and Neural Networks, John Hopfield and Peter Young, Institute for Theoretical Physics, University of California at Santa Barbara, September to December 1986. See, *e.g.*, Dana H. Ballard. "Modular learning in neural networks" In: *Proceedings of the sixth National conference on Artificial intelligence – Vol. 1* (AAAI'87). AAAI Press, 279–284 (1987).

[48] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Hanoch Gutfreund*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 16 p. https://doi.org/10.34847/nkl.1adb9r42

[49] Anders Krogh: https://en.wikipedia.org/wiki/Anders_Krogh

[50] Anders Krogh, *Learning and generalization in neural networks*, PhD Thesis, University of Copenhagen, The Niels Bohr institute (1991). https://bibliotek.dk/linkme.php?rec.id=800010-katalog%3A99122679335805763

[51] A. Krogh and J. A. Hertz, "Mean-field analysis of hierarchical associative networks with 'magnetisation'," *J. Phys. A* **21**, 2211 (1989). https://doi.org/10.1088/0305-4470/21/9/033

[52] Holm Schwarze, *Learning and Generalization in Multilayer Neural* Networks, PhD Thesis, University of Copenhagen, The Niels Bohr Institute (1993). https://bibliotek.dk/linkme.php?rec.id=800010-katalog%3A99122957829705763

[53] "Barry Richmond," Neurotree. https://neurotree.org/beta/peopleinfo.php?pid=567 (Consulted December 5, 2021.)

[54] J. A. Hertz, T. W. Kjær, E. N. Eskandar and B. J. Richmond, "Measuring natural neural processing with artificial neural networks," *Intl J. Neur. Syst.* **3**, 91-103 (1992). https://doi.org/10.1142/S0129065792000425

I'd say that for most of 20 years after that, leading up to just the last few years anyway, I became more of a neuroscientist than a physicist. Until around the turn of the century, I was still working on some things in spin glasses, trying to study on dynamics of polymers. At this time, David and Giorgio's network had grown into a bigger thing funded by the European science Foundation[55]. We had regular meetings with 50 people so. I was doing some calculations on spin glass dynamics with David Sherrington[56], spending a lot of time visiting Oxford. (It was partly motivated by the fact that my son was studying at Oxford. It was an excuse to go there, but I always liked to go to Oxford under any circumstances.)

In recent years, I've had an office in both the physics department and the neuroscience department here. In the middle of this Nordita moved to Stockholm, so I have an office there too. (I haven't been up to since the pandemic started, but I hope to go back soon.) Certainly in the last 10 years, [I've] been more concerned with problems having to do with neuroscience.

PC: Let's take a step back again. You mentioned that you met Richard Palmer when he was a grad students in Cambridge. And then that you met him again and interacted with him again at the ITP in 1983. In 1988, he invited you to spend a sabbatical at Duke. Were you in touch throughout? Were you close friends? How did that visit to Duke get organized?

JH: [0:41:37] I wouldn't say we were close friends, but we were in touch. It was quite largely his initiative suggesting that I come to Duke. The duty that I would have was to help teach a course on this, because people were getting interested and the students wanted to know, but there was nobody besides Richard who had any knowledge whatsoever. I was really more involved already then understanding the engineering-style developments in back propagation and so on. We taught the course together. They had a scheme there, where there is a bunch of universities right in that neighborhood. (You're right there, so you know.) This was a course put on television for people in rooms at NC state and UNC as well as Duke[57]. My

[55] "Statistical physics of glassy and non-equilibrium systems (SPHINX)", *European Science Foundation*, 1999-2004. http://archives.esf.org/coordinating-research/research-networking-programmes/physical-and-engineering-sciences-pen/completed-esf-research-networking-programmes-in-pesc/statistical-physics-of-glassy-and-non-equilibrium-systems-sphinx/science-meetings.html (Consulted December 5, 2021).

[56] J. A. Hertz, D. Sherrington and Th. M. Nieuwenhuizen, "Competition between glassiness and order in a multispin glass." *Phys. Rev. E* **60**, R2460 (1999). https://doi.org/10.1103/PhysRevE.60.R2460

[57] The Microelectronics Center of North Carolina (MCNC) created in 1985 the Communications for North Carolina Education, Research and Technology (CONCERT)—later known as North Carolina Research and Education Network (NCREN)—which set up the first broadcast-quality, two-way interactive, multipoint video and audio system in the United States. The initial microwave system linked NC State University, UNC

first experience with remote teaching, trying to remember to look at the screen to see whether somebody had a question.

Because we didn't have a textbook or anything, I wrote up lecture notes in LaTeX and handed them out to the students. Richard did the same for the stuff he gave lectures on. By the end of the course we had a pile of these things. It wouldn't have gone anywhere except that Jack Cowan[58] came from Chicago for a visit. We were talking in Richard's office and he saw these things on the table. He said: "What's that?" He was supposed to collect things for a volume for Springer on this sort of idea. He said: "Could we use these?" Richard and I talked about it. I should mention that my then-student Anders Krogh had a lot to do with helping with the teaching and in preparing the notes. He writes better than I do, and so does Richard. I'm not the good writer of the group, but fortunately I had really good co-authors there. We said: "Yes, we would like to make it into a book of some kind." Richard was already associated with the Santa Fe Institute then. He cared about this more than Anders Krogh or I did, so we decided we didn't go to Springer, but to this Santa Fe series with the blue cover, by which it's known since then[59]. We spent the next year converting the notes into something we could not be ashamed of to publish as a book. I enjoyed writing it. I was writing a book on spin glasses around the same time[60]. It started earlier with Konrad Fischer, but the spin glass book was finished later than the neural networks book.

PC: The book on neural network was amongst the very first to be published on the topic, right? It seemed to have been reasonably well received, right? If yes, was there ever a discussion of a possible second edition, or was it seen as complete?

JH: [0:46:22] The barrier to doing it was that Richard and Anders had gone off into totally different fields. In fact, I was not really involved in network research much by that time. It would be a big learning job for us to capture all the new. But it almost happened because Sara Solla was visiting Copenhagen once and she said: "Well, I can take Anders' place." The problem was that before we got started Richard had a stroke, and he really wasn't able to do it. We just kind of got discouraged at that point. I think

Chapel Hill, Duke University, NC A&T, UNC Charlotte, and the Research Triangle Institute. See, *e.g.*, C. R. Coble, "Perspective: MCNC, connecting North Carolina," EdNC.org, May 11, 2020. https://www.ednc.org/perspective-mcnc-connecting-north-carolina/ (Consulted December 5, 2021.)

[58] "Jack D. Cowan," *Neurotree* (undated). https://neurotree.org/beta/peopleinfo.php?pid=1756 (Accessed May 30, 2022.)

[59] J. A. Hertz, A. S. Krogh and R. G. Palmer, *Introduction to the Theory of Neural Computation* (Redwood City, Calif.: Addison-Wesley Pub. Co., 1991).

[60] K. H. Fischer and J. A. Hertz, *Spin Glasses* (Cambridge: Cambridge University Press, 1993).

now the book is still useful to people. Actually, Bernhard Mehlig at Gothenburg [Göteborg] has written—I think it's just out now—a modern version[61], which seems to have the same kind of flavor that ours had, but leaves out the things from ours which have now become less important and put things about the new exciting developments in their place. If somebody's looking for an updated version, I would recommend Bernhard's book.

PC: You mentioned the other book that you were co-authoring at the same time with Konrad Fischer[62]. How did that book project come about, and why was it the natural time to start writing such a book when you did?

JH: [0:49:06] Because there was no book on spin glasses. In a way, the book by Marc and Giorgio and Miguel sort of came out first[63], but it wasn't the same sort of monograph as ours was. It [nevertheless] wound up the first thing people went to learn about the field. Because of Konrad's involvement in experiments, ours had a different kind of emphasis on issues which maybe have gotten onto the back burner. When you know the excitement of RSB and what Rome and Paris people could do with it, this is really something new and important in physics. Not just important. It's more than that that the Nobel committee has recognized finally this year. (I think it was a little overdue.) We [now] have some math for describing at least some classes of highly complex things. That it started out with worrying about some messy magnetic alloys is now kind of irrelevant.

PC: How did you know Konrad Fischer? How did the two of you get started on this project?

JH: [0:51:47] I can't remember how it started. But when I was working on those problems that I worked on back in Cambridge—the initial problem with Phil Anderson—that led me to connections with people in Jülich. The relevant experiments there were photoemission, and they had a very good photoemission group there. So early in my time in Cambridge I established connections with the then-called Institut für Festkörperforschung, in Jülich, which I don't think exists anymore—at least not under that name. That's how I met a number of other people in spin glasses. When I got

[61] B. Mehlig, *Machine Learning with Neural Networks: An Introduction for Scientists and Engineers* (Cambridge: Cambridge University Press, 2021).
[62] Alex Braginski, "Konrad H. Fischer," *Superconductivity News Forum* (June 1, 2016). https://snf.ieeecsc.org/obituary/konrad-h-fischer (Consulted December 5, 2021.)
[63] M. Mézard, G. Parisi and M. A. Virasoro, *Spin Glass Theory And Beyond: An Introduction To The Replica Method And Its Applications*, (Singapore: World Scientific Publishing Company, 1987).

interested in spin glasses, there were other people there[64]. Wolfgang Kinzel[65] was there at the time. I had a lot of interactions with his group over the years; more after he moved to Würzburg, but I first met him in Jülich.

PC: As you mentioned, after the book your interest in pure spin glasses slowly vanished, but did you keep abreast of the field? Were you following the advances that were happening? In particular, in the mid-90s, Silvio Franz was at Nordita, which is the time he formulated the Franz-Parisi potential[66]. Were you then discussing with him? Were you following what was happening?

JH: [0:53:47] Yes. I was hearing a lot about it from Silvio at the time. But what we were doing was a little different. It was dynamics. Also, with my commitments in neuroscience, it was a matter of there's only 24 hours in a day. These things like the Parisi-Franz potential, I only sort of followed them in passing.

PC: After this initial contact with David Sherrington in the meetings, we understand that you became part of some of the European networks on spin glasses and related ideas.

JH: [0:54:49] Yes. When they made the expanded version of it with the European Science Foundation, it had a council with representatives from the various countries where people were in. I was the Danish representative, and informally I was supposed to represent other Scandinavian people too. The group met for big conferences every year and I always went to those. That's where I got to know people like Irene [Giardina] and Andrea [Cavagna], before that Jorge[67] and Leticia[68], and other Parisi students.

PC: From your viewpoint of having bridged both sides of the Atlantic scientifically and geographically, how do you understand the difference in interest in spin glasses and in replica symmetry breaking ideas between the US and Europe?

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[64] See, *e.g.*, P. Charbonneau, *History of RSB Interview: John Mydosh*, transcript of an oral history conducted 2021 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 19 p. https://doi.org/10.34847/nkl.e1e3ob87

[65] "Wolfgang Kinzel," *Physics Tree, Academic Tree* (undated). https://academictree.org/physics/peopleinfo.php?pid=736010 (Consulted December 6, 2021.)

[66] S. Franz and G. Parisi, "Recipes for metastable states in spin glasses," *J. Phys. I* **5**, 1401-1415 (1995). https://doi.org/10.1051/jp1:1995201

[67] Jorge Kurchan: https://fr.wikipedia.org/wiki/Jorge_Kurchan

[68] Leticia Cugliandolo: https://en.wikipedia.org/wiki/Leticia_Cugliandolo

JH: [0:56:33] I got the feeling that in the late '70s funding agencies were not interested in spin glasses. This was not why I left Chicago, but if I had stayed I would have worked on something else. I got the impression that this was a lively and funded topic in Europe, but not so much in the States, at least then I didn't know of any. Ok, Haim spent some time with Bell Labs, but that was just on Bell Labs funding. He had the independence to do that there.

PC: Phil Anderson at Princeton did some work in the mid-'80s as well[69]. At that point, were you still in touch with him? Do you have an idea how he was thinking about spin glasses and replica symmetry breaking?

JH: [0:58:08] I did visit Princeton a couple of times, but I wasn't really in touch with Phil. Things were changing in the field radically around the time I came to Europe. I had the impression then that all the action was here, in Europe. All the people in Paris and Rome and also England. I didn't know anybody working on this in America.

FZ: Do you have an opinion or an idea of why it was like that? Was it a fluctuation or were there some underlying reasons for this difference in interest and in funding on the two sides of the Atlantic?

JH: [0:59:34] One advantage we had in Europe was there were many countries, many research councils. Even if one of them is negative, another might be positive. That gave more opportunities for new ways of thinking sneaking into the system. I have the impression that in the States it was more that people were thinking about materials problems, and there were lots of excitement in those. Maybe the field lacked a big vocal advocate.

FZ: What has always kind of puzzled me is that Phil Anderson was probably one of the strongest advocates for the idea of universality of spin glasses, and of the fact that spin glasses could be a prototype of complex systems with broad applications[70]. He was in the US, and he was very influential, but still it seems that this didn't really create a community at the time working on this interest.

JH: [1:00:58] In retrospect that is curious. But Phil had his fingers in so many different problems. In Europe, for people working on spin glasses, this was

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[69] Y. Fu and P. W. Anderson, "Application of statistical mechanics to NP-complete problems in combinatorial optimization," *J. Phys. A* **19**, 1605 (1986). https://doi.org/10.1088/0305-4470/19/9/033

[70] See, *e.g.*, P. W. Anderson, "Spin Glass VI: Spin Glass As Cornucopia," *Phys. Today* **42**(9), 9 (1989). https://doi.org/10.1063/1.2811137

our main interest. Obviously, especially Giorgio has to get credit for building up a school. Marc learned this stuff from him. David Sherrington was instrumental as well. He had a lot of students working on this for years. It was his main interest as well.

PC: You mentioned a class you thought that had a spin glass flavor to it at Duke, but did you ever teach a class that mentioned spin glasses and replica symmetry breaking in Chicago, Nordita or elsewhere? If yes, can you detail?

JH: [1:02:24] Ok. Not in Chicago. I gave courses with applications of quantum field theory to condensed matter there. In Nordita, I gave courses on statistical dynamics generally, and also on disordered systems. I guess, it is probably fair to say [about] these that the paper from Cirano from 1978 mentioned earlier, was sort of at the root of those. I never gave one which was specifically on spin glasses, but spin glasses were involved in both of those.

PC: When would this have started?

JH: [1:03:38] At Nordita, we didn't have teaching obligations, so I didn't teach every year, but I taught maybe every other year. In the '80s it was on disordered systems generally and in the '90s it was a course given several times on statistical dynamics. When Nordita moved, I did give one course there because people wanted to know about renormalization group. I said: "Ok. I'll try to do that." But that wasn't really about spin glasses at all. It was about using renormalization group methods. I taught myself a new way of doing it in order to teach that. It's the operator algebra method that's in John Cardy's book[71].

PC: Is there anything else you'd like to share with us about this idea that we may have missed or skipped over?

JH: [1:05:32] I still have questions about replicas. Maybe if I'd stayed in the middle of the action and kept working with Silvio I wouldn't have these questions. One of them is: "Why is it either zero, one, or infinity?" You either had no replica symmetry breaking, or one step, or infinite. Are there no systems where two or three would be relevant? Could you make up some?

PC: There are.

[71] J. L. Cardy, *Scaling and Renormalization in Statistical Physics* (Cambridge: Cambridge University Press, 1996).

JH: [1:06:20] Maybe people have made these up now, but I always wondered about that.

FZ: There are, but they are not natural in some sense[72]. You have to cook up a system that has that. It's strange, yes.

JH: [1:06:38] I also wondered about all these things that we treat with one step. Why is one step enough? Are we sure that one step is enough? I don't work on this, but I still wonder about things like that?

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

JH: [1:07:28] I don't have any notes from then. I never saved old calculations once they get published. Sometimes, if there was some special thing that didn't make it into the paper, but I thought might be useful for the future, [I would save it]. But I've had to move offices several times, and I never had whatever old notes I had organized well enough to save them in a systematic way. I'm afraid I don't have anything to contribute to the archives from that.

PC: And no correspondence with Phil, Richard, David Sherrington throughout those years either?

JH: [1:08:36] Probably all my emails with everybody are still on Niels Bohr Institute somewhere. What I have here on my desk is only as old as this machine because I had trouble transferring old mail. That goes back seven years. It's getting to be an old machine. Next time I can get back to Stockholm to Nordita I intend to replace it, but it's still working fine for now. Anyways, that's why my correspondence goes back [seven years] at home. I do save all of that, but I have to say I haven't looked at old stuff for a long time.

PC: Thank you very much for your time for this conversation.

JH: [1:09:33] Thank you. It's been interesting talking to you.

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[72] A. Crisanti and L. Leuzzi, "Spherical 2+*p* spin-glass model: An exactly solvable model for glass to spin-glass transition," *Phys. Rev. Lett.* **93**, 217203 (2004). https://doi.org/10.1103/PhysRevLett.93.217203