History of RSB Interview: A. Peter Young

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PC: Good morning, Peter. Thank you very much for joining us. As we discussed ahead of time, the theme of this interview is the replica symmetry breaking period, from 1975 to 1995. But to get to that, we will start with a few background questions, if you don't mind. What led you to physics and then to pursue a PhD in theoretical physics?

PY: I think it was because I wasn't much good at anything else, actually. I never was able to remember very much stuff. Foreign languages—my apologies for my previous attempts to speak French—and history [had] too many things to remember. But for physics you don't have to remember so much. You have to understand very well some basic stuff, and everything follows from that. Why a theorist? Because I've never been very good with my hands, so I thought I would be better off doing theory than experiments.

PC: Towards the end of your time in Oxford, you got interested in the renormalization group, like many people at the time. How did you come to that?

PY: [0:01:27] I actually spent a very long time in Oxford. At that point I was a postdoc. It was clear that the renormalization group was something important, but it wasn't so simple for those of us outside that area to understand. So there was a series of sort of self-help lectures organized by one of the then-faculty members there, Gillian Gehring[1]. She organized these, and different people would talk on a paper that they had read to do with the renormalization group. I got a bit familiar with it then. Then, I realized

[1] Gillian Gehring: https://en.wikipedia.org/wiki/Gillian_Gehring

that [for] the problem that I had done my thesis work on[2]—which is the Ising model in a transverse field, and for which earlier work of my thesis advisor, Roger Elliott[3], and others had found numerically from series expansions and stuff that the quantum transition was like the thermal transition but in one higher dimension—that with the renormalization group you could see that [relationship] very easily. So I wrote a little paper[4]. Then, I realized that a more detailed work along similar lines but for a different topic, that is itinerant magnets, had been done by John Hertz[5].

PC: And from that you quickly realized that you could use real-space renormalization group to study the Edwards-Anderson model in finite dimension. Could you tell us how you heard about this model, and where did this idea come from?

PY: [0:03:16] You mean the Edwards-Anderson model?

PC: Yes.

PY: Again, I have a memory of it being one of these self-help lectures, in which we also learnt about the renormalization group. Maybe it was in that series or not in that series, but somebody gave a talk on this paper by Edwards and Anderson[6]. I remember that the replica part, which was presumably Edwards' parts, seemed very serious. There was actually another part—which nobody talked about since then and which is probably a bit of Anderson—where he talked about dynamics and factoring dynamical correlations. [This] at the time seemed maybe more intuitive, but that never caught on. It was the replica approach that definitely caught on.

PC: So did you just connect the two: the renormalization group, that you had been working on, and this model?

PY: [0:04:13] Yes. From these self-help lectures, one was on the real-space renormalization group, with Niemeyer and van Leeuwen[7], and then it was not a big step to realize that for disordered systems what you had to do is

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[2] A. P. Young, *Phase Transitions in Spin-phonon Systems*, PhD Thesis, Oxford University (1973). http://solo.bodleian.ox.ac.uk/permalink/f/89vilt/oxfaleph019435187

[3] Robert Elliott: https://en.wikipedia.org/wiki/Roger_Elliott_(physicist)

[4] A. P. Young, "Quantum effects in the renormalization group approach to phase transitions," *J. Phys. C* **8**, L309 (1975). https://doi.org/10.1088/0022-3719/8/15/001

[5] John A. Hertz, "Quantum critical phenomena," *Phys. Rev. B* **14**, 1165 (1976). https://doi.org/10.1103/PhysRevB.14.1165

[6] S. F. Edwards and P. W. Anderson, "Theory of spin glasses," *J. Phys. F* **5,** 965-74 (1975). https://doi.org/10.1088/0305-4608/5/5/017

[7] T. Niemeyer and J. M. J. van Leeuwen, "Wilson theory for 2-dimensional Ising spin systems," *Physica* **71**, 17-40 (1974). https://doi.org/10.1016/0031-8914(74)90044-5

do a rescaling of the distribution of the interactions. That was in principle what you had to do. Maybe in practice you had to make some approximations, like considering a few moments, but in principle what you had to do was this transformation of the distribution. This is about when I went as a postdoc to Grenoble, to the Institut Laue-Langevin.

PC: Were you in touch with David Sherrington at that point? He had been on your thesis committee, right?

PY: [0:05:03] Yes. That's right. He was on my thesis committee. I would say at that point I was not in touch with David Sherrington. That came a bit later, when we were both together at Imperial College. I was at Imperial from '78 to '85, and during that time David was also at Imperial College.

PC: In your contribution to Roger Elliott Festschrift you wrote that he "had a big influence in convincing [you] that [disordered systems] was a rich and challenging area."[8] Was this early on in your study?

PY: [0:05:43] This was when I was a graduate student. Roger was working on disordered electronic systems, things like the coherent potential approximation. I guess I realized from him that it wasn't just dirt, that you had to try to understand enough to push it out of the way. There was new physics in the disorder which one should try to understand. This came very much to fruition in the spin glass field, where a whole lot of new stuff appeared.

PC: Had he been specifically interested in spin glasses?

PY: [0:06:25] No. I don't ever think he worked on spin glasses.

FZ: You mentioned your stay in Grenoble. We interviewed other people who mentioned it as being place of ideas. Can you tell us a little bit what was the atmosphere in Grenoble? Who was there? Who you collaborated with?

PY: [0:06:55] Yeah. So I was in Grenoble from '75 to '77. The head of the group was [Philippe] Nozières[9]. I never wrote a paper with Nozières, but I felt he was always very supportive of my work. One of the people who was there

[8] Symposium in honor of the sixtieth birthday of Roger Elliott, Oxford, UK, July 10-12, 1989. Peter Young, "Spin glasses" In: *Disorder in Condensed Matter Physics*, J. A. Blackmail and J. Tagueha Eds. (Oxford: Oxford University Press, 1991).
[9] Philippe Nozières: https://en.wikipedia.org/wiki/Philippe_Nozi%C3%A8res

was Byron Southern[10]. He's now been in Winnipeg for many years. We developed a bit more the real-space renormalization group for disordered systems like spin glasses[11]. Curiously, the person who replaced me at the ILL—I think we overlapped for one day—was none other than Duncan Haldane[12], who went on—while he was still at Grenoble—to develop the idea of the Haldane gap.

PC: Byron had been a postdoc of David Sherrington also before. Was there a Sherrington connection there?

PY: [0:08:05] Yes. I guess there was a Sherrington connection there, maybe through Byron. I'm not remembering exactly how these papers on spin glasses developed. I imagine it was definitely through Byron, and his influence and David Sherrington's, that we probably got started working on spin glasses together. I know that at the same time Robin Stinchcombe[13], in Oxford, was working on spin glasses. (Elliott was not, but Stinchcombe was.) It turned out we'd both been doing various things independently, so we decided to write it up[14]. I think that the first paper I wrote on real-space renormalization group for spin glasses was actually with Stinchcombe[15]. Then, I continued further with Byron Southern.

PC: In early 1979, you wrote a review about spin glasses, in which you argued that based on the result of the RSB scheme of Bray and Moore, one should expect an infinite number of symmetry breaking in the ordered phase[16]. What gave you that confidence? Did you actually anticipate the Parisi result?

PY: [0:09:18] No. I didn't anticipate the Parisi result. You've got me there, I have to admit. I don't really remember that. I know there's an early replica symmetry breaking idea of Bray and Moore which seemed quite bizarre, but you did have the idea of dividing these replicas into blocks. What it

[10] Byron Wayne Southern (1947?-). See, *e.g.*, Byron Wayne Southern "Magnetoelastic Effects in Rare-Earth Metals and Compounds," PhD Thesis, McMaster University (1973).
[11] B. W. Southern and A. P. Young, "Real space rescaling study of spin glass behaviour in three dimensions," *J. Phys. C* **10**, 2179 (1977). https://doi.org/10.1088/0022-3719/10/12/023
[12] Duncan Haldane : https://en.wikipedia.org/wiki/Duncan_Haldane
[13] Robin B. Stinchcombe: https://academictree.org/physics/peopleinfo.php?pid=82545 (Last consulted February 17, 2021).
[14] A. P. Young and R. B. Stinchcombe, "A renormalization group theory for percolation problems," *J. Phys. C* **8**, L535 (1975). https://doi.org/10.1088/0022-3719/8/23/001
[15] A. P. Young and R. B. Stinchcombe, "Real-space renormalization group calculations for spin glasses and dilute magnets," *J. Phys. C* **9**, 4419 (1976). https://doi.org/10.1088/0022-3719/9/24/012
[16] Invited review paper presented at the 24th Annual Conference on Magnetism and Magnetic Materials, November, 14-18 1978, Cleveland, Ohio, USA. A. P. Young, "Fluctuation effects in spin glasses," *J. Appl. Phys.* **50**, 1691-1694 (1979). https://doi.org/10.1063/1.327239

didn't have was the infinite hierarchy that the Parisi scheme had. I'm afraid I don't remember what motivated me to write that comment. Could you tell me which review you're talking about?

PC: I have it here. It was for the Conference on Magnetism and Magnetic Materials in Cleveland, Ohio. You wrote that review that got published in early '79, in the Journal of Applied Physics.

PY: [0:10:18] I will have to go back. You're better aware of my work than I am, obviously. I don't know what induced me to make that comment, which sounds prophetic, but surely at the time I had no idea that he was going to come out with this amazing scheme[17].

PC: How did you find out, then, about the Parisi result? And was your reaction to it?

PY: [0:10:44] Well, it came out in bits and pieces, didn't it[18]? There wasn't just one paper, where it was all done finished and polished. It came out in several papers, and I was just aware of the papers as they were coming out. I thought the idea of this infinite hierarchy was very interesting, but—I suppose like everybody else—what it actually meant physically was very mysterious. It was only later that we understood how one could interpret these functions that were in the Parisi theory. I suppose the crucial thing, when you started thinking "Well, maybe this is right" was when he was doing his sequence of approximations—as you know the full equations are extremely complicated and you can't really solve those analytically—the first of them was the replica symmetric expression of Sherrington-Kirkpatrick, and that gave the negative entropy[19]. That was the smoking gun, the negative entropy. It was almost like a little afterthought in the Sherrington and Kirkpatrick paper, but that was actually the most important thing in it. The rest was pretty much doing for Ising spins whatever Anderson had done for Heisenberg spins. From this sequence of approximations of Parisi, he found that at very low temperatures the negative entropy diminished in magnitude. It looked reasonably plausible that, if you could extrapolate to an infinite hierarchy, then the entropy would go to zero, and that would

[17] **PY**: Looking at the paper later on, I see that the Bray-Moore theory, which has one level of RSB, kills the $O(t^2)$ instability of RS theory (t being the reduced temperature), but leaves an $O(t^3)$ instability. Hence it didn't take a step of brilliance to postulate that to correct the instability to all orders might require an infinite number of levels of RSB. I certainly did not envisage a theory with the richness of Parisi's.

[18] *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

[19] David Sherrington and Scott Kirkpatrick, "Solvable model of a spin-glass," *Phys. Rev. Lett.* **35**, 1792 (1975). https://doi.org/10.1103/PhysRevLett.35.1792

be satisfactory. That really was what induced one to think: “Maybe this is the right answer".

PC: Shortly after the first couple of those papers, you started collaborating with Scott Kirkpatrick on doing computer simulations of the SK model[20]. And shortly after that you had your first PhD student, Neil Mackenzie[21], who did Monte Carlo simulations on the SK model[22]. What led you to this computational approach and down this path?

PY: [0:13:00] I think the analytical approach with replicas seemed so strange that I thought: “Maybe with some careful numerics one could get a little bit of intuition.” I guess Scott Kirkpatrick invited me to spend a month, or so, in the summer at Yorktown Heights, where he was then. We wrote a paper together on some numerics.

That was quite interesting. I had been working at Imperial College, developing a code to do whatever aspect of spin glasses, and I wanted to take this code with me and run it on all the big computers at Yorktown Heights. But that was well before the internet. How do I actually bring this code? The professional way to do it would be magnetic tape in those days, but every tape system was different. There was no standardization, so that was likely to be difficult. So I went back to what even then was fairly old technology: punched cards. I got a whole bunch of punched cards printed out in England and took them on the plane with me. There was still a punched card reader at Yorktown Heights, and everybody gathered around to see it being used. The cards were fed in and then we could develop a bit the code there, and run it. That was a little historical aside.

PC: So you had computational experience before then. How did that come about in your case?

PY: [0:14:45] When I was at Imperial College, which is part of the University of London, there was a British computer company called ICL, International Computers Limited[23]. They had a really amazing parallel processor called distributed array processor. It consisted of a 64x64 array of very small one-

[20] See, *e.g.*, A. P. Young, S. Kirkpatrick, “Low-temperature Behavior of the Infinite-Range Ising Spin Glass: Exact Statistical Mechanics for Small Samples,” *Phys. Rev. B* **25**, 440 (1982). https://doi.org/10.1103/PhysRevB.25.440

[21] Neil Mackenzie, *Studies of Phase Transitions in Frustrated Magnetic Systems*, PhD Thesis, Imperial College London (1982). https://library-search.imperial.ac.uk/permalink/44IMP_INST/2e5g7s/cdi_imperial_dspace_oai_spiral_imperial_ac_uk_10044_1_36336

[22] N. D. Mackenzie and A. P. Young, "Lack of ergodicity in the infinite-range Ising spin-glass," *Phys. Rev. Lett.* **49**, 301 (1982). https://doi.org/10.1103/PhysRevLett.49.301

[23] International Computers Limited : https://en.wikipedia.org/wiki/International_Computers_Limited

bit processors which ran in parallel[24]. This was actually perfect for doing Ising spin glass simulations. The lattice structure was there, built into the hardware. I got into simulating two-dimensional or three-dimensional spin glasses using this distributed array processor. I've always regretted that… For these sort of Ising simulations it was really a very nice machine, but as with the typical British business the technology was very good, but the management and the marketing was very poor. It was never developed; it just faded away. It was clear, right from the early days, that it was very tough analytically and that numerics was going to have a really important role. I said: "I should get into this, particularly with Monte Carlo simulations".

PC: Had Imperial College bought one of those machines, or did the company give you access?

PY: [0:16:23] No. There was one at another of the colleges of University of London, Queen Mary College. I used it there.

PC: You also started to collaborate with Kurt Binder shortly thereafter, and that led to your writing your magnum opus—in a sense—the Reviews of Modern Physics paper[25].

PY: [0:16:49] That's right. I forget how these visits were arranged, but I was able to visit Kurt Binder in Mainz in at least a couple of occasions. I think one connection there was… Well, at the beginning of my time in Santa Cruz I had a postdoc called Joseph Reger[26], who then went back to Germany and worked with Kurt Binder. Then Binder invited me to come to Mainz and work with him. I reckon that must have been after the review, what was the impetus for the review?

PC: Kurt mentioned that both he and you had been asked by RMP to write a review, and you decided to join forces.

PY: [0:17:41] That rings a bell. I think that's right. Joining forces. Well, the efficiency with which those forces were activated was quite different. Kurt had written his part of the paper very quickly and efficiently. Then, I had just started in Santa Cruz. He kept pushing me and pushing me: "When are you going to do your bit?" Eventually it got done. It was obviously a lot of work,

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[24] ICL 2900 Series : https://en.wikipedia.org/wiki/ICL_2900_Series

[25] Kurt Binder and A. Peter Young. "Spin glasses: Experimental facts, theoretical concepts, and open questions," *Rev. Mod. Phys.* **58**, 801 (1986). https://doi.org/10.1103/RevModPhys.58.801

[26] Alex Scroxton, "Interview: Fujitsu CTO Joseph Reger on human-centric innovation," ComputerWeekly.com, 28 Nov 2014. https://www.computerweekly.com/feature/Interview-Fujitsu-CTO-Joseph-Reger-on-human-centric-innovation (Last consulted February 24, 2021)

even in some sort of mundane aspects. Now if you want to keep track of citations it's really easy, because you're using LaTeX. You just have a label for each citation and the software keeps track of it. We didn't have that then. Everything was typed up by hand. We were having to try to keep track by hand which was the paper Bray and Moore 1978a and 1978b, and so on. This was a nightmare to keep track of all those references, which would have been more straightforward now. This has been cited quite a lot. I'm pleased by that. Of course, it's massively out-of-date, but that's inevitable.

PC: At about the time the review came out, you started working more closely with Allan Bray and Mike Moore as well. Is this connection more obvious to you?

PY: [0:19:24] Not really. How did that happen? Before I went to the US, I had been studying in the UK, and working as a postdoc in the UK and then at Imperial College so I ran into Mike Moore quite a lot. We had this common interest in spin glasses, so it was pretty natural that we might try to join forces from time to time. I don't remember exactly now how that happened. We did one or two papers with Mike Moore.

PC: You were pretty interested with the droplet model, I presume, at that time. Is that where it emerged?

PY: [0:20:06] Yeah. That's right. How does one characterize the spin glass state? The droplet model or replica symmetry breaking or something else. Let me try and recollect my thoughts here. Mike was always pretty skeptical—less so now—about replica symmetry breaking. The droplet model, he felt, was more useful. I'm just trying to recall the paper we did on replica symmetry breaking, now. What did we do there? I should have gone through my publications first.

PC: You have a paper entitled "Lack of self-averaging in spin glasses[27]," which might be the first, and then "Weighted averages of TAP solutions[28]…"

PY: [0:21:36] That's right. The lack of self-averaging. This was about the same time as Mézard *et al*. came out with the very elegant ultrametric structure

[27] A. P. Young, A. J. Bray and M. A. Moore, "Lack of self-averaging in spin glasses," *J. Phys. C* **17**, L149 (1984). https://doi.org/10.1088/0022-3719/17/5/005

[28] A. J. Bray, Michael A. Moore and A. Peter Young, "Weighted averages of TAP solutions and Parisi's *q* (*x*)," *J. Phys. C* **17**, L155 (1984). https://doi.org/10.1088/0022-3719/17/5/006

of the Parisi solution[29]. One part of that is that you don't have self-averaging for certain quantities. That little tiny bit of their big work, I had figured out independently that you didn't have self-averaging; maybe this would help understand certain aspects of the Sherrington-Kirkpatrick model, some of the numerics. Then there were some other difficulties comparing the TAP solutions with the results of numerics and doing full thermal averages. Within the TAP solution the deviation of the order parameter from one at low temperatures went like T^2—if I remember correctly—whereas the overall average went like T. When you're doing the Gibbs average, you weren't just taking a single one of the TAP solutions. You had to do a thermal average of different TAP solutions, and then you could reconcile these different results[30].

PC: In the middle of all this, you moved to UC Santa Cruz, around 1985. Was there any spin glass connection to that move, or was this completely independent?

PY: [0:23:29] No. There was no spin glass connection there. I thought that going to the States would be challenging. It would be good for my career, and I think that it was. I also thought that Santa Cruz would be a good place to live and have children grow up. Where I grew up in England was Lancaster, which is a small town. Before going to Santa Cruz I was seven years in London. Although I now very much like going back to London and go to the theatre and all that, at that time living in London to me was not great. On a poor lecturer salary you could not live in the center of London, you had to commute. I hate commuting. Also with young children you couldn't really take advantage of the cultural amenities that the city had. Santa Cruz is a small town, similar in size to Lancaster, the town that I grew up in the north of England. Of course, because it's small town there was somewhat limited amenities there, but we're not far away from San Francisco, which is my favorite American city. We could, and did until COVID, take advantage of all the cultural amenities of San Francisco, go to the Symphony Orchestra, the Theatre and Opera and so on. It was a bit like having the best of both worlds. You had the advantages of a small town. For about 25

[29] 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. Phys. France* **45**, 843-854 (1984). https://doi.org/10.1051/jphys:01984004505084300

[30] A. P. Young, "The TAP equations revisited; a qualitative picture of the SK spin glass model," *J. Phys C* **14**, L1085 (1981). https://doi.org/10.1088/0022-3719/14/34/004 **PY**: I remember Phil Anderson telling me that he found this to be a useful paper, but I don't remember when he told me that. Incidentally, Parisi's theory contains both of these cases. The average in a single TAP solution is $q(1)$, while the Boltzmann average is $\int_0^1 q(x)dx$.

years, I would bike to work. But we could travel to the big city when we wanted to.

PC: Shortly after arriving at Santa Cruz, you co-organized, with John Hopfield, a meeting at the ITP—the Institute of Theoretical Physics[31].

PY: [0:25:21] That's right. I was invited to do that. That was interesting and challenging to try to see the connections between spin glasses and the burgeoning field of neural networks, which John Hopfield was paramount in providing a sort of Ising-type model of a neural network. This sort of horrified the biologists at the time, as I understand it, because it clearly wasn't very realistic biology, but it provided a lot of useful insights. So that was quite stimulating, to attend those meetings. There were people from a variety of fields, including people working on real neurons and so on.

I think this sort of statistical mechanics approach to neural networks seemed to me had its heyday about that time, in the late '80s, and then it rather diminished. But neural networks have now come back again, in a big way, with artificial intelligence. To me, that's been quite interesting. It never seemed to quite pan out 25 years ago. Why is it panning out now? Maybe it's just because of the size of the computers and the size of the databases… If you really beat it, it works. Those big computers and big datasets were not available in the late '80s and '90s.

PC: Despite organizing the workshop, you never jumped on the neural network bandwagon. Was there a reason for that?

PY: [0:27:08] Just a lack of any good ideas on what to do. Just working on certain abstract stat mech model, there were a lot of people doing that and I didn't feel I could contribute in that area. And I wasn't sure it was very useful. I didn't really have any sort of biology people I knew who could provide me with some stimulation with experiments. "Why don't you explain these results?" I never jumped on that bandwagon, that's right.

PC: At that point, you embarked instead on a long-term collaboration with Ravindra Bhatt, from Princeton,[32] on the simulation and finite-size scaling analysis of spin glasses.

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[31] John Hopfield and Peter Young, "Spin Glasses, Computation, and Neural Networks" September to December 1986 Institute for Theoretical Physics, University of California at Santa Barbara. 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).

[32] Ravindra N. Bhatt : https://academictree.org/physics/peopleinfo.php?pid=188110 (Last consulted February 24, 2021)

PY: [0:27:56] That had actually started earlier, when I was at Imperial College. Ravin had a sabbatical from Bell Labs; he was at Bell Labs then. He spent several months at Imperial, and we started working together. He actually came with some very interesting problems, which I didn't understand about antiferromagnetic spin chains. This is a field which afterwards developed very much with the work of people on these local singlet states and stuff like that. But at the time I didn't understand any of that, and we never worked on that.

We started [instead] working on Monte Carlo simulations of 3D spin glasses[33]. It was clear that what we wanted was a better way of doing finite-size scaling than what was being done at that time. To try to locate more convincingly whether or not there was a phase transition in the three-dimensional Ising spin glass. The idea is, rather than try to fit at the same time the transition temperature and the critical exponents, you try to determine them one at a time. One way of doing that is to find some sort of dimensionless quantity. The data for different sizes then intersect at the critical temperature if your neglect corrections to scaling. I thought about trying various ratios of moments of the Edwards-Anderson order parameter, Q. Because there is a trace Q^3, which is not zero in the spin glass, I tried computing the ratio of the average of Q^3, suitably defined, divided by the 3/2th power of the average of Q^2. The data for that didn't really intersect, it sort of came together and merged. Then I saw a paper by Binder—for the ferromagnet—calculating the M^4 divided by the square of the second power[34]. I thought: "Well, that's the same idea and that's sort of more familiar, so we'll do that for the spin glass: the fourth-power of Q divided by the square of the second power of Q." That's what got published. That's what people have done since then. Actually the results at that time were very similar. The data merge as you lower the temperature, but didn't splay out as you increase the size below the transition temperature.

That was while I was still at Imperial College. We continued a little bit more on that after I went to Santa Cruz.

PC: Did Ravin know anything about spin glasses before he came to that visit, or did you introduce him to that?

PY: [0:31:04] I'm sure he knew something about it. He didn't work on it before. So yes, I introduced him to spin glasses, I recollect.

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[33] 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/PhysRevB.37.5606

[34] Binder Cumulant : https://en.wikipedia.org/wiki/Binder_parameter

PC: You were doing simulations at that time, but that's also when specialized computer architectures started to appear to simulate spin glasses. Andy Ogielski's machine at Bell labs, in particular[35]. Were you concerned that you didn't have access to those machines to keep on competing? How did you face that problem?

PY: [0:31:45] I felt that special machines definitely had a place. Certainly, at that point computer hardware was developing extremely fast. If somebody spent maybe two years developing a specially-dedicated computer, the design at the beginning would have been much faster than what you could get from a cluster of workstations, but by the time the special computer was built it wasn't obviously that much superior. So I felt it was still true that one could do interesting work with a bunch of workstations. That's still true. Of course, these special dedicated computers are very much continuing. There's this huge machine of the Rome and Zaragoza group led by Parisi, which does monumental calculations that would not be feasible, I think, for anybody else[36]. I would say now there are things that they can do that I don't think anybody else could do because this machine is just so awesome and powerful.

PC: They have beaten Moore's law in that sense.

PY: I guess so, yes.

PC: You have maintained the study of spin glasses funded through NSF essentially without interruption. Was that ever a challenge? How receptive was NSF in supporting research on spin glasses?

PY: [0:33:27] I think it was becoming increasingly not very receptive to doing it. There were other things that I was working on. In particular, I would go into the quantum world and do disordered quantum phase transitions[37]. That was something of new development. I think the NSF was more amenable to that than to traditional spin glasses. I would say it was… In addition, I was doing—in the not so distant past—quantum annealing[38]. Also

[35] Andrew T. Ogielski and Ingo Morgenstern, "Critical behavior of three-dimensional Ising spin-glass model," *Phys. Rev. Lett.* **54**, 928 (1985). https://doi.org/10.1103/PhysRevLett.54.928

[36] Janus Supercomputer : http://www.janus-computer.com/ (Last consulted February 24, 2020)

[37] *E.g.,* Heiko Rieger and A. Peter Young, "Zero-temperature quantum phase transition of a two-dimensional Ising spin glass," *Phys. Rev. Lett.* **72**, 4141 (1994). https://doi.org/10.1103/PhysRevLett.72.4141

[38] *E.g.*, A. P. Young, S. Knysh and V. N. Smelyanskiy, "First-order phase transition in the quantum adiabatic algorithm," *Phys. Rev. Lett.* **104**, 020502 (2010). https://doi.org/10.1103/PhysRevLett.104.020502; Itay Hen and A. P. Young, "Solving the graph-isomorphism problem with a quantum annealer," *Phys. Rev. A* **86**, 042310 (2012). https://doi.org/10.1103/PhysRevA.86.042310

going into quantum land, but in a different area, trying to see by doing simulations on a computer how efficient might be a quantum annealer of the sort made by the company D-Wave[39]. That looked better. I also got some funding from the Army Research Office for that work. But I should say funding from the NSF world was not easy. There was definitely one major hiccup when, for some reason, they were only able to get three reviewers. Normally they get five or six, I would say. They all gave it a rating of excellent, yet the proposal was turned down. I was quite unhappy about that. In the US, this is fairly serious because the students need the support to live. Very much at the last minute this funding was turned off. The program director said: "Well, even though they said excellent, when you actually look at what they wrote it wasn't as good as that." But I think this was definitely part of the fact that the NSF wasn't that interested in funding spin glasses. Although there were a lot of other stuff in the proposal in addition to spin glasses, there was also a spin glass side.

PC: Can you situate us a bit in time? When would that have happened? When did the winds start to change direction?

PY: [0:35:39] I think it changed quite a long time ago. It also, I think, depends a bit on the program manager. The program manager changed after about 10 years of my being in the US. He [used to be] very supportive[40], and then the new program manager, Daryl Hess, I found was less supportive. That would have been in the [1990s][41].

PC: You've kept on working on spin glasses for decades—and other things as well, obviously—but the question is how did you become so enamored with them? In other words, how do you remain motivated to hit on this problem?

PY: [0:36:30] At times, there seems to be not much one can do, and that's a good time to work on other stuff. Then some new development and some new ideas [emerge]. For example, the big question as to whether the replica symmetry breaking is applicable in real short-range spin glasses—in three dimensions, [in particular]—is still unresolved.

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[39] D-Wave Systems Inc. : https://en.wikipedia.org/wiki/D-Wave_Systems

[40] G. Bruce Taggart was Program Director of Materials Theory from at least 1990 until 1997. His prior research had a relatively strong statistical mechanics component. See, *e.g.*, *Grant Opportunities for Chemists*, (Washington, D.C. : National Science Foundation, [1990]), 14. G. Bruce Taggart, "Computational materials research at the national science foundation," *Comput. Mater. Sci.* **2**, 143-148 (1994). https://doi.org/10.1016/0927-0256(94)90057-4

[41] Daryl W. Hess joined NSF as Program Director of Materials Theory in 1997 and became permanent in 2000. See, *e.g.*, https://www.techconnectworld.com/World2015/bio.php?id=103 (Last consulted February 25, 2021)

A characteristic feature of replica symmetry breaking is the de Almeida-Thouless line, the line of transitions in a magnetic field. Then I realized: "Well, actually, one could quite straightforwardly, at least in principle, Monte Carlo that." There's a quantity, which is like a susceptibility—which is actually what de Almeida and Thouless calculated—a sort of spin glass susceptibility in a magnetic field. You could calculate this in a standard way using Monte Carlo simulations. To avoid systematic effects, you need four copies because you have products of four thermal averages, but this you can do. As I mentioned earlier, you would like dimensionless quantities to do the finite-size scaling, and you can also find a dimensionless quantity, sort of a correlation length. [If] you divide that by the system size, you can do a nice finite-size scaling. I put quite a bit of effort into doing this[42]. For what it's worth, those simulations seem to indicate that in high dimensions it looked like you do have a de Almeida-Thouless line, and probably not in low dimensions. From time to time some new idea comes up, and then one has another push at it.

Right now, as you know, I'm retired and I'm not really giving a hard push on anything. But I'm thinking that if I wasn't retired I wouldn't be spending a huge amount of time on spin glasses. I, at least, don't have any particular idea on how to solve these major problems of the nature of the spin glass state. Certainly, I can't compete with the numerics that the Rome-Zaragoza group can do. Right now, I'm not really doing spin glasses. I do a little bit on quantum computing and that's about it.

PC: You were never interested in the structural glass problem, right?

PY: [0:38:58] That's not quite true. It's one thing to be interested, it's another thing to figure out how one can make a contribution. I've read around quite a bit, and learnt about the Kauzmann paradox[43], and all these sort of stuff. It seemed to be really quite mysterious. I did think about the question of whether, if you could wait an infinite amount of time, there will be an ideal glass transition. Or course, you never really get there. There were some

[42] *E.g.*, A. P. Young and Helmut G. Katzgraber, "Absence of an Almeida-Thouless line in three-dimensional spin glasses," *Phys. Rev. Lett.* **93**, 207203 (2004). https://doi.org/10.1103/PhysRevLett.93.207203; Helmut G. Katzgraber and A. Peter Young, "Probing the Almeida-Thouless line away from the mean-field model- *Phys. Rev. B* **72**, 184416 (2005). https://doi.org/10.1103/PhysRevB.72.184416; Helmut G. Katzgraber, Derek Larson and A. P. Young, "Study of the de Almeida–Thouless line using power-law diluted one-dimensional Ising spin glasses," *Phys. Rev. Lett.* **102**, 177205 (2009). https://doi.org/10.1103/PhysRevLett.102.177205; Derek Larson, Helmut G. Katzgraber, M. A. Moore and A. P. Young, "Spin glasses in a field: Three and four dimensions as seen from one space dimension," *Phys. Rev. B* **87**, 024414 (2013). https://doi.org/10.1103/PhysRevB.84.014428

[43] Kauzmann's Paradox: https://en.wikipedia.org/wiki/Glass_transition#Kauzmann's_paradox

papers of Mike Moore, which I thought were quite interesting[44]. He was arguing that maybe the structural glass is a bit like a spin glass but in a magnetic field. Of course, there's no quenched disorder in a structural glass, somehow the system makes its own disorder. In a magnetic field a spin glass, [doesn't have] an up-down symmetry. It's quite strange that you have a phase transition, but there's no symmetry change, you don't have up-down symmetry. If you think of a structural glass, you have density fluctuations which get frozen in, and there's no symmetry of these density fluctuations about the average density. Increasing the density by a bit is not exactly equivalent to decreasing the density by bit. Maybe there's some rough connection there between the putative ideal glass transition and the de Almeida-Thouless line of the transition in a magnetic field. If, as Moore argues, there is no de Almeida-Thouless line in three dimensions, then strictly speaking there would not be an ideal glass transition in three dimensions.

PC: From your career having bridged both sides of the Atlantic, do you have any insight about the difference in interest in replica symmetry breaking ideas in the US and in Europe?

PY: [0:41:18] The ideas, of course, originated in Europe. I think that's where the main interest has carried on. Giorgio has a big group, and he continues to be very active. There's a big group in Spain, with Víctor Martín-Mayor and others. And also people like Mike Moore, in England. I would say that there's not very much activity in the US. The activity there is tends to be of a more mathematical nature. I'm thinking of the work of Newman and Stein[45]. Their approach is more of a mathematical nature. I have to confess I've always found their work hard to follow. I think there's a difference of vocabulary between what they're talking about when they talk about a state and when I talk about a state. Maybe we don't mean exactly the same thing. This has created a certain amount of confusion. Reading their papers, they seemed in the past to be saying that they were pretty confident that RSB in some format could not exist in short-range spin glasses. Now, I think they've rather backed off that. This, in particular, has been looked at

[44] *E.g.*, M. A. Moore and J. Yeo, "Thermodynamic glass transition in finite dimensions," *Phys. Rev. Lett*. **96**, 095701 (2006). https://doi.org/10.1103/PhysRevLett.96.095701

[45] *E.g.*, Charles M. Newman and Daniel L. Stein, "Non-mean-field behavior of realistic spin glasses," *Phys. Rev. Lett.* **76**, 515 (1996). https://doi.org/10.1103/PhysRevLett.76.515; "Simplicity of state and overlap structure in finite-volume realistic spin glasses," *Phys. Rev. E* **57**, 1356 (1998). https://doi.org/10.1103/PhysRevE.57.1356; "Metastable states in spin glasses and disordered ferromagnets," *Phys. Rev. E* **60**, 5244 (1999). https://doi.org/10.1103/PhysRevE.60.5244; "Ordering and broken symmetry in short-ranged spin glasses," *J. Phys.: Condens. Matter* **15**, R1319 (2003). https://doi.org/10.1088/0953-8984/15/32/202;

again recently by Nick Read[46]. He has a picture in which it's perfectly feasible to have in short-range spin glasses a replica symmetric solution suitably defined for a short-range system[47].

Basically, there's not been all that much interest in replica symmetry breaking in the US. For example, most of the simulation work in recent decades has been in Europe.

PC: Do you think it's mostly center of gravity issue between the two continents, in term of where the people were trained?

PY: [0:43:29] Yes. The center of gravity is certainly in Europe and lots of people were trained in that area. I should of course have mentioned the Paris school and people like Mézard, and people around him, being very important in this area.

FZ: I wanted to ask another question of a similar nature. The communities working on classical disordered systems and on quantum disordered systems are quite disconnected. I think you are one of the few persons who explore both sides: the quantum and the classical. Do you understand why there is difficulty sharing ideas between the two communities? Did you try to export ideas from classical spin glasses to the quantum world? Do you feel that it's something interesting, and that you succeeded in doing that?

PY: [0:44:33] I certainly tried to work it; I have a foot in both camps. Whether I succeeded in interesting the quantum people to more traditional spin glass aspects, I guess I don't really know.

If you go to the quantum case, even if you don't have frustration but you have disorder and quantum mechanics, there's a lot of interesting physics which people wanted to sort out. That's not true to quite the same extent in the classical disordered, say Ising, model. For example, the Griffiths singularities are extremely important in the quantum case and they can give you big, measurable effects even in the paramagnetic phase at low temperatures[48]. Although, in principle, Griffiths singularities exist in classical magnets above the transition temperature, their effects are extremely

[46] Nicholas Read : https://en.wikipedia.org/wiki/Nicholas_Read

[47] E.g., N. Read, "Short-range Ising spin glasses: the metastate interpretation of replica symmetry breaking," *Phys. Rev. E* **90**, 032142 (2014). https://doi.org/10.1103/PhysRevE.90.032142

[48] H. Rieger and A. P. Young, "Griffiths singularities in the disordered phase of a quantum Ising spin glass," *Phys. Rev. B* **54**, 3328 (1996). https://doi.org/10.1103/PhysRevB.54.3328; C. Pich, A. P. Young, H. Rieger and N. Kawashima, "Critical behavior and Griffiths-McCoy singularities in the two-dimensional random quantum Ising ferromagnet," *Phys. Rev. Lett.* **81**, 5916 (1998). https://doi.org/10.1103/PhysRevLett.81.5916

weak. You get some sort of essential singularity which is not measurable in experiments or in simulations.

The additional effects of frustration, at least in some cases, are argued to not make all that difference. For example, for the one-dimensional transverse field Ising model with disorder, Daniel Fisher has this very beautiful theory with the infinite random fixed point, where when you rescale a distribution doing some sort of real-space renormalization group, the distributions of the random transverse field and the random interactions get broader and broader[49]. So you can figure out what's going on by tracing out, in perturbation theory, the largest term in the Hamiltonian. If you have that situation, where just, say, the largest interaction dominates, then it's argued that frustration is not playing very much of an additional role. I think this is one of the reasons why the spin glass [physics] is not taken into the quantum community so much.

Though, in the fairly new field of many-body localization, I've seen work where the different phases one can have are related to spin glasses. I think there is some interest there. In particular, one of the people who is very important in many-body localization is David Huse[50]. He has, of course, done a lot of important work in the past on spin glasses with Daniel Fisher[51], the major proponent of the droplet picture. There, there's definitely some intuition in spin glasses being taken over to an important quantum problem, many-body localization.

FZ: What about quantum computing? You have recently worked a lot on that, and that's a place where, in principle, spin glass ideas about dynamical slowing down, barriers, and nontrivial structure of phase space could play a role.

PY: [0:48:23] Quantum computing means different things to different people. There's the traditional gate or circuit model of quantum computing, for which [one has] the famous algorithm of Shor for factoring integers[52]. This

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[49] See, e.g., Daniel S. Fisher, "Random transverse field Ising spin chains," *Phys. Rev. Lett.* **69**, 534 (1992). https://doi.org/10.1103/PhysRevLett.69.534; "Critical behavior of random transverse-field Ising spin chains," *Phys. Rev. B* **51**, 6411 (1995). https://doi.org/10.1103/PhysRevB.51.6411; Daniel S. Fisher and A. P. Young, "Distributions of gaps and end-to-end correlations in random transverse-field Ising spin chains," *Phys. Rev. B* **58**, 9131 (1998). https://doi.org/10.1103/PhysRevB.58.9131

[50] E.g., Arijeet Pal and David A. Huse, "Many-body localization phase transition," *Phys. Rev. B* **82**, 174411 (2010). https://doi.org/10.1103/PhysRevB.82.174411; David A. Huse, Rahul Nandkishore, Vadim Oganesyan, Arijeet Pal and Shivaji L. Sondhi, "Localization-protected quantum order," *Phys. Rev. B* **88**, 014206 (2013). https://doi.org/10.1103/PhysRevB.88.014206

[51] *E.g.*, Daniel S. Fisher and David A. Huse, "Ordered Phase of Short-Range Ising Spin-Glasses," *Phys. Rev. Lett.* **56**, 1601 (1986). https://doi.org/10.1103/PhysRevLett.56.1601

[52] Shor's algorithm: https://en.wikipedia.org/wiki/Shor%27s_algorithm

uses quantum parallelism to try to get a quantum speed up. Quantum parallelism is extremely susceptible to the slightest bit of decoherence from the external noise, which inevitably is there.

The area that I was working on—I got interested in this by being invited to go to a conference on the topic in Key West, Florida[53]—was quantum annealing. Maybe [the name was] inspired by Scott Kirkpatrick's [so-called] thermal annealing or simulated annealing, to try to use quantum fluctuations to see if you could find the optimal solution to an optimization problem. I regard this is as an example of a sort of quantum simulator. The device is actually simulating a particular Hamiltonian, and the Hamiltonian is the classical optimization problem plus something like a transverse field which induces transverse fluctuations. On the one hand, there's no obvious reason—like for quantum parallelism—to expect a quantum speed up; on the other hand, there might be. You hope—and it's probably true—that you're not as sensitive to external noise. With these sort of quantum simulators—of which quantum annealers I would say is one type—you have a better chance of building a device in the near to intermediate term. In particular, the efforts of D-Wave of building a device with several thousand qubits… They do not claim that these maintain phase coherence during the time of the simulation, but they still claim that you can find a ground state efficiently. Whether it's more efficient or not than simulated annealing or some other classical method is not clear. Optimization problems are very much similar to spin glasses—in fact, finding a ground state of a spin glass is an optimization problem—and many of the toy models that we study are related to spin glasses. Definitely spin glass ideas have been involved in quantum annealing, which as I say worked on for a while but I haven't actually be working really on that recently.

PC: During your time at the Imperial and later at Santa Cruz, did you ever get to teach about replica symmetry breaking and spin glasses? If you did, could you give us a bit of details?

PY: [0:51:56] No. I don't recall. When I was teaching, it was the mainstream physics classes. Curiously, in Imperial College, in the math department, they had a mathematical physics group, who surprisingly were not at all the heavy-duty mathematical physics types. They were more phenomenological people like me and David [Murray] Edwards, who was working on itinerant magnets and so on. I was teaching a lot of classes of math for

[53] NASA International Conference on Quantum Computing and Many-Body Systems, Key West, Florida, USA, January 31-February 3, 2006.

engineers. I did some graduate classes, but that was in statistical mechanics, solid-state physics, that sort of thing. So I did not give a course on spin glasses or anything like.

As you already noted, I did give some review talks at conferences—you mentioned one—and also a number of schools, giving summaries of the spin glass work[54]. I've never given a course as such.

PC: Did you include notions of replica symmetry breaking in your stat mech classes, for instance?

PY: [0:53:09] I did not include replica symmetry breaking in the stat mech classes. They were at a more elementary level than that. Ising ferromagnet sort of level.

PC: Is there anything else that we may have skipped over that do you think was important from that epoch?

PY: [0:53:31] As we mentioned earlier, at first this Parisi scheme seemed so mysterious. This $q(x)$, what is this function? I have a little bit of a story about that. I spent some time also visiting Cirano De Dominicis[55] in Saclay. While I was there, also Parisi came. I remember we were talking, and we had some discussions about, in the SK model, if you take the spin-spin correlation function squared, that this is related to Parisi's function, as it is the integral of $q(x)$ square and so on. Some higher-order correlation is the integral of $q(x)$ to the fourth and so on. Parisi wasn't sure about what I was saying, but what I should have done is to realize that since I had understood what all the moments are, then I know what the distribution is. Somehow for me the penny did not click. That all I was saying was that the distribution is dx/dq. When I went back to Imperial College, then finally the penny did click. Giorgio went back to Rome and immediately wrote a paper with that and sent it to David Sherrington[56]. I felt, from a personal point of view, I should have been more on the ball, and I could have gotten credit for figuring out the physical significance of the Parisi result. I just missed

[54] *E.g.*, (i) Euroconference on Computer Simulation in Condensed Matter Physics and Chemistry, July 3-28 July 1995, Como, Italy. Proceedings: A. P. Young, "Phase Transitions in Random Systems," In: *Monte Carlo and Molecular Dynamics of Condensed Matter Systems*, K. Binder and G. Ciccotti Eds. (Bologna: Italian Physical Society, 1996). (ii) Computer Simulations in Condensed Matter Systems: From Materials to Chemical Biology, July 2005, Ettore Majorana Foundation and Center for Scientific Culture (EMFCSC), Erice, Italy. Proceedings: A.P. Young, "Numerical Simulations of Spin Glasses: Methods and Some Recent Results," *Lect. Notes Phys.* **704**, 31–44 (2006). https://doi.org/10.1007/3-540-35284-8_2

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

[56] Giorgio Parisi, "Order Parameter for Spin-Glasses," *Phys. Rev. Lett.* **50**, 1946 (1983). https://doi.org/10.1103/PhysRevLett.50.1946; A. Houghton, S. Jain and A. P. Young, "Role of initial conditions in spin glass dynamics and significance of Parisi's $q(x)$," *J. Phys. C* **16**, L375 (1983).

that last step, which Giorgio beat me to. It's not a shame to be beaten by Giorgio, so I can live with it[57].

PC: The last question is whether you still have notes, papers, correspondence from that epoch? If yes, do you intend to deposit them in an academic archive at some point?

PY: [0:55:30] Deposit them in an academic archive… I've never thought about that. I don't think I have all that much old correspondence. Some old emails, I have, but I'm not sure how far they go back. I don't think I would really want to go back and rummage through old emails and stuff like that to figure it out.

PC: If ever you find the time and energy, I strongly encourage you contact UC Santa Cruz first, to see if their rare manuscripts and books might take it.

PY: [0:56:18] Related to that, may I ask where the results of all your detailed endeavors are going to appear?

PC: We'll stop the recording, and then we can chat about that. Thank you so much for your time.

PY: You're welcome. Thanks for all those interesting questions.

[57] **PY** : It was a major step to understand the physical significance of Parisi's hitherto mysterious function $q(x)$, and that furthermore it could be computed in simulations. I did this for the SK model in A. P. Young, "Direct Determination of the Probability Distribution for the Spin-Glass Order Parameter," *Phys. Rev. Lett.* **51**, 1206 (1983). https://doi.org/10.1103/PhysRevLett.51.1206 It was gratifying that the results looked similar to the predictions of Parisi's theory, though with large finite-size corrections.

It took much longer to compute the order parameter distribution for a short-range spin glass, in part because it was not even clear that there is a finite-T_c in 3D for a long time afterwards. However, 4D is simpler and so we computed $P(q)$ for this model well below T_c in 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 The data showed a tail down to $q = 0$, independent of size, up to the size that one could go, as in Parisi's theory. This was not easy to do because of slow computers and lack of parallel tempering which, later, would considerably speed up equilibration at low-T. It was only possible because a very enterprising postdoc, Joseph Reger, managed to get (for free, I think) a bunch of parallel processors called transputers from a British company called INMOS (https://en.wikipedia.org/wiki/Inmos), and was then able to connect them to a driver PC and program them. Reger is now Chief Technical Officer of Fujitsu (the part outside Japan, which is still very large). Incidentally, despite the big clamor of those who argue that the droplet picture (not RSB) applies in short-range spin glasses, the latest simulations with larger sizes persist in showing no decrease of $P(0)$ with size. (The droplet picture does predict such a decrease.)