History of RSB Interview:
Charles M. Newman and Daniel L. Stein

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PC: Good morning, Prof. Newman and Prof. Stein. Thank you very much for joining us and for agreeing to this interview. The main topic of this discussion series is the history of spin glass and replica symmetry breaking, which we roughly bound from 1975 to 1995. Both of you have contributed to that material during that epoch. But before we get to that, we have a few questions on background that would help us situate both of your work on this. We'll start with you, Prof. Newman. In a variety of interviews[1] you explained how you got to pursue a math degree, but how did you get interested in physics? Can you tell us a bit about what brought you to this field?

CN: [0:00:53] I guess you already know some of this. Almost all of my degrees are actually in physics. I started out as a physics undergrad at MIT, a long time ago. I originally didn't plan to do anything particular in mathematics but I had such a wonderful kind of a special introductory rigorous course in calculus when I was a freshman, which was fairly unusual—I think that's more usual maybe in France or Italy but definitely unusual in the US—that got me interested in mathematics also. By the time I finished MIT I ended up with two degrees, an extra degree in mathematics. And even though I was a graduate student in physics at Princeton—as was Dan some

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[1] See, *e.g.*, K. D. Mossmaan, "Profile of Charles M. Newman," *Proc. Nat. Acad. Sci. U.S.A.* **107**, 15668-15669 (2010). https://doi.org/10.1073/pnas.1010735107; T.-P. Liu, S.-H. Yu, J. Quastel and F. Rezakhanlou, "Interview with Prof. Charles Newman," *Institute of Mathematics, Academia Sinica* (June 11, 2011). https://web.math.sinica.edu.tw/mathmedia/interviewE.jsp?mID=40301 (Accessed April 25, 2022.)

years later—I focused on mathematical physics[2]. So in a way I don't have to explain how I got involved in physics. I got involved in physics first and then I got involved in mathematics.

PC: If we step back a bit, why did you choose physics? What drew you initially to physics?

CN: [0:02:20] Part of it was a misunderstanding of the term. I had an uncle, actually a quite distinguished uncle by marriage, Marvin Camras[3], who passed away years ago, married to my mother's sister, who is really an electrical engineer, but he was at the Illinois Institute of Technology, in their research part. He had a title; his title was senior physicist. I remember hearing this title when I was a child, and [thinking]: "He seems like a smart guy and he's a physicist." But he actually wasn't a physicist; he was an electrical engineer. I said: "Ok. I guess I'll go into physics also. It seems like a nice area." But I didn't know what physics was at the time. Of course, I learned what physics was as a physics major at MIT. But my original inclination was actually based on this misunderstanding of what my uncle's profession was.

PC: Prof. Stein, could you tell us what first got you interested in physics and what led you to pursue graduate studies in theoretical physics after that?

DS: [0:03:42] I had been interested in science in general and in physics in particular since I was very young. I don't know where that arose from. Probably like Chuck, I don't come from an academic background. Nobody in my family was in science or in any kind of academic endeavor, but I seemed to have just a special interest in it. I was drawn especially to astronomy when I was a kid. Not that I lived in a place where you could see the stars very well, but what you could see I found somehow spoke to something deep inside of me. And I loved math and science in school. But by the time I got to college—I went to Brown University as an undergraduate—I was interested in a very wide variety of things. I was even contemplating being an English major, until I realized that my self-evaluation of my writing abilities probably wouldn't translate to the outside world. So finally, I guess, I gave in to my inner voice, my inner impulses, telling me that this is really what I wanted to do, and had been sort of avoiding the whole time. So in my sophomore year I finally decided I was going to become a physics major. I came in sort of late, in the second semester of my sophomore year. I loved it from the very beginning. I also

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[2] Charles Michael Newman, *Ultralocal Quantum Field Theory In Terms of Currents*, PhD Thesis, Princeton University (1971). https://catalog.princeton.edu/catalog/9919648323506421

[3] Marvin Camras: https://en.wikipedia.org/wiki/Marvin_Camras

loved mathematics. I had some wonderful mathematics professors. A similar story to Chuck's in that sense. Like Chuck, when I graduated I got bachelor's degrees in both math and physics, a double major. And then I went to Princeton to get a PhD in physics[4]. That's sort of my background story.

PC: What drew you to Princeton, and to work with Phil Anderson[5] in particular?

DS: [0:06:06] Well, when I first applied to Princeton, I applied to a number of schools, and did well in terms of the application process. Initially, I thought I was going to go to Harvard. (I'm not sure if I should continue with this but I will anyway.) I would have ended up going to Harvard, except at that time I graduated early from Brown after three and a half years and I was working at the Naval Research Laboratory[6] on plasma physics problems, in Anacostia, near Washington, DC. I decided to drive up to Harvard and I got to speak to people like Steven Weinberg[7] and all this kind of stuff. It was very exciting. Then, I spoke to the graduate students and they were uniformly miserable. They said: "Don't come here!" I was really sort of crestfallen. I had actually had a nice offer from Caltech, but at that point in my life I wasn't really thinking about leaving the Northeast, so I went down to Princeton. I spoke to the students there. They were also fairly miserable, but not quite as miserable as the students at Harvard. I said: "Okay! I'll go there." At that time, I still wasn't sure which branch of physics I wanted to do. I was thinking of high energy physics at that time. I knew I was going to be a theorist. I did like lab work, but my real heart was in theory, and particularly mathematics.

The first year I was there I spoke to the other graduate students. I sort of looked around. At that time, that's when Phil Anderson—my first year at Princeton was 1975—came to Princeton as well. Some of the other students had said: "If you want to work with a real theoretical physicist, Phil Anderson, he works in all kinds of areas, does all kinds of interesting things." I started looking at some of his papers, and I thought that this would be the person that I would want to work with. So I went to talk to him. This was, I guess, around 1976. He said: "Well, we'll see how you do in the general exams that Princeton gives, and after that we'll see." I did well in the general exams and Phil immediately took me on. Interestingly,

[4] Daniel Lewis Stein, *The topology of order parameter spaces of condensed matter systems*, PhD Thesis, Princeton University (1979). https://catalog.princeton.edu/catalog/9916026723506421

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

[6] U.S. Naval Research Laboratory: https://en.wikipedia.org/wiki/United_States_Naval_Research_Laboratory

[7] Steven Weinberg: https://en.wikipedia.org/wiki/Steven_Weinberg

I'd been working with him for a total of one month when he won the Nobel Prize. I suppose it did not come as a surprise to other people, but I did not know Phil very well then, and I guess that was my first brush with it you might say. It was a very interesting time.

The thing that drew me to condensed matter physics was that there was a much wider range of problems that one could work on than in other areas. I had been interested in astrophysics originally, but the work that was being done at that time—it has changed a lot since then, but this was the late '70s—didn't appeal to me terribly much. In particle physics, everybody was pretty much working on the same problem or a very small subset of problems, whereas in condensed matter physics there was a huge variety of problems from very abstract and mathematical to very applied, bordering on engineering, particularly electrical engineering. I thought that there's a whole palette of problems to choose from. I decided this would be the area that I would go into.

PC: You PhD work with Anderson was not spin glass related, but were you following developments in the field at that time?

DS: [0:10:13] Well, in fact, yes, [although] not that closely. Phil had suggested that I work on the topological properties of order parameters in condensed matter systems, and that's what I wrote my thesis on. But I also wrote a number of other papers, sort of on my own[8], on liquid crystals, on renormalization group stuff, because that was all the rage back then. Many people who were getting degrees in condensed matter physics wrote their papers on the renormalization group. Because in the early '70s Ken Wilson did his thing, that was very popular then. But I was a graduate student from 1975 to 1979, which you might consider to be the heroic age of theoretical spin glasses. In 1975, both Edwards and Anderson[9] and Sherrington and Kirkpatrick[10] came out with their papers. Then, in '77 and '78, you had

[8] See, *e.g.*, D. L. Stein, "Kosterlitz-Thouless phase transitions in two-dimensional liquid crystals," *Phys. Rev. B* **18**, 2397 (1978). https://doi.org/10.1103/PhysRevB.18.2397; D. L. Stein and M. C. Cross, "Phase Transitions in Two-Dimensional Superfluid ^{3}He," *Phys. Rev. Lett.* **42**, 504 (1979). https://doi.org/10.1103/PhysRevLett.42.504; D. L. Stein, "Dissipative structures, broken symmetry, and the theory of equilibrium phase transitions," *J. Chem. Phys.* **72**, 2869-2874 (1980). https://doi.org/10.1063/1.439386

[9] 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

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

Thouless-Anderson-Palmer[11] and you had de Almeida-Thouless[12]. But there were also other things going on. Localization theory—the "gang of four [paper]", if you remember—came out around 1978, plus or minus a year[13]. There was a huge ferment. And then, of course, in 1979 Giorgio came out with the replica symmetry breaking solution of the Sherrington-Kirkpatrick model[14]. I wasn't working on spin glasses or glasses at that time, but I was quite interested, and I do remember hearing a talk—I'm not sure by whom, it probably was Richard Palmer[15]—and I remember that a lot of discussion was on the Thouless-Anderson-Palmer solution, which was brand new. I thought to myself: "At some point, I definitely want to have a closer look at this problem."

PC: If you were following that field closely, do you remember your reaction and that of Phil Anderson to the Parisi solution in '79? Or was it a non-event?

DS: [0:12:18] I remember no reaction at all. I think at that time Phil was heavily involved in other things. He was very deeply invested in the single-particle localization theory[16], because there were a lot of theories at that time about whether the metal-insulator transition[17], whether there was a gap or not—a jump in the conductivity at the metal-insulator transition in two and three dimensions. ([There were] some papers that I know Phil was not happy with.) He probably spoke to others about it, but I don't recall any particular reaction at that time. But I think also that Phil's attention was… He was very involved in neutron stars at that time, extremely involved in localization, amorphous semiconductors, and a lot of other problems. I'm sure he noticed it, but what his reaction was I could not tell you.

PC: One of your first works on spin glasses concerned the one-dimensional Ising spin chain with a frustrated Hamiltonian that has a varying interaction

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[11] D. J. Thouless, P. W. Anderson and R. G. Palmer, "Solution of 'solvable model of a spin glass'," *Philo. Mag.* **35**, 593-601 (1977). https://doi.org/10.1080/14786437708235992
[12] J. R. L. de Almeida and D. J. Thouless, "Stability of the Sherrington-Kirkpatrick solution of a spin glass model," *J. Phys. A* **11**, 983 (1978). https://doi.org/10.1088/0305-4470/11/5/028
[13] E. Abrahams, P. W. Anderson, D. C. Licciardello and T. V. Ramakrishnan, "Scaling theory of localization: Absence of quantum diffusion in two dimensions," *Phys. Rev. Lett.* **42**, 673 (1979). https://doi.org/10.1103/PhysRevLett.42.673
[14] See, *e.g.*, G. Parisi, "Toward a mean field theory for spin glasses," *Phys. Lett. A* **73**, 203-205 (1979). https://doi.org/10.1016/0375-9601(79)90708-4
[15] Richard G. Palmer: https://en.wikipedia.org/wiki/Richard_G._Palmer
[16] See, *e.g.*, P. W. Anderson, D. J. Thouless, E. Abrahams and D. S. Fisher, "New method for a scaling theory of localization," *Phys. Rev. B* **22**, 3519 (1980). https://doi.org/10.1103/PhysRevB.22.3519
[17] Metal-insular transition: https://en.wikipedia.org/wiki/Metal%E2%80%93insulator_transition

range[18]. Can you walk us through what motivated this model and what led to its genesis?

DS: [0:13:36] That was so long ago; I don't remember. Gabi Kotliar[19] really was the primary driver of that work. I wish I could remember how we first started thinking about that. That came out in the early eighties. I think that was probably the first paper that I was involved in directly with spin glasses, but I remember we had had a lot of conversations about spin glasses. I don't recall how that particular model came up, how it was proposed and so on. I remember that we had started thinking about getting high temperature expansions, which I believe was the first part of the paper, then Gabi pushed a lot forward the low-temperature part of that paper as well. We were particularly excited by the fact that when you did this conversion from a power-law decay to a dimensionality in nearest-neighbor models—like the Edwards-Anderson model—we found that the specific heat was continuous right at that point, which of course was quite exciting because the specific heat of spin glasses experimentally had been measured not to have any jumps at the critical temperature. I wish I could tell you more. I honestly don't remember many of the details, or how we came to the model. I do remember that Gabi really was the main driver of that work and deserves a lot of the credit for that.

PC: Professor Newman, were you following any of this work at that time?

CN: [0:15:27] The short answer is no. I was certainly interested as a graduate student. My thesis was always more in the general quantum field theory point of view. Particularly, in those years, which would have been the late '60s-early '70s and then during my first position after my PhD at NYU, where Jim Glimm[20] was a faculty member, I was very interested in constructive quantum field theory and things like φ^4 quantum field models and their connections with Ising models[21]. I didn't know anything about spin glasses. I think I may have heard the word, but I think my first real contact with it was when Dan and I were faculty members together at the University of Arizona. That would have been something like 1987. I was very interested in Ising models and renormalization group theory in the Ising ferromagnet context, but I didn't really know anything about spin

[18] G. Kotliar, P. W. Anderson and D. L. Stein, "One-dimensional spin-glass model with long-range random interactions," Phys. Rev. *B* **27**, 602 (1983). https://doi.org/10.1103/PhysRevB.27.602
[19] Gabriel Kotliar: https://en.wikipedia.org/wiki/Gabriel_Kotliar
[20] James Glimm: https://en.wikipedia.org/wiki/James_Glimm
[21] See, *e.g.*, C. M. Newman, "Zeros of the partition function for generalized Ising systems," Comm. Pure and Appl. Math. 27, 143-159 (1974). https://doi.org/10.1002/cpa.3160270203; C. M. Newman, "Inequalities for Ising models and field theories which obey the Lee-Yang theorem," *Comm. Math. Phys.* 41, 1-9 (1975). https://doi.org/10.1007/BF01608542

glasses. I'm sure that he quickly let me know that that was an interesting topic to think about. And then we started working together.

I wanted to add one thing about this uncle of mine, Marvin Camras. He was described as an engineer, which is correct, but he was actually one of the original developers of magnetic tape recording. He eventually won a national medal of engineering—I think the name is somehow changed now[22]—and won all kinds of awards from Japan because with a number of other things which originated in the US they developed that much more quickly and further.

PC: In the mid- to late-70s, you were interested in notions of metastability[23], following your work at the Technion.

CN: [0:18:10] In the mid-'70s, I spent a year at the Technion. I was already a faculty member at Indiana University, but I took off a year. They had a kind of postdoc at the Technion. Larry Schulman[24] was the one who suggested metastability as an interesting question. Larry Schulman was simultaneously a professor at Indiana University and at the Technion. He used to go back and forth. Yes, that's right. We started working on that then. I worked on metastability-related questions and other things. I remember one time when Michael Aizenman and I were one semester both in residence in Bures-sur-Yvette. We tried very hard to figure out something to do with metastability. That didn't work out particularly well, but at the same time we had a second project which was $1/r^2$ one-dimensional Ising ferromagnets and related models[25]. That worked out better. We said: "Ok. We can't figure out metastability, so let's work on this easier problem."

PC: You mentioned you both being in Arizona, but Prof. Newman you were there before Prof. Stein. Were you in any way involved in his recruitment there?

CN: [0:20:16] I don't think so. I don't think at all.

DS: [0:20:19] We didn't know each other, I think, at that point.

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[22] National Medal of Technology and Innovation : https://en.wikipedia.org/wiki/National_Medal_of_Technology_and_Innovation

[23] C. M. Newman and L. S. Schulman, "Metastability and the analytic continuation of eigenvalues," *J. Math. Phys.* **18**, 23-30 (1977). https://doi.org/10.1063/1.523131; "Complex free energies and metastable lifetimes," *J. Stat. Phys.* **23**, 131-148 (1980). https://doi.org/10.1007/BF01012588

[24] Lawrence Schulman: https://en.wikipedia.org/wiki/Lawrence_Schulman

[25] M. Aizenman and C. M. Newman, "Discontinuity of the percolation density in one dimensional $1/|x-y|^2$ percolation models," *Comm. Math. Phys.* **107**, 611-647 (1986).

CN: [0:20:23] Yes. I was completely in the mathematics department, although I talked to some people in physics. He came in the physics department. Then, we met and discovered we had lots of interests in common. Spin glasses turned out to be the main one. Well, I hadn't had it in common before, but we quickly got involved.

PC: It came naturally.

DS: [0:20:47] Let me add just a couple of things to that. Actually, our first paper together was not on spin glasses[26]. It was on some obscure problem having to do with whether or not percolation [takes place] on the majority spins below T_c. Something like that.

In fact, I can just say I do remember the first time I met Chuck. At that time, Peter Carruthers[27] had just come in as the head of the physics department. I think he just tried to recruit me but at the same time he tried to recruit Lincoln and Jennifer Chayes[28]. The three of us came the same day to interview. Lincoln and Jennifer at that time were working with Chuck. I just sort of tagged along with them. I think, Chuck, that's the first time that you and I met. I remember that Lincoln and Jennifer kept saying: “Ok. Come on! Let’s get to work.” They were not so interested in the interview. They wanted to start working right then and there. Anyway, I ended up coming to Arizona. I guess they went to UCLA where Lincoln still is, if I’m right about that.

CN: [0:22:02] Yes, I think that's right. They definitely did not come to Arizona.

DS: [0:22:10] I could say more later if you want. I'll stop here for now so you can come back to this if you like.

PC: I’d like to step back in time, because we skipped over a key period. Following a meeting at the Aspen Center for Physics, Prof. Stein, you co-authored a paper entitled “Models of hierarchical constrained dynamics

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[26] C. M. Newman and D. L. Stein, “Broken symmetry and domain structure in Ising-like systems,” *Phys. Rev. Lett.* **65**, 460 (1990). https://doi.org/10.1103/PhysRevLett.65.460

[27] Peter Carruthers: https://en.wikipedia.org/wiki/Peter_A._Carruthers

[28] Jennifer T. Chayes: https://en.wikipedia.org/wiki/Jennifer_Tour_Chayes. See also J. T. Chayes, L. Chayes and C. M. Newman, “Bernoulli percolation above threshold: an invasion percolation analysis,” *Ann. Prob.* **15**, 1272-1287 (1987).

for glassy relaxation"[29]. Can you tell us more about that work, and about how that collaboration came about?

DS: [0:22:35] Sure. That, I remember much better. When I got my PhD, from the period of 1979 up to maybe 1982 or '83, I was working on other problems altogether. I was very interested at that time in quantum liquids, in superfluids and things like that. I did a lot of work on neutron stars [and] superfluid helium-3[30]. Jim Sauls[31] had come to Princeton from Stony Brook at that time, and he and I worked together on a lot of problems. That was mostly what I was working on, but I had remembered and, of course, I wasn't completely… (There was the work with Gabi on the 1D long-range spin glass.)

I remained very interested in the problem of disordered systems. I don't remember exactly how I got started to migrate over to that area, but I do know that at some point in '82 or '83 Richard Palmer and I started talking about various problems, not in spin glasses but in structural glasses. We talked about spin glasses too, but our main interest was in dynamical relaxation, in particular these alpha relaxations in glassy liquids. In particular, the Kohlrausch—Watts—Williams law[32], the Vogel-Fulcher [equation][33], all of these kinds of anomalous relaxation behaviors really got our interest. We started thinking about the timescales. Richard had written a very long paper about broken ergodicity that appeared in *Advances in Physics* in '82[34]. We had started working together for… Of course, the renormalization group was still very much in the air. Richard and I started thinking about… And, of course, hierarchies were very much in the air because of the Parisi solution of the Sherrington-Kirkpatrick model. We started thinking: "What if there are fast degrees of freedom that have to unlock slower degrees of freedom and so on and so forth." We spoke to Elihu[35] and Phil about that as well. That became the so-called PSAA

[29] R. G. Palmer, D. L. Stein, E. Abrahams and P. W. Anderson, "Models of hierarchically constrained dynamics for glassy relaxation," *Phys. Rev. Lett.* **53**, 958 (1984). https://doi.org/10.1103/PhysRevLett.53.958

[30] See, *e.g.*, T. Perry, K. DeConde, J. A. Sauls and D. L. Stein, "Evidence for Magnetic Coupling in the Thermal Boundary Resistance between Liquid He-3 and Platinum," *Phys. Rev. Lett.* **48**, 1831 (1982). https://doi.org/10.1103/PhysRevLett.48.1831; J. A. Sauls, D. L. Stein and J. W. Serene, "Magnetic vortices in a rotating $^3P_2$ neutron superfluid," *Phys. Rev. D* **25**, 967 (1982). https://doi.org/10.1103/PhysRevD.25.967

[31] James Sauls: https://en.wikipedia.org/wiki/James_Sauls

[32] Stretched exponential function: https://en.wikipedia.org/wiki/Stretched_exponential_function

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

[34] R. G. Palmer, "Broken ergodicity," *Adv. Phys.* **31**, 669-735 (1982). https://doi.org/10.1080/00018738200101438

[35] Elihu Abrahams: https://en.wikipedia.org/wiki/Elihu_Abrahams

paper[36]. In a way, I still don't know quite what to think of that. In some sense, it was a model not based on any first principles or anything like. It was sort of based on an idea, but it's one of my most cited papers. And it's still being cited. I suppose the physics community must think more highly of the paper than I do at this point. It seems to have been an interesting paper, but it did grow out of this discussion with Richard Palmer and the influences, at that time, which were very much in the air, [namely] the renormalization group theory and hierarchies *à la* replica symmetry breaking.

PC: What role did the Aspen Center for Physics play in there?

DS: [0:26:06] I don't recall much about that, but I think there probably was. That may well be where Richard Palmer and I met. I wish I could say more, especially because I've been involved with the Aspen Center for physics for many years, and I do want to give it its just due[37]. It's not their fault that my memory is a little hazy. I don't know where I would have met Richard, because I don't think he was at Princeton at that time. I think there probably was some kind of meeting at that time on disordered systems. I probably was at that meeting, because I was going every summer at that point. Skipping a few years, but I certainly must have been there in ’82, ’83, when these things were very much in the air. That is probably—I can't be 100% sure—where Richard Palmer and I met and started our discussions. Elihu and Phil probably were there too, because they were always at the Center as well. The Aspen Center for Physics, thank you for reminding me of that, because I had completely forgotten it. I'm not 100% sure about it, but I think it's more than 90% likely that that's probably where all of this got started.

PC: In 1988, again with Richard Palmer[38], you made a different proposal about glassy dynamics that presents some similarities with the Potts glass analogy of Kirkpatrick and Wolynes[39]. How closely were you following these works? How influenced were you back and forth between these ideas?

[36] See Ref. 29.

[37] See, *e.g.*, Ravindra N. Bhatt, “Condensed Matter Physics at the Aspen Center for Physics during the First Fifty Years,” *Aspen Center for Physics* (undated). https://www.aspenphys.org/science/sciencehistory/cm.html (Accessed May 6, 2022.)

[38] D. L. Stein and R. G. Palmer, “Nature of the glass transition,” *Phys. Rev. B* **38**, 12035 (1988). https://doi.org/10.1103/PhysRevB.38.12035

[39] T. R. Kirkpatrick and P. G. Wolynes, “Stable and metastable states in mean-field Potts and structural glasses,” *Phys. Rev. B* **36**, 8552 (1987). https://doi.org/10.1103/PhysRevB.36.8552

DS: [0:27:47] In January to June 1987—just before when I was making my transition from Princeton to Arizona—I codirected a workshop in glassy systems and glassy relaxation and proteins and all this kind of stuff[40]. Two other co-directors were Richard Palmer and Peter Wolynes, so we had had a lot of discussions about these things. There was a lot of interest in whether there was a hidden phase transition somehow underlying the glass transition. I also remember that there were some interesting renormalization group work by Hasenfratz and Hasenfratz[41]. I don't remember the details of that anymore. (I think later Alan Sokal had followed up with that work and found some problems with that[42].) I think that provided the germ of the idea that led to this work that Richard and I had done on glasses. That was work that I sort of forgot about pretty quickly afterward, because at that point I was making the transition, which became a very hard transition, when I went to Arizona and Chuck and I started working together. At that point, I more or less dropped out of doing the stuff on glasses. I don't think I've done anything on glasses since then, structural glasses that is. All of these things again were in the air and that workshop in Santa Barbara, I'm sure is where much of these ideas and discussions took place.

PC: In 1985, you also proposed an analogy between spin glasses and substates of proteins. That predates that workshop. Where did your interest of this problem and the idea for the analogy come from[43]?

DS: [0:29:51] This I do remember. Two things again that were in the air: spin glasses. In the early 80s, when Phil Anderson had given a talk at an inaugural conference and what became the Santa Fe Institute: "Spin glass Hamiltonians, the bridge to other problems in computer science and biology"[44]. Kirkpatrick, Gelatt and Vecchi had done their work on simulated

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[40] R. G. Palmer, D. L. Stein, P. G. Wolynes, January-June 1987, Institute of Theoretical Physics, Santa Barbara, CA, USA.

[41] Anna Hasenfratz: https://en.wikipedia.org/wiki/Anna_Hasenfratz; Péter Hasenfratz: https://hu.wikipedia.org/wiki/Hasenfratz_P%C3%A9ter See also: A Hasenfratz and P. Hasenfratz, "Singular renormalization group transformations and first order phase transitions (I)," *Nucl. Phys. B* **295**, 1-20 (1988). https://doi.org/10.1016/0550-3213(88)90224-6; K. Decker, A. Hasenfratz and P. Hasenfratz, "Singular renormalization group transformations and first order phase transitions (II). Monte Carlo renormalization group results," *Nucl. Phys. B* **295**, 21-35 (1988). https://doi.org/10.1016/0550-3213(88)90225-8

[42] A. C. Van Enter, R. Fernández and A. D. Sokal, "Regularity properties and pathologies of position-space renormalization-group transformations: Scope and limitations of Gibbsian theory," *J. Stat. Phys.* **72**, 879-1167 (1993). https://doi.org/10.1007/BF01048183

[43] D. L. Stein, "A model of protein conformational substates," *Proc. Nat. Acad. Sci. U. S. A.* **82**, 3670-3672 (1985). https://doi.org/10.1073/pnas.82.11.3670

[44] P.W. Anderson, "Spin Glass Hamiltonians: A Bridge between Biology, Statistical Mechanics, and Computer Science," in: *Emerging Syntheses in Science: Proceedings of the Founding Workshops of the Santa Fe Institute*, D. Pines, ed. (Santa Fe, NM: SFI Press, 2019), pp. 31-41

annealing[45]; John Hopfield had done his work on neural networks[46]; Phil had done his work on prebiotic evolution;[47] all of these with spin glass analogies, so that was in the air. But what really got that work started was [that] in ‘83 or ’84 I was at Princeton and I really did enjoy biological physics quite a lot. I had many discussions with Sol Gruner[48] and Bob Austin[49], who were biophysics experimentalists at the time. Bob approached me and said that they had some very interesting data on proteins. He'd been working with Hans Frauenfelder on relaxation of proteins after photodissociation of heme group in myoglobin[50]. They did flash photolysis, which broke the bond. The iron atom would diffuse around the protein matrix and then it would get rebound, and he would look at its relaxation time. Bob had done a number of these experiments and he had very slow relaxations. We spoke about it, and asked me if I could… (If I recall correctly, you may want to ask him he may have a different memory.) My recollection is that he said: “You know, do you think you want to think about this?” And I said: “You know that looks an awful lot like this kind of relaxations that I've seen in spin glasses.” That I did on my own. I mean I had a lot of discussions with the experimentalists but no discussions with the theorists. I think I talked to Phil about it at some point. Phil said: ”Yeah, that's a good idea. You should pursue that,” more or less just encouraged me. Anyway, I thought of a model of the relaxation due to flipping between conformations of substates of the protein, which one could think about as many metastable states—not ground state kinds of things but many metastable states around the tertiary structure. Then, it got published in PNAS in 1985 as a model of protein conformation substates using the spin glass analogy.

PC: [In the following years], Wolynes with Bryngelson[51] and then with Kirkpatrick and Thirumalai[52], published two papers on those two fields that were clearly related. What was your reaction to these works?

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[45] S. Kirkpatrick, C. D. Gelatt Jr. and M. P. Vecchi, “Optimization by simulated annealing,” *Science* **220**, 671-680 (1983). https://doi.org/10.1126/science.220.4598.671

[46] J. J. Hopfield, "Neural networks and physical systems with emergent collective computational abilities." *Proc. Nat. Acad. Sci. U. S. A.* **79**, 2554-2558 (1982). https://doi.org/10.1073/pnas.79.8.2554

[47] P. W. Anderson, “Suggested model for prebiotic evolution: The use of chaos,” *Proc. Nat. Acad. Sci. U. S. A.* **80**, 3386-3390 (1983). https://doi.org/10.1073/pnas.80.11.3386

[48] “Sol M. Gruner,” Academic Tree (undated). https://academictree.org/physics/peopleinfo.php?pid=93601 (Accessed May 6, 2022.)

[49] Robert H. Austin: https://en.wikipedia.org/wiki/Robert_Hamilton_Austin

[50] R. H. Austin, K. W. Beeson, L. Eisenstein, H. Frauenfelder and I. C. Gunsalus, “Dynamics of ligand binding to myoglobin,” *Biochemistry* **14**, 5355-5373 (1975). https://doi.org/10.1021/bi00695a021

[51] J. D. Bryngelson and P. G. Wolynes, ”Spin glasses and the statistical mechanics of protein folding,” *Proc. Nat. Acad. Sci. U. S. A.* **84**, 7524-7528 (1987). https://doi.org/10.1073/pnas.84.21.7524

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

DS: [0:33:08] I had been in many conversations with Peter Wolynes[53]. He was very interested in these problems. I remember thinking many times that the protein folding problem would be very interesting to apply this, although I know I was not the only one. The phenomenology of protein conformational dynamics was fairly straightforward. The phenomenology of protein folding is… The experimental data, the list of phenomena and exceptions, and exception to the exceptions and so on were so overwhelming that I felt that to try to do any kind of modeling of protein folding would be very difficult if one wanted to take into account all of the experimental data. That was probably my mistake, because a number of people went ahead and said: "Well, we don't have to worry about taking account of everything. Let's just come up with a model that will do this." My reaction was that I was very happy to see spin glass ideas being applied to other problems in protein dynamics. Peter and I were good friends and we've had a lot of conversations about these things. We probably discussed these things, although to be honest I don't recall any specific conversation, but it must have come up. I was delighted to see that. Sometimes you do something it is like a drop in the ocean and it disappears, but this seemed to be an idea that even if my work in '85 played any role at all in leading to that other work I would have been extremely gratified. I can't speak to that myself, because other people did that work, but clearly it did follow from it, and it's been a very successful theory.

PC: You mentioned the school you ran. In the published lecture notes for that school, you mentioned that the school organization presented some challenges[54]. Were any of those challenges science related, or were they just the usual [logistical ones]?

DS: [0:35:31] Are you referring to the summer school on complex systems that I was the first director of?

PC: Sorry. I might be confusing the two.

DS: [0:35:40] I have a feeling, yes. There was a summer school on complex systems that I was the first director of. It's still going to this day, run by the Santa Fe Institute. The first one was in 1988. That, I certainly had a lot of

[53] Peter G. Wolynes: https://en.wikipedia.org/wiki/Peter_Guy_Wolynes

[54] *Complex Systems Summer School*, Daniel L. Stein, June-July 1988, Sante Fe, NM, USA. Proceedings: *Lectures in the Sciences of Complexity*, D. L. Stein, ed. (Redwood City, CA: Addison-Wesley, 1989). See, *e.g.*, "I would like to express my deepest thanks to my wife, Bernadette, and to my daughter, Laura, who kept me going at times when my feelings about the summer school were less than enthusiastic." See also: "Appendix X. Participants of the 1988 Complex Systems Summer School" In: *A broad research program on the sciences of complexity: Annual report* (Santa Fe, NM: Santa Fe Institute, 1988), 101-110. https://doi.org/10.2172/6190729.

challenges, including having to find the money, having to decide what complex systems meant, deciding whom to invite and all of that. But that was a whole different thing. We can talk about that later if you'd like, but that would be getting us off topic.

PC: I would like to know what motivated you to organize this summer school, then.

DS: [0:36:12] Well. I was just being hired at the University of Arizona. I was very junior at the time. The Santa Fe Institute was still getting started at that point. They were housed in a former convent that was eventually taken back by the church, but they were housed in this convent and it was a shoestring operation at that time. Pete Carruthers called me up that summer of ’87. At that point, my wife was pregnant with our first child, I had a lot of stuff going on. Pete Carruthers asked me: “They said that they'd like to start a summer school on complex systems. Would [you] be interested in directing it?” I said: “Sure!” I had no idea what I was getting into. In December I went to Santa Fe and spoke to George Cowan[55], who was the president of the Santa Fe Institute.

At that point, I was told they would be happy to support it, but not financially, because they did not have any money. I had to go out and find the funding and everything else. If there were other complex systems summer schools at that time I was unaware of them. I'm not sure there were any. First, we had to find the funding. I was helped by some people like Mike Simmons[56] [who] in particular helped a lot, David Campbell[57], and other people. We did manage to get about $200,000 to run this school. I was terrified that we weren’t going to be able to get the funding, but amazingly we did. We got funding from Research Corporation[58], from DOE, the Sloan Foundation[59]. NSF came in the next year. Sloan Foundation only does it for one year and then they drop out; they just do the initial push. We got the money and then we had to decide what are complex systems, who should I invite, all of this kind of stuff. Again, I got help from more senior people, but it was mostly on me so it was quite stressful at the time. In the end, it was a very successful school. We had a lot of students and participants. There was a Woodstock feeling in the air at that time that something new was being created.

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[55] George Cowan: https://en.wikipedia.org/wiki/George_Cowan

[56] L. M. Simmons, “Presidential Essays: L. M. “Mike” Simmons 1985-1988,**”** Aspen Center for Physics (Undated). https://www.aspenphys.org/aboutus/history/presidentialessays/simmons.html (Accessed May 7, 2022.)

[57] David Campbell: https://en.wikipedia.org/wiki/David_Kelly_Campbell

[58] Research Corporation: https://en.wikipedia.org/wiki/Research_Corporation

[59] Alfred P. Sloan Foundation: https://en.wikipedia.org/wiki/Alfred_P._Sloan_Foundation

It was wonderful, however, I said that I was out. It took an enormous amount of my time. Erica Jen[60] ran it the second year and asked me to be co-director under her. I said: "Sure!" But then after that Erica said she'd had enough. I ended up running the summer school for the next 10 years, some years in co-direction with Lynn Nadel[61], a psychology professor in Arizona[62]. But after 10 years I said it's really time for you to find somebody else to do this. But, as I said, it's still going on and that, that's something that I'm very proud of, in fact. I think it's been a very successful enterprise.

PC: Absolutely! For the first edition, there were lecture notes published, and you wrote a number of the chapters with Richard Palmer on spin glasses and structural glasses. In one of these chapters, you mentioned that a more physical scaling theory approach of spin glasses had been developed and that this approach was attractive[63]. I'm curious to know, in your eyes, what was particularly attractive about this proposal at the time?

DS: [0:40:02] I have no recollection of this. Did we reference what this scaling proposal was?

PC: It was the approach of Fisher and Huse[64].

DS: Yes, of course, that's the scaling theory. There was a lot of talk of scaling theories of glasses at that time, so I wasn't sure you were referring to those. I apologize for that. What's the question about?

PC: What is it that you found attractive about that proposal, given that this is before you doing any work in this area?

DS: [0:40:44] The Parisi solution at that time had dominated everything. In fact, I'm sure you know I'd written the paper with Andy Ogielski[65], solving a dynamical problem in ultrametric spaces. When the Fisher-Huse paper

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[60] Erica Jen: https://en.wikipedia.org/wiki/Erica_Jen
[61] Lynn Nadel: https://en.wikipedia.org/wiki/Lynn_Nadel
[62] See, e.g., *1990 Lectures in Complex Systems*, L. Nadel and D. L. Stein, eds. (Redwood City, Calif.: Addison-Wesley, 1991).
[63] D. L. Stein, "Disordered Systems: Mostly Spin Glasses," In: *Lectures in the Sciences of Complexity*, D. L. Stein, ed. (Redwood City, CA: Addison-Wesley, 1989). p. 340: "Recently, a more physical theoretical approach which is both attractive and self-consistent has been developed; the remainder of this section will be devoted to its discussion. This scaling theory, based on a type of 'domain wall renormalization group,' relies conceptually on the influence of boundary conditions on the state in the interior."
[64] D. S. Fisher and D. A. Huse, "Ordered phase of short-range Ising spin-glasses," *Phys. Rev. Lett.* **56**, 1601 (1986). https://doi.org/10.1103/PhysRevLett.56.1601
[65] A. T. Ogielski and D. L. Stein, "Dynamics on ultrametric spaces," *Phys. Rev. Lett.* **55**, 1634 (1985). https://doi.org/10.1103/PhysRevLett.55.1634

came out I thought it was a beautiful paper. It was sharp. It was crystalline in its beauty. (Crystalline may be an interesting word to use in this context.) I thought that they really had something there. There was no way of knowing who could possibly be right, but I think that in fact this is probably what led me to start thinking about trying to do more serious mathematical work, and could have been an impetus for how initially Chuck and I ended up working together. What happened at that point—this I remember very vividly—is that I was beginning to…

I sort of was interested in neutron stars for a while. I sort of left that field because I thought that while the theories were very interesting and enjoyable and fun to work with, there was no way of really knowing which of these theories of what was going on in the core of the neutron star and things like that were correct, whether it was quark matter or these other exotic phases people were wandering about. I left it for that reason, and I was beginning to get that same feeling with spin glasses. There were all these theories floating around. They were extremely difficult to verify experimentally or numerically, especially numerically. Numerical simulations were being done, and some of them seemed to support the scaling theory, some of them seemed to support RSB, and even the same numerical simulations people argued about which theory it supported. I was also aware that the work of John Imbrie[66] had come out a little bit earlier, in which he rigorously resolved this controversy in random field Ising magnets, whether the lower critical dimension was two or three. He was able to resolve that rigorously. It was a few years later that Aizenman and Wehr did their work on this problem[67]. All of these things together started to make me think that it may be that… Look, I was a student of Phil. Phil did have a bit of an attitude toward mathematical physics. He would love to say that mathematical physicists were able to prove things that ordinary theoretical physicists understood 30 years prior. And that was true to a large extent for a lot of things, but it was not true for the random field Ising magnet. It started to make me think that maybe the right way to go about this is to stop coming up with all of these models and all of this, and start trying to do some rigorous work. The problem was that at that time I did not have the background and training needed for that. So when Chuck and I met and started talking about these things—I remember I was trying to push a paper on him when I first got there that I'd written with Michael Barber, Shoudan Liang and Ganapathy Baskaran[68] on the plus and

[66] J. Z. Imbrie, "Lower Critical Dimension of the Random-Field Ising Model," *Phys. Rev. Lett.* **53**, 1747 (1984). https://doi.org/10.1103/PhysRevLett.53.1747

[67] M. Aizenman and J. Wehr, "Rounding of first-order phase transitions in systems with quenched disorder," *Phys. Rev. Lett.* **62**, 2503 (1989). https://doi.org/10.1103/PhysRevLett.62.2503

[68] D. L. Stein, G. Baskaran, S. Liang and M. N. Barber, "Ground state of the $\pm J$ Ising spin glass," *Phys. Rev. B* **36**, 5567 (1987). https://doi.org/10.1103/PhysRevB.36.5567

minus *J* model in two and three dimensions, ground states and things like that. (That paper disappeared, although I've seen papers that have come out recently with many of the same ideas in them. It's always amusing when you're around in a field long enough, you see these kinds of things happening.) Chuck had said that he was doing lots of very interesting work on all of these statistical mechanics models, these Ising spin models mostly ferromagnetic. And I'd done some work at this point on spin glasses but I didn't really have the mathematical background. When we got together, there was a back and forth about learning about spin glasses on one side and learning about the mathematics in probability theory that was needed to really look at these things in a serious way in my opinion. That's how, I think, our collaboration got started, although Chuck may add to that. He may have different memories. Plus, I've been talking a lot.

PC: That's precisely the question I wanted to ask. Prof. Newman, in your eyes, what did you find compelling about Prof. Stein's proposal or the spin glass models at this point?

CN: [0:45:53] I would agree with his assessment that basically the spin glass models, in particular short-range ones, are so difficult to understand no matter what methods you use that more rigorous mathematical approaches may be more worthwhile than they are in settings where things can be studied non-rigorously and get accurate results. I think I used to say sometimes in talks or maybe just in personal communications that people who try to study physics models from a rigorous mathematical point of view are usually at a big disadvantage, because it's very slow and it takes years or even decades to get results. Meanwhile, as Phil Anderson used to say 30 years before the problem was already resolved to everybody else's satisfaction by non-rigorous methods. Often, there are non-rigorous theoretical physics modeling, and then there are also computational things which have been more and more important in lots of areas. Then, a third kind of option are rigorous mathematical methods. In most areas the rigorous mathematical methods are decades late compared to the other methods, but spin glasses and in particular short-range spin glasses seemed to be so difficult that the competition is more equal. It was one of the reasons that I found it kind of exciting. Those approaches were not necessarily at a disadvantage compared to the other ones, both theoretical physics modeling and computational [modeling]. They're both extraordinarily difficult. So everybody is at the same disadvantage.

DS: [0:48:20] I should just add quickly that while most of the work that Chuck and I have done together is rigorous, we were not above doing some non-rigorous analysis when we felt that we had something to contribute that was at least believable.

PC: In the first paper you wrote together on spin glasses[69], you here again acknowledged a meeting taking place at the Aspen Center for Physics. Was this meeting really helpful? If yes, what was it about it that was helpful?

CN: [0:48:53] I'm not sure who you are directing it to.

PC: To either of you. Presumably you were both there.

CN: [0:48:59] I'm not sure what meeting that would be. I went to one of the relatively early summer schools. It might be that, but I don't remember any details now.

DS: [0:49:16] I remember. You were in Aspen in 1990. The reason I remember that is [two-fold]: one is Jennifer babysat Laura—his daughter babysat my oldest daughter—and also we were caught in a thunderstorm at the top of Mount Aspen.

CN: [0:49:41] Oh, yes!

DS: [0:49:44] So you were there in 1990, and so that must have been the meeting where our discussions… At that point Chuck and I had been working together for a couple of years. Originally, we were thinking about trying to prove a phase transition in the Edwards-Anderson model. I think that's where these ideas of double FK percolation[70] first started coming about. I think that Michael Aizenman may have also been talking about these things at the same time, although I don't recall very well. I think that that 1992 paper, I do think that its genesis was at the Aspen Center for Physics in the summer of 1990. As often happens, both in our '92 paper and our '95 papers[71]—and in a little paper that was published in '96 where we first directly addressed replica symmetry breaking application in short-range spin glasses[72]—we were originally trying to prove something else. Or at least we were looking at another problem and then somehow realized something that applied to these kinds of problems. Chuck, do you remember much about the origins of the '92 paper? I do remember our

[69] C. M. Newman and D. L. Stein, "Multiple states and thermodynamic limits in short-ranged Ising spin-glass models," *Phys. Rev. B* **46**, 973 (1992). https://doi.org/10.1103/PhysRevB.46.973
[70] FK representation: https://en.wikipedia.org/wiki/Random_cluster_model
[71] C. M. Newman and D. L. Stein, "Spin-glass model with dimension-dependent ground state multiplicity," *Phys. Rev. Lett.* 72, 2286 (1994). https://doi.org/10.1103/PhysRevLett.72.2286; "Broken ergodicity and the geometry of rugged landscapes," *Phys. Rev. E* **51**, 5228 (1995). https://doi.org/10.1103/PhysRevE.51.5228
[72] C. M. Newman and D. L. Stein, "Non-mean-field behavior of realistic spin glasses," *Phys. Rev. Lett.* **76**, 515 (1996). https://doi.org/10.1103/PhysRevLett.76.515

intense discussions about it back at Courant. At that time, I was still in Arizona, Chuck was at Courant—he moved in '89—but I would visit. I would make many visits. Also, my wife is from New York and her whole family was in New York and so that worked out very well. Whenever we could we'd be in New York, so I spent a lot of time at Courant at that time. But Chuck, what do you remember about the origins of the '92 paper?

CN: [0:51:29] Not very much, I'm afraid.

DS: [0:51:37] I do remember one thing. What I do remember is this. It turned out that is an extremely simple argument... Originally, [in] that paper we were able to prove a bound on the amount of… Let's see. If you look at the expectation of $(s_x s_y)^2$—an edge variable—thermally averaged and then averaged over all of the couplings, we found a bound on that, that it was greater than or equal to 1- $\langle s_x s_y\rangle^2/\beta$ for any β but as β goes to infinity it's interesting. We thought originally at that point that this was… We had a long argument for it, and I don't remember anymore what it was. Turns out that you can actually prove it in about one line. It ended up being a footnote in the '92 paper. We have this bound in this footnote. At first [we said]: "Does this mean that there really are only two states, two ground states, a single pair?" We quickly realized that it did not imply that. I think that's what led to, in the '92 paper, which actually I think it's one of our nicest pieces of work together on this problem. That was a nice paper. Chuck, do you remember anything more about that?

CN: [0:53:20] No.

PC: Prof. Newman, at that time, especially in 1992-1993, there were various mathematicians and mathematical physicists who got interested in the SK model, such as Francesco Guerra[73], who started about that time, and Michel Talagrand[74] as well. Did the thought of jumping on that story cross your mind? Or was it for you always a finite-connectivity interest?

CN: [0:53:50] I think it's related to what I said before, even though, unlike for ferromagnets, the infinite-range model—the SK model—is not at all simple. I think it's extremely interesting. Certainly after the influence of Dan, I decided that even more fundamental problems were the short-range models. There, as I said before, the more rigorous mathematical

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[73] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Francesco Guerra*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 27 p. https://doi.org/10.34847/nkl.05bd6npc

[74] See, *e.g.*,P. Charbonneau, *History of RSB Interview: Michel Talagrand*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 20 p. https://doi.org/10.34847/nkl.daafy5aj

approach was more on an equal footing with the other approaches, because everybody found it almost impossible, all the methods. So why not continue working on that one? I remember having some brief discussions with the French mathematician Michel Talagrand, who thought that I was or we were completely nuts of working on that impossible problem. Why not work on something where you could actually get some progress?

DS: [0:55:21] He was probably right!

CN: [0:55:23] He was probably right, yes.

PC: How was your collaboration synergistic? What were the complementary skills both of you were bringing to the problem at that point?

CN: [0:55:37] I guess we can both say something about that. You know, I had somewhat more training in mathematical techniques and rigorous kinds of approaches, and Dan certainly had more understanding of physics literature and what were the physically interesting issues to think about. I think that we actually complemented each other rather well.

DS: [0:56:14] What Chuck says, that's certainly how our collaboration started out. I think that's more or less been true throughout, although I think that of lot of each of us has rubbed off on the other. Maybe one could put it that over the years I've become more rigorous and Chuck has become less rigorous. I mean that in a good way. The point is that I certainly have become over the last few decades much more mathematical in my thinking about things. I think that Chuck has probably become more physical in his thinking about things. At this point, we both come up with ideas, we both come up with methods on how to attack, methods for how to prove things and so on. It's been a wonderful collaboration. It really has probably been one of the highlights of my career, and one of the most enjoyable collaborations that I've had, and I've had many of them, many of which have been very fruitful and very enjoyable.

CN: [0:57:41] Let me add that I have no, unlike some people… Because I somewhat started on physics I suppose, unlike some mathematicians who go into mathematical physics, I have nothing against doing work which is not completely rigorous. The only thing I try to be careful about is how you advertise it. If you say that something was rigorously proved, [then] that should be rigorously proved. If you do something which is not completely rigorous, then that's fine and very useful and can be a great contribution but just say what the situation is.

DS: [0:58:22] We try to be very careful in our papers to say what assumptions are going in. That's something that we do try to be extremely careful about. We also try to be very careful about trying to make sure that we do—and I'm sure that we have not been perfect in this because nobody is, it's impossible—try to be very careful about referencing people who should be referenced and all the rest of that. I just hope that we've done a reasonably good job of that.

PC: Having worked on metastable states, did you have a particular intuition about the behavior of finite-range spin glasses when you first started thinking about it? (I understand that Prof. Stein brought a lot of the physics lore to it.)

CN: [0:59:15] Since I didn't know much about it in the beginning, I don't think I had any preconceived notions. The metastate[75], which I hope will be regarded as one of our contributions to not just spin glasses but general disordered systems, was something that we somehow kind of felt forced into. We didn't know how… For example, trying to understand the replica symmetry breaking picture within a rigorous point of view, we understood the general idea, but we just couldn't somehow make precise sense out of it. At some point or other, we had no alternative almost than thinking about the phenomenon of chaotic size dependence in which you don't have just a simple thermodynamic limit, but [at] different scales you see different states. That led to thinking about the metastate. At that point we realized that a very similar picture, but from a somewhat different point of view had been done by Aizenman and Wehr[76]. So we've been careful to describe this object as there's two different ways in which you can think of it, but they end up giving basically the same object.

DS: [1:00:55] Patrick, you said metastable states, did you mean metastate?

PC: You had worked on metastable states in the mid-'70s. I was trying to see if there was a connection.

CN: [1:01:09] No, not much of a connection, I would say.

DS: [1:01:19] We did write a paper on metastable states[77], however, in random ferromagnets and in spin glasses, and we were able to prove a lot of results. This paper appeared in '99 or 2000 (or '98) somewhere around there. We

[75] Metastate: https://en.wikipedia.org/wiki/Metastate

[76] M. Aizenman and J. Wehr, "Rounding effects of quenched randomness on first-order phase transitions," *Comm. Math. Phys.* **130**, 489-528 (1990). https://doi.org/10.1007/BF02096933

[77] C. M. Newman and D. L. Stein, "Metastable states in spin glasses and disordered ferromagnets," *Phys. Rev. E* **60**, 5244 (1999). https://doi.org/10.1103/PhysRevE.60.5244

were able to prove that in all dimensions, including one, if you have a continuous disorder distribution, both spin glasses and random ferromagnets have an uncountable infinity of metastable states. One-spin flip, two-spin flip, and three-spin flip, all the way up to any *k*-spin flip.

CN: [1:01:48] I don't think we've done anything in which those two things have been connected to each other.

DS: [1:01:52] No. But I'm just addressing the metastable state.

CN: [1:01:56] Maybe that's a good idea (laughter)!

DS: [1:01:59] I think that one thing that's important is that people often talk about disorder and frustration. Disorder and frustration together do make the spin glass problem extremely difficult to analyze. But it's also true that you don't need both for a lot of the phenomena that you do see in a lot of these systems. Sometimes disorder alone is enough, as in the random ferromagnet having many metastable states, and therefore we also have slow relaxation. Since Chuck brought up the metastate… I agree with Chuck that that is probably one of our most important contributions. Maybe we could talk a little bit about that if you would like.

CN: [1:02:39] I have a continuing comment about that. I remember very clearly spending a large part of a day sitting with Dan in Washington Square Park trying to decide what we should call this object. I think I had written down a long list of twenty possible names and we kind of ruled out one after the other for various reasons. After a while, that was the only one that was left standing.

DS: [1:03:14] Chuck always had a great talent for coming up with wonderful names for things. Unfortunately, most of those could not be used. About the metastate, though, I do think that this is an important thing. Back in 1995, I remember I spent the entire summer—not the entire summer, but much of the summer—in New York working with Chuck. Originally, we were working on looking at ground states of the two-dimensional spin glass. It was out of that that this flurry of papers that were published in 1996 came. Back then, people were talking about spin glasses a lot. The thing is that I don't think anybody at that time seemed to have a very clear idea of what replica symmetry breaking even meant in this short-range, Edwards-Anderson model. We talked and said: "Well, you know, there's many pure states and they're all ultrametric, like that." Originally, what happened is that first Chuck and I said: "Well, how can you even construct the thermodynamic state…" Some people were even saying—I remember even seeing papers written by very prominent people: "Maybe, it doesn't even have any meaning for spin glasses in the thermodynamic limit, the concept of pure state." That's certainly not true. We were thinking: "Well, we know they exist, but the question was how can one construct them?" It was a lot of work, but we did have a procedure where you do lots of averaging, take the limit and so on. We were [then] able to generate the thermodynamic state, which we now know is the barycenter of the metastate. Then, it was immediately clear that this state could not support the basic… It did not have the non-self-averaging of overlaps and ultrametricity and all this kind of stuff. We published that paper and that created a lot of controversy. We also had discussions with… Francesco Guerra knew Chuck—he didn't know me—so he had written to Chuck with some comments that led to our thinking: "Well, maybe we should think about how one could incorporate replica symmetry breaking in short-range spin glasses. Maybe there was a way." Then, as Chuck said, we had to deal with problems like chaotic size dependence, which we had uncovered in the 1992 paper. That led to the metastate. At that time, we knew about the Aizenman-Wehr paper, but the thing is that what they had done with the metastate was tucked away in an appendix. I think we were probably unaware of that. I know I was and Chuck was too. Anyway, we came up with the idea of the metastate for the spin glass. I think that shortly thereafter, Chuck you mentioned to me that Aizenman-Wehr had something similar. This is after we had written the paper, but I don't remember if somebody had mentioned that to you. Do you remember how you found out about that?

CN: [1:06:57] I don't really remember.

DS: [1:07:03] I do remember. You told me. I had read the Aizenman-Wehr paper. In fact, that's one of the things that got me extremely excited. I was at Arizona—Chuck had already gone to NYU—[and] Bill Faris[78], the head of the math department at Arizona, gave a special seminar on the Aizenman-Wehr paper. He was mostly interested in the martingale central limit theorem, which is a central piece of the Aizenman-Wehr proof, that you had rounding of the phase transition in the two-dimensional, zero-temperature random field Ising model. That got me really excited. That talk did a lot to really turn me on to really thinking hard about how to do rigorous [treatments] of short-range spin glasses. In any case, I read the paper but the technical appendices I probably looked at but I don't remember anymore. I was mostly concerned with the bulk of the paper. I know that, when we wrote that, we were unaware that Aizenman-Wehr had done something similar for the RFIM. But I do remember that shortly

[78] "William Guignard Faris", *Mathematics Genealogy Project* (undated). https://www.mathgenealogy.org/id.php?id=22540 (Accessed May 7, 2022.)

after we had done that, Chuck sent me an email saying: "Take a look at Aizenman-Wehr's Appendix A. They've done something similar for the random field Ising model." I assume, Chuck, that somebody must have mentioned that to you, because I don't think we were looking at the paper at that time. It was years after it was written.

CN: [1:08:40] It could have been Michael Aizenman who pointed it out.

DS: [1:08:45] That's very possible.

PC: You mentioned the 1996 paper as stirring some controversy, which must be related to you writing: "We provide rigorous proofs, which show that the main features of the Parisi solution of the SK spin glass, as applied to more realistic spin glass models, are not valid in any dimension at any temperature." At the time you wrote these words, was this the proof of the program you had embarked on? Was this what you thought was the endpoint of your work?

DS: [1:09:22] No, not at all. I don't think we thought that at the time. Even as that paper was published, we were working hard on trying to figure out how replica symmetry breaking could be… You know, we probably should have phased this a bit differently. What we were trying to say in that paper is that the picture that I think was in most people's minds at that time… I had spoken to a lot of people at Aspen and other places about this, and they'd be giving talks saying: "The spin glass, there are many pure states and when you look at their overlap it's not self-averaging, and they're organized in an ultrametric structure." It turned out it was not that simple. What we really showed is that you cannot construct any thermodynamic state that by itself could have all of [these properties]. The answer did not lie in a single thermodynamic state. That is really what we were trying to say. I think that [ran] counter to what many people were thinking at the time. We later came up with what we then called a non-standard RSB picture—or a non-standard SK picture[79]—which was a very different way of looking at how replica symmetry breaking would apply to short-range spin glasses. I spoke to a lot of people at Aspen about it, and clearly it was not a picture that had been in anyone's mind that I was aware of. Now it is. In fact, we were able to prove—and then later Nick Read[80] showed using

[79] See, *e.g.*, C. M. Newman and D. L. Stein, "The state (s) of replica symmetry breaking: Mean field theories vs. short-ranged spin glasses," *J. Stat. Phys.* **106**, 213-244 (2002). https://doi.org/10.1023/A:1013128314054; "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

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

field theoretic methods[81] that this is really the only way in which replica symmetry breaking can manifest itself in short-range models. Chuck, do you want to add to that?

CN: [1:11:31] Yes. I think that the earlier way we had presented things was because at the time we thought: "Ok, we think that the RSB picture of short-range models is likely or has a good chance of being incorrect, but in order to show that it is incorrect you first have to formulate it clearly so you can then try to prove that it's incorrect." We have succeeded in the formulation, but it's not yet determined whether it's incorrect or not.

DS: [1:12:15] We don't know the answer to that. We have come up with a rigorous argument, using the metastate and its invariance under certain gauge transformation, that led us to believe very strongly at that time and still to some extent that it would be very hard… Not that it's impossible because there's a certain part of it that's rigorous and from that we were led to believe that it would be difficult for replica symmetry breaking… There are so many constraints it would have to satisfy. It was hard for us to see how it could satisfy all of that. We did say, though, in that paper that it's not a proof of anything, but we do believe that this invariance of the metastate—this was later, this was in the late '90s—made it hard to imagine how replica symmetry breaking could survive in finite dimension. However, in follow-up papers, we have written papers that sort of… We actually wrote a paper in [2015 with] Louis-Pierre Arguin[82], who was a former postdoc of ours, in which we proved a number of thermodynamic identities and we checked the replica symmetry breaking. What we found in that paper is that it looked like these thermodynamic identities… We said: "Suppose you had a mixed state model, a model in which you have many thermodynamic states that themselves are decompositions of infinitely many pure states, what did the thermodynamic identities say about those?" What we found and we published in *Phys. Rev. Letters* back in [2015] or so, was that the only mixed state model that satisfies these thermodynamic identities is the replica symmetry breaking picture. Obviously, as you go along you hone your thinking and your new thinking becomes apparent. What we can say now with a certain degree of certainty is that if there are many thermodynamic states and if those thermodynamic states are mixed states, or mixtures of many pure states, then it must be the replica symmetry breaking picture, there are no

[81] 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

[82] L.-P. Arguin, C. M. Newman and D. L. Stein, "Thermodynamic identities and symmetry breaking in short-range spin glasses," *Phys. Rev. Lett.* **115**, 187202 (2015). https://doi.org/10.1103/PhysRevLett.115.187202

alternative pictures of that. I think that's a strong argument in support of replica symmetry breaking.

With that being said, however… We also mention this chaotic pairs picture, which came out of the metastate. That's not a picture that we advocate as being correct, it simply is a picture that is a viable alternative many-state picture, but there the thermodynamic states are not non-trivial mixtures of infinitely many pure states, they're just mixtures of a single pair. So there's uncountably many pure states, but organized in a very different way. What we tried to do is to really narrow down all of the possibilities. At this point, we wrote a paper just recently that is still under review at *Phys. Rev. E*[83], in which we say: "Basically, here are the four basic pictures and they can be related to each other in certain ways." We found ways to relate all of them: the replica symmetry breaking, chaotic pairs, [trivial-nontrivial] (TNT) picture, and droplet scaling. If we were forced to choose a picture at this point, droplet scaling still seems to me like the most plausible alternative, but the question is completely open. Mostly, I just would like to know what the answer is. I don't have a particular horse in the race as far as this is concerned. I would just like to know what spin glasses really do look like below T_c.

CN: [1:16:33] And the answer may well depend on the spatial dimension.

DS: [1:16:38] Yes. Absolutely. There are plenty of people—Mike Moore[84] is a primary advocate of this—[who think] that above six dimensions you have replica symmetry breaking and below six dimensions you have maybe droplet scaling. The droplet scaling people—at least in their early papers—would say that replica symmetry breaking doesn't hold in any finite dimension. That may be true. I still think that there's a reasonable chance that that is true. But, you know, it's the kind of thing that if somebody forced me to bet, I'd probably bet on scaling droplet, but I would prefer not to bet at all, because spin glasses are very tricky things. We have learned quite a lot over the years. At least at this point we can sort of say: "We're pretty sure now it has to be… We have sort of narrowed down…" Our work with Chuck has done a lot to narrow down many possibilities into just a small handful now. I know that we have a reputation as people who are somehow anti-RSB. I think we've tried to do a lot for RSB as well. We've sort of tried to say this is how RSB must look in short-range spin glasses. In our more recent papers we went further and say that if you do have this

[83] C. M. Newman and D. L. Stein, "Ground-state stability and the nature of the spin glass phase," *Phys. Rev. E* **105**, 044132 (2022). https://doi.org/10.1103/PhysRevE.105.044132

[84] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Michael Moore*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 26 p. https://doi.org/10.34847/nkl.997eiv27

complicated structure of many thermodynamic states, each of which is a convex combination of infinitely many pure states, then it must be the replica symmetry breaking picture. Chuck, do you want to add anything to that?

PC: Before we keep on in this line of thought, I wanted to ask a specific historical question. In 1998, you presented this particular problem, the problem of finite-range spin glasses as one of the key open problems in mathematical physics for Michael Aizenman's list that was being established at the time[85]. Why, in your mind, is this problem important to mathematical physics and should belong to that list?

DS: [1:18:53] I'll wait for Chuck. I have my own thoughts, but I'll let Chuck take this first and I'll add my own thoughts.

CN: [1:19:00] I would say that it simultaneously has the two features that an important problem in mathematical physics should have. One, is that it's important from the physics point of view what the answer is. Second, doing things mathematically rigorous seems to be not only useful in this situation but maybe even necessary to answer the question. If you combine those two things that somehow puts it on the short list of significant problems.

DS: [1:19:43] I'll add two things to that. First, replica symmetry breaking is a very beautiful mathematical solution. (I gave a talk at the Santa Fe Institute on the Parisi solution of the Nobel Prize last week. It's on the Santa Fe YouTube channel if you want to check that out[86].) First, it's extremely important to find out if such a beautiful theory, which it is, really does apply to short-range spin glasses in any dimension. Whether it goes down as far as three is debatable, but that would be extremely important because we really have not seen anything like that in any other statistical mechanics problem. We certainly would want to know whether it does apply to finite-dimensional spin glasses. If it doesn't, then you have something that is really peculiar. We know that there's an upper critical dimension for mean-field theories to be valid, but when people are talking about that behavior [it is] near T_c, the transition temperature, where fluctuations become important. Nobody's ever disputed that mean-field theory doesn't work well at low temperatures in terms of figuring out the order parameter. Ginsburg-Landau theory, mean-field theory, figured out the order

[85] M. Aizenman, "Open Problems in Mathematical Physics," *Department of Mathematics of Princeton University* (1998-1999). http://web.math.princeton.edu/~aizenman/OpenProblems_MathPhys/ (Accessed May 7, 2022.)

[86] D. L. Stein, "Complexity Science and the Nobel Prize in Physics 2021," *Santa Fe Institute YouTube Channel* (December 17, 2021). https://www.youtube.com/watch?v=IFBbGTX4TRw (Accessed May 7, 2022.)

parameter for superconductors long before BCS [theory][87] came along. If mean-field theory doesn't get the low-temperature ordering correct, this would also be something different from anything that we've ever seen before. The dimension goes to infinity limit would somehow be singular in behavior. There are reasons to think that this may be the case, which we mentioned in our book. The other thing that I think makes this so important is that since the 1930s, when people were able to come up with theories of the solid-state and of ferromagnetism, and then along came broken symmetry, we have this nice picture of the condensed state in homogeneous systems, crystals, ferromagnets, superconductors, superfluids and so on. But a lot of matter is not ordered and crystalline. We have glasses and spin glasses and all the rest. So there's this big gap still in our understanding of the condensed matter state. It seems to me that until we are able to solve this problem, which by the way Phil Anderson once called the most important problem in condensed matter physics[88]. (He said that before high-temperature superconductors came along, in early '80s.) If we don't solve that problem, then I think we have a huge gap in our understanding of the condensed matter state, which I think that is something that needs to be addressed. So it is an extremely important problem.

PC: In the mid-2000s, 2003, 2004, 2005, the formal proof of the full RSB picture in infinite dimensions started to emerge. Up until that point, someone could argue that this was still a hypothesis and not rigorous. Were you surprised by that result? You now speak about it retrospectively, as being convinced that in infinite dimension that has to be the case, but were you convinced that actually that was the correct picture in the limit of infinite dimension?

DS: [1:23:20] In infinite dimension, we never doubted for a second that the Parisi solution was correct for the Sherrington-Kirkpatrick model. There's not a single one of our papers in which we said anything remotely like that. All of our discussions about RSB maybe not applying had to do with whether they applied to the Edward Anderson model in finite dimension.

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[87] BCS Theory: https://en.wikipedia.org/wiki/BCS_theory

[88] See, *e.g.,* P. W. Anderson, "Lectures on Amorphous Systems," in: *Ill-Condensed Matter*, R. Balian, R. Maynard and G. Toulouse, eds. (Singapore: World Scientific, 1979), pp. 159-261. **PC:** See also: "The deepest and most interesting unsolved problem in solid state theory is probably the theory of the nature of glass. This could be the next breakthrough of the coming decade. The solution of the problem of spin glass in the late 1970s had broad implications in unexpected fields like neural networks, computer algorithms, evolution and computational complexity. The solution of the more important and puzzling glass problem may also have a substantial intellectual spin-off. Whether it will help make better glass is questionable." P. W. Anderson, "Through the Glass Lightly," *Science* **267**, 1615-1616 (1995). (letter) https://doi.org/10.1126/science.267.5204.1615.f

In fact, we were glad to see Guerra's papers and Talagrand's papers, and Toninelli[89] and all these other people who worked on these problems. We never doubted for a second that the Parisi solution was correct. The second it came out it was clear to us. (We weren't together then.) It was clear to me as soon as the Parisi solution came out that that was the answer. And I'm sure that Chuck thought the same way.

CN: [1:24:15] Well, I guess as soon as I learned it. It was somewhat later. I didn't have any arguments with it. I understood that it was interesting and quite non-trivial to actually prove that it worked, but there was no particular reason to doubt it.

PC: In 2013, you published a book for the educated public, a long *Scientific American*-like treatise[90]. Why did you think that such a book was then warranted? And what led to it?

DS: [1:24:52]. What happened was that… The summer school on complex systems was the origin of that book. Back in 2011 or 2012, roughly around that time, I gave a series of lectures at the Santa Fe Institute summer school. At that point, Daniel Rockmore[91] was the director. He asked me to give some talks. I gave a week-long series of talks on spin glasses. When it was over, Dan Rockmore came to me and said—I think he was an editor along with John Miller[92] of a series published by Princeton University Press called Primers in [Complex Systems][93]: "Would [you] be interested in converting those lectures into a volume," I said: "Well, everything that I've done on spin glasses at least that's been any good, has been…" (I shouldn't say that, because of the '83 paper with Gabi.) Almost everything that I've done on spin glasses has been with Chuck, and we would have fun to do that together, so I had to first check if Chuck was interested. Chuck said that he was. So the two of us together sat down and we wrote this book. We tried to be fair in the book to all sides. It was fun to sit down and think about these things and how to explain it to the general public, because these are not easy topics to explain.

CN: [1:26:20] I guess there's a chance we might do a revised version. Is that [right]?

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[89] Fabio Toninelli: https://en.wikipedia.org/wiki/Fabio_Toninelli

[90] Daniel L. Stein and Charles M. Newman, *Spin Glasses and Complexity* (Princeton: Princeton University Press, 2013).

[91] "Daniel Nahum Rockmore", *Mathematic Genealogy Project* (undated). https://www.genealogy.math.ndsu.nodak.edu/id.php?id=69663&fChrono=1 (Accessed May 8, 2022.)

[92] John H. Miller: http://jhmsfi.com/ (Accessed May 8, 2022.)

[93] "Primers in Complex Systems," *Princeton University Press* (undated). https://press.princeton.edu/series/primers-in-complex-systems (Accessed May 8, 2022.)

DS: [1:26:24] A second edition. Princeton has been in touch with us about possibly doing a second edition. We'll see. It depends on how much energy we have left at this point.

CN: [1:26:35] I have a silly kind of notion that maybe someday it will be turned into a film. Maybe we'll find some avant-garde French director, who…

DS: [1:26:55] It will be movie rights, thanks to Chuck.

CN: [1:26:56] They don't have to take too much from the book itself, just use the title is fine and a little bit from the book.

PC: You both held administrative positions throughout your careers. Did you ever use your leadership position to recruit researchers or to otherwise value spin glass-related work?

DS: [1:27:16] Never for me. I was department head of physics and later dean of science. I had to look at the interests and needs of the department as a whole. No, I never did.

CN: [1:27:35] I guess that I used that role a little bit to help recruit Dan to come to NYU from Arizona.

DS: [1:27:47] There is that, yes.

PC: So there was that one instance.

DS: [1:27:54] That instance, yes.

PC: There's the cornucopia, as you mentioned, all these other fields. It's not necessarily just spin glasses, but that never came up, or you never had the chance to weigh in the scale.

DS: [1:28:09] The thing is that what was needed, when I was head of physics at the University of Arizona was to build up in all kinds of different areas. What I mostly focused on in the early days was building up in soft condensed matter physics and biological physics, and things like that. But there were a bunch of other areas as well. When I was dean at NYU, I had all of the science departments to worry about. At that point, really most of the recruiting for things in probability theory and statistical mechanics was really done at Courant[94], which was not under my purview as dean of science. It's its own separate institute. But Courant, I felt, really had… We didn't need to recruit in that area in the physics department, because there was so much need to spread out in all kinds of other areas. But as Chuck pointed out, I suppose, I myself am a personal example of this kind of thing, since Chuck played a very major role not just in recruiting me to NYU but in convincing me to come to NYU.

CN: [1:29:29] Something a little bit further afield that I did play some role was something which has been a significant change within the world of mathematics over my career, which is how probability theory is regarded. Early in my career and hearing stories from people who are older than me, like Mony Donsker[95], probability was barely considered part of mathematics if you go back to forty or fifty years. It had a funny origin in gambling and things of that sort. There were some very good people and there were some places that specialized [in it], but in general it was somehow regarded as a funny field. I've been told that in France, for example, if you weren't in algebraic geometry, you were regarded as not doing anything of interest. That has completely changed. Probability is now somehow one of the most fashionable areas of mathematics, including probability in the statistical physics context. I'm certainly not responsible for that, but whenever I had a chance I helped move that along.

DS: [1:30:53] How is algebraic geometry doing these days?

CN: [1:30:56] It's holding its own, but no longer the only game in the field. [Laughters.]

DS: [1:31:00] It's not the only game in town.

PC: Do you have any insight into how spin glass and spin-glass related questions became to be less important in the United States than it is in Europe in the physics community? Not so in probability, actually. In the mathematics departments it's not so true, but definitely in the physics departments it is.

CN: [1:31:25] I think that Dan is better equipped to answer that.

DS: [1:31:28] In the early '80s, there was lots of work on spin glasses. You noticed, though, that almost immediately much of the work started getting

[94] Courant Institute of Mathematical Sciences: https://en.wikipedia.org/wiki/Courant_Institute_of_Mathematical_Sciences

[95] Monroe D. Donsker: https://en.wikipedia.org/wiki/Monroe_D._Donsker

diverted from spin glasses proper to applications of spin glasses to problems in biology and computer science and so on. Then, at some point in the mid-1980s—I don't remember the timeline exactly—but the bottom sort of dropped out of the market. [At] the NSF, it started getting harder and harder to get grants. In fact, Chuck and I had a joint grant for many years from NSF, but it was really from the Division of Mathematical Sciences, not from the physics part[96]. I think we were one of the very few people who had grants at all in spin glasses. You know, fashions change; things move along. There was a lot of ferment of activity from 1975 to 1985 in spin glasses, and then once the mean-field problem had been solved a lot of people moved on to other things. Then, of course, in 1986 Bednorz and Müller discovered high-temperature superconductivity[97], and there was a massive shift of condensed matter [physicists] to that field. That and all kinds of correlated electron systems became a synonym almost for condensed matter physics. I sort of felt like there is this big problem out there that has not been solved yet, that of the short-range spin glass. We know the mean-field solution, but we still don't know the solution for the short-range spin glass. Is it the mean-field solution? Is it scaling droplet? Is it something else? We just sort of doggedly persisted in pursuing this problem. Despite the fact that it might have been better for our careers, I suppose, if we had just done other things. This was a problem that really grabbed our interest. We really did care about trying to understand and to find the solution. So here we are.

PC: At Princeton, Arizona or NYU, did you ever teach a class about spin glasses and or replica symmetry breaking? If yes, can you detail?

DS: [1:34:04] I've done it at the summer school. As you know, at the first summer school, I spoke about spin glasses and replica symmetry breaking. I have given lectures on it. I gave another set of lectures at another summer school[98]. I don't recall giving a course on spin glass…

PC: As part of a statistical physics graduate course, say…

DS: [1:34:31] I've given courses on statistical physics, but not like a specialty topics course on spin glasses or disordered systems in particular. Chuck, you must have given a series of lectures, though, when you were in Europe,

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[96] NSF Division of Materials Research.

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

[98] **DS:** This was the 2008 Summer School, but I don't think there's a proceedings for that. There was another summer school as well, but I don't recall the year.

because your book *Topics in Disordered Systems*[99], I think, grows out of that. Didn't it?

CN: [1:34:52] Yes. The book *Topics in Disordered Systems*, which is more general than spin glasses but includes that, was a book that came out of a kind of special topics course when I was a visitor at ETH, in Zürich. I guess that's probably… Spin glasses was a part of that, maybe not the major part, but that maybe the only instance of that sort. I don't remember… Certainly not at Arizona and I don't remember… I've given special topics courses, but they have been on other aspects of statistical physics, like usually percolation or ferromagnets Lee-Yang theorem[100] and things of that sort. Not really spin glasses.

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

DS: [1:36:16] Patrick, are you talking about the years from 1975 to 1995?

PC: Loosely, from 1975 to 1995, or within the scope of what we talked about.

DS: [1:36:28] I was extremely gratified and happy to see that the Nobel Prize was given to Parisi. I think that it's about time that spin glasses were recognized as an important problem. I think this will be very good for the field. I'm quite pleased about that. I hope it leads to a resurgence of interest in the field. Chuck and I sent a congratulatory email to Giorgio.

PC: Like many thousands of us.

DS: [1:37:05] Like many thousands of others, yes.

CN: [1:37:09] Maybe sometime we'll go back and dig up some of the emails. There were a few emails we had with him back twenty years ago or so, which might be of historical interest to look at[101].

PC: Speaking of which, do you still have notes, papers, correspondence from that epoch? If yes, do you have a plan to deposit them in an academic archive at some point?

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[99] Charles M. Newman, *Topics in disordered systems* (Basel: Birkhäuser Verlag, 1997).

[100] Lee-Yang Theorem: https://en.wikipedia.org/wiki/Lee%E2%80%93Yang_theorem

[101] See, *e.g.*, G. Parisi, "Recent rigorous results support the predictions of spontaneously broken replica symmetry for realistic spin glasses," arXiv:cond-mat/9603101 (1996); C. M. Newman and D. L. Stein, "Response to Parisi's Comment on 'Non-mean-field behavior of realistic spin glasses'," arXiv:adap-org/9603001 (1996).

DS: [1:37:35] These are correspondences that go back to the ‘90s.

PC: And before. This also intends to cover your work from the ‘80s as well.

DS: [1:37:46] Of course, that was before email was even common. I have tons and tons of them, of unorganized, unsorted out, loose papers, all kinds of things. Chuck and I, of course, have a very long correspondence, email correspondence. And we do have some correspondence with Parisi in the very early days. We could try to dig those up if you're interested. We must have them somewhere.

CN: [1:38:25] There’s even a chance we could find them. We could find them, take a look at them, and if they're not too embarrassing to us…

DS: [1:38:35] I still remember them pretty well. No, they're not embarrassing to either party. They were collegial, cordial back and forth. Obviously…

CN: [1:38:50] There was something like: “Here’s the reason you’re wrong.”

DS: [1:38:54] Yeah. He presented an argument as to why what you should do should be a single delta function, but we also had some pretty good back and forth. Actually, it would be rather interesting, and I'm certainly not embarrassed about it. I can find them and I'll be happy to send them to you.

PC: Thank you both very much for your time, for this discussion.

DS: [1:39:33] Thank you.

CN: [1:39:35] Thank you. You obviously prepared for this very conscientiously.

DS: [1:39:38] Yeah. You knew more about what we did than we remember.