History of RSB Interview: Scott Kirkpatrick

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PC: Scott, thank you very much for sitting with us today. As we've discussed ahead of time, this conversation is going to be mostly about spin glasses and replica symmetry breaking, but to get to that period we have a few lead-up questions. In particular, I wanted to first understand where did your interest in physics come, and how did you get to pursue a PhD in theoretical physics?

SK: I've always followed the path of most interest and least resistance. That seemed to be physics. I went to a pretty good high school. It was a private day school in the town where I grew up, Wilmington, Delaware. They had a more than adequate science program. I didn't benefit from much biology, but we had good chemistry and physics and an excellent couple of math teachers, one of whom promised that next year he was going to teach us how to solve cubic equations, but then he died. So I never did learn how to solve cubic equations, but I know where to look it up. That got me into Princeton.

I got to Princeton and in the first year I took maximal rate physics, math, chemistry and electrical engineering. Electrical engineering was so boring that I dropped it after one term, and chemistry after one year. So I did physics and math, English literature and German, and did all the things that you can do in college. When it was time to apply to grad school, I was an experimentalist who was at least competent at doing theory and that got me into Harvard. Once I got into Harvard I became a theorist, because I realized you could do experiments on computers. So that's how I got to do a thesis on effective medium theories of interesting alloys[1]. The same alloys—for example, copper with some manganese, iron and manganese and nickel—mixed magnetic systems that were subsequently, in certain ranges of dilution, discovered to be spin glasses.

(The man's dead now so I can give you my unbiased opinion of him without burning any bridges.) Henry Ehrenreich[2], whom I worked for, was kind of excellent at what he did but a low status physicist. In order to build the status of the subjects he was interested in—metals and alloys—he had to make the alloys look like metals, meaning that they had to be described by an effective medium theory. And in order to have the necessary mathematical sophistication to go to have tea with Julian Schwinger[3], Paul Martin[4] and people like that, we had to use Green's functions for everything. It took a number of years for me to realize that some of this was decorative and not entirely necessary. I developed a taste for doing things on the computer that were like the experiments that you couldn't adequately characterize in theory of that time. That's how I got into that.

I [then] went off to work with Morrel Cohen[5] at Chicago. Morrel was interested in glasses, semiconducting chalcogenides—Ovshinsk's materials[6]—that might have interesting applications. It was a move from needing a uniform model of everything to appreciating the interesting things that happened when the inhomogeneities are the dominant feature. The characteristics you want to extract are the points at which an inhomogeneity dominates the homogenous behavior to create something different. When I was in Chicago, Morrel Cohen one day suggested that I read—every now and then he would toss us these papers—a paper on percolation. I had a grad student I was working with, and we decided this was pretty interesting. We would try to read some more, and maybe have a seminar or some-

[1] E. S. Kirkpatrick, *Topics in the Electronic Structure of Alloys*, PhD Thesis, Harvard University (1970). http://id.lib.harvard.edu/alma/990038793160203941/catalog

[2] Henry Ehrenreich : https://en.wikipedia.org/wiki/Henry_Ehrenreich

[3] Julien Schwinger: https://en.wikipedia.org/wiki/Julian_Schwinger

[4] Paul C. Martin: https://de.wikipedia.org/wiki/Paul_C._Martin_(Physiker)

[5] Morrel H. Cohen : https://history.aip.org/phn/11503016.html

[6] Amorphous materials with semiconducting properties. See, *e.g.*, Stanford R. Ovshinsky, "Reversible electrical switching phenomena in disordered structures," *Phys. Rev. Lett.* **21**,1450 (1968). https://doi.org/10.1103/PhysRevLett.21.1450; Morrel H. Cohen, H. Fritzsche and S. R. Ovshinsky. "Simple band model for amorphous semiconducting alloys," *Phys. Rev. Lett.* **22**, 1065 (1969). https://doi.org/10.1103/PhysRevLett.22.1065; Lillian Hoddeson and Peter Garrett, *The Man Who Saw Tomorrow: The Life and Inventions of Stanford R. Ovshinsky* (Cambridge: MIT Press 2018).

thing. We ended up writing a widely read—and more readable than its predecessors—review paper in *Annals of Physics*[7]. [With Vinod Shante]—he has since died, he went off into finance and did very well by himself[8]—our paper on percolation had a handful of mini-insights about things that we hadn't seen in the literature. It helped to recognize that percolation had important effects in disordered materials, but it wasn't the whole world because percolation did not explain Anderson localization.

At that time, the giants were Phil Anderson and Pierre de Gennes. The midgets, who were trying to hang out with the giants and maybe grow up to be giants themselves were people like John Ziman[9]. De Gennes, coming from polymers and other mechanical and chemical things, insisted on trying to understand Anderson localization thresholds from the standpoint of percolation, which doesn't work. It misses the subtlety and the suddenness with which it sneaks up on you. I would say Ziman managed to be more or less from what I would regard as my old advisor Ehrenreich's community of turning everything into a uniform effective medium, if you like, averaging too early.

I got a really nice education in the course of grad school and the postdoc in the different ways you could approach inhomogeneous problems, and some of the pitfalls you could fall into. Then I came to IBM…

PC: Before we jump there… You mentioned that you were using computers already a lot during thesis and your postdoc work. Was this your main instrument? What was the balance of work? And what sort of a computational resources did you then have at your disposal?

SK: [0:08:22] My thesis was half analytic, half computational. The computational part was to compute final expressions and display them as curves, not to simulate in great detail. I probably simulated a few things in Chicago. At Harvard we had a 7094[10]. You went to the basement of what was just beginning to be a computer science department and you punched cards

[7] Vinod K. S. Shante and Scott Kirkpatrick, "An introduction to percolation theory," *Adv. Phys.* **20**, 325-357 (1971). https://doi.org/10.1080/00018737100101261

[8] See, *e.g.*, "About Alumni," *University of Chicago Magazine*, 95(6) (2003). https://magazine.uchicago.edu/0308/alumni/deaths.shtml (Last consulted February 2, 2021.)

[9] John Ziman: https://en.wikipedia.org/wiki/John_Ziman

[10] IBM 7090 Series: https://en.wikipedia.org/wiki/IBM_7090

on an 029 keypunch[11]. You could get pretty good at that. Sebastian Doniach[12], Seb Doniach, was around. I remember he was the only faculty member I would ever see spending the night in the computer keypunch room. If you hung out down there you could get three runs in the night instead of only one. The stuff would come back as having failed the first two times and you could fix things. I did spend a little time in that era, but I moved on…

I go back even further than that. I actually worked with one of the dawn of the modern era computers as an undergraduate. For a summer job, I worked at Baird-Atomic[13], a company in Cambridge that had an Autonetics drum memory machine[14] with instructions which you optimized by knowing how long each instruction took and then using the second half of an instruction pair to branch to the point that would be just underneath the read heads on the memory drum. This was great fun. This was as complicated as programming GPUs is today. I guess I've always loved that particular kind of math puzzle, and we had plenty of it.

PC: You were saying you went to IBM. Was it computer driven? What took you to IBM?

SK: [0:10:52] What took me to IBM was working on the phase diagrams, overall properties, and physical properties of random materials. I joined the physical sciences department in their theory group. I was in and out of the theory and experimental groups for a while, so I was in both kinds of groups. I managed a low-temperature physics group that had Richard Webb[15] and Dick Voss[16], both doing really interesting Josephson junction work at one point.

My initial projects were things like the theory of random magnets, spin waves in random magnets. I can't say that I solved any of the world's problems, but it was a good introduction and it was an introduction to people who made crucibles full of interesting substances. Ours were Fred

[11] IBM 021 keypunch : https://en.wikipedia.org/wiki/Keypunch#IBM_029_Card_Punch
[12] Sebastian Doniach : https://en.wikipedia.org/wiki/Sebastian_Doniach
[13] Baird-Atomic Inc. https://collection.sciencemuseumgroup.org.uk/people/cp103283/baird-atomic-inc (Last consulted March 12, 2021).
[14] Probably a Autonetics Recomp II: https://en.wikipedia.org/wiki/Autonetics_Recomp_II
[15] Richard A. Webb : https://en.wikipedia.org/wiki/Richard_A._Webb
[16] Richard F. Voss. See, e.g., "Festival Profile," *APS News* **8**(11), 3 (1999). https://www.aps.org/publications/apsnews/199912/index.cfm (Last consulted February 2, 2021)

Holtzberg[17], Steven von Molnár[18], and [Thomas] (Tom) Penney. At Bell Labs, you had Bernd Matthias[19] and a couple of other people who could produce things that were either purer than anybody else thought possible or had small concentrations of things that nobody else would have thought to put there, which allowed you to make doped semiconductors and RKKY coupled magnets. This is where spin glasses got started. Long-range coupling with random signs between spins in magnetic systems were a natural extension of the stuff for which I did very approximate band theories as a grad student. I went on beyond and continued that area for some time as a postdoc and at IBM.

PC: So was your work on percolation at IBM mostly secondary to your main responsibilities? It was [definitely] an important part of your early publication record.

SK: My only responsibilities at IBM in the Physical Sciences group were to do original and innovative research, and maybe also to help the others do good work as well. That's what we were told and what we were judged on.

Between the time I finished up at Chicago and came to IBM, I did a really nice paper. I was at a conference in Michigan, where David Thouless and a student had explored[20]… Let's say you make a simulated percolating system by punching holes in a sheet of conducting paper, and you measure the conductance of that sheet of paper. How does it go to zero? Does it go to zero sharply, with a critical exponent less than one? The answer is no. The exponent is greater than one. It's got a long tail, and it's clearly affected by the local geometry through which the current flows. On the way back—fortunately I wasn't driving—from Ann Arbor to Chicago I wrote a program in my head, and I had a Phys. Rev. Letter with a mean-field theory, a simple simulation of the threshold and there was a third part. (I forget how I got three things.[21]) I had a letter off by the end of the following

[17] Stephan von Molnár, Paul M. Horn and David D. Awschalom, "Obituary of Frederic Holtzberg (1922-2012)," *Physics Today* (2012). https://doi.org/10.1063/PT.4.1782
[18] Stephan von Molnár: https://en.wikipedia.org/wiki/Stephan_von_Moln%C3%A1r
[19] Bernd T Matthias : https://en.wikipedia.org/wiki/Bernd_T._Matthias
[20] B. J. Last and D. J. Thouless, "Percolation theory and electrical conductivity," *Phys. Rev. Lett.* **27**, 1719 (1971). https://doi.org/10.1103/PhysRevLett.27.1719
[21] **SK**: The third approach that I took in the PRL that I brought to IBM with me was an approach introduced by Ambegaokar, Halperin, and Langer for treatment of Mott's $T^{1/4}$ law in hopping conduction, using percolation to single out the contribution of critical paths in a very dilute system. It didn't work as well in this case as the effective medium approach. But Rolf Landauer understood all three points of view and it was a perfect introduction. See: Vinay Ambegaokar, B. I. Halperin, and J. S. Langer. "Hopping conductivity in disordered systems," *Phys. Rev. B* **4**, 2612 (1971). https://doi.org/10.1103/PhysRevB.4.2612

week[22]. When I got to IBM, it was something that caught the attention of Rolf Landauer[23], who was the guy who best understood where de Gennes had gone wrong in thinking about Anderson localization. He understood quantum mechanics; he understood how wave interference could accumulate in one-dimensional narrow channels.

One of the cool things about IBM was I could take these things that I was thinking about as Ising models and percolation models and ask if they were relevant for five-micron wide strips of aluminum on the surface of a chip somewhere that IBM would care about. It was a great environment for thinking about random systems on a scale just large enough to be interesting, and small enough to be where the technology was at that point.

PC: Was the quality of computers at IBM also a draw? Was it significantly better than what you could get elsewhere? Or was it roughly the same?

SK: [0:16:07] I had gotten more and more interested in computers at IBM. I just used them in Chicago, I would have to ride out to Argonne in order to hang out and get three runs a night rather than one a night. Computers were an obstacle for most of that time. At IBM you could really do some things with them, except you have to scale your expectations down. Raj Reddy[24] at Carnegie Mellon around that time set as his goal for the '80s to have a megapixel in his display, a megabyte of memory, and one mip[25]. The mips moved along faster. I think they got to 10 mips fairly quickly. This was all three, four, five orders of magnitude below where we operate on our laptops today.

PC: Absolutely. I read your piece for Phil Anderson's 90th birthday, in which you said that you read the EA paper as soon as David Sherrington showed up at IBM in September of '75[26]. How did you get to know David Sherringon, and how did you get to discuss this particular problem with him? How did this come about?

SK: [0:17:41] He showed up. He came for the summer.

PC: You didn't know him beforehand?

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[22] Scott Kirkpatrick, "Classical transport in disordered media: scaling and effective-medium theories," *Phys. Rev. Lett.* **27**, 1722 (1971). https://doi.org/10.1103/PhysRevLett.27.1722
[23] Rolf Landauer : https://en.wikipedia.org/wiki/Rolf_Landauer
[24] Raj Reddy : https://en.wikipedia.org/wiki/Raj_Reddy
[25] 3M Computer : https://en.wikipedia.org/wiki/3M_computer
[26] Scott Kirkpatrick, "Spin Glasses and Frustration", In: *PWA90: A Lifetime of Emergence*, Piers Coleman, Premi Chandra, Gabi Kotliar, Clare Yu, Daniel L. Stein, Phuan Ong Eds. (Singapore: World Scientific, 2015).

SK: No. I don't think so. I probably recognized his name, but I hadn't worked with him before. He was a student of Sam Edwards and he'd gone off to Imperial. Edwards was at [Manchester and then at Oxford]. He kept in touch with Sam Edwards, and Edwards was telling him what he and Phil Anderson had done.

A lot of stuff at the industrial labs got done through collaborations, often in summer visits. Things got a more academic flavor every summer, because we had a lot of visitors. So David came to IBM. He walked around the theory group, and I must have been pointed out to him as somebody who can put things on the computer, and asked questions about it. He had the idea—at roughly the stage that I described in the paper—that we could take the analytical path that Sam Edwards had followed with Phil but turn it into a spherical model, or an Ising Erdős-Rényi graph model rather than a spherical model with squishy spins. And off we went. Neither of us is the world's greatest mathematician, so we just went down that path enough times to be convinced that we knew how the theory would work out. My added contribution was to calculate some stuff when we were all done.

PC: When did computers play a role, if at all, in this early stage for you?

SK: [0:19:58] We started modeling enough to test whether the predictions for the internal energy as a function of temperature and for the specific heat measured from fluctuations in Monte Carlo would be reasonable or not.

I had done a whole bunch of papers with Brooks Harris[27] at Penn on proper theory of random insulating magnets [also at that time]. I'm trying to think whether that was all algebra or did we simulate anything for that[28]. I suspect we didn't simulate anything. I think we just wrote out equation after equation. This spin glass project was the most extensive simulations that I found myself doing, more elaborate than any percolation studies.

PC: Was this already during that three-month visit? Were you doing simulations contemporaneously to writing the paper?

SK: [0:21:18] The first paper was a Phys. Rev. Letter[29]. The second one was a Phys. Rev. with all sorts of numerical results in it[30]. The letter we had to do

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[27] A. Brooks Harris : https://en.wikipedia.org/wiki/A._Brooks_Harris

[28] See, *e.g.*, S. Kirkpatrick and A. Brooks Harris, "Theory of the spin excitations of $Rb_2Mn_xNi_{1-x}F_4$," *Phys. Rev. B* **12**, 4980 (1975). https://doi.org/10.1103/PhysRevB.12.4980

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

[30] Scott Kirkpatrick and David Sherrington, "Infinite-ranged models of spin-glasses," *Phys. Rev. B* **17**, 4384 (1978). https://doi.org/10.1103/PhysRevB.17.4384

a certain amount of numerical work to get numbers out of the equations that the formula gave us. It was not obvious… You could derive things at T=0, and you could derive things at T_c, but in between you had to solve equations that were kind of ugly. That required integration on a computer. That was my piece. I think I should get a little credit for recognizing that if we pushed all the way to zero temperature, the entropy became unphysical. You might not have wanted to bother with the entropy because who's going to measure that? If we went to the entropy, we found something that wasn't zero as it should have been, but was a small negative number. That's what got our little piece of the field started. We wrote a paper that said that this is all totally consistent above the threshold. It predicts there is a spin glass phase, and the characteristics of the phase in this theory are not trustworthy. We therefore were asking for lots of citations. I recommend that approach to students today!

PC: Can you tell us the immediate reception to that work? How did the news of it spread, for instance?

SK: [0:23:10] We said there really is a spin glass phase and it could very well, in this simple model, be delineated by a sharp transition. Experiments had been showing sharp transitions, however a transition that is very much sensitive to how you approach it, how long you stay there, and has all sorts of hysteresis complications. Now, we understand [these] to be part of the fact that there are just many states that all of these things have access to, like any glassy system. The experimentalists really like the fact that the pictures they had been drawing now had some theoretical substrate. Whereas, over the years, the theorists have enjoyed the fact that the mean-field theories we create now have an exact, rigorous theoretical substrate. These things take time.

PC: So there was not that a surge of enthusiasm right as the paper came out. It took some time for it to be digested. Is that your impression?

SK: [0:24:20] I would say it was a well-received paper both by theorists and experimentalists. While we had to admit that our conclusions had inconsistencies, physics is full of problems that people work on that they can't solve but they still publish to show progress. Look at high-energy physics!

PC: The next summer you attended the 1976 Aspen summer workshop entitled “Current topics in the theory of condensed matter,” which had a number of spin glass people attending as well, including Phil Anderson, David Thouless and Gérard Toulouse. Can you tell us more about the discussions that were taking place from the theory side at that point?

SK: In terms of what I was trying to understand, and it did turn out to be fundamental… Basically, the old-timers like Phil [Anderson] knew about the gradual extensions to mean-field theory that in this context eventually became cavity approaches. They had ways of posing a mean-field theory not as a solution of everything at one point, but one point with a surrounding that acts on it, which you then iterate to reach a self-consistent description of a spin, or an electron, or an object of some sort in a random environment made up of objects like it. That's a richer sort of what are still mean-field theories, because you're desperately trying to characterize a complicated system by just a very few points that you hope are typical. That's what I spent the time at Aspen trying to understand. The TAP equations, which were cavity equations, came out of that[31]. It was really David Thouless who had the deepest understanding of it all.

A little bit later he was able to come up with the de Almeida-Thouless line, which was where the Edwards-Anderson type theory broke down.[32] That was the first example… I don't even know if that's a replica symmetry breaking line. I suspect it is, because the words that go with it and the physical intuition is that it is the point where a stationary phase—the assumption that you can solve by looking at derivatives of the Hessian around a single point—characterizes all of the corrections, that breaks down along that line.

PC: You were saying that that summer you spent a lot of time thinking about cavity equations and the TAP equations is what came out of it. Was it a very collective discussion? Can you describe how those conversations went? Why is it TAP, in the end, and not TAPS?

SK: [0:27:42] Why wasn't I part of it? I don't know. Probably because I would say my contributions were much more "What intuition are we heading for?" rather than any of the equations that got written down in the paper. Richard Palmer was also there. I would say he and I were mostly trying to put things into words, so that they made sense to us. If they made sense to us, maybe then I could simulate the right experiments. We participated.

PC: So this was a very a fluid, wide-ranging discussion group, as you were suggesting.

[31] David J. Thouless, Philip W. Anderson and Robert G. Palmer, "Solution of 'solvable model of a spin glass'," *Phil. Mag.* **35**, 593-601 (1977). https://doi.org/10.1080/14786437708235992
[32] Jairo R. L. de Almeida and David 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

SK: [0:28:22] Yeah. I would say that. In subsequent work I took my contribution to be: "Hey, I can make a model of this, put it on a computer, simulate it, and do some of the experiments that you're trying to characterize; we'll see if your mean-field theory and my slightly more extended model are talking about the same phenomena." I only stayed in the field for—at the outside you might say it was—10 years. By 1980, I was already managing a computer science group worrying about algorithms for designing computers. So this was a fairly short period for me, but it was a great deal of fun, and I really missed the people that I was able to work with during that time.

PC: Another meeting was the 1978 Les Houches summer school on Ill-Condensed Matter[33], where you were one of the lecturers. How influential do you think the school was? Can you describe your impressions of it?

SK: [0:29:44] There was a lot of experimental material from the Grenoble group. I basically spent my lectures describing the things that I could do experiments on, and characterizing how much you could learn from those experiments, and what their limits were. Of course, you go forward 10, 20, 40 years and you could do those experiments at an incredibly greater resolution, but you wouldn't learn too much more from them. I was really lucky in the time that I wrote that stuff up. Phil [Anderson] gave an overall perspective, and talked about a whole lot more problems. For Phil, the summer school was from the Kondo effect to the future, and the future… I'd have to go back and look at his notes to see where he felt the future was. It was still vague enough that it hasn't adopted his nomenclature or his set of classic problems completely. Another reason why it was timely was there was a great series of talks on topology, and what topology could constrain, what it could separate. That has proved to be an ongoing theme in condensed matter and exotic states of matter ever since then.

We had one guy that does topology—the French all loved him—[Valentin] Poénaru[34], who was probably Romanian given the name, was teaching at École Normale or in the Parisian orbit. That school established what you might think of as the Paris-Rome axis, because [Giorgio] Parisi was there but he was very young. There were other Romans, and Rome was where the French groups that had started the school would send their students to do postdocs, and they would go themselves. Marc Mézard[35] was probably the person who crystallized the Paris-Rome axis, and to this day he prefers speaking Italian at dinner to speaking English or French. The thing

[33] Les Houches, Session XXXI, July 3-August 18, 1978. Cf. *La Matière mal condensée/Ill-Condensed Matter*, Ed. R. Balian, R. Maynard, G. Toulouse (Amstredam: North-Holland Publishing, 1979).
[34] Valentin Poénaru: https://en.wikipedia.org/wiki/Valentin_Po%C3%A9naru
[35] Marc Mézard : https://en.wikipedia.org/wiki/Marc_M%C3%A9zard

that I would say is the greatest accomplishment of that summer school is forging a community to which all sorts of smart Europeans would gravitate over time. Germans, Czechs like Lenka [Zdeborová][36] and her husband Florent [Krzakala], and Eastern Europeans would make their way to Paris and off to Switzerland and Rome and places like that. That's what these European-funded Summer Schools were supposed to do and it's worked.

FZ: Florent is French and he did his postdoc in Rome, so he's really one of the examples of people that went back and forth between Paris and Rome.

PC: During that five-year period during which you were most active on spin glasses, how closely were you following the theoretical discussions, like the works of Bray and Moore[37], Blandin[38], Rudnick and Pytte[39], for instance?

SK: [0:33:51] Only somewhat. It's because I kept applying as a test for whether this was something I should get involved in: "Can I make it happen on a computer, watch it, and see if there's something they've missed? Did they get it right, and are there things they've missed?"

I did a couple of papers which were the experiments to go with current theory. For example, I did a paper on percolation thresholds in two to six dimensions[40]. I also looked at spin glasses in various dimensions as there was an argument, which is still unsettled, between a corrupted first-order transition and a true spin glass transition in three and four dimensions. I was looking for an experimental means of shedding some light on the influence of dimension on these things. In percolation theory it works. You can learn a lot. You can get exponents that are accurate and believable and interesting. In spin glasses it's harder, because you need to wait for the dynamics to settle down, and you need to take averages rather than just do geometric measurements.

PC: This is exactly where I wanted to go. To talk about that 1976 work of yours on higher dimensions. I think you might be the first person to consider a system in higher dimensions in numerical work. Is that correct?

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[36] Lenka Zdeborová: https://en.wikipedia.org/wiki/Lenka_Zdeborov%C3%A1
[37] A. J. Bray and M. A. Moore, "Replica-Symmetry Breaking in Spin-Glass Theories," *Phys. Rev. Lett.* **41**, 1068 (1978). https://doi.org/10.1103/PhysRevLett.41.1068
[38] André Blandin, "Theories versus experiments in the spin glass systems," *J. Phys. Coll.* **39**, C6-1499 (1978). https://doi.org/10.1051/jphyscol:19786593
[39] E. Pytte and Joseph Rudnick, "Scaling, equation of state, and the instability of the spin-glass phase," *Phys. Rev. B* **19**, 3603 (1979). https://doi.org/10.1103/PhysRevB.19.3603
[40] Scott Kirkpatrick, "Percolation Phenomena in Higher Dimensions: Approach to the Mean-Field Limit," *Phys. Rev. Lett*. **36**, 69 (1976). https://doi.org/10.1103/PhysRevLett.36.69

SK: [0:35:54] Probably I'm not. Certainly, at that point I had better computing power available to me, and people with even more—like Los Alamos—hadn't gotten interested in the problem, so I had the field to myself for a year, or six months, or something like that.

PC: For the spin glass problem, another simulation group that looked at the role of dimension is Stauffer and Binder, who wrote a paper in 1979 looking at the role of dimensionality[41].

SK: [0:36:34] They probably did a better job than I did, too, because I was doing other stuff by then.

PC: So you were not paying close attention to what was going on already at that point.

SK: [0:36:48] I confess. I did one thing at a time, and I did my thing each time. If it worked, I published it; and if it didn't, I moved on to do something else. I would say the last couple of years during the time I was working on the simulated annealing paper... The paper appeared in '81 but a lot of it was written from '79 to '80[42]. That was a time when Dan Gelatt and I were working very hard on applying iteration schemes that were not straight downhill minimization to solve frustrated engineering problems. Dan's job was to explain things to engineers, and my job was to explain things to the math department so they would realize we were working on something interesting. We ended up teaching them about the importance of frustration and making it acceptable to think about problems with multiple minima, not some simple global minimum.

The annealing analogy was Gelatt's. He had a better gift for good words than I did. He never got proper credit for that because he—at his father's command—had to take charge of a couple of companies that his family owned back in Wisconsin. Basically, Dan and I—together with the help of some programmers in Poughkeepsie—transferred simulated annealing code to IBM's design automation team that supported what we would now think of as FPGA[43] design, or something slightly less constrained. You put the circuits where you want, but they're circuits from a library, not full custom design. So library-based design, which is the way chips are designed

[41] Dietrich Stauffer and K. Binder, "Comparative monte Carlo study of Ising spin glasses in two to five dimensions," *Z. Phys. B* **34**, 97-105 (1979). https://doi.org/10.1007/BF01362783

[42] Scott Kirkpatrick, C. Daniel Gelatt and Mario P. Vecchi, "Optimization by simulated annealing," *Science* **220**, 671-690 (1983). https://doi.org/10.1126/science.220.4598.671

[43] Field-programmable gate array (FPGA): https://en.wikipedia.org/wiki/Field-programmable_gate_array

to this day. Full-custom went out a couple of generations ago. It was just unworkable and unreliable.

You had to put these little blocks down with all sorts of rules. Highly-constrained optimization, but there's so much stuff and there are so many different ways you can do it that it becomes a combinatorially marginal to intractable problem. I spent those years in the business of solving intractable problems, other than spin glasses, with spin glasses as a poster child for what sorts of interesting things could happen, what stuff to avoid, and all that kind of thing. They were just an inspiration to us, but we made money for IBM and got some bonuses out of it by making it possible for IBM to put a computer on fewer chips than if they hadn't had our software.

The other thing that I keep trying to pass on to people was that life in research trying to influence engineering is endlessly frustrating in itself. I had this happen at least twice in my IBM career. The first time I was transferring programs to Poughkeepsie. I wrote a gorgeous, six-page long Fortran program that did simulated annealing for a highly simplified circuit placement problem. We transferred it to a programmer who took it apart and, using the best methodology of the day, split it up into short routines, none longer than a single page, everything easily understandable by people less smart than she was. That's what we were able to transfer. When we certified that our six-page long, no more than a thousand lines of Fortran, was functionally equivalent to her carefully boiled down and separated code, they accepted it and went off to use it. They found it was slow so they rewrote it as six pages of Fortran.

Everything to do with getting stuff across to engineering that I've been part of, or watched since then, has had to do with boiling down the ideas to where they can be accepted one at the time, and then watching them get put back together into something nobody who didn't do it themselves could understand. IBM was great for that kind of observations about the real world.

PC: Can you tell us more about the genesis of this idea? How did you get from spin glass to optimization? Where did that insight come from?

SK: [0:42:27] That's very simple. The question was we had a prediction of the ground-state energy of a spin glass. I could put a spin glass on the computer, but to find its ground state energy I had to cool it down to its ground state. How are you going to do that? You do it slowly. You immediately discover that the natural greedy algorithm gets stuck, and so you try to get it unstuck, and so you heat it. After a while, you learn that you apply heat at all stages in the approach to the ground state observing fluctuations. You watch the susceptibility and/or the specific heat as you're doing so.

PC: Was mapping to the computer architecture problem because you were learning about this yourself, being at IBM, or was it through conversations with your co-authors?

SK: [0:43:27] I like computers, and so both Gelatt and I were spending a little time trying to learn how computers were designed. We found that in research there were a very small number of computer design gurus. Each guru had one special trick. The source of frustration in computer design is if you want stuff to be as fast as possible you want to put the pieces as close together as possible. When you do they heat the silicon, they go outside of temperature limits, and they stop working. Furthermore, if you try to space them out, you need wires—these little copper aluminum alloy strips—and there isn't a lot of space for them. So you have a fight to make space for the wiring, you have a fight to keep things cool enough so that they'll run correctly. In research, we tended to design to zero tolerance, and then we would turn it over to engineers who knew that if you didn't have many sigma of tolerance your computer wouldn't work, the customers would send it back, and you'd all get fired. It was interesting to learn to operate in that world. The gurus would boil the problem down to its simplest form. [Bradford] Dunham, for example, was a guru. He would say: "Your problem is you want to squeeze it together for performance, and you want to have room for the wiring channels. So first squeeze it together as hard as you can, and then stop with the solution. Then pull it just far enough apart to make room for the wiring channels and that will cool it off." The idea that you could do both at the same time was not in their mindscape, not in their vision.

The second thing is that if you're a guru you can have an excellent career at IBM: get awards, trips to nice places to receive these awards, bonuses and things like that. They didn't get a bigger office—you had to be a manager to get a bigger office—but you could be highly regarded if you had a good idea, and you had a handful of disciples to whom you would teach the good idea, and they would go out and do it in the development laboratories. But if you were too clear about your good idea, then everybody could do it, and you'd have to have another good idea and that's hard. So I found the gurus tended to be a little bit mystical, and not very good as educators.

Gelatt and I realized that what we wanted to do was do everything at once, but under the control of an objective function, which like a Hamiltonian would normalize all the things you were trying to do, so you can do more of them by managing an energy, or cost function, or some other overall thing. That's really where spin glasses came in. Spin glasses have Hamiltonians, they have all sorts of random couplings, and you just can't optimize everything at the same time. If you want to think simply and clearly about a spin glass, you isolate the frustration between ferromagnetic and antiferromagnetic tendencies, or domain walls between regions that are organized well, but you can only do that up to a certain scale. Then you put energy in a domain wall, and so you then have to look at shortening the domain walls, or reducing the tension in the domain walls. Conceptually, things can be looked at locally, but, in terms of optimization, things have to be looked at globally by iterating everywhere. That's kind of back in fashion in spin glass modeling by parallel simulated annealing, and many replica approaches to solving models of spin glasses and other models of problems in combinatorics that have the same characteristics as spin glasses. Of being inherently hard and ugly, and of requiring an extended non-local approach. But you think about them simply in terms of replica symmetry breaking and phase boundaries and all the old ideas.

PC: We will get back to that. But you were still doing some work on spin glasses in the early '80s, including a paper with Peter Young, and another couple of papers with Nihat Berker. What led to those works?

SK: [0:49:01] The area that I thought was insufficiently explored was using a very simple recursion relation, introduced by Sasha Migdal and by Leo Kadanoff, to identify flows towards different kinds of order. This gave rise to a paper by Leo Kadanoff, Jorge Jose, David Nelson and me[44]. We had simulated the recursion relations for a spatial rescaling of an XY model, and to our surprise it was absolutely straight for 32 decades of high-precision IBM computing. But Leo pointed out that you could get that without ever going near a computer, which was humbling and impressive. But we thought that both paths were interesting enough that we made a paper out of it. Shortly after that time, Mike Kosterlitz and David Thouless pointed out that there was in fact a transition, and you just had to work a little harder than Leo had. We laid down much of the pathway, we just missed the turn off to the right solution at the end of that.

That was the same recursion relation that I was using with Nihat Berker to study spin glasses[45]. What we would do is start with a random distribution

[44] J. V. José, L. P. Kadanoff, S. Kirkpatrick and D. R. Nelson, "Renormalization, vortices, and symmetry-breaking perturbations in the two-dimensional planar model," *Phys. Rev. B* **16,** 1217 (1977). https://doi.org/10.1103/PhysRevB.16.1217

[45] See, *e.g.*, Susan R. McKay, A. Nihat Berker and Scott Kirkpatrick, "Spin-glass behavior in frustrated Ising models with chaotic renormalization-group trajectories," *Phys. Rev. Lett.* **48**, 767 (1982). https://doi.org/10.1103/PhysRevLett.48.767

of spins and interactions and shrink them to see if they flow towards a critical point. We could understand the behavior in different dimensions that way. I don't think we ran into computational limitations; I think it wasn't the right way to tackle that problem. I also was working with Gérard Toulouse during that time. I was at École Normale, and they wanted me to give a seminar. I said: "Which would you like to hear about: Migdal-Kadanoff recursions for random systems or simulated annealing. They said: "Oh, Migdal-Kadanoff, please!" I just remember at that point both Toulouse and I exchanging an unstated thought that this was not the most important subject to hear about, but if that's what they wanted, that's what they were going to hear.

PC: Speaking of your work with Toulouse. You started working on less materials or more computer science problems, like the traveling salesman problem (TSP), with him in the mid-'80s, right? You have a paper[46]…

SK: [0:55:00] That was because there's a particular traveling salesman problem in the Parisi-Virasoro-Mézard book[47]. Again, it's a fully-connected dimensionless model or an infinite-dimensional model. You take *N* vertices with random distances between them, and you try to find a single path of minimal distance, with no repeats, returning to the origin at the end, *i.e.*, the traveling salesman path. If you do it in a greedy way, you get a length that goes as log *N*. If you do it in a smarter way—you can do it by Held]Karp—type dissection[48], and lots of different ways—you'll get a constant. The question is what should that constant be. That's the problem. There aren't such powerful codes for solving traveling salesman problems. You can write your own and come up with a better code. There was a guy in my group, down at IBM Hawthorne at that time, Harold Stone[49]. He later went to NEC Labs in Princeton. He was the kind of person who could—in the shower—think of a clever way of doing a branch and bound or branch and cut program with some elegant new feature to it. He felt one day like writing a TSP program, so we had access to Harold's program, which for some reason was not very good at realistic TSPs, but was really good at the random distance TSP. And it was even better at asymmetric TSPs, in which the upper right and the lower [left] distances in the distance matrix did not have to be the same. I don't recall exactly what role Toulouse played in that, but it was something we talked about and we were probably also talking about other problems at the same time. I worked on that for a

[46] Scott Kirkpatrick and Gérard Toulouse, "Configuration space analysis of travelling salesman problems," J. Phys. **46**, 1277-1292 (1985). https://doi.org/10.1051/jphys:019850046080127700
[47] M. Mézard, G. Parisi, and M. A. Virasoro, *Spin glass theory and beyond: An Introduction to the Replica Method and Its Applications* (Singapore: World Scientific, 1987).
[48] Held-Karp Algorithm : https://en.wikipedia.org/wiki/Held%E2%80%93Karp_algorithm
[49] Harold S. Stone : https://de.wikipedia.org/wiki/Harold_S._Stone

while, and subsequently that was, I believe, solved by 1RSB. I noticed in the paper that I said it's a replica symmetric problem. I don't know to this day whether it really has a replica symmetric solution, and the numbers in the Parisi-Virasoro-Mézard book are correct. But I was working for some time with Harold's program, which was better than the other ones that were available, to see whether those numbers were right or not. They were almost right. I went back a little while ago and the number still don't seem right, but I've been told that it's now been upheld by Michel Talagrand[50], and therefore we shouldn't question it. Any problems in the numerical experiment are therefore a consequence of the size of the experiment, or something like that. I have never had the time to get to the bottom of that one.

PC: Did you follow other works that were on a similar theme, like that of Fu and Anderson[51], that took place at about the same time? Or were you concerned with other problems at this point?

SK: [0:59:21] Mostly with other problems, because by that time I had 50 people working for me. We were making IBM's first tablet, which got shipped, by the way! That's an accomplishment I'm quite proud of. During that time I was doing real engineering. Dan Gelatt was briefly placed in the real-stuff side of IBM research. Then he left to run his father's companies and they gave me his job. I thought computers were kind of interesting, so I went over there and I spent another 15 years making stuff. I would say the accomplishments were: we made a couple of chips that were better than what they replaced; we had a small role in RISC architecture computers[52], and when we finally got things together we produced a 8.5x11 inch, or A4, pad of paper sized tablet that you could write on with a pen so the ink would be displayed on the screen. For writing recognition, we had two groups running in parallel. The speech group wanted to recognize cursive, but we discovered that we could much more easily recognize hand printing. We could see an application for filling out forms with printed letters that would probably be useful. IBM shipped it and sold it for a couple of years.

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[50] Johan Wästlund, "The mean field traveling salesman and related problems," *Acta Math.* **204**, 91-150 (2010). https://doi.org/10.1007/s11511-010-0046-7; Giorgio Parisi and Johan Wästlund, "Mean field matching and TSP in pseudo-dimension 1," arXiv:1801.00034 [math.PR]

[51] G. Baskaran , Y. Fu and P.W. Anderson. "On the statistical mechanics of the traveling salesman problem," *J. Stat. Phys.* **45**, 1–25 (1986). https://doi.org/10.1007/BF01033073

[52] Reduced instruction set computer (RISC) : https://en.wikipedia.org/wiki/Reduced_instruction_set_computer

We went through the process that I described before. We created a prototype tablet. We took it down to Boca Raton, [Florida,] where they were, at that time, making personal computers, and they completely re-engineered it. Threw out all of our choices of components, and over time found this problem and that problem and redid it. It looked remarkably like our original design by the time they were ready to ship a product. They had to go through that. That's how they learned how it worked. We had terrible software problems. IBM insisted on creating a version of OS/2[53], which never really influenced much of the personal computing world. At the same time, we were working with Go Corp.[54], which was a start-up out in Foster City, [California,] that I saw a lot of. Go had an elegant-looking interface, but they couldn't write code that worked, so they were not a great partner.

The thing that I learned that I'll just toss in for your enlightenment was that all along this time IBM and Bell Labs were the place where really good work was getting done. If you weren't teaching at a first-rate university, you wanted to be at one of those two. Also, I had friends at Bell Labs throughout that time, who also were interested in making real stuff, like John S. Denker[55] and Lawrence D. Jackel. That group gave rise to Yann LeCun[56], who made real stuff and whom you're probably aware of. The AT&T people came in after we'd established a relationship with Go Corp. Our money had just about run out, and the Go software still didn't work for our tablets. AT&T gave them lots of money, and that's about the point that we were ready to pull up and have nothing more to do with them. Once Go Corp. read the contract carefully, they discovered that we had said that if you get money from anybody else you're going to give us our money back. And we exercised that clause. I learned some things from working with Go Corp. We got a lot of good ideas for user interface, and we got all our money back. AT&T ended up basically funding their shutdown.

I had a really stimulating and interesting 15 years building real stuff at IBM. I also spent a little of that time on random systems, and some of the stuff that we built were in fact random systems. It was a different world, and every bit as fascinating.

PC: I think you took a couple sabbatical leaves during those years. One in Jerusalem and one in ENS, is that right?

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[53] OS/2: https://en.wikipedia.org/wiki/OS/2
[54] Go Corp.: https://en.wikipedia.org/wiki/GO_Corp.
[55] See short bio in: John S. Denker *A new spin on the perceptions, procedures, and principles of flight*. https://www.av8n.com/ (Last consulted March 12, 2021).
[56] Yann LeCun : https://en.wikipedia.org/wiki/Yann_LeCun

SK: [1:04:34] I spent a summer in Grenoble and ENS—that was the summer of the Les Houches School—but I've had lots of visits. I am comfortable settling quickly, and finding people to talk to in Rome, in Paris, and nowadays in Lausanne. There was a magnetism group in Grenoble who may be there to this day, but I was really only interacting with the experimental spin glass people during the time of the Les Houches school.

The other reason for sabbaticals was very simple. My first marriage broke down in the early '80s. In the mid-'80s, I met my second wife, Daphna Weinshall, who is Israeli. After a couple of interesting postdocs, and time that she spent at MIT, at Phillips, in Princeton and even some time at IBM, we ended up married with a couple of kids, and returned to Israel. I came to Israel on sabbatical twice during the '90s, because at that point she had taken up a tenure-track position at the highest altitude available, in the coolest weather in Israel, *i.e.*, Jerusalem.

Let me also give this time in Jerusalem credit for the fact that that was when Bart Selman[57] and I were working on the question of whether a combinatorics problem had phase transitions[58]. Bart and his colleagues at Bell were the acknowledged masters of heuristic algorithms for solving k-SAT, and k-SAT has phase transitions. I ended up working with Riccardo Zecchina and Rémi Monasson on the question of phase boundaries in combinatorics, and the fact that the change between the easy problem in k=2 and the hard problems at k=3 occurred in mixed systems at k=2.6, which was later found to be because you could solve all the 2-clauses and the 3-clauses didn't matter up to 2.6. So we did a nice piece of work in the spin glass and message passing part of the world as late as the mid-'90s[59]. The field of spin glasses had by then broadened to include all of combinatorial optimization.

PC: So this work all stemmed from your visit to Israel. I thought it had stemmed from visits to Paris. That's where my confusion comes from.

SK: [1:08:20]. The only time I was really based at ENS was in the 1970s. I made a bunch of visits there, but I hung out in Torino. Riccardo's family is from Torino. He was at Trieste, but would come to Torino. There was a center at Torino that I was invited to be on the advisory board for, and was for a

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[57] Bart Selman : https://en.wikipedia.org/wiki/Bart_Selman

[58] Scott Kirkpatrick and Bart Selman, "Critical behavior in the satisfiability of random boolean expressions," *Science* **264**, 1297-1301 (1994). https://doi.org/10.1126/science.264.5163.1297

[59] R. Monasson, R. Zecchina, S. Kirkpatrick, B. Selman and L. Troyansky, "Determining computational complexity from characteristic 'phase transitions'," *Nature* **400**, 133-137 (1999). https://doi.org/10.1038/22055

number of years. It was working in Torino that led up to the 2+*p*-SAT work with Rémi and Riccardo. I worked with them in Trieste, Paris and Torino.

PC: Throughout those years, you've been pretty well connected to this community on both sides of the Atlantic. What did you feel was the American response? We've talked already about the European response, but was the American response to ideas that came from spin glasses and replica symmetry breaking, for instance, different?

SK: [1:09:37] I would say it was just one of many interesting problems that Americans were working on. I would occasionally try to interest IBM people in the notion that you had so many stable states in the spin glass that maybe this was a good memory. But since I couldn't tell them how you were going to address a particular state, or write a particular state in a controllable way, that never got off the ground. The year that the simulated annealing paper was published—that was recognized fairly quickly as "Oh, all right, that does make sense!"— did convert a lot of people. They felt that we were doing mathematically ugly things, but they recognized that, while it took astronomical time, there was in fact a path to a ground state. You could get there with the extra power of a slowly modulated temperature. That made it a problem that had a proof of a solution, and therefore it was acceptable. I put together a talk on simulated annealing, and went essentially everywhere. I gave more than one talk a month for a year. I would say that was the piece of work that was widely accepted in the United States.

PC: During your time at IBM and now at the Hebrew University, did you ever get to teach a class about RSB and spin glasses?

SK: [1:11:27] Not about RSB, but I taught a little bit about optimization. I would teach a special class in an overall course on combinatorial methods. I joined a department that had Nati Lineal[60], Avi Wigderson[61], Noam Nisan[62], and a whole bunch of people that you may not know. So what I ended teaching once I came here—not just on sabbatical—was project methodology. How to create a project, what it's like to release a product, how to test usability. I ran this course for half a dozen years, and it rather quickly moved into the curriculum, taught by one of my former students. It was an early entrepreneurial course in doing something that would make a difference and that people would care about. Basically, everybody had

[60] Nati Lineal: https://en.wikipedia.org/wiki/Nati_Linial
[61] Avi Wigderson : https://en.wikipedia.org/wiki/Avi_Wigderson
[62] Noam Nisan : https://en.wikipedia.org/wiki/Noam_Nisan

to do a simple project. I'm back to doing that these days, because my current responsibility is the final thesis of all of our engineering students, which is a project done in a small team. So we formed small teams and did little projects. Over time, we discovered that the piece of technology that they most needed to learn was, first of all, how to build a website. That got to be too easy. Then, how to program a smart phone, which very rapidly moved from the barely programmable in Java flip phones to today's smartphones. So the course continues to be taught, but by other people who are more interested than me in exposing our students to the programming systems that let you build apps. You build apps out of apps, nowadays, rather than raw code.

PC: Is there anything else you would like to share with us about that epoch that we may have skipped over, or missed, that were particularly important?

SK: [1:13:57] I've always felt that science was supported in the US for its ability to make us an international power, for its World War II success in super weapons. Super this, and super that. Kind of with a slightly military flavor, always on the edge of the military. When I got to Europe, and looked for funding for projects there, it was always much more about European commercial vitality, European commercial success. The nice thing was that optimization and understanding of complex systems supports both sets of goals. We worked for a couple of years on mapping and understanding the topology of the internet, using some of the same ideas that had come out of percolation and conduction in percolating systems, and the flows through random lattices—random graphs really—because those were the things that would help Europe compete. Not very successfully, if I may say so, with the Googles and the Amazons of the world that are everywhere in the internet.

PC: The final question is whether you still have notes, papers, correspondence from that epoch? If you do, do you have any plan to deposit them at an academic archive?

SK: [1:15:41] I wish I could, but I would need help extracting them. First of all, when I left IBM, I only took a laptop stuffed with files with me. I didn't really take all my emails. They were in an IBM Notes system[63] and Script language that I don't think I could run anymore. I don't know where those are. Up until around 2008, I used an email system that I abandoned for Gmail at that time. Everything's been in Gmail since then. So I really only have about 10 or 15 years worth of email to which I have access.

[63] IBM Notes: https://en.wikipedia.org/wiki/HCL_Domino

I brought along all my old papers in a couple of large cardboard boxes. They had filled a couple file cabinets.

Recently, I was reading a book by Jill Lepore[64], *If Then*, talking about the history of the very first election predictors. They were brave enough to call it AI, but it really was simplistic modeling that they used to predict the outcome of elections, and to tailor a politician's message by predicting whether it would or not influence the voters they thought they should get, or that they wanted to bring together into a majority.

What struck me was the piece of AI and prediction in machine learning that I was witness to during that time was Bernie Widrow[65], Terry Sejnowski and Geoff Hinton[66], and the PDP Collaboration on network models of the brain. They used very simple few-layer neural networks, all of which was exiled, driven to the AI winter[67] of failure by people like Marvin Minsky who proved to his satisfaction that it couldn't work, because he showed that a single-layer network couldn't capture XOR[68]. These brilliant assholes, respected in their community, wrote an entire book on the failure of the neural networks to provide a jumping-off point for machine learning. It cost us ten years.

Meanwhile, Bernie Widrow was doing multi-layer networks out at Stanford—which back then was the Siberia of American academia—and his stuff works and continues to work. It took about a 10-year period of the community pursuing neural networks, even though they did work like bumblebees, to make stuff which ultimately became the foundation of deep networks, which worked. Even though my work on simulated annealing suggested that you shouldn't be able to get there in a reasonable time because there ought to be all these traps at higher energies that would keep you from finding the true ground state, what was discovered in the course of doing their bumblebee thing, was that there aren't that many higher energy states that will trap you, and there are so many solutions that could be used that you didn't need to get there anyway.

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[64] Jill Lepore : https://en.wikipedia.org/wiki/Jill_Lepore
[65] Bernard Widrow : https://en.wikipedia.org/wiki/Bernard_Widrow
[66] Geoffrey Hinton : https://en.wikipedia.org/wiki/Geoffrey_Hinton
[67] AI Winter: https://en.wikipedia.org/wiki/AI_winter
[68] See, *e.g.*, Mikel Olazaran, "A Sociological Study of the Official History of the Perceptrons Controversy," *Social Studies of Science* **26**, 611-659 (1996). https://doi.org/10.1177/030631296026003005; "A Sociological History of the Neural Network Controversy," *Advances in Computers* **37**, 335-425 (1993). https://doi.org/10.1016/S0065-2458(08)60408-8

How's that for wrapping up all of the things we've learned from spin glasses?

PC: As a final note, I'd like to say that if you do have paper correspondence that you brought with you from the '70s and '80s, I definitely encourage you to deposit those.

SK: [1:20:30] Who wants it, will take it, and will keep it in a way that is usable?

PC: You should talk to the Hebrew University first, that's where you are.

SK: We have a lovely library in the middle of our campus. The computer science building used to be right next to it. It was so dead. It hired nobody in 18 years. When I got here, I wrote a list of things that I'm interested in, one of which is: how will we archive born-digital information? People at our library agreed that that's a very interesting topic, but then they had so little money from the university that they couldn't hire anybody to work on it. We had a small library school, which we got rid of because it wasn't contributing to the university's reputation. In Harvard's terms, "every tub on its own bottom." This tub wasn't floating; it was sinking. So we closed the library school, and we are now building an absolutely gorgeous National Library. Our library used to be called the National Library, so all of the new emphasis in libraries has moved to just outside of the Hebrew University campus, and across the street from the Knesset. So I don't believe we have a good place for the Scott Kirkpatrick archive to go.

In the book *If Then,* a lot of the stuff that Jill Lepore got to, and could base her work on, was the Ithiel de Sola Pool[69] papers. He was the guy doing the political projection, political mapping, and election AI at MIT. He did leave an archive, which she used as a basis for much of her book. A lot of stuff about the increasing sophistication and the increasing intellectual content of what were the underpinnings that you could do this with is in that archive. It also did not progress very far during that time. The title of this book reflects the structure of a decision tree. If a person recognizes that you're against abortion, then that puts them in a subcategory of the decision tree. Then you can ask another question, and if they've gotten a yes to two or three of those questions you have one of YOUR voters. Maybe you can sign that person up to go out and get more voters. What they didn't do was go off axis, to go into multi-dimensional analysis. Widrow was there, Minsky wasn't there because he was on axis. All of the work in deep learning recognizes that you have the possibility, by going to nonlinear and off-axis high dimensional spaces, to have a far richer world. It's not a curse

[69] Ithiel de Sola Pool : https://en.wikipedia.org/wiki/Ithiel_de_Sola_Pool

of dimensionality, it's the power of dimensionality to get you out where these things can be really captured, where clusters form. It's the one thing that's missing from this rather stimulating book about all the maximization and the computerization of political intelligence. It's missing because de Sola Pool didn't recognize it. It wasn't in his bag of tricks. Minsky didn't realize that he'd left it out, and Bernie Widrow didn't know that he was doing it. Until people like Geoff Hinton, later Yann LeCun, and the Carnegie-Mellon crowd came along, and formed the PDP collaboration, which produced a couple of books in 1986[70], which made all of that accessible as a playpen that you could really do stuff in. It has a rather similar trajectory to spin glasses but it didn't ever develop replicas.

PC: Scott, thank you so much.

[70] G. E. Hinton and T. J. Sejnowski, "Learning and Relearning in Boltzmann Machines" In: *Parallel Distributed Processing: Explorations in the Microstructure of Cognition,* 2 vols, David E. Rumelhart, James L. McClelland and PDP Research Group (Cambridge, Mass: MIT Press/Bradford Books, 1986).