# History of RSB Interview: Tom C. Lubensky

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**PC**: Good morning, Professor Lubensky. Thank you for sitting down with us. As we've discussed ahead of this interview, the main topic will be the period during which replica symmetry breaking was formulated, roughly from 1975 to 1995. Before we get to that, we wanted to ask you a few questions on background. In a recent oral history interview that was deposited at AIP[1], you detailed how you decided to pursue studies in physics, then a PhD in theoretical physics, and how do you got to do a postdoc in France as well. In that interview, you mentioned that your initial plan was to work with André Blandin[2], but that once you arrived in France he was emotionally unavailable, which eventually lead you to work with de Gennes[3]. Could you tell us a bit what got you interested in working with Blandin? Did you know of his work? What was the original impetus?

**TCL:** In many ways, my graduate experience—though I learned a lot—was unfortunate. My thesis was far and away the worst piece of work I've ever done[4]. Paul Martin[5], who was my advisor, at the time was applying these Schwinger techniques to condensed matter physics. They had this paper

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[1] Interview of Tom Lubensky by David Zierler on May 4, 2020, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, www.aip.org/history-programs/niels-bohr-library/oral-histories/44792
[2] J. Friedel, "André Blandin 1933-1983," *Ann. Phys. Fr.* **10**, 1-4 (1985). https://doi.org/10.1051/anphys:019850010010100
[3] Pierre-Gilles de Gennes : https://en.wikipedia.org/wiki/Pierre-Gilles_de_Gennes
[4] Tom Carl Lubensky, "Some aspects of magnetic correlations and line-widths," PhD Thesis, Harvard University (1969). http://id.lib.harvard.edu/alma/990038782500203941/catalog
[5] Paul C. Martin : https://de.wikipedia.org/wiki/Paul_C._Martin_(Physiker)

from the late '50s that they wrote[6]. A lot of his thesis students—Pierre Hohenberg[7], [Leo] Kadanoff[8], and all those people—were basically doing various versions of it.

At the time I guess there were experiments going on doing neutron scattering at Brookhaven to study the excitation spectra of magnets. My project was to set up a hydrodynamic theory of magnetic systems, ferromagnetic mostly, not too much antiferromagnets. (I plugged away on Martin's technique, while Halperin and Hohenberg were developing a more general approach to broken symmetry hydrodynamics[9] that treats nonlinearities.) Through all of that I was involved with magnetism.

I had gone to Les Houches in '67[10], and Martin had lots of contacts in France, so I thought I would really like to go to France. I applied to Blandin—he said yes—(I had an NSF Fellowship) because he was reasonably well-known. I had fiddled around a bit with the various models of magnetism.

I arrived in France, and Blandin was not really functioning. De Gennes—you probably heard this in my interview—I had met him at Les Houches and we had a bit of a guitar connection. When I arrived at Orsay, condensed-matter physics was still in the old building *deux cent dix* (210). Fortunately, it was moved soon thereafter to the new building 510, and de Gennes' office was just down the hall from mine. He came in one day and said: "I know you. We met in Les Houches!" And he invited me down to his house for dinner. I saw all of the liquid crystal people there. Clearly, liquid crystals were far more interesting at the time than magnetism, even though work spawned there led to the Nobel Prize to Albert Fert[11]. I thought at the time that it was kind of dull.

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[6] Paul C. Martin and Julian Schwinger, "Theory of Many-Particle Systems. I," *Phys. Rev.* **115**, 1342 (1959). https://doi.org/10.1103/PhysRev.115.1342
[7] Pierre Claude Hohenberg, "Excitations in a dilute condensed bose gas," PhD Thesis, Harvard University (1962). http://id.lib.harvard.edu/alma/990038697200203941/catalog
[8] Leo P. Kadanoff, "Theory of many-particle systems: superconductivity; and the acceleration of a charged particle by a quantized electric field," PhD, Harvard University (1960). http://id.lib.harvard.edu/alma/990038551940203941/catalog
[9] B.I. Halperin and P. C. Hohenberg, "Hydrodynamic theory of spin waves," *Phys. Rev.* **188**, 898 (1969). https://doi.org/10.1103/PhysRev.188.898
[10] Problème à N corps/Many-Body Physics, Session de 1967 de l'École d'été des Houches, Les Houches, France. Proceedings : C. deWitt and R. Balian, eds., *Many-Body Physics* (New York: Gordon and Breach, 1968).
[11] Albert Fert : https://en.wikipedia.org/wiki/Albert_Fert

I just followed along there pretty much on my own. I didn't write a paper with de Gennes, but I did write a paper at that time[12], which was well received, all alone. I was then on an adventure, so to speak, because liquid crystals really were a new thing that most physicists didn't know about. That's the best place to be: someplace where you have virgin territory.

**PC:** So you didn't end up talking to Blandin at all, and didn't stay in touch…

**TCL:** [0:04:16] No, I talked to him and I even had a nice dinner at his house. I wasn't avoiding him, but the liquid crystal stuff was a lot more interesting.

**PC:** You first started working on disordered systems a few years later, after having joined Penn, with one of your Penn colleagues, Brooks Harris[13]. Can you tell us how that collaboration and these ideas developed?

**TCL:** [0:04:47] I should say I spent a postdoc with Leo Kadanoff[14] at Brown. I got to know a little bit about his thinking on critical phenomena, even though he was working on solving the problems of the cities at that time[15]. I got the job at Penn, and seeing that all of the people I knew and respected in Paul Martin's orbit—Halperin[16], Hohenberg, Michael Fisher[17], and a couple of others—were doing critical phenomena, I decided that looked like the thing I should do. Halperin told me about the Wilson notes on the renormalization group, which were eventually published[18]. I went through that and found a problem to do applying the renormalization group to semi-infinite systems, which I did with a colleague, Morton Rubin, at Penn[19]. I learned how to do the normalization group—the old fashion form that Wilson did—and I had been working on liquid crystals. It struck me that there was something there. I tried to do the nematic to smectic-A transition with the renormalization group, and I kept finding no fixed point. I had enough

[12] T. C. Lubensky, "Molecular description of nematic liquid crystals," *Phys. Rev. A* **2**, 2497 (1970). https://doi.org/10.1103/PhysRevA.2.2497

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

[14] Leo P. Kadanoff: https://en.wikipedia.org/wiki/Leo_Kadanoff

[15] See, *e.g.*, Leo P. Kadanoff, "From Simulation Model to Public Policy: A case study of the relationship between simulation models of urban social systems and evaluations of proposed public policies." *American Scientist* **60**, 74-79 (1972). https://www.jstor.org/stable/27842948

[16] Bertrand Halperin : https://en.wikipedia.org/wiki/Bertrand_Halperin

[17] Michael Fisher: https://en.wikipedia.org/wiki/Michael_Fisher

[18] K. G. Wilson, "Renormalization group and critical phenomena. I. Renormalization group and the Kadanoff scaling picture," *Phys. Rev. B* **4**, 3174 (1971). https://doi.org/10.1103/PhysRevB.4.3174 "Renormalization group and critical phenomena. II. Phase-space cell analysis of critical behavior," *Phys. Rev. B* **4**, 3184 (1971). https://doi.org/10.1103/PhysRevB.4.3184

[19] T. C. Lubensky and Morton H. Rubin, "Critical phenomena in semi-infinite systems. I. ε expansion for positive extrapolation length," *Phys. Rev. B* **11**, 4533 (1975). https://doi.org/10.1103/PhysRevB.11.4533 "Critical phenomena in semi-infinite systems. II. Mean-field theory," *Phys. Rev. B* **12**, 3885 (1975). https://doi.org/10.1103/PhysRevB.12.3885

sense to do the superconductor as well. When I went to Aspen, Bert was there, and he wanted to do the superconducting thing. So I got involved with that[20].

Then, things are a little bit funny and serendipitous. At that time, you were allowed to just make up models to play with. We had the de Gennes-Landau theory of liquid crystals, so we thought we should study it. Actually, it was Richard Priest who was a student of Harris'[21]. He had left and gone down to NRL[22], but we started a little collaboration. We wrote a paper on models with involving symmetric-traceless tensors and calculated the critical exponents there[23]. We noted if we let the third-order term be zero, the model's upper critical dimension was 4. Richard made an observation that was very interesting,: "Maybe there's a 6-dimensional something here because of the third-order coupling." So I was aware of that. Then we had a postdoc who came in, William Holcomb[24], who was interested in percolation. He had access to these papers by the Dutch group [Kasteleyn and Fortuin][25], who had shown that the one-state Potts model describes percolation. Gérard Toulouse had written a little note, where he did a de Gennes-type calculation showing that the upper critical dimension for percolation is six[26]. So I had this image in my mind that there's something possibly going on in six dimensions. Then we found out about the *s*=1 limit of the Potts model, and we created a real Wilson-type theory[27], which was later improved using various tricks by other people. So we were aware of six dimensions and percolation was not that far from spin glasses. Then the Sherrington-Kirkpatrick theory came out and…

[20] "The research reported in this Letter was begun while the authors were participants at the Aspen Center for Physics." B. I. Halperin, T. C. Lubensky and Shang-keng Ma, "First-order phase transitions in superconductors and smectic-A liquid crystals," *Phys. Rev. Lett.* **32**, 292 (1974). https://doi.org/10.1103/PhysRevLett.32.292

[21] Richard Giles Priest, "Development of a model for nematic liquid crystals," PhD Thesis, U Penn (1972). https://franklin.library.upenn.edu/catalog/FRANKLIN_999007513503681

[22] US Naval Research Laboratory: https://en.wikipedia.org/wiki/United_States_Naval_Research_Laboratory

[23] Richard G. Priest and T. C. Lubensky, "Biaxial model of cholesteric liquid crystals," *Phys. Rev. A* **9**, 893 (1974). https://doi.org/10.1103/PhysRevA.9.893

[24] William Keith Holcomb, "Optical absorption in simple liquids," PhD Thesis, University of Minnesota (1972). https://primo.lib.umn.edu/permalink/f/1q7ssba/UMN_ALMA21366795260001701

[25] P. W. Kasteleyn and C. M. Fortuin, "Phase transitions in lattice systems with random local properties," *J. Phys. Soc. Jpn., Suppl*. **26**, 11 (1969). (Proceedings of the International Conference on Statistical Mechanics, Kyoto, September 9-14, 1968.) C. M. Fortuin and P. W. Kasteleyn, "On the random-cluster model: I. Introduction and relation to other models," *Physica* **57**, 536 (1972). https://doi.org/10.1016/0031-8914(72)90045-6

[26] G. Toulouse, "Perspectives from the theory of phase transitions," *Nuov. Cim. B* **23**, 234-240 (1974). https://doi.org/10.1007/BF02737507

[27] A. B. Harris, T. C. Lubensky, W. K. Holcomb and C. Dasgupta, "Renormalization-group approach to percolation problems," *Phys. Rev. Lett.* **35**, 327 (1975). https://doi.org/10.1103/PhysRevLett.35.327

**PC:** Before you get there, I wanted to ask you about the 1974 paper with Harris, in which you study a model that looks a lot like the Edwards-Anderson model, but came before. It's a letter with just Brooks Harris and you[28]. The Hamiltonian you were looking at is a version of the Edwards-Anderson model with a non-zero ferromagnetic contribution, although you could take that to be zero.

**TCL:** [0:10:11] Right, and of course we were not so sophisticated in creating the most efficient models. One must say that Wilson himself played a role in that. Brooks had just come back from England for a sabbatical, and he was interested in random spin systems just as a general problem. Wilson came to visit, gave a lecture, and came to my office. We were sitting around talking about whether one should apply the renormalization group to random systems. In fairness, Wilson sat there for a couple of minutes, thought and said: "What you need to do is to renormalize the probability distribution." Then we understood that we could do an expansion in the graphs. We got diagrams which reproduced the $n$=0 trick, which I first heard about from Alan Luther[29], who was at Harvard and later went to Copenhagen. There's a long story about that too. Geoff Grinstein and Alan Luther didn't realize that the probability distribution for the *J*s had to have a positive definite cumulant[30]. They found a fixed point which is in the wrong quadrant. Anyhow, that's what started this out.

We had no idea about spin glasses at the time. It wasn't until the Sherrington-Kirkpatrick paper came along that we did[31]. We saw that, and we knew that there had to be some kind of transition. I was equipped already with the idea of six dimensions. I remember I was sitting in a particularly boring seminar. (I think it was on the band structure or something or other.) I had a gotcha moment and I figured out how to convert it into a field theory, on which I could do renormalization group stuff.

**PC:** This is work that you did with your graduate student at the time, Jing-Huei Chen[32], right?

[28] A. B. Harris and T. C. Lubensky, "Renormalization-Group Approach to the Critical Behavior of Random-Spin Models," *Phys. Rev. Lett.* **33**, 1540 (1974). https://doi.org/10.1103/PhysRevLett.33.1540
[29] Alan Harold Luther: https://de.wikipedia.org/wiki/Alan_Luther
[30] G. Grinstein and A. Luther, "Application of the renormalization group to phase transitions in disordered systems," *Phys. Rev. B* 13, 1329 (1976). https://doi.org/10.1103/PhysRevB.13.1329
[31] 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
[32] Jing-Huei Chen, "Phase transitions in gauge coupled systems," PhD Thesis, University of Pennsylvania (1978). https://franklin.library.upenn.edu/catalog/FRANKLIN_998132403503681

**TCL:** [0:12:36] Jing-Huei Chen, right. Brooks continued the work with with us as well[33].

**PC:** So Brooks Harris had been looking at systems with disorder, including a Hamiltonian very close to the Edwards-Anderson or Sherrington-Kirkpatrick one, and you had been interested in RG in disordered systems, and...

**TCL:** [0:13:00] Yeah. Well, we viewed it as putting random *J*s, but the *J*s were random around a finite value, so we didn't get to confront the spin glass business which was around $J$=0.

**PC:** Had you heard of spin glasses at that point at all, or no?

**TCL:** [0:13:25] Not at all. One of the interesting things about the renormalization group (RG) at the time was there were so many problems throughout physics to which you could apply the ideas that were new to the renormalization group. As I noted, Brooks and I first applied the RG to phase transitions in random systems. I did the paper alone where I meticulously did the diagrams with the Wilson technique, not the $n$=0 trick, but I think we got the right answer there[34]. Then, of course, we learned the replica trick and wrote some papers on that. Once we saw the Edwards-Anderson and Sherrington-Kirkpatrick papers, it was relatively easy to move to spin glasses.

**PC:** What was the reaction to your work on the epsilon expansion applied to spin glasses?

**TCL:** [0:14:25] It was well received. At that time, Phil Anderson[35] invited me to Princeton to give a talk and everything. I think it was a pretty big thing at the time that you could treat random systems with RG. Of course, we had no idea how much more complicated the spin glass was. As you know, I got involved in other things.

I have to say that I would have never come up with a replica symmetry stuff and all of that. It consumed the efforts of some of the greatest French scientists for a long time. It really took a long time to get it to work out.

[33] **TCL:** Jing-Huei's thesis focused more on liquid crystals, though she did co-author two papers with on spin glasses. A. Brooks Harris, Tom C. Lubensky and Jing-Huei Chen, "Critical properties of spin-glasses," *Phys. Rev. Lett.* **36**, 415 (1976). https://doi.org/10.1103/PhysRevLett.36.415; Jing-Huei Chen and Tom C. Lubensky, "Mean field and ε-expansion study of spin glasses," *Phys. Rev. B* **16**, 2106 (1977). https://doi.org/10.1103/PhysRevB.16.2106

[34] T. C. Lubensky, "Critical properties of random-spin models from the ε expansion," *Phys. Rev. B* **11**, 3573 (1975). https://doi.org/10.1103/PhysRevB.11.3573

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

**PC:** How had you found out about the Sherrington-Kirkpatrick or Edwards-Anderson[36] papers. Was this through your French connection?

**TCL:** [0:15:25] I don't know. Maybe Brooks got them. This was back in the days when you circulated preprints. (We had big mailing costs.) I don't remember the details. There was a paper that appeared. We had it. We looked at it. We were not particularly fond of that mean-field theory where you had everything interacting with everything else, but it's a nice tool. I'm not really sure how much impact all of this stuff had on real experiments, because the spin glass is really complicated. We used to go to talks and it would be the experimentalists who were talking about some crazy alloy, and it was hard to make contact between the two.

**PC:** How connected were you to France and Europe throughout those years, when you were an assistant and then associate professor? I'm asking this with a particular context in mind. Blandin[37], for instance, is one of the first few people to propose a replica symmetry breaking scheme. Had you kept abreast of this?

**TCL:** [0:16:50] I hadn't. I didn't keep contact with him. I had more contact with the field theorists, Brézin[38], Zinn-Justin[39] and Gérard Toulouse[40], with whom I spent a year at ENS. We worked a lot together there. There were others. Roger Maynard[41] was one. I also had more contact with a long list of French soft-matter scientist. At that time, France really was the champion of the renormalization group field-theoretic formalism. I learned from them. I even did some instantons [calculations][42], and things like that.

**PC:** After your work on RG, you stopped paying interest on spin glasses for a few years, at least according to your publication record. Then, in 1981-1982, you went on a Guggenheim fellowship to Paris, where you started working with Gérard Toulouse.

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[36] 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
[37] André Blandin, "Theories versus experiments in the spin glass systems," *J. Phys. Coll.* **39**, C6-1499 (1978). https://doi.org/10.1051/jphyscol:19786593
[38] Édouard Brézin : https://en.wikipedia.org/wiki/%C3%89douard_Br%C3%A9zin
[39] Jean Zinn-Justin : https://en.wikipedia.org/wiki/Jean_Zinn-Justin
[40] Gérard Toulouse : https://en.wikipedia.org/wiki/G%C3%A9rard_Toulouse
[41] Eric Akkermans *et al.*, "Roger Maynard 1938–2015," Eur. Phys. J. Special Topics 226, 1349–1352 (2017). https://doi.org/10.1140/epjst/e2017-70032-0
[42] See, e.g., A. B. Harris and T. C. Lubensky, "Mean-field theory and ε expansion for Anderson localization," *Phys. Rev. B* **23**, 2640 (1981). https://doi.org/10.1103/PhysRevB.23.2640; T. C. Lubensky, "Fluctuations in random walks with random traps," *Phys. Rev. A* **30**, 2657 (1984). https://doi.org/10.1103/PhysRevA.30.2657

**TCL:** [0:18:26] We really didn't do much together. There's just a couple of papers that we did[43]. Mostly I spent my time preparing my French lectures. Really it was amazing. People from all over Paris came. It must have been a hundred people who attended.

**PC:** Is that when you became acquainted with the replica symmetry breaking ideas? Because you wrote a paper on spin glasses then…

**TCL:** [0:18:56] No. When did I first hear about it? I heard about it pretty quickly from Parisi. Before I went on that sabbatical, in '81-'82, there was a conference in Italy, in Rome, where I talked about applying all these techniques to the localization problem[44].

I don't know how much you've followed that work. We developed what we loved. It was a wonderful theory of the transition to the conducting state and so forth. It was totally wrong, but we were able to show that it was in the same universality class as lattice animals. We had a theory where we did the instantons and we showed where the localized states were. But it gave that the density of states at the transition was zero, and apparently you could show rigorously that was not true. That's something that still bothers me. I heard from others that Parisi paid some attention to this work, but it did not go far.

**PC:** Did working on spin glasses influence the rest of your research? Are there some connections we might be able to see?

**TCL:** [0:20:35] With Jing-Huei, we left some mysteries behind, if you wish. We looked at the intersection between spin glasses and the ordered state[45]. There's a critical point between the ferromagnetic and the spin glass. When we did it in the Heisenberg, as opposed to the Ising model, we got this fixed point with a complex exponent. It drove us crazy because it does a spiral, and we didn't know what to do with it.

I did much more on percolation after that. Percolation was sufficiently close that we watched out a little bit, but I really got discouraged by the

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[43] G. Toulouse, M. Gabay, T. C. Lubensky and J. Vannimenus, "On the order of the spin glass transitions in mean field theory," *J. Physique Lett.* **43**, 109-113 (1982). https://doi.org/10.1051/jphys-let:01982004304010900 R. Rammal, T. C. Lubensky and G. Toulouse, "Superconducting networks in a magnetic field," *Phys. Rev. B* **27**, 2820 (1983). https://doi.org/10.1103/PhysRevB.27.2820

[44] Disordered Systems and Localization, Rome, Italy, May 1981. T. C. Lubensky, "Critical properties of the Anderson mobility edge: Results from field theory," *Lecture Notes in Physics* **149**, 199-214 (1981). https://doi.org/10.1007/BFb0012559

[45] See Ref. 33.

complexity of spin glasses. It was pushing the limits of my analytical abilities. Then other things came along. Shortly thereafter quasi-crystals came up and all that[46]. I moved on to other things. Later on, I got more sucked into experimental things, which was a lot of fun too.

**PC:** You have a pretty intimate understanding of both the US and European physics communities. Could you help us understand the difference in response to ideas of replica symmetry and replica symmetry breaking between the two communities?

**TCL:** [0:22:10] You know there was the conflict between the Fishers—Daniel[47] and Michael—and Parisi. They were not too fond of the spin-glass stuff that was going on in Europe, and, I think, vice versa. We were surprised, over here, how much effort was going into replica symmetry breaking. It was really quite a long period before it was resolved. For example, Cirano De Dominicis[48], with whom I had a lot of contact, spent an enormous amount of time on it.

**PC:** During your Les Houches lectures in 1978 at the Ill-Condensed Matter session, you mentioned the "infamous replica trick"[49]. First, what was infamous in your mind at that point? And second how important was that school from your perspective?

**TCL:** [0:23:59] Alan Luther wrote the first papers on replica symmetry breaking that I saw. I don't know if he invented it, but his was the first I saw. He made this mistake about the sign. He had a fixed point, which corresponded to having a positive definite quantity negative. That's why I called it infamous. But in fact I employed it afterwards. I think it really is a valid trick, until you hit the spin glass stuff. It basically was a good way of keeping track of the diagrams. It really was that. As long as you were perturbative, it was a safe thing. But then there was a lot of doubt whether replica symmetry made any sense, and I think it was justified.

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[46] See, e.g., D. Levine, T. C. Lubensky, S. Ostlund, S. Ramaswamy, P. J. Steinhardt and J. Toner, "Elasticity and dislocations in pentagonal and icosahedral quasicrystals," *Phys. Rev. Lett.* **54**, 1520 (1985). https://doi.org/10.1103/PhysRevLett.54.1520 J. E. S. Socolar, T. C. Lubensky and P. J. Steinhardt, "Phonons, phasons, and dislocations in quasicrystals," *Phys. Rev. B* **34**, 3345 (1986). https://doi.org/10.1103/PhysRevB.34.3345

[47] Daniel S. Fisher: https://de.wikipedia.org/wiki/Daniel_S._Fisher

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

[49] Les Houches, Session XXXI, July 3-August 18, 1978. T. C. Lubensky, " Thermal and geometrical critical phenomena in random systems," in R Balian, Gerard Toulouse, R Maynard, eds., *Ill-condensed Matter* (Amsterdam: World Scientific, 1979), 405-476.

**PC:** I forgot to ask something earlier. You started to work with Toulouse, and you also wrote a paper with his collaborator, Rammal Rammal[50].

**TCL:** [0:25:10] Rammal, yes. We did at least one paper. One of the papers, Gérard wrote and put my name on it; I'm not sure I even knew it[51]. The thing with Rammal was these superconducting loops that we worked on. That also led to another RG thing. I did one with these superconducting lattices with my postdoc Sajeev John[52], whose name is now associated with light localization.

**PC:** So you never interacted much with Rammal himself?

**TCL:** [0:25:59] I did not interact too much with him. He visited us at Penn for a while and we talked about things. He was a bright guy, a bit strange. Then he had his unfortunate early demise. I knew him, but not too much. Mostly, Gérard was my French teacher. He was a *perfectionniste, si tu veux*. It was very useful to be corrected constantly.

**PC:** Getting back to the replica trick. You taught the replica trick at the Les Houches school, but is there any other context in which you taught the replica trick after that?

**TCL:** [0:26:52] I certainly taught it in the renormalization group class I taught at Penn. Where else did I teach it? I don't think… It was something everybody used at the time. I didn't have any special relationship to it, other than it was a useful tool.

**PC:** I mention this because your book *Principles of Condensed Matter Physics*[53], does not mention spin glasses, the replica trick, replica symmetry breaking…

**TCL:** [0:27:24] Or random systems. I debated about that, but I ran out of steam. The reason why I did not put it is that it really is an enormous topic in itself. To pick and choose, I felt, was too much. It's certainly something that we discussed, Paul Chaikin[54] and I. But after 10 years of writing the book we just didn't have the energy to do any more. In some ways, some of the random things were a little more disappointing than they could have been.

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[50] Rammal Rammal: https://en.wikipedia.org/wiki/Rammal_Rammal
[51] R. Rammal, T. C. Lubensky and G. Toulouse, "Superconducting diamagnetism near the percolation threshold," *J. Physique Lett.* **44**, 61-71 (1983). https://doi.org/10.1051/jphyslet:0198300440206500
[52] Sajeev John: https://en.wikipedia.org/wiki/Sajeev_John
[53] P. M. Chaikin and T. C. Lubensky, *Principles of Condensed Matter Physics* (Cambridge: Cambridge University Press, 1995).
[54] Paul M. Chaikin : https://en.wikipedia.org/wiki/Paul_Chaikin

We had all this beautiful machinery that didn't always agree with experiments. I was just a little discouraged about it. I felt that if I were teaching the generation to which my book was available, it was less fundamental than the stuff that was actually put in the book.

**PC:** So the book is not necessarily a reflection of what you were really teaching in the classes at Penn, where you were teaching about disordered systems and replica symmetry breaking…

**TCL:** [0:28:32] Renormalization group, I taught… It's true. That was before the book that I taught classes that had disordered systems. There's really a lot to get through. I talked about it with Alan McKane, who was a postdoc of mine. (We did a couple of things[55].) He and I wrote half a book during my visit to Paris. Again, it's one of these things where we had it all written but then didn't go anywhere.

**PC:** About disordered systems?

**TCL:** About disordered systems, yes. I don't know if I can find my notes on it but…

**PC:** If you find your notes, that would be very interesting. Is there anything else from that era that you think are of interest and that we might have skipped over?

**TCL:** [0:29:42] The percolation problem actually had a big impact on how we thought about random systems. Probably there are more people who worked on percolation than on spin glasses. The other thing about that era is that the work we did on what we called lattice animals was a big surprise[56]: all of this stuff about dimensional reduction[57]—theories in six dimensions which really apply to four dimensions—the Yang-Lee edge[58] and all of that business. There were all of these really surprising and very nice applications of renormalization group to problems that were of interest to

[55] See, *e.g.*, T. C. Lubensky and A. J. McKane, "Cluster size distribution above the percolation threshold," *J. Physics A* **14**, L157 (1981). https://doi.org/10.1088/0305-4470/14/5/011 A. R. Day, T. C. Lubensky and A. J. McKane, "Dislocations and the nematic—to—smectic-*A* transition for arbitrary values of $K_1$." *Phys. Rev. A* **27**, 1461 (1983). https://doi.org/10.1103/PhysRevA.27.1461

[56] T. C. Lubensky and Joel Isaacson, "Statistics of lattice animals and dilute branched polymers," *Phys. Rev. A* **20**, 2130 (1979). https://doi.org/10.1103/PhysRevA.20.2130

[57] See, *e.g.*, Giorgio Parisi and Nicolas Sourlas, "Random magnetic fields, supersymmetry, and negative dimensions," *Phys. Rev. Lett.* **43**, 744 (1979). https://doi.org/10.1103/PhysRevLett.43.744

[58] See, *e.g.*, Michael E. Fisher, "Yang-Lee edge singularity and $\varphi^3$ field theory," *Phys. Rev. Lett.* **40**, 1610 (1978). https://doi.org/10.1103/PhysRevLett.40.1610

all kinds of people. The polymer people liked the statistics of polymer clusters and things like that. I would put that in the general group of things that were in the air during the spin glass period.

**PC:** Would it be fair to say that you found those more rewarding because there was a better reception?

**TCL:** [0:31:00] It was something that I knew how to control. I didn't know how to deal with the… Replica symmetry breaking is a very technically complex thing. I saw all of the stuff coming out of France and Italy and I knew that for me to be able to contribute I would have to sit down and spend a lot of time. There were all these other things going on, which were a lot more accessible to me.

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

**TCL:** [0:31:39] I have stacks and stacks of notes. I have notes on most of the papers I wrote, but when I look at them now they're sometimes hard to get a hold of. I have a whole office that I've got to clean up now with papers all over the place. I was pretty careful about saving notes which were relevant to the papers I have. But you know how it is when you write notes. You write them and then you find there's a mistake, and rewrite them a little bit. So when you go back and look at them… Remember this is just shy of 50 years ago. Memory is… What kind of things would you find interesting?

**PC:** Your correspondence with your co-authors, or correspondence with the community in general. The notes on your work, or…

**TCL:** [0:32:47] I may have some. The person I exchanged more letters with was Amnon Aharony[59], who at the time was one of the leading guys in the RG business. Before the internet came on board, there was more letter-writing than there is now. In the early seventies, I certainly wrote a number of letters. Whether I have those letters or not I don't know. If I ever get back in my office, I can have a look if you really think that's the sort of things you'll find interesting.

**PC:** I would definitely encourage you to contact the rare books and manuscripts archives at UPenn to deposit at least some of the more salient or relevant pieces. The notes from the book you didn't publish, for instance.

[59] Amnon Aharony : https://en.wikipedia.org/wiki/Amnon_Aharony

**TCL:** [0:33:34] If I find those notes, yes. I have half written another book with David Pine[60], now at NYU, for sophomores about waves, which is probably not so interesting. Some people were aware of their impact on history and thought about it from the very beginning of their career. Frankly, I never thought I was going to have an impact. It took me a while to develop the self-confidence that one needs to think in advance about those things. I always felt under the shadow above all the Fisher, Martin, Halperin crew who were always the ones who never made mistakes and were always in the front row harassing the speakers.

**PC:** Famously so, yes. Thank you very much for your time.

**TCL:** My pleasure. I'm curious to see where this all goes.

[60] David J. Pine: https://en.wikipedia.org/wiki/David_J._Pine