History of RSB Interview: Geoffrey Grinstein

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Interviewers:
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Over Zoom, from Dr. Grinstein's home in New York, New York, USA.
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PC: Good morning, Dr. Grinstein. Thank you very much for sitting down with us. As we've discussed ahead of time, the general theme of these discussions is the history of replica symmetry breaking in physics, which we bound roughly from 1975 to 1995. But before we get to the heart of that topic, I'd like to ask a few background questions to enrich and contextualize our understanding. First, can you tell us a bit about your family and your studies before starting university at McGill.

GG: [0:00:31] Sure. I was born and raised in Montreal in a section of town called Côte Saint-Luc[1], which was a newly formed suburb. It used to be farmland, but there was this new tract of houses that young families moved into. I went to public high school, Wagar High School[2], where I got a very decent education. Not sure what else you'd like to know. I have a sister and a couple of parents, who were alive until relatively recently.

PC: How did you get interested in science and physics, in particular?

GG: [0:01:26] It was really in high school. We had a wonderful math teacher, who inspired our interest both in math and science. When I still get in touch sometimes with some of my old friends from high school, we all remember him with great fondness. Not everybody ended up in science, but a lot of them ended up engineers, architects etc. Without exception they all credit him for sort of pointing them in the direction of some kind

[1] Côte Saint-Luc: https://en.wikipedia.org/wiki/C%C3%B4te_Saint-Luc
[2] Wagar High School: https://en.wikipedia.org/wiki/Wagar_High_School

of profession that involves analytics. So, I would credit him—his name is Bob Kurys[3]—with inspiring a lot of students in those days.

PC: What then lead you to pursue a PhD in theoretical physics at Harvard with Alan Luther[4]?

GG: [0:02:30] I started out in the joint math and physics program at McGill, but it turned out that that was a five-year program[5], and I was getting restless after three years. I thought: "I'd like to graduate at the same time all my buddies were graduating." So, I switched at some point to a pure physics major, because I felt that I had taken a certain amount of both math and physics, and that physics somehow suited my interest and personality a little better. So, I graduated with a degree in physics and then I was anxious to pursue those studies. I applied to graduate school in a bunch of places. Harvard was one of the places I got accepted to, and there it was. As far as working with Alan was concerned, there was a small number… I decided quite early on I did not want to do high energy physics. I wanted to do lower energy stuff. Condensed matter seemed like a satisfying subfield for me. There were relatively few theorists doing that. Paul Martin[6], whom I'm sure you know by reputation, was there. He was on leave that year when I was choosing a thesis advisor, so it made it very easy. I think Alan was just about it. There were some people doing band structure theory and I wasn't particularly keen on doing that, so it made the decision very easy. He couldn't have been nicer and more encouraging to me. It turned out to be a very good choice.

PC: You worked on disorder for your thesis[7]. There are a few aspects of that work I'd like to probe a bit more. One of the foundational ideas in your thesis is to describe how to average over disorder, in order to obtain an average free energy functional, which is now known as a quenched average. Do you remember where this idea originated from?

GG: [0:04:48] I do, actually. It came from the journal club. I can't remember who gave the presentation. In the small group of students who Alan was in charge of and a couple of other students with different advisors, we had

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[3] S. Robert Kurys. See, *e.g.*, S. R. Kurys, "Bob Kurys fonds," *Toronto Metropolitan University Archives* (1997). https://archives.library.torontomu.ca/index.php/bob-kurys-fonds (Consulted December 24, 2022.)

[4] Alan Luther: https://en.wikipedia.org/wiki/Alan_Harold_Luther

[5] See, *e.g.*, " Joint Honours in Mathematics and Physics," *McGill Department of Physics* (2022). https://www.physics.mcgill.ca/ugrads/math.html (Consulted December 3, 2022.)

[6] "Paul Cecil Martin," *AIP Physics History Network* (n.d.) https://history.aip.org/phn/11606015.html (Consulted December 3, 2022.)

[7] Geoffrey Mark Grinstein, *Magnetic phase transitions in alloys; a renormalization group approach*, PhD Thesis, Harvard University (1974). https://id.lib.harvard.edu/alma/990038509680203941/catalog

a theory journal club that met once a week. One of the students would read an interesting looking preprint and try to describe it to the rest. One of those preprints was a work by Shang-keng Ma[8], which was an attempt to… I'm not sure what the problem was that he was addressing. I think that was the localization problem, but I don't have a copy of that ancient preprint which was later withdrawn. In that preprint, he had this idea of using the n=0 limit as a way of turning a quenched random problem into a translationally invariant problem. He may have gotten the idea from De Gennes' work[9], who had done something about the excluded volume problem using n=0, but I'm not sure. I can't really remember the detailed history of where his paper came from, if I ever knew. It turned out that this paper had a serious flaw, in that it produced a translationally invariant Hamiltonian that was unstable. The Hessian had the wrong sign. When he realized that—I think people realized that quite quickly—he withdrew the paper. But Alan Luther, my advisor, had had this idea for a while that disordered systems ought to be able to have sharp phase transitions the way pure systems did, even though a lot of the experimental evidence at the time seemed contrary. The transition seemed as if it might be rounded. There was this one existing analytic calculation by McCoy and Wu for a disordered system that showed that the transition looked very different[10]. But, of course, the system they studied had correlated disorder. The disorder correlated along rows of the two-dimensional system as I recall. So, it wasn't clear how well that would apply to systems with short-range correlated disorder. Alan had this intuition that it ought to be possible. I'm not sure where that intuition came from. It was just a feeling he had. We had been talking about this a little bit and had no idea how to proceed. When somebody came up with this preprint and talked about it, a light bulb went off in both his and my head. It was like: "Whoa! This might be something that we could conceivably use to have a go at all the problems we were interested in." That's how the project got started.

PC: Did you at any point got in touch with Ma during that work, was it just through the preprint?

[8] Grinstein's thesis states (p. 18) "a formal mathematical trick devised by Ma to attack the problem of an electron in a random potential", citing (p. 121) "Shang-keng Ma, preprint, later withdrawn". Other sources date the preprint to 1972. See, *e.g.*, P. Charbonneau, "From the replica trick to the replica symmetry breaking technique," *IAMP News Bulletin* **2022**(October), 5-25 (2022).

[9] P.-G. de Gennes, "Exponents for the excluded volume problem as derived by the Wilson method," *Phys. Lett. A* **38**, 339-340 (1972). https://doi.org/10.1016/0375-9601(72)90149-1; See also: T. A. Witten, "The n=0 Discovery," In: *P.G. De Gennes' Impact on Science — Volume II: Soft Matter and Biophysics*, F. Brochard-Wyart, J. Prost and J. Bok, eds. (Singapore: World Scientific, 2009), 1-17. https://doi.org/10.1142/9789814280648_0001

[10] See, *e.g.*, B. M. McCoy and T. T. Wu, "Theory of a Two-Dimensional Ising Model with Random Impurities. I. Thermodynamics," *Phys. Rev.* **176**, 631 (1968). https://doi.org/10.1103/PhysRev.176.631

GG: [0:08:30] It was just through the preprint, I think. Later on, he and I wrote papers together[11]. I would like to say a word about him[12]. The most delightful guy. Brilliantly creative, modest, friendly. I can't say enough nice things about him. He passed away, unfortunately, very young. I'm glad to be able to remember him as one of the founders of this whole enterprise. As I remember—having gotten to know him later—he was embarrassed about that preprint. He didn't want to talk about it. He didn't want to hear about it. It's just an example of how interesting mistakes can be as important sometimes as work that turns out to be correct.

PC: The treatment of disorder in your thesis was redone and expanded by Victor Emery[13] in a paper published in '75 and submitted in '74[14]. That paper does cite your thesis. How were you in touch with Emery, and what did you think of his demonstration?

GG: [0:09:56] It made me feel stupid, because it was so much easier than what I had done, among other things. We were definitely in touch. I spent two summers at Brookhaven, where he was working. That would have been the summer after my final year, so I guess the summer of '74 and possibly the summer of '75 or maybe '73 and '74. I'm not sure I got the dates 100% right. So, he and I talked a lot. I told him about what we were doing. Also, he had worked very closely with Alan. I think Alan was a postdoc of his at Brookhaven[15]. So, they were in constant touch. I'm sure Alan was also telling him the kinds of things we were doing. He got interested in it and produced this much more elegant way of deriving the translationally invariant Hamiltonian.

PC: So, by the time you wrote up the paper based on the thesis, you cited Emery's treatment. You then also cited a 1959 paper by Brout[16] that explains the rationale for computing the quenched disorder. (It's not cited

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[11] See, *e.g.*, G. Grinstein, S.-k. Ma, and G. F. Mazenko, "Dynamics of spins interacting with quenched random impurities," *Phys. Rev. B* **15**, 258 (1977). https://doi.org/10.1103/PhysRevB.15.258; G. Grinstein, and S.-k. Ma, "Roughening and lower critical dimension in the random-field Ising model," *Phys. Rev. Lett.* **49**, 685 (1982). https://doi.org/10.1103/PhysRevLett.49.685; "Surface tension, roughening, and lower critical dimension in the random-field Ising model," *Phys. Rev. B* **28**, 2588 (1983). https://doi.org/10.1103/PhysRevB.28.2588

[12] Shang-keng Ma: https://en.wikipedia.org/wiki/Shang-keng_Ma

[13] Victor J. Emery: https://en.wikipedia.org/wiki/Victor_Emery

[14] V. J. Emery, "Critical properties of many-component systems," *Phys. Rev. B* **11**, 239 (1975). https://doi.org/10.1103/PhysRevB.11.239. The manuscript was received on 30 July 1974, and Grinstein's thesis is dated May 1974.

[15] **PC:** Luther was a postdoc at Brookhaven National Laboratory from 1969 to 1971.

[16] R. Brout, "Statistical mechanical theory of a random ferromagnetic system," *Phys. Rev.* **115**, 824 (1959). https://doi.org/10.1103/PhysRev.115.824

in your thesis, but it appears as a footnote in the paper.) Do you recall how you became aware of that work?

GG: [0:11:28] I'm sorry, I don't. I have no recollection of that.

PC: Beyond what Victor Emery did, what was the initial reaction to your work from the community?

GG: [0:11:47] That's a good question. I remember people being interested. I think part of the problem was that I was very slow turning the thesis into a paper. I left graduate school, I took a postdoc at the University of Illinois, and for some reason I had no idea how this business works. I was just a kid, like we all are when we just finish up. In those days, I think students were much less plugged into the system than they are now. Now, people go into graduate school with a much clearer sense of how things are supposed to work and what's supposed to happen to them after they graduate. Me, I guess I was a slow learner. I had very little sense… Actually, I was a little bored with the problem after finishing the thesis. I was a bit shocked that it worked, that you could get this thing to work. I had spent a lot of time on it and I was kind of bored. I was looking for other things to do. Alan also wasn't pushing me or wasn't very ambitious to get the paper written, so it took a long time. I don't think that many people were aware of it until a little bit later. The people who knew about it seemed excited about it: Vic, for example, and some others. By the time I got around to writing the paper, Tom Lubensky[17] had written a paper doing similar things, doing the renormalization group with the quenched disorder itself. When that paper came out, I think there was a shift in the perception of the possibility of having sharp phase transitions in disordered systems. Those papers did convince people. It too had an influence on experimentalists. If you think there's something there, it's somehow easier to find it experimentally than if you're very unsure that it's there. I feel as if the whole field slowly shifted—it took a few years—in terms of the perception of sharp phase transitions in disordered systems. I don't know if you wanted to talk about this, but that interest reached a fever pitch with the random field problem around the same time.

PC: Professor Lubensky has told us that he heard about the replica trick from you[18]. Do you remember what would have been the context of that interaction?

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[17] T. C. Lubensky, "Critical properties of random-spin models from the ε expansion," *Phys. Rev. B* **11**, 3573 (1975).

[18] P. Charbonneau, *History of RSB Interview: Tom C. Lubensky*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 13 p. https://doi.org/10.34847/nkl.f2cap2m9

GG: [0:15:17] I don't. I remember knowing Tom of course. I don't remember exactly how or when I would have told him. Perhaps at a meeting. Take this with a giant grain of salt, but I may have given a short talk at the big APS meeting that year, in 1974. I may have talked to him there[19]. I just don't remember the details of how I first talked to him.

PC: From what I could tell, you've never used the replica trick again in your own work. Is that correct? If yes, is there any particular reason why not?

GG: [0:16:11] I haven't even thought about that, but I think that's right. I probably never did. I guess the context in which it could be used is somewhat limited. It's pretty much in glasses, where it's been used subsequently, or other things, possibly. I just don't remember.

PC: Yes, and more[20].

GG: Really? Interesting. I guess by the time spin glasses came around, which I never really worked on intensively, it was broken replica symmetry that seemed to be the thing that people were interested in. I never really got into that. I never really felt that I appreciated what people have done with that. I did make some attempt, but not a serious enough attempt. There were a lot of complicated ideas going around at the time. Haim Sompolinsky[21], this super brilliant guy, had all these ideas, which I always felt I was never quite fully understanding. That was probably before the broken replica symmetry came along. I just never really worked intensively on a problem where it seemed relevant.

PC: Did you follow the conversation about the breaking of replica symmetry or was this done post facto, looking back through literature?

GG: [0:18:01] Not so much, I'd say. I went to a few talks on the subject. I wasn't sure how much of it I believed, but it wasn't a super expert opinion by any stretch of the imagination. I tried to read a couple of articles.

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[19] Ref. 16 cites A. Luther and G. Grinstein, "Magnetic Phase Transitions in Alloys," AIP *Conference Proceedings* **18**, 876 (1974). https://doi.org/10.1063/1.3141834. This work, which may have been the basis for Grinstein and Lubensky to have discussed, is part of the proceedings of the *19th Annual Conference on Magnetism and Magnetic Materials*, Hugh C. Wolfe, C. D. Graham and J. J. Rhyne, 13–16 November 1973, Boston, Massachusetts, USA.

[20] **PC:** Sir Sam Edwards and others have used *innocent replicas* in various soft matter contexts, for instance. See, *e.g.*, P. Charbonneau, *History of RSB Interview: Fumihiko Tanaka*, transcript of an oral history conducted 2022 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 11 p. https://doi.org/10.34847/nkl.adfcm02v

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

PC: Any dates or times or people that you might have heard this from?

GG: [0:18:33] Certainly, the name Giorgio Parisi[22], I remember in that context. I remember meeting him in Italy at some point. It might have been in Florence. I spent a month there at some point in the '80s with Roberto Livi and others. I think that's where I first met him. I have a feeling that he was talking about that at the time, but again the details are a little bit hazy in my mind.

PC: As you were alluding to earlier, you worked on the random field Ising model and other disordered models afterwards[23]. What questions were you pursuing at that point? What was the general program?

GG: [0:19:31] It seems like such a small detail now, but people were absolutely fascinated with the question of what the lower critical dimension was for the random field—Ising in particular, but in general—random field models. There had been this early argument by Imry and Ma[24] about domain walls, very elegant, simple argument, which suggested that the lower critical was two in those systems. I did a calculation in '75, or something like that, which concluded that there was a dimensional reduction in the critical exponents[25]. In other words, you could do an expansion around six, which is the upper critical dimension, and it turned out that the exponents you got were the same as pure system exponents in two lower dimensions, which suggested that—although hardly convincingly—three was actually the lower critical dimension, because one is the critical dimension for pure Ising systems. Anyway, this started kind of an industry because by then the experimentalists had figured out ways to make good realizations of the random field problem with random antiferromagnets in a uniform field. This great controversy arose because there were two main groups, as I

[22] See, *e.g.*, P. Charbonneau and F. Zamponi, *History of RSB Interview: Giorgio Parisi*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 80 p. https://doi.org/10.34847/nkl.7fb7b5zw

[23] See also G. Grinstein, A. N. Berker, J. Chalupa and M. Wortis, "Exact renormalization group with Griffiths singularities and spin-glass behavior: The random Ising chain," *Phys. Rev. Lett.* **36**, 1508 (1976). https://doi.org/10.1103/PhysRevLett.36.1508; G. Grinstein, C. Jayaprakash and M. Wortis, "Ising magnets with frustration: Zero-temperature properties from series expansions," *Phys. Rev. B* **19**, 260 (1979). https://doi.org/10.1103/PhysRevB.19.260

[24] Y. Imry and S.-k. Ma, "Random-field instability of the ordered state of continuous symmetry," *Phys. Rev. Lett.* **35**, 1399 (1975). https://doi.org/10.1103/PhysRevLett.35.1399

[25] G. Grinstein, "Ferromagnetic phase transitions in random fields: the breakdown of scaling laws," *Phys. Rev. Lett.* **37**, 944 (1976). https://doi.org/10.1103/PhysRevLett.37.944. See also G. Grinstein, "On the lower critical dimension of the random field Ising model," *J. Appl. Phys.* **55**, 2371-2376 (1984). https://doi.org/10.1063/1.333669

remember it, doing this: the MIT group under Bob Birgeneau[26], and a group in California under Vince Jaccarino[27]. These two were vigorously at odds. (I hope I'm remembering this right.) I think that the MIT group was arguing that d_c=3—the lower critical dimension was three—and therefore you couldn't have true phase transitions below three dimensions. And the reverse by the UCSB group. Of course, this experimental effort was complicated by severe critical slowing down. Everybody understands that now, but at the time I don't think people understood how severely the dynamics could influence what you saw experimentally, that to try to get to the true equilibrium behavior could be extraordinarily difficult. I don't know if you're talking to either of those two guys, but there was so much hostility between those groups that lawsuits were threatened. At least, that was the rumor that was going around over this issue. People cared about this very passionately. Around that time, Shang-keng Ma and I wrote a paper arguing that d_c=2 was correct[28]. So, we came in on that side of the debate, and eventually that turned out to be correct, as I remember.

PC: Was that paper with Gene Mazenko? Or was it a different one? "Dynamics of spins interacting with quenched random impurities" in 1977?

GG: [0:23:40] No. This wouldn't have been dynamics. This was purely talking about the statics, whether there was an ordered phase.

PC: Jumping forward in time, in the mid-1980s, you worked on neural networks from a spin glass perspective with Sara Solla[29] and John Hertz[30]. How did these ideas and this collaboration come about[31]?

GG: [0:24:28] My recollection is a little bit hazy, but it must have been because I was spending a fair bit of time in those days at Nordita in Copenhagen, where John was a professor. He and I had known each other for a long time, since his days at the University of Chicago. (He and Gene Mazenko

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[26] Robert J. Birgeneau: https://en.wikipedia.org/wiki/Robert_J._Birgeneau

[27] Vincent Jaccarino (1924—2019); See, *e.g.*, https://chancellor.ucsb.edu/memos/2019-09-03-sad-news-professor-emeritus-vincent-jaccarino (Consulted January 13, 2021.)

[28] See Refs. 11.

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

[30] P. Charbonneau, *History of RSB Interview: John A. Hertz*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 18 p. https://doi.org/10.34847/nkl.cad347wh

[31] J. A. Hertz, G. Grinstein and S. A. Solla, "Memory networks with asymmetric bonds," *AIP Conference Proceedings* **151**, 212-228 (1986). https://doi.org/10.1063/1.36259; "Irreversible spin glasses and neural networks," In: *Heidelberg colloquium on glassy dynamics*, J. L. van Hemmen and I. Morgenstern, *Lecture Notes in Physics* **275** (Berlin: Springer, 1987). https://doi.org/10.1007/BFb0057533

and Paul Horn[32] were the young guys I first met at the University of Chicago years earlier.) Sarah was also working in Copenhagen at that point, so I think that's how the collaboration got started. The details and origin of the ideas and the discussions are really beyond me at this point. I don't have a clear recollection of them.

PC: Shortly afterwards, you coauthored a piece for *Physics Today* with Daniel Fisher[33] and Anil Khurana that includes a discussion of spin glasses[34]. How did you come to work on this piece? How did this happen?

GG: [0:25:43] Daniel is somebody that I have known since he was a kid. I first met him when he was about to start graduate school[35]. Possibly at a meeting where his dad, Michael[36], was giving a talk and part of the family was tagging along. That may have been the first time I met him. That's certainly how I met his younger brother, Matthew[37]. It was at one of those meetings.

Matthew was at IBM when Dan and I wrote the *Physics Today* article. I was friendly with both of them. In general, Dan used to visit often. We would often chat. We were both interested in random magnets. I think that's how the idea came about. I can't remember whether we were asked by *Physics Today* to write that article or whether we suggested it to them. Anil Khurana was one of the editors of *Physics Today* at that time. The idea to do the article may have come from him.

PC: Did you have a particular opinion about finite-dimensional spin glasses, or this was largely Daniel's contributions to that piece?

GG: [0:27:05] It was largely his contribution. I certainly liked to try to think about the stability of ordered states in terms of droplets, surface tensions. In particular, I liked the idea of the dynamics being crucial because it would be logarithmically slow, given all the frustration in the system. I thought those ideas were actually quite fruitful, particularly if you were trying to understand experiments. So, I was comfortable with those ideas. I don't

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[32] "Paul Horn," *AIP Physics History Network* (n.d.) https://history.aip.org/phn/11601019.html (Consulted December 5, 2022.)

[33] Daniel S. Fisher: https://en.wikipedia.org/wiki/Daniel_S._Fisher

[34] D. S. Fisher, G. M. Grinstein and A. Khurana, "Theory of random magnets," *Physics Today* **41**(12) 56-67 (1988). https://doi.org/10.1063/1.881141

[35] **GG:** What I said about meeting Daniel Fisher for the first time is wrong. We met for the first time in Copenhagen in the mid-70's, probably 1976, when he would have been a year or less into his grad school career.

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

[37] Matthew Fisher: https://en.wikipedia.org/wiki/Matthew_P._A._Fisher

know whether they have held up. He certainly played the leading role in writing that section of the paper, but I think we were pretty much in agreement on the physics that was written there. Later, as I mentioned to you in an email, one of my friends in the field said: "Hey! You guys, you really slighted a pretty significant French group who had done a lot of work on spin glasses.—I think you could have done a much better job acknowledging their contributions." In hindsight, I realize that was true. I really should have taken a broader view of the field before writing that article. I apologize, guys! It should have been included. I can't even remember the names of the guys we're talking about, but I know we slighted them.

PC: At about the same time, you published what's considered to be a milestone work about boson localization with Matthew and Daniel Fisher[38]. How did that come about?

GG: [0:29:22] Again, through IBM. Matthew was at IBM at the time; Daniel was visiting a lot.

PC: But who was interested in this problem first?

GG: [0:29:33] It was really Matthew who was the driving force behind that paper, I would say. The rest of us were very interested, contributed here and there, but as I remember it, Matthew was the driving force behind that paper.

PC: During your time at IBM or elsewhere, did you ever teach about the replica trick, or replica symmetry breaking glasses or spin glasses in any context?

GG: [0:30:08] Not teach. I certainly gave lectures at conferences and visited universities. I gave lectures about random magnets early on, because certainly by 1980, all the interest had turned to glasses and spin glasses. I didn't feel I had that much to say about those subjects. Every once in a while, there would be a review conference or something talking about the good old days. I would go and give a talk. But really it was mostly early on when people were still trying to figure out what was going on with quenched random systems.

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[38] M. P. A. Fisher, P. B. Weichman, G. Grinstein and D. S. Fisher, "Boson localization and the superfluid-insulator transition," *Phys. Rev. B* **40**, 546 (1989). https://doi.org/10.1103/PhysRevB.40.546. The article is contained within the collection: *Physical Review B 50th Anniversary Milestones* https://journals.aps.org/prb/50th

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

GG: [0:31:12] Not that comes to me right away, but this conversation has reminded me of other things which I hadn't thought of. If something else comes to me, I'll have the chance to get back to you.

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

GG: [0:31:37] I doubt that I do, although hidden down in the basement somewhere are maybe some old notes. I certainly have no plan to deposit them. Would that be part of this archive? Are you thinking about that?

PC: We can discuss the logistics afterwards, but if you do have material, such as correspondence with others about the work, that could be of interest.

GG: [0:32:10] The only thing I remember that I might have is a nasty—not really—one of those letters that say: “No! This cannot be right.” I wonder if I still have that. That's the only thing that comes to mind I might have kept, just because afterwards when I read it, I said: “This was so stupid. Why did I write this letter?” This is one of those things when I said to myself: “Email is a disaster. It doesn't give you enough time to think before you send it off.” Anyway, I can certainly have a look. I’m not sure if I have anything, but I’ll have a look.

PC: Thanks, Dr. Grinstein, for the conversation.

GG: [0:33:06] Very nice talking to you and meeting you.