History of RSB Interview: Devarajan Thirumalai

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PC: Good morning, Professor Thirumalai. Thank you very much for joining us. As we were discussing before to start this interview, the theme of this series is the history of replica symmetry breaking in physics, which we roughly bound from 1975 to 1995. But before we dive into these precise ideas with you, we have a quick background question. In a recent autobiographical piece that you wrote[1], you explained what led you to pursue research in theoretical chemistry, in general, but not really what led you to the University of Minnesota, in particular. Did you have a prior connection with the school? What brought you there?

DT: [0:00:47] I was an engineering and chemistry major as an undergrad. I had a pretty good job offer at the end of my undergraduate education. I came from a very prestigious school. My grades were not ~~very~~ great, but the reputation of the school was so good they would hire a janitor from there, essentially. I was building an instrument for a well-known chemist[2], and at some time in March, or so, of the last semester I was there, he asked me what my plans were. I told him that I had this job. In fact, I was going to make three times what my father was doing at that time. He said: "Look, you can always get this job. But why don't you go abroad for a PhD?" But I hadn't applied. It was too late. In fact, most of the [admission] decisions are made by that time. But he knew this rather famous chemist[3], a spectroscopist, in Minnesota. He said: "I'll call him, and we'll see if it can

[1] D. Thirumalai, "Autobiography of Dave Thirumalai," *J. Phys. Chem. B* **125**, 13834–13839 (2021). https://doi.org/10.1021/acs.jpcb.1c10031
[2] C. N. R. Rao: https://en.wikipedia.org/wiki/C._N._R._Rao
[3] Bryce Crawford: https://en.wikipedia.org/wiki/Bryce_Crawford

be worked out, because the US system is somehow flexible." That's why I got to the Chemistry Department without knowing a lot of chemistry. That's how I got to Minnesota.

PC: In that same autobiographical piece, you explained how you were led to think about classical statistical mechanics once you joined the University of Maryland. You were told explicitly that $\hbar$=0 in IPST[4] by Bob Zwanzig[5]. You also explained how you quickly you got to know Ted Kirkpatrick, who was your host during your initial visit. Is that when you first heard about spin glasses? Or had you encountered these systems before?

DT: [0:02:44] At the end of my postdoc—maybe a year before—in Columbia, I think… I can't quite remember. I was at a Rutgers meeting[6] probably, or something like that. I knew there was a lot of interest in glasses. As you know, it's not traditionally done in chemistry departments that much. But people like Frank Stillinger[7] and Hans Andersen[8], both associated with chemistry, were thinking about that. I think there was a talk by David Nelson[9]. He was talking about a frustration model, these icosahedra and stuff[10]. I thought it was sort of interesting. So, I picked up some stuff on glasses at the time. When I interviewed at Maryland, Ted was already there, and he had worked on some aspects of supercooled liquids already. From the time he picked me up at the train station for the interview, we discussed things about glasses. I knew that if I were to join, I would work with him at some point on this stuff. I sort of knew a little bit about the spin glasses as well, but not very much.

PC: Do you know how Ted himself got interested in this problem?

DT [0:04:17] Yeah. I do know that, more or less. Ted, in his early career and until the time I met him, was heavily interested in looking in dense liquids and transport in dense liquids. He had produced some very important results, which are [very well] known. In his thesis he looked at the

[4] Institute for Physical Science & Technology (IPST) of the University of Maryland. https://ipst.umd.edu/ (Consulted November 25, 2022.)
[5] Robert Zwanzig: https://en.wikipedia.org/wiki/Robert_Zwanzig
[6] As part of the activities of the Center for Mathematical Sciences Research at Rutgers University, semiannual conferences in statistical mechanics are held. Recent Statistical Mechanics Conferences: https://cmsr.rutgers.edu/news-events-cmsr/statistical-mechanics-conference/ (Consulted November 25, 2022.)
[7] Frank Stillinger: https://en.wikipedia.org/wiki/Frank_Stillinger
[8] Hans C. Andersen: https://de.wikipedia.org/wiki/Hans_C._Andersen_(Chemiker)
[9] David R. Nelson: https://en.wikipedia.org/wiki/David_Robert_Nelson
[10] See, *e.g.*, S. Sachdev and D. R. Nelson, "Theory of the structure factor of metallic glasses," *Phys. Rev. Lett.* **53**, 1947 (1984). https://doi.org/10.1103/PhysRevLett.53.1947; "Order in metallic glasses and icosahedral crystals," *Phys. Rev. B* **32**, 4592 (1985). https://doi.org/10.1103/PhysRevB.32.4592

temperature gradient effects on fluids[11]. He derived an exact result for the emergence of long-range correlation in the non-equilibrium steady state, which at a first glance looks like it is a perturbation result. The other factors enhanced tremendously and grow like the square of the gradient of the temperature. It's something that was very verified at IPST when I was there, in '95 or something like that[12]. Then, he also worked on long-time tails and shear viscosity[13]. The long-term tail was established, but the coefficient of the long-time tail decay was not theoretically calculated. He computed that. Then, in '84—this is the year before I joined—there were these papers by Leutheusser[14] and Götze and company[15] on mode coupling. Ted also did something important immediately after the mode coupling[16]. He therefore was already interested and knew more about the dynamics of liquids when you supercool them—compared to the other people in the field including me —from a theoretical perspective. He was into it, and I also thought it was a good problem to think about. I learnt a lot from Ted who is extraordinary in his capacity to perform calculations without errors. In addition, Zwanzig had a keen interest in the dynamics of supercooled liquids and conversed with Ted and I often. Because I had abandoned quantum [problems], I started to work on this stuff with Ted.

FZ: I know the paper of Ted from 1985 on mode coupling, but at that point did you already know about spin glasses specifically? You mentioned the glass problem and frustration from David Nelson's work, but what about magnetic materials and all the discussion that was ongoing in the early '80s about making a theory of these materials? Or did that happen later?

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[11] Theodore Ross Kirkpatrick, *On the theory of light scattering from fluids in nonequilibrium steady states*, PhD Thesis, The Rockefeller University (1981). https://rockefeller-primo.hosted.exlibrisgroup.com/permalink/f/ji7ros/01RU_ALMA2122306170004157 (Consulted November 25, 2022.)

[12] See, *e.g,* B. M. Law, R. W. Gammon and J. V. Sengers, "Light-scattering observations of long-range correlations in a nonequilibrium liquid," *Phys. Rev. Lett.* **60**, 1554 (1988). https://doi.org/10.1103/PhysRevLett.60.1554; J. R. Dorfman, T. R. Kirkpatrick and J. V. Sengers, "Generic long-range correlations in molecular fluids," *Annu. Rev. Phys. Chem.* **45**, 213-239 (1994). https://doi.org/10.1146/annurev.pc.45.100194.001241

[13] See, *e.g.*, T. R. Kirkpatrick and J. R. Dorfman, "Divergences and long-time tails in two-and three-dimensional quantum Lorentz gases," *Phys. Rev. A* **28**, 1022 (1983). https://doi.org/10.1103/PhysRevA.28.1022; T. R. Kirkpatrick, "Large long-time tails and shear waves in dense classical liquids," *Phys. Rev. Lett.* **53**, 1735 (1984). https://doi.org/10.1103/PhysRevLett.53.1735

[14] E. Leutheusser, "Dynamical model of the liquid-glass transition," *Phys. Rev. A* **29**, 2765 (1984). https://doi.org/10.1103/PhysRevA.29.2765

[15] U. Bengtzelius, W. Götze and A. Sjölander, "Dynamics of supercooled liquids and the glass transition," *J. Phys. C* **17**, 5915 (1984). https://doi.org/10.1088/0022-3719/17/33/005

[16] T. R. Kirkpatrick, "Mode-coupling theory of the glass transition," *Phys. Rev. A* **31**, 939 (1985). https://doi.org/10.1103/PhysRevA.31.939

DT: [0:06:59] I can't quite remember, but very shortly anyway—in ’85 or maybe in ’84—I had learned about these spin glass papers but did not know their contents all that well. I certainly knew Edwards-Anderson *Journal of Physics F* paper in ’75[17] before I came. But not the connection with glasses, which was discovered with Ted. They do share the word glass, but beyond that I didn't really know. But I was aware that there was something interesting going on in Ising spin-glass—type models and experiments.

PC: How did you familiarize yourselves with the techniques: the replica symmetry approach and replica symmetry breaking? Was this something that you knew before, of that you learned together? Or that Ted knew or that you knew?

DT: [0:07:59] We learned that together. I didn’t know anything about these methods I don’t think I knew even the SK model stuff at that time. Then, of course, the moment we got into this we became aware of many of these papers. And of the Gross-Mézard paper of ‘84[18] on the *p*-spin with *p* [goes to] infinity model. It was quite a struggle really. It took a long time to sort this out.

PC: Were you learning it from the primary literature, or were you in touch with people who had done the work and could guide you?

DT: [0:09:01] Actually, this is one of the things. Not that it's important, but we knew nobody. So, we did it entirely on our own. It’s actually ok. I didn’t know whom to contact. It’s not like [there was] an email I could write. We thought we could just learn it and we did. We never met any of these characters, like the ones who are now familiar now, people like Giorgio[19] or Cyrano[20]. (Cyrano in fact visited me much later for about six months.) We didn't know anybody. (Peter Young[21] maybe.) I knew Daniel Fisher[22] a little bit, but [as] you know he was (maybe still is) skeptical about things related to mean-field spin glasses. I didn't talk to him about these matters.

[17] 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

[18] D. J. Gross and M. Mézard, “The simplest spin glass,” *Nucl. Phys. B* **240**, 431-452 (1984). https://doi.org/10.1016/0550-3213(84)90237-2

[19] 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

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

[21] See, *e.g.*, P. Charbonneau, *History of RSB Interview: A. Peter Young*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 20 p. https://doi.org/10.34847/nkl.2fef8760

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

I got to know him better after Michael[23] joined the University of Maryland. He would visit from time to time. In fact, by the time we quit, which means that Ted and I quit, which is technically around '89 or '90—we moved on to other things—between the two of us, we had probably given no more than three or four talks on RFOT. (Somebody is now trying to persuade me to give a talk. It's going to take me an enormous amount of time to do it.) Bottom line is that we didn't meet anyone at all in the game.

FZ: At the time, did you know the work of Sompolinsky and Zippelius[24] who were working on the dynamics of the spin glass problem using techniques that were kind of related to mode-coupling theory, which arose later? Did you did you know this work?

DT: [0:11:28] This work is, I think, in PRL and we certainly knew that when we wrote our first paper. We did cite the paper in [our] very first paper.

PC: In your first couple of papers with Ted[25], you associated the glass transition in structural glasses with the dynamical transition in *p*-spin spin glasses. Where did the idea for the analogy originate?

DT: [0:12:02] We knew that p=2 is not good—this was either an insight or a hunch—because it satisfies inversion symmetry. We felt that the model that would be appropriate is one that does not. In p>2, then [that's satisfied]. We knew about the Gross-Mézard paper in '84. We knew about these Martin-Siggia-Rose[26]—type functional integrals for classical statistical mechanics. So, we basically worked out that for p=3, it's exactly the same as mode-coupling for the density fluctuations. That's how we realized [it]. Looking back, maybe we should have been very happy. We consider that an extremely important paper, by the way, the '87 paper in PRL because it is an explicit connection between equilibrium and dynamics in a precisely solvable model. The two temperatures, (the mode coupling temperature and the Kauzmann temperature) characteristics of structural glasses emerged naturally as well. It's exciting to see that. Once we saw that… The other thing we wanted always was that there has to be a

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

[24] H. Sompolinsky and A. Zippelius, "Dynamic theory of the spin-glass phase," *Phys. Rev. Lett.* **47**, 359 (1981). https://doi.org/10.1103/PhysRevLett.47.359; "Relaxational dynamics of the Edwards-Anderson model and the mean-field theory of spin-glasses," *Phys. Rev. B* **25**, 6960 (1982). https://doi.org/10.1103/PhysRevB.25.6860

[25] T. R. Kirkpatrick and D. Thirumalai, "Dynamics of the structural glass transition and the *p*-spin—interaction spin-glass model," *Phys. Rev. Lett.* **58**, 2091 (1987). https://doi.org/10.1103/PhysRevLett.58.2091; "*p*-spin-interaction spin-glass models: Connections with the structural glass problem," *Phys. Rev. B* **36**, 5388 (1987). https://doi.org/10.1103/PhysRevB.36.5388

[26] P. C. Martin, E. D. Siggia and H. A. Rose, "Statistical dynamics of classical systems," *Phys. Rev. A* **8**, 423 (1973). https://doi.org/10.1103/PhysRevA.8.423

connection between dynamics and equilibrium. We just kept insisting that any sensible theory should make such a connection throughout our research. We were particularly pleased that these scenarios could be derived from a Hamiltonian without quenched disorder in a study that we reported two years later[27].

PC: How was this work initially received by the spin glass and/or the structural glass communities?

DT: [0:14:14] They didn't care, especially I think that in the United States… Nobody paid attention. [There's] two reasons. Ted had just become an associate professor, I think. Maybe even not quite. [It was] maybe a year or two later. I was just an assistant professor. Who cares about these guys? So, nobody paid attention. I gave this one talk, I remember, the year my daughter was born[28], in the Gordon conference on liquids[29]. It was one of those Thursday talks. I didn't want to be there. I did a terrible job. I didn't help myself or our cause in any particular fashion. Some moments, I remember. I remember a moment after dinner I was walking with Hans Anderson to the meeting room. I told him: "I really don't want to be here." My daughter was just born like 2-3 weeks before that. Anyway, I did a terrible job. I think we owe a lot to Jean-Philippe[30] and probably some others associated with him, who took this seriously, maybe about seven, eight, ten years after our initial papers came out. I think that is why some of the ideas in '89 and before took hold. Actually, I've seen even today that people in the US don't pay that much attention, but I'm not 100% sure of that.

PC: From the *p*-spin model, you moved to the Potts glass next[31]. Why the Potts glass?

DT: [0:16:18] It seemed logical to do, because it also had similar features. Some of the calculations are simpler in the Potts glass, as I recall. We wanted to make sure that there was nothing peculiar about that as well. It was sort

[27] T. R. Kirkpatrick and D. Thirumalai, "Random solutions from a regular density functional Hamiltonian: a static and dynamical theory for the structural glass transition," *J. Phys. A* **22**, L149 (1989). https://doi.org/10.1088/0305-4470/22/5/003
[28] Alexandra D. Thirumalai was born in 1989.
[29] Gordon Research Conference Chemistry and Physics of Liquids, S. C. Greer and H. C. Andersen, Hoderness School, August 14-18, 1989. See, *e.g.*, Alexander M. Cruickshank, "Gordon Research Conferences," *Science* **243**, 1201-1217 (1989). https://www.jstor.org/stable/1702845
[30] Jean-Philippe Bouchaud: https://en.wikipedia.org/wiki/Jean-Philippe_Bouchaud
[31] T. R. Kirkpatrick and D. Thirumalai, "Mean-field soft-spin Potts glass model: Statics and dynamics." *Phys. Rev. B* **37**, 5342 (1988). https://doi.org/10.1103/PhysRevB.37.5342; D. Thirumalai and T. R. Kirkpatrick, "Mean-field Potts glass model: Initial-condition effects on dynamics and properties of metastable states," *Phys. Rev. B* **38**, 4881 (1988). https://doi.org/10.1103/PhysRevB.38.4881

of the logical thing to do, so that's we did. I remember that things like the role of initial conditions could be worked out much easier in the Potts glass than in the *p*-spin, but I don't remember all the technical details.

PC: Next, you considered a different class of models altogether, what you called a "somewhat unrealistic density functional Hamiltonian for the liquid state"[32]. That allowed you to identify two separate transitions: the dynamical and the Kauzmann transitions. What led you to this model?

DT: [0:17:10] First of all, already in the *p*-spin and in the Potts glass, the two transitions exist: the dynamics and [the Kauzmann]. It's already there in the PRL in '87. And the dynamics is very much like mode-coupling, and then at the Kauzmann transition the state entropy goes to zero. A lingering thought, which subsequently people pointed out is that in a sense, we want to describe classical particles and molecules which map into trajectories, where Newton's laws are valid and where you don't put randomness by hand. We were aware of that. So, we wanted to dole up a picture, where the emergence of a very large number of states below this mode-coupling, say, temperature is the physics and that is sufficient to obtain all the results with regard to spin models with quenched randomness. That's what drew us to that paper, which we initially submitted to PRL. They rejected it. Hence, it's in *J. Phys. A*. That's also a paper that we think today is important, that 1989 *J. Phys. A* paper.

PC: As you were just mentioning, this is a model without quenched disorder. In the paper, you say that the actual calculation details and motivations were left for a future work, which I think was never published. Is there a reason why the long version of the letter never appeared?

DT: [0:19:19] Every Christmas time, Ted would come to my office, and we would drink some wine and thought: "Could we do something more?" This particular year, we decided that we had exhausted our ideas. He was getting interested in metal-insulator transitions[33], and I was getting interested in proteins[34]. So, we split, eventually. We split in the sense that we stopped thinking about it, although we wrote a few more things in the '90s. (It could well be that the last stuff we did was in '95[35], which is this review article about transport theory. [It is] the last relevant piece.) Since

[32] See Ref. 27.

[33] See, *e.g.*, D. Belitz, and T. R. Kirkpatrick, "The Anderson-Mott transition," *Rev. Mod. Phys.* **66**, 261 (1994). https://doi.org/10.1103/RevModPhys.66.261

[34] See, *e.g.*, J. D. Honeycutt and D. Thirumalai, "Metastability of the folded states of globular proteins," Proc. Nat. Acad. Sci. U.S.A. **87**, 3526-3529 (1990). https://doi.org/10.1073/pnas.87.9.3526

[35] T. R. Kirkpatrick and D. Thirumalai, "Are disordered spin glass models relevant for the structural glass problem?" *Trans. Theor. Stat. Phys.* **24**, 927-945 (1995). https://doi.org/10.1080/00411459508203940

we left, we never returned to complete anything more on that. Of course, a few years later, I don't remember four or five years, I'm not sure, Silvio Franz, Jean-Philippe and some others also got the idea that you could get glass-like features without randomness[36]. We thought: "There's no need to revisit the problem, this work." It's never going to be done.

PC: Throughout these works, sometimes you and Ted acknowledged conversations with Peter Wolynes[37]. How closely were you in contact with him throughout these first papers?

DT: [0:21:12] As I recall not very much during the writing and development of the *p*-spin glass papers. He would come to Maryland from time to time, and was and is very much interested in glasses. And he [got in] touch by snail mail. That happened when we were writing the scaling picture paper in 1989[38]. So, he came from time to time. I think Ted met him again at a conference or something like that. During these visits, we would talk to him, and discuss the glass transition problem. Neither of us, as I recall, went to Illinois, where he was at that time, but we managed to stay in touch. He was a full partner in the formulation and writing of the KTW paper.

PC: Ted published a couple of papers with Peter[39], in parallel to those with you. Were you aware of these papers? Were you following that work?

DT: [0:22:10] Actually, Ted wanted me to be an author of one of these papers, but I decided that I didn't want to be. [Despite] all the conversations that I had with Ted, I didn't do any of the calculations, so I didn't want to be [an author].

PC: So, you were very familiar with these works.

DT: [0:22:42] Yeah. I was familiar, absolutely.

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[36] See, *e.g.*, E. Marinari, G. Parisi and F. Ritort, "Replica field theory for deterministic models. II. A non-random spin glass with glassy behaviour," *J. Phys. A* **27**, 7647 (1994). https://doi.org/10.1088/0305-4470/27/23/011; J.-P. Bouchaud and M. Mézard, "Self induced quenched disorder: a model for the glass transition," J. Physique I **4**, 1109-1114 (1994). https://doi.org/10.1051/jp1:1994240; S. Franz, J. A. Hertz, "Glassy transition and aging in a model without disorder," Phys. Rev. Lett. **74**, 2114 (1995). https://doi.org/10.1103/PhysRevLett.74.2114

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

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

[39] T. R. Kirkpatrick and P. G. Wolynes, "Connections between some kinetic and equilibrium theories of the glass transition," *Phys. Rev. A* **35**, 3072 (1987). https://doi.org/10.1103/PhysRevA.35.3072; "Stable and metastable states in mean-field Potts and structural glasses," *Phys. Rev. B* **36**, 8552 (1987). https://doi.org/10.1103/PhysRevB.36.8552

PC: This effort culminated, as you were saying, with the KTW scaling paper, which was written in 1989. Can you tell us a bit more about the genesis of that particular work? How did it come about?

DT: [0:23:04] These were a bit logical. At the time, if you remember, in 1986-1987, Fisher and Huse were saying that this droplet picture should be valid in Ising spin glasses as well[40]. We knew some of the language and ideas in the paper. We remember a paper in the *Journal of Physics A*[41], where they were talking about different kinds of states in the problem. And we know, of course, that below the mode-coupling [temperature] the dynamics becomes a lot slower. There are many states, but the states don't have infinite barriers in realistic glass forming materials, obviously. They do undergo some kind of activated transition, whose nature is still not clear from a microscopic perspective. We were logically thinking about a nucleation picture, how this could actually come about. Then, we worried about the fact that it's an Edwards-Anderson type model, and the analog of that is the square of the density, so the nucleation picture needed to be modified. So, we actually did that stuff. At the time, we were also completely familiar with two really [important papers]. The first paper is a remarkable paper by Imry and Ma[42] on the random field model. There was an analogous scenario in the random fields in some sense. Then, there was this very nice paper by Villain[43] exploring that. We knew about all that stuff, so we put together a picture. We also knew that the *p*-spin model also had that the specific heat was discontinuous, so hyperscaling would mean that [the critical exponent] alpha would be zero. Then, there were these papers by the math guys, Spencer and company and Fisher[44], where they showed that ν (the correlation length exponent) would be bigger than $2/d$. We knew of all this stuff. At lot of that played, if not consciously then certainly unconsciously, in formulating that droplet picture with the state entropy being the driving force for activated processes. At the end of the paper, we in fact make a connection to the [random field] Ising model,

[40] See, *e.g.*, D. S. Fisher and D. A. Huse, "Ordered phase of short-range Ising spin-glasses," *Phys. Rev. Lett.* **56**, 1601 (1986). https://doi.org/10.1103/PhysRevLett.56.1601

[41] D. A. Huse and D. S. Fisher, "Pure states in spin glasses," *J. Phys. A* **20**, L997 (1987). https://doi.org/10.1088/0305-4470/20/15/012

[42] 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

[43] J. Villain, "Equilibrium critical properties of random field systems: new conjectures," *J. Physique* **46**, 1843-1852 (1985). https://doi.org/10.1051/jphys:0198500460110184300

[44] J. T. Chayes, L. Chayes, D. S. Fisher and T. Spencer, "Finite-Size Scaling and Correlation Lengths for Disordered Systems," *Phys. Rev. Lett.* **57**, 2999 (1986). https://doi.org/10.1103/PhysRevLett.57.2999

which was not noticed at first but has been exploited by Biroli in a number of interesting papers[45].

PC: Concretely, was this happening because Peter came to visit? Or mostly through letter exchanges of the write-up?

DT: [0:26:15] We wrote the paper, actually with substantial contributions by Peter. I don't think he was there during the time when the paper was being written. I think this was all done by snail mail. Even papers were submitted by the U.S. Postal Service. He participated fully, of course, but it was all through telephone calls or something like that.

PC: You mentioned that over that period you two gave three or four talks.

DT: [0:26:56] I gave two.

PC: So, you gave one at the Gordon conference. What was the other one?

DT: [0:27:02] I don't remember that, but I think I gave two talks. I'm not sure that I did it a much better job the in the second one. I don’t really [know]. I know Ted gave a talk somewhere, maybe Princeton or something. I don't remember exactly.

PC: In parallel to this effort, you were running numerical simulations on glasses with Ray Mountain. Can you tell us more about how that came about?

DT: [0:27:34] That's a story that involves Zwanzig. Ray Mountain[46] was Zwanzig’s postdoc when Bob was at NIST, in the ‘60s, I think. Bob never asked me to do things politely. He would give me commands. So, he said: “Go see Ray! This will be good for you.” (There’s a small story, about which I’ll tell you at some point privately.) Ray, of course, was an expert in computer simulations. He is one of those guys who wrote all the codes from scratch. If after a paper is published, I said: “Let’s go and calculate something else.” These data were not available. He had deleted them, so he would do them again. He was interested in glasses too, it turns out, so I sort of discussed things with him. We started looking at these binary

[45] See, *e.g.*, G. Biroli, C. Cammarota, G. Tarjus and M. Tarzia, “Random-field-like criticality in glass-forming liquids,” *Phys. Rev. Lett.* **112**, 175701 (2014). https://doi.org/10.1103/PhysRevLett.112.175701; “Random-field Ising-like effective theory of the glass transition. I. Mean-field models,” *Phys. Rev. B* **98**, 174205 (2018). https://doi.org/10.1103/PhysRevB.98.174205; “Random field Ising-like effective theory of the glass transition. II. Finite-dimensional models,” *Phys. Rev. B* **98**, 174206 (2018). https://doi.org/10.1103/PhysRevB.98.174206

[46] “Raymond D. Mountain,” *National Institute of Standards and Technology* (2019). https://www.nist.gov/people/raymond-d-mountain (Consulted November 26, 2022.)

mixtures of soft spheres and Lennard-Jones[47]. Incidentally, the Kob-Anderson[48] model became extremely popular, but before that Ray and I had published a couple of papers already. Kob-Anderson uses a model with non-additive diameters, which is not physical, whereas we used a more physical model. Anyway, our papers did not become as famous as the Kob-Anderson study, but that's okay. Ray and I published several papers. The first one, we were interested in order parameters for glasses. This issue is still sticking around. We talked about this icosahedral stuff, but that is not useful at all, actually. Besides, in hard sphere systems, which could form glasses, that's not even immediately definable. So, we wrote that paper and shortly after that, when these models on *p*-spin and especially the *J. Phys. A* paper appeared; we were wondering what could be learned from trajectories. In Ma's statistical mechanics book, there's a chapter—I think it's in the chapter on entropy—[that] has a numerical method called the coincidence method for computing entropy[49], which not many people have used, as far as I know. There is a sentence in the chapter, [where] he says: "Regardless of whether the system is in equilibrium or out of equilibrium, trajectories have meaning. They are measurable. Positions and momentum have meaning." This made a big impression on me. I thought: "What does he mean by this?" Also, this notion of replica was floating around within me, Ted and Ray, when we wrote that paper in '89 about replica symmetry breaking. We basically wanted to understand what Ma meant by that[50] from our perspective. We figured that we could do replica molecular dynamics—or replica Monte Carlo or whatever you want—and from that learn how two independent copies evolve in supercooled liquids. To me, that's one of the long-lasting influences of this

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[47] D. Thirumalai and R. D. Mountain, "Relaxation of anisotropic correlations in (two-component) supercooled liquids," *J. Phys. C* **20**, L399 (1987). https://doi.org/10.1088/0022-3719/20/19/005; R. D. Mountain and D. Thirumalai, "Molecular-dynamics study of glassy and supercooled states of a binary mixture of soft spheres," *Phys. Rev. A* **36**, 3300 (1987). https://doi.org/10.1103/PhysRevA.36.3300

[48] W. Kob and H. C. Andersen, "Scaling behavior in the β-relaxation regime of a supercooled Lennard-Jones mixture," *Phys. Rev. Lett.* **73**, 1376 (1994). https://doi.org/10.1103/PhysRevLett.73.1376; "Testing mode-coupling theory for a supercooled binary Lennard-Jones mixture I: The van Hove correlation function," *Phys. Rev. E* **51**, 4626 (1995). https://doi.org/10.1103/PhysRevE.51.4626; "Testing mode-coupling theory for a supercooled binary Lennard-Jones mixture. II. Intermediate scattering function and dynamic susceptibility," *Phys. Rev. E* **52**, 4134 (1995). https://doi.org/10.1103/PhysRevE.52.4134

[49] Shang-Keng Ma, *Statistical Mechanics* (Singapore: World Scientific, 1985), Chapter 25 "Entropy Calculation from the Trajectory of Motion". See, in particular, p. 425-426: "If we know the details of motion of every molecule during the observation time, then any property of the system can be calculated. […] But entropy is unlike quantities such as energy and pressure. It is not the averaged value over time of a dynamical variable. […] Nevertheless, if the determination of entropy had to go beyond knowledge of the whole motion, then the concept of entropy would be outside the realm of science […]. Why must we discuss this problem of calculating entropy from the motion? […] A method of calculating entropy from motion can be used to analyze various models, especially those exhibiting metastable states."

[50] D. Thirumalai, R. D. Mountain and T. R. Kirkpatrick, "Ergodic behavior in supercooled liquids and in glasses," *Phys. Rev. A* **39**, 3563 (1989). https://doi.org/10.1103/PhysRevA.39.3563

Parisi thinking, in terms of creating copies and looking at how the copies are doing—two or more—that's embodied in that. From my viewpoint, the Franz-Parisi scheme[51] is an evolution of the idea of coupled replicas. It took us about a year to figure out that little formula that we have in the paper, but once we did, it is obvious that [it is the] right way to think about ergodicity breaking in any system, numerically. That was in a paper that was published in '89. So, the three papers from '89 that we published I think are not so bad: the *J. Phys. A* showing that self-generated randomness causes glass-like behavior, the ergodicity paper, and the scaling picture of glasses with Ted and Peter.

PC: Were the simulation results informing your theoretical work and vice versa?

DT: [0:32:50] For sure! The other thing we did with Ray was to show… Again, this was inspired by the growing correlation length problem. The correlation length increases, but in practice the inability to reach the Kauzmann temperature means that only lengths on the order of a few particle diameters can be accessed in realistic simulations. We had this idea, which turned out to be completely wrong, that if you calculate viscosity and diffusion coefficient, you can actually try to get a length which would increase, which does the exact opposite, but it led to Stokes-Einstein violation, which was close to the first if not the first example of SE violation. [Frank Stillinger showed that] sometime in the early '90s[52].

PC: Very early in your theoretical work, you identified that your results were exact when the dimension of space went to infinity. Did the idea of doing numerical simulations in higher dimensions to validate these proposals ever cross your mind?

DT: [0:33:54] Not at all! This came in the hands of you guys, later on[53]. I know you know because I think I've seen this paper cited, [but] Jerry Percus[54]

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[51] See, *e.g.*, S. Franz and G. Parisi, "Recipes for metastable states in spin glasses," *J. Physique I* **5**, 1401-1415 (1995). https://doi.org/10.1051/jp1:1995201; "Phase diagram of coupled glassy systems: A mean-field study," *Phys. Rev. Lett.* **79**, 2486 (1997). https://doi.org/10.1103/PhysRevLett.79.2486; A Barrat, S. Franz and G. Parisi, "Temperature evolution and bifurcations of metastable states in mean-field spin glasses, with connections with structural glasses," *J. Phys. A* **30**, 5593 (1997).

[52] J. A. Hodgdon and Frank H. Stillinger, "Stokes-Einstein violation in glass-forming liquids," *Phys. Rev. E* **48**, 207 (1993). https://doi.org/10.1103/PhysRevE.48.207

[53] See, *e.g.*, P. Charbonneau, J. Kurchan, G. Parisi, P. Urbani and F. Zamponi, "Glass and jamming transitions: From exact results to finite-dimensional descriptions," *Annu. Rev. Condens. Matt. Phys.* **8**, 265-288 (2017). https://doi.org/10.1146/annurev-conmatphys-031016-025334

[54] Jerome K. Percus: https://en.wikipedia.org/wiki/Jerome_K._Percus

knew that hard spheres became simple as *d* goes to infinity[55]. We knew that. In fact, Ted had written a paper on hard cylinders in *d* goes to infinity[56]. Anyways, he told me about it. This thought didn't enter our minds at all.

PC: Your first NSF grant, your Presidential Young Investigator Award[57], was on the study of the glass transition[58], but I don't think you did any of this work with graduate students. Is there a reason why this was always your work Ted, and not with anyone else?

DT: [0:35:08] That seemed sufficient. With Ted and Ray, and of course Peter. I don't know what I did do with that grant at all. Ted really didn't have many [students]. Neither did I at that time. For the first ten years or so as a faculty member, I used to have a group of three people, no more. Actually, I did one paper on Wigner glasses in '89[59] with—I hate to call him my postdoc, because he's such a close friend of mine, and he was hanging around Washington for personal reasons so he would stop by, so I don't really consider him my postdoc[60]. We did write this paper in Wigner glasses, where we were trying to chase two-level systems and quite didn't succeed there.

PC: Starting in 1990, you were drawn away from the glass field by your work on proteins. Did you nevertheless keep abreast of the work that was been going glasses?

DT: [0:36:28] Not very much. I cannot believe then, nor do I believe now, that foldable proteins, the ones that get to the native state are trapped anywhere for arbitrarily long times. I should take that back a little bit, but I didn't believe that the notion that it would stay there for a very long time.

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[55] See, *e.g.*, H. L. Frisch and J. K. Percus, "Nonuniform classical fluid at high dimensionality," *Phys. Rev. A* **35**, 4696 (1987). https://doi.org/10.1103/PhysRevA.35.4696

[56] T. R. Kirkpatrick, "Microscopic theory of dynamics in an orientationally ordered fluid." *J. Chem. Phys.* **89**, 5020-5032 (1988). https://doi.org/10.1063/1.455646

[57] Presidential Young Investigator Award: https://en.wikipedia.org/wiki/Presidential_Young_Investigator_Award

[58] D. Thirumalai, "Dynamics of the Structural Glass Transition, #8657356" *National Science Foundation Division of Chemistry* (1987-1992). https://www.nsf.gov/awardsearch/showAward?AWD_ID=8657356&HistoricalAwards=false (Consulted November 27, 2022.)

[59] R. O. Rosenberg, D. Thirumalai and R. D. Mountain, "Liquid, crystalline and glassy states of binary charged colloidal suspensions," *J. Phys.: Condens. Matter* **1**, 2109 (1989). https://doi.org/10.1088/0953-8984/1/11/019

[60] See, *e.g.*, Robert Owen Rosenberg, *Conformational equilibrium and isomerization dynamics in solution and the gas phase*, PhD Thesis, Columbia University (1986). https://clio.columbia.edu/catalog/551355 (Consulted November 27, 2022.)

In biology, the maximum time—that's Infinity for you—is the cell doubling time. After that, it doesn't matter. The cell has doubled, and all information has been transmitted from the mother to daughter cells. And often, the folding times of proteins is much smaller than that, so I didn't really quite believe that that the concepts that [appear] in the glass transition are relevant for foldable proteins. (I can qualify myself a little bit later if you want.) Maybe so in RNA, but not in proteins. I don't believe it. So, I didn't pay a lot of attention to that.

PC: Were ideas surrounding structural glasses or replica symmetry breaking at all influential on your subsequent work?

DT: [0:38:06] In the proteins, I hardly used—except for [quantitative] measures like overlap functions and fluctuations of overlap functions as a tool to analyze trajectories of computer simulations of some kind, because I essentially felt that—especially small proteins with 100 amino acids or there about—either fold spontaneously or not at all. But when I started on RNA, though (RNA is a super complicated problem), from my perspective, there are remnants of glass-like behavior for many reasons. There is something like the ground state, but there are lots of low energy excitations around them, which are accessible—I don't know on what timescale, but certainly they get trapped for arbitrarily long times. Especially the ribozymes, which are at least 200 nucleotides or more. There, notions of slow dynamics and glasses are important. I thought about them often. But then, when we started working on cells, about eight or nine years ago, there is evidence of sub-diffusive and super-diffusive behavior in their collective motions. These observations come not only from theory or computations, but experiments have shown that this is so. In fact, I work with a biologist here, who is convinced that this is the case. You can see in the process of the early development, signatures that are unmistakably glass-like in some sense. Of course, it cannot be permanently there. It's got to a evolve and do something. Ted and I suggested these possibilities in our RMP article in 2015.

Sometime in '90 or something, I was in France for something or the other, and I met Virasoro[61] in that meeting. I believe he was an editor of the *Reviews of Modern Physics* at the time. Somehow—I can't remember this in detail—he said: "Why don't you write a review on your glass stuff?" I thought it was a good idea, so we did. It was Ted, me and Peter. It was a 200-page typed article, but we never submitted it for some reason that I

[61] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Miguel Virasoro*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 7 p. https://doi.org/10.34847/nkl.a941vym8

don't remember. But three copies exist and each of us has a copy of that. I think even I can find it somewhere, even though I have moved many times now. Then, Ted and I thought we should write some review after all this time. I think we both said: "Well, we don't want to write one on RFOT or glass, per se." Because at the time there were the reviews by Cavagna in 2009[62], and one—very famous by now—by Ludovic Berthier in [2011] in RMP[63]. And then Francesco did something with Giorgio, in [2010][64]. So, we wanted to write something that was beyond RFOT. This article contains a little bit of RFOT at the beginning and then we discuss both applications to biology—from molecules to cells—and also to some quantum phase transitions that Ted was interested and knew about[65]. In preparing this, I don't know why—even though it was only nine years ago—I was getting interested in the physics of cancer, [about which I knew] zero. I now know 0.001%; at the time I knew nothing. Somehow, I was looking at the literature somewhere and there was an article in—this is something that is good to know incidentally—*Annual Review of Pathology* on heterogeneity in cancer cells[66]. It turns out that these cancer biologists have known about heterogeneity in very much the same way as we were thinking about glasses in the mid '50s already. They didn't put math into it or something like that. We did a lot of work on that afterwards[67]. But in this *Annual Review of Pathology* paper, which is reproduced in the *Reviews of Modern Physics*, they explain metastasis. If you remove the labels and the color, you take your computer simulation of a glass and place it at the bottom, and you use the same color code, you can't tell the difference very much. I would say: "Boy, this looks the same." In that paper, we basically suggested that maybe the ideas of glasses, which means the methodology in some sense, could be used here. In fact, that's what we have done in the last half a dozen or so years. Some of the things that we learned there, like χ_4, the fourth order susceptibilities and stuff like that, are very much meaningful. You can quantify aging, and lots of things using those sorts of ideas. For 20 years, I resisted the notion that nature would use glass, in an evolving, adaptable system, in which one would see manifestation of glass-

[62] A. Cavagna, "Supercooled liquids for pedestrians," *Phys. Rep.* **476**, 51-124 (2009). https://doi.org/10.1016/j.physrep.2009.03.003

[63] L. Berthier and G. Biroli, "Theoretical perspective on the glass transition and amorphous materials," *Rev. Mod. Phys.* **83**, 587 (2011). https://doi.org/10.1103/RevModPhys.83.587

[64] G. Parisi and F. Zamponi, "Mean-field theory of hard sphere glasses and jamming," *Rev. Mod. Phys.* **82**, 789 (2010). https://doi.org/10.1103/RevModPhys.82.789

[65] T. R. Kirkpatrick and D. Thirumalai, "Colloquium: Random first order transition theory concepts in biology and physics," *Rev. Mod. Phys.* **87**, 183 (2015). https://doi.org/10.1103/RevModPhys.87.183

[66] V. Almendro, A. Marusyk and K. Polyak, "Cellular heterogeneity and molecular evolution in cancer," *Annu. Rev. Pathol.* **8**, 277-302 (2013). https://doi.org/10.1146/annurev-pathol-020712-163923

[67] See, *e.g.*, A. N. Malmi-Kakkada, X. Li, H. S. Samanta, S. Sinha and D. Thirumalai, "Cell growth rate dictates the onset of glass to fluidlike transition and long time superdiffusion in an evolving cell colony," *Phys. Rev. X* **8**, 021025 (2018). https://doi.org/10.1103/PhysRevX.8.021025

like behavior. But there you are. It's not idle talk. You can image these trajectories from experiments and analyze them using things that you guys are familiar with, both in two and three dimensions, and see glass-like behavior.

PC: You mentioned earlier that Jean-Philippe Bouchaud and co-workers rediscovered your work later and then built on it. Did they get in touch with you? How did you become aware of that happening later on?

DT: [0:46:11] I don't know how it got rediscovered, if that is the right word. I don't know. Jean-Philippe can answer this better than I can. I think he wanted to see if this stuff makes any sense. They have done lots of really great stuff with it, and beyond. They probably understand the problem far better than I do, in fact. We haven't paid attention to the field very much. But I did get in touch with JP [Bouchaud] in 1994 after his paper about glass-like behavior in systems without quenched randomness[68]. I think it was written with Mézard, but I'm not really sure. I did tell him that this is a similar thing that Ted and I had published in *J. Phys. A*. He was completely unaware of it. His ideas and criticisms on the limits of RFOT are quite valid. I don't know if we have answered any of it at all actually. We owe Jean-Philippe Bouchaud a great debt of gratitude, especially his very kind remarks during the Rome meeting in 2019[69].

FZ: Related to this. In Rome, in the early '90s, there was an explosion of work inspired by your earlier papers. In particular, there was the work of Cugliandolo and Kurchan[70]—who were in Rome at the time—on the aging solution of the *p*-spin and the connections to structural glasses. There was a series of works when people were looking for models without quenched disorder that could be exactly solvable and reproduce RFOT phenomenology. For example, the work of Marinari, Ritort, Parisi and coworkers[71]. Then, there were the work of Franz and Parisi[72] and Mézard and Parisi[73], in which they tried to use the hypernetted chain approximations to study glasses. All these works somehow cited your

[68] See Ref. 36.

[69] *40 years of Replica Symmetry Breaking*, P. Charbonneau, E. Marinari, F. Ricci-Tersenghi, G. Parisi, F. Zamponi, La Sapienza, Rome, Italy, September 10-13, 2019. https://sites.google.com/view/rsb40 (Accessed March 8, 2023.)

[70] See, *e.g.*, L. F. Cugliandolo and J. Kurchan, "Analytical solution of the off-equilibrium dynamics of a long-range spin-glass model," *Phys. Rev. Lett.* **71**, 173 (1993). https://doi.org/10.1103/PhysRevLett.71.173

[71] See Ref. 36.

[72] See, *e.g.*, M. Cardenas, S. Franz and G. Parisi, "Glass transition and effective potential in the hypernetted chain approximation," *J. Phys. A* **31**, L163 (1998). https://doi.org/10.1088/0305-4470/31/9/001

[73] M. Mézard and G. Parisi, "A tentative replica study of the glass transition," *J. Phys. A* **29**, 6515 (1996). https://doi.org/10.1088/0305-4470/29/20/009

work, albeit in a kind of incoherent way. Some of your papers and some not. I was wondering what the connection was, at the time. Were you in touch with these people? Did you visit them, or did they visit you? Or did you meet at conferences? How was the communication working?

DT: [0:48:59] Categorically, I never met any of these people, and to the best of my knowledge neither did Ted. I did meet Giorgio at some point, but I don't know when it was. I can't remember that. I did meet Mézard in Santa Barbara once. In thinking about our conversation today, I was trying to think when I met him. All these papers that used the liquid state equation of state and connections with replicas and finding solutions and explaining structure factors and things of glasses, I never paid attention to [them] then, and I haven't caught up with [them] now. Obviously, we knew about this. We would just look at abstracts and things like that. I think they made great progress in integrating what seemed like totally disconnected subfields. And maybe they all paved the way towards your great papers on the infinite-dimensional solutions of hard spheres. Maybe they provided the impetus. I don't really know. I never really met them then. We were completely out of contact after the ‘90s, and even before. Bottom line is we never really met anyone at all until much, much later, when we saw some people at meetings. I met Enzo [Marinari] before I met Giorgio, but there were no connections, no correspondence, nothing.

FZ: In your opinion, was this because of geographical barriers, like being on two sides of the Atlantic, or was it more of a community effect, that you were in different communities? Why was there this barrier to communication?

DT: [0:51:16] When these guys were doing all this great stuff, which we were admiring from a distance, we were not players in the game. The subject evolved without our input, as it must.

FZ: But at the same time these people were somehow citing your work, and they knew it. Their work was directly inspired by yours. There was a connection, but maybe it was not evident at the time.

PC: Was the connection only paper based? Were there no personal relations, no students or postdocs circulating, or anything of the sort?

DT: [0:52:01] Our works only involved four people: Ted, Ray, Peter and myself. It's possible that Peter met all these people because he travels a lot more, but we never did. I don’t believe—if I’m not mistaken—that I have ever attended a meeting on glasses at all. I’ve never been to any meeting on glasses.

PC: During your time at Maryland or elsewhere, did you ever teach about replica symmetry breaking, structural glasses or spin glasses?

DT: [0:52:44] No. I taught biophysics and statistical mechanics at the graduate level, but I never really taught these things. I think that when the biology bug bit me, so to speak, my brain was rewired and thought about problems there. Not without influence from these other areas, but the answers the biochemists and, now more, the biologists seek but with very little to with spin glasses per se. Although the notion of heterogeneity, for example, is so evident in biology that people have to worry about it—[although] they like to average—but what the biological significance of that is not clear yet for any given system. Maybe it will become so, and then maybe it's going to happen. It's the biology, the molecular aspect, and more and more cellular aspects that go in my thinking almost all the time, so I never really paid attention to it. We are coming back to some real problems that are generally glassy, again mostly in the context of Wigner glasses and soft glasses that we began a long time ago. We're thinking about that from some numerical perspective. We've done some work on them, but they are not published yet. So, I didn't really get an opportunity to teach spin glass related materials. I should have done it, but it's too late.

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

DT: [0:54:44] I wanted to mention one thing about Giorgio. Because, after all, it's at least in part a celebration of his great achievements. He's a super impressive and unique guy. He doesn't say very much, but his papers say a lot. I'll tell you two things about Giorgio that early on influenced me and had nothing to do with replicas, as far as I know.

One was a statement that he made and maybe he proved this as a theorem—you guys should correct me if I'm not saying this right. The statement was something like: "Permutation symmetry can be broken infinite number of times if the number of objects to be permuted goes to zero." Have you seen that? I don't even know what the hell it means, but I thought this is a basic stuff. That's one thing that I kind of looked up. He's a very unique and creative guy.

The other thing—even before replicas—I knew about Parisi is the following. In 1980, he had done the replica symmetry breaking solution already, but he had a great career as a field theory person thinking about some high-energy problems. He could have retired at that time and would still have been a fantastic scientist. When I was a postdoc. Bruce Berne[74] asked me to think about numerical algorithms for calculating real-time dynamics in quantum systems. Because e^{iHt} has oscillations, [it] is numerically completely unstable. I don't know how I came across this paper, but I saw a paper by Parisi and Wu in *Scientia Sinica*, in 1980, when they were thinking about stochastic quantization to solve some gauge fixing problem in field theory[75]. I don't know the context very well. I looked at the paper and I thought: "Gee! This is a really cool thing that I could actually do numerically. It will generate a complex action, but I can split them in real and imaginary time, I can introduce a fictitious time and integrate the real part and the imaginary part separately. In the end, I could get real time dynamics in quantum systems." So, I thought: "I'm going to go do this when I start at Maryland." I did, actually. I first showed that his method works very well for a harmonic oscillator, where I can calculate [the result] analytically, but the harmonic oscillator is essentially semi-classical. It's not a big deal. I never pursued that program after that, because Zwanzig told me: "You can't work on quantum problems." That was the end of that, but it stuck in my mind. Then, when we first started working on cell dynamics—a paper that we published in 2018 in PRX[76]—it turns out that it's a classical analog of that Parisi-Wu stochastic quantization method, which people have worked on for a very long time, including in high energy and classical non-equilibrium systems. We used that again to calculate—sort of in a mode-coupling sense—the dynamics of the~~se~~ cancer cells. So, the earliest paper of his that influenced me was that paper. I still don't understand why it's not used routinely in non-equilibrium classical statistical mechanics of many-body systems. It seems like it's a pretty good thing to do, but I don't see people using it very much. That's my non-replica influence of Giorgio.

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

DT: [0:59:23] No correspondence, because we didn't correspond with anyone. There are notes floating around, I think, but I moved four times, so I would have to really look very hard on it. I certainly saw, when I moved from Maryland to Austin, the notes on the fourth-order susceptibility paper in 1988 that Ted and I wrote. But I don't know where it is now.

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[74] Bruce J. Berne: https://de.wikipedia.org/wiki/Bruce_J._Berne; B. J. Berne, " Autobiography of Bruce J. Berne," *J. Phys. Chem. B* ***105***, 6455-6461 (2001). https://doi.org/10.1021/jp012029a

[75] G. Parisi, Y. S. Wu, "Perturbation theory without gauge fixing," *Sci. Sin.* **24**, 483-496 (1981). https://www.openaccessrepository.it/record/18105/files/LNF_81_017.pdf (Consulted November 29, 2022.)

[76] See Ref. 67.

PC: And the correspondence with Peter, for instance?

DT: [1:00:04] I can't remember. Michael Fisher was a very good at this. Every letter that he got and that he wrote he filed it away. So, when he passed on, they were all sitting there somewhere. People had to go through it. But I'm a lazy guy, so I probably tossed things out. There may be there, I don't know. I have some correspondence with the late Sir Sam Edwards[77], but that's on polymers.

PC: Thank you very much for your time.

DT: [1:00:41] Thanks a lot guys. Hopefully, it was not useless.

[77] Sam F. Edwards: https://en.wikipedia.org/wiki/Sam_Edwards_(physicist)