History of RSB Interview: Chandan Dasgupta

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PC: Hello, Professor Dasgupta. Thank you very much for joining us. As we discussed ahead of time the goal of this discussion is to go over the genesis of the ideas surrounding spin glasses and replica symmetry breaking, which we bound roughly from 1975 to 1995. Before we get to this material, I'd like to ask you a few background questions, if you don't mind. First, can you tell us a bit about your family and your studies before reaching university, namely the context in which this took place.

CD: [0:00:37] I studied in India through the masters, then in 1973 I got admission at the University of Pennsylvania for the PhD.

PC: Before we get to that, can you first tell us what got you interested in physics?

CD: [0:01:00] I had been interested in science throughout high school. That was at a place near Calcutta, in the Eastern part of India. In Calcutta, there is this college, which has a very good tradition and is quite famous, called Presidency College[1]. People who wanted to do science typically went to that college for their undergraduate training. I was not interested in the professional courses, like medicine or engineering or things like that. I aimed to do science. More specifically, physics is something that was decided more or less when I found out that many of the people who did very well in our school-leaving exam[2] were joining that college and were

[1] Presidency College: https://en.wikipedia.org/wiki/Presidency_University,_Kolkata
[2] Higher Secondary Examination of West Bengal Board of Secondary Education: https://en.wikipedia.org/wiki/West_Bengal_Council_of_Higher_Secondary_Education

taking physics. Although I liked physics from the beginning. Physics, whatever we learned at school, I found it very interesting. And there was this sort of inducement that people that did very well in the school-leaving exam, many of them are joining Presidency College to do physics. I wanted to talk to a bunch of people who I would have as classmates.

PC: What drew you then to pursue graduate studies in theoretical physics in particular?

CD: [0:02:37] That was dictated in some sense by circumstances. First of all, I liked theoretical physics. Undergraduates didn't really learn advanced physics, but I liked whatever we learned in the theoretical part. Being an experimentalist in India is very difficult for various reasons, because the labs are not so well equipped and so on. Even the experimental part that we did as part of our curriculum—some experiments we had to do as it relates to the theory part—there again the labs were not well equipped. Sometimes things didn't work and all that. We were not very impressed with the experimental aspects, but the theoretical part we could read some books and learn from lectures, so it was in some sense more interesting.

After three years in this college, I went to Delhi University to do a two-year masters. That was the standard thing to do in India at that point: three years bachelor and two years masters. After that, if one wanted to do PhD, then they were going to do that. Then again, a lot of people in our class were thinking of going abroad to take the PhD. That was one reason why I also decided to do that. I knew at that point that I'd do a PhD, but where I'd do that was not clear. Since many of my friends were going abroad—many of them to the US—I also decided to do that.

PC: What drew you to work with Brooks Harris[3] at U Penn, in particular?

CD: [0:04:35] I was in Pennsylvania in 1973. In the first year, as you know, in the US system one typically takes courses. I did that too, with the qualifying exam in the summer, in 1974. Then, I started discussing with various faculty members as to what they were doing and what would be the possibility of me joining as student. In statistical physics, condensed matter…

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

First of all, in India, when I did a masters, my training was in high-energy physics[4]. Condensed matter, solid state physics was not—at the university where I studied—very popular, in the sense that there were not many people who were working in this area, but there were quite a few people who were working in high-energy physics.

After going to Pennsylvania, I talked to some of the professors there who were doing high-energy physics. Because high-energy physics was not in a very good shape at that point, there were not very many interesting things going on in that field. Then, of course, I started talking to other people who were in other areas of physics. Bob Schrieffer[5] was there at the time. Brooks Harris was there, and so were a few other people. So I talked to them, and eventually found out the kind of things that these guys were doing. Brooks Harris had been there for a while, and then Tom Lubensky[6] also was there. He joined as a young assistant professor just few years before I went there. They were working together. I took a course on statistical physics with Tom Lubensky, which I liked. Then, when there was this possibility of working with them, I decided to do that.

PC: Just to understand: why did you choose UPenn to begin with? Did you have the choice, or was it difficult to get a graduate position at that time?

CD: [0:06:50] It was not very easy. One had to apply to something like 10-15 different places, and then choose among those who are willing to take you. I had two, three offers maybe. Among them, I knew about UPenn, because some of the people that I knew were studying there at that point, and they gave a good report on graduate studies there.

PC: You said you joined Brooks Harris, but you said you were also in close proximity with Lubensky. Can you tell us how the group, with students and PIs, was functioning at that point?

CD: [0:07:41] It was a small group in the sense that the number of students and postdocs in the group was not very large. Brooks and Tom were actually working together. Many of the papers that either of them wrote were joint

[4] Chandan Dasgupta, *Chew-Low theory for meson-nucleon scattering*, MSc Thesis, University of Delhi (1973).

[5] John Robert Schrieffer: https://en.wikipedia.org/wiki/John_Robert_Schrieffer

[6] 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

papers[7]. I think there were four or five graduate students and one postdoc. So it was seven or eight people in the group.

PC: Would you meet as a group, or were you working individually? Were the students working with each other? I'm just trying to get a feel for the interactions.

CD: [0:08:25] A lot of interactions. I don't think we had formal group meetings, but there were informal meetings in which people were talking about what they were doing, and various discussions took place. It was a good atmosphere. People were very helpful. A lot of communications among the students in fact.

PC: You didn't work on spin glasses at that point, but did you hear about them? Were you aware of the topic? If yes, how?

CD: [0:08:56] Yeah. Sure. I'll just give you a little background on what this group was working on at that point. There were a couple of exciting things going on, which in some sense also made me interested in doing a PhD in this field. One was, of course, this idea of scaling and renormalization group and things like that which was about ten years in the making. But a lot of people were still working on critical phenomena and application of renormalization group to various problems and things like that. That was one interest. Then, there was the study of disordered systems, in general, starting with disordered magnetic systems, percolation and things like that. That was also becoming popular in the sense that a lot of new problems were coming up both experimentally and also in theoretical developments. Brooks Harris, in particular, just had done this Harris criterion for deciding whether a disordered system would have a sharp phase transition or not[8]. This Harris criterion was started two or three years before I joined, maybe, or something like that. In general, they were interested in looking at systems with quenched disorder. Their main interest when I joined was dilute magnets. If you have a magnetic system and some sites or some bonds are missing with some probability, then what kind of transition one expects, what kind of phase diagram, whether the phase transition is of the same universality class as that for the pure system. All these things were being discussed.

[7] See, e.g., 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

[8] A. B. Harris, " Effect of random defects on the critical behaviour of Ising models," *J. Phys. C* **7** 1671 (1974). https://doi.org/10.1088/0022-3719/7/9/009

In ’75-’76, all these spin glass papers started coming out: the Edwards-Anderson paper[9] and Sherrington-Kirkpatrick paper[10]. There were discussions about that in the group in general. Actually, Brooks and Tom started working on spin glasses while I was there. There was another student, Jing-Huei Chen, who was put on that problem[11]. They wrote some papers doing the epsilon expansion for spin glasses while I was there[12]. So I was aware of spin glasses, although at that point I didn’t do any research on spin glasses[13]. It was recognized as a very important problem and how to deal with it, and so there was a lot of discussions about it.

PC: After graduating you decided to move to a postdoc with Shang-keng Ma[14], at UCSD, where you did then work on spin glasses. Can you tell us a bit how you got to go work on that postdoc and how that collaboration on spin glasses came about?

CD: [0:11:58] Why I went there is again in some sense that you apply for various places and choose the one which you think is the best for you. That was the reason.

Why spin glasses? I should tell you a little about the summer of 1978. I graduated in 1978. Before going to La Jolla for a postdoc, I actually spent a fairly long time in Europe, and a large part of that was the Les Houches school in condensed matter[15]. Since for the PhD I had done some work on

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[9] S. F. Edwards and P. W. Anderson, “Theory of spin glasses,” *J. Phys. F* **5**, 965 (1975). https://doi.org/10.1088/0305-4608/5/5/017

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

[11] A. B. Harris, T. C. Lubensky and J.-H. Chen, “Critical properties of spin-glasses,” *Phys. Rev. Lett.* **36**, 415 (1976). https://doi.org/10.1103/PhysRevLett.36.415; Jing-Huei Chen, *Phase transitions in gauge coupled systems*, PhD Thesis, University of Pennsylvania (1978). https://franklin.library.upenn.edu/catalog/FRANKLIN_998132403503681

[12] 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; J.-H. Chen and T. 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;

[13] Chandan Dasgupta, *Renormalization-group Study of the Potts Model with Applications to Random Networks*, PhD Thesis, University of Pennsylvania (1978). https://franklin.library.upenn.edu/catalog/FRANKLIN_996250433503681

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

[15] Matière mal condensée, Les Houches, Session XXXI, July 3-August 18, 1978. R. Balian, G. Toulouse, R. Maynard, eds., *Ill-condensed Matter* (Amsterdam: World Scientific, 1979).

percolation[16], random resistor networks[17] and things like that, I was interested in disordered systems. That school was specifically on that topic, so I attended that school. That was a great experience in the sense that very many leaders in the field were there and they gave lectures. As you know, in Les Houches there are extended lectures. Phil Anderson[18] was there, Scott Kirkpatrick[19], Tom Lubensky and a few others were doing work in this field—a new field at that point. There were of course a lot of discussions about spin glasses in the school. Actually, I first heard about replica symmetry breaking at that school. This de Almeida-Thouless paper had come out[20], and so we knew that the replica symmetric solution is not the correct solution in the ordered state. Then, what to do with it was not clear. People were looking at various possibilities and so on and so forth. I heard about the possibility of replica symmetry breaking. People were looking at it and trying to find the right way of doing that. P. W. Anderson gave many lectures about spin glasses and also glasses and possible connections between these two. Scott Kirkpatrick was there so the Sherrington-Kirkpatrick model and the numerical work was discussed[21]. Tom Lubensky was there talking about the renormalization group and things like that. This told me that this is a very important field.

When I went to work with Shang-keng Ma, he was also interested in disordered systems. He had done this work, this Imry-Ma argument[22], to find out if random field models would have an ordered phase and things like that. So he was interested, and we started discussing about spin glasses. Eventually we ended up doing some work[23]. At that point, this replica method, somehow people were skeptical about it, in some sense. You know, this business of *n* going to 0. How to take that limit? Many

[16] See, *e.g.*, 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; C. Dasgupta, "Renormalization-group calculation of the critical exponents for percolation," *Phys. Rev. B* **14**, 1221 (1976). https://doi.org/10.1103/PhysRevB.14.1221

[17] See, *e.g.*, C. Dasgupta, A. B. Harris and T. C. Lubensky, "Renormalization-group treatment of the random resistor network in 6− ε dimensions," *Phys. Rev. B* **17**, 1375 (1978). https://doi.org/10.1103/PhysRevB.17.1375

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

[19] P. Charbonneau, *History of RSB Interview: Scott Kirkpatrick*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 24 p. https://doi.org/10.34847/nkl.cba615t7

[20] J. R. L. de Almeida and D. J. Thouless, "Stability of the Sherrington-Kirkpatrick solution of a spin glass model," *J. Phys. A* **11**, 983 (1978). https://doi.org/10.1088/0305-4470/11/5/028

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

[22] J. 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

[23] C. Dasgupta, S.-k. Ma, and C.-K. Hu, "Dynamic properties of a spin-glass model at low temperatures," *Phys. Rev. B* **20**, 3837 (1979). https://doi.org/10.1103/PhysRevB.20.3837

people had used it at that point, but there were also questions about whether this is the right way of calculating quenched averages. So we spent some time trying to find some alternative to using replicas to get this quenched average. Of course, we didn't succeed. In general, we got interested in spin glasses. The work I did there didn't have much to do with infinite-range models. It was basically short-ranged, Edwards-Anderson kind of models. Also, the work was not really asking very deep questions about what is the nature of the spin glass phase, if there is a phase transition, and so on and so forth.

There was also a lot of simulation work that was coming out at that time. Particularly Kurt Binder[24] did a lot of simulations on short-range models of spin glasses. We were trying to understand some of those.

PC: Do you know where Shang-keng Ma's interest in spin glasses came from? Did it start with you, or was he interested in them before as well?

CD: [0:16:36] I don't think he did any work on spin glasses before I joined. But this being a very important problem in disordered systems, I'm sure he had thought about it or discussed with other people about it.

PC: You were at La Jolla when the Parisi solution came out[25]. How did you and Shang-keng react to it? What was your initial impression?

CD: [0:17:07] I would say that at that point it was looked upon as a very ingenious way of breaking replica symmetry. But what it means, it wasn't clear to anybody at that time, I think. If I remember correctly, I didn't spend a lot of time on trying to figure out what this is all about and trying to understand what this order parameter function actually means.

PC: Was it clear that it was the solution? Or was it not clear? Did it look like a lucky trick or that it might actually be the correct description?

CD: [0:17:57] To begin with, the replica method—the replica trick, for many years people used to call it the replica trick—there was all this, in some sense, skepticism about using replicas and taking $n \to 0$. On top of that breaking replica symmetry, and on top of that instead of having a simple order parameter having something which is a function. All these things were very new. People thought that it was a very interesting way of looking

[24] Patrick Charbonneau, *History of RSB Interview: Kurt Binder*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 20 p. https://doi.org/10.34847/nkl.5f2b685y

[25] See, *e.g.*, G. Parisi, "Infinite number of order parameters for spin-glasses," *Phys. Rev. Lett.* **43**, 1754 (1979). https://doi.org/10.1103/PhysRevLett.43.1754

at things, but the actual meaning of that was not clear. At that point, at least, I don't remember spending a lot of time on it.

PC: From La Jolla, you moved to Harvard to work in Halperin's group[26]. How did that happen?

CD: [0:18:52] I spent two years in La Jolla and at the end of two years I got a job offer. I got a faculty position at the University of Minnesota. But before I got the offer, I also had written to a few places to do a second postdoc. Then, I got an offer from Harvard and I decided that I'd postpone joining Minnesota and spend one year at Harvard. That's how it worked out.

It wasn't clear what I was going to work on at Harvard. This was sort of a free postdoc. Although it was in some sense Bert Halperin's group, there were lots of other people. I'll talk a little more about this one year I spent there. A lot of other people, mostly young people, were around and Bert told me: "You should interact with these people, because there are interesting things I think you could work on. You are free to do that."

PC: One of the people who was around at the same time as you—I think he arrived at the same time as you—was Haim Sompolinsky[27]. This is the time when he started working on spin glasses as well, by himself and with Annette Zippelius[28]. I guess you were in the same offices, or the same suite. Can you tell us a bit how this happened?

CD: [0:20:26] Not the same office, but we were in the same building. When I joined both Annette and Haim were there already for maybe one year or a little more than one year. They were working on this dynamical theory of spin glasses[29]. By the time I got there this work was more or less done. Since I had done some work on spin glasses earlier, it was natural for me to talk to these people. We had a lot of discussions. In their papers, my name is mentioned as one of the people with whom they had discussions. Annette, I didn't have so much discussion with, because she left maybe a few months after I joined, but Haim was there throughout so I had a lot of discussions with him. I ended up doing some work with him also.

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[26] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Bertrand I. Halperin*, transcript of an oral history conducted 2021 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 14 p. https://doi.org/10.34847/nkl.7ac326ng

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

[28] Annette Zippelius: https://en.wikipedia.org/wiki/Annette_Zippelius

[29] 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**, 6860 (1982). https://doi.org/10.1103/PhysRevB.25.6860

PC: Exactly. You wrote a paper with him on spin glasses[30]. Can you tell us how that came about from these conversations?

CD: [0:21:49] This now has a lot of overlap with replicas and replica symmetry breaking. When I got there, Parisi's paper was out and there was a lot of discussion about what this way of breaking replica symmetry means. Haim and Annette had done this work on dynamics, so one obvious question was whether it was possible to have some kind of interpretation of this order parameter function in terms of dynamics. That was one thing we had some discussions about.

Actually, Haim wrote a paper which I'm sure you are also familiar with, where he gave a dynamical interpretation of this $q(x)$[31]. In the earlier work, they had found that the dynamics can be treated in a way that didn't require replica. This was one of the attractive points about this way of looking at things. Then, the next issue is whether from the dynamics it is possible to have some interpretation of this order parameter q(x). One thing that Haim assumed was that there is this hierarchy of timescales, since it's a complex system with very long timescales. Of course, long timescales were well known for spin glasses to begin with. In that hierarchy of timescales—all of which go to infinity in the thermodynamic limit—there is ordering. In a finite system, these timescales, if you order them, will [have] a shortest one. [This] shortest one still went to infinity in the thermodynamic limit, but among those infinite timescales the shortest one would be such that averaging over that timescale would give you the $q(1)$. When you say $q(x)$ with x going from 0 to 1, the $q(1)$ end that's what it seemed to be, that's what you would get if you calculate the order parameter averaging over times which are the shortest along this hierarchy. The other assumption was about the other end $q(0)$. Here, Haim's assumption was that if you average over the longest of these many timescales, then what you'd get is $q(0)$. Then, using this dynamical formalism he was able to again work out the dynamics and get results which looked very similar to Parisi's result with this new order parameter. That was done by himself, but of course there were discussions about that with Annette, Bert, me and others.

The problem that I worked on was to try to sort of connect that dynamical description to some kind of static description. There, at a certain point was

[30] C. Dasgupta and H. Sompolinsky, "Equivalence of statistical-mechanical and dynamic descriptions of the infinite-range Ising spin-glass," *Phys. Rev. B* **27**, 4511 (1983). https://doi.org/10.1103/PhysRevB.27.4511
[31] H. Sompolinsky, "Time-dependent order parameters in spin-glasses," *Phys. Rev. Lett.* **47**, 935 (1981). https://doi.org/10.1103/PhysRevLett.47.935

these TAP—Thouless-Anderson-Palmer—equations[32]. At that point, one knew that there are many locally stable solutions of these TAP equations. The idea was then that the origin of this whole spectrum of timescales, we tried to relate that to averaging over these TAP solutions over longer and longer sort of distances in phase space. Again, $q(1)$ was just looking at one TAP solution and $q(0)$ is what you'd get for this order parameter when one did an average over all the TAP solutions. At this point, we didn't know very much about how the overlap between two TAP solutions looked like, what was the distribution of these overlaps and things like that, so we had to make some assumptions. Under these assumptions we got essentially the same thing as what Haim got in his dynamical description. Again, this was very similar to the Parisi picture. The whole thing was basically an attempt to try to understand what this $q(x)$ function means, which was not clear at the time.

Eventually, in '82-'83, around that point, people were able to figure out what this is[33]. The eventual understanding was actually not quite the same as what we had proposed, in the sense that the statistical mechanical average that you get when you average over all the solutions it turns out that it is not $q(0)$, but the integral of $q(x)$ between zero and one. There were some differences in the interpretation. That's how it worked out.

PC: As you said, you went to Les Houches and spent the summer of '78 in Europe. Throughout those years, were you keeping in touch with your peers you met at the school, or back in Paris where there were some parallel efforts? Were you in regular contact, or only preprints?

CD: [0:27:48] Not regular contact. I have kept in touch with some of the people that I met at that school, but not many of them actually worked on spin glasses. I remember there was Thomas Garel, who was my roommate in Les Houches, he did some work with De Dominicis and others on spin glasses[34]. Bernard Derrida later did important work on spin glasses and related systems[35]. Who else was there? Bob Pelcovits was there, but he

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[32] D. J. Thouless, P. W. Anderson and R. G. Palmer, "Solution of 'solvable model of a spin glass'," *Philo. Mag.* **35**, 593-601 (1977). https://doi.org/10.1080/14786437708235992

[33] G. Parisi, "Order parameter for spin-glasses," *Phys. Rev. Lett.* **50**, 1946 (1983). https://doi.org/10.1103/PhysRevLett.50.1946

[34] C. De Dominicis, M. Gabay, T. Garel and H. Orland, "White and weighted averages over solutions of Thouless Anderson Palmer equations for the Sherrington Kirkpatrick spin glass," *J. Phys.* **41**, 923-930 (1980). https://doi.org/10.1051/jphys:01980004109092300; M. Gabay, T. Garel and C. De Dominicis, "Symmetry breaking a la Parisi in the n-component SK model of a spin glass," *J. Phys. C* **15**, 7165 (1982). https://doi.org/10.1088/0022-3719/15/35/014

[35] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Bernard Derrida*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 23 p. https://doi.org/10.34847/nkl.3e183b0o

didn't continue to work on spin glasses[36]. Many other people were there, who later became very famous, and I kept up with some of them like Jean-François Joanny[37], who is now in Paris. (We're doing similar work in a different area – active systems – these days[38].)

PC: After a year at Harvard, you did take the position at Minnesota and moved there. I think your first PhD student there, Amitabha Chakrabarty[39], worked with you on spin glass problems, especially on the RKKY spin glass model[40]. What were you pursuing? Can you walk us through what you were trying to achieve?

CD: [0:29:09] I was at Harvard for just one year. With Haim we had done this work. There's another interesting thing about that work. (I haven't seen much work on that later on.) We were looking at this staggered magnetization. Basically, there's this J_{ij} matrix in the Sherrington-Kirkpatrick model. As you know, you can look at it as a random matrix and look at its eigenvalues and eigenfunctions. There's a semi-circular law for the distribution of eigenvalues. We were looking at the projection of the onsite magnetization onto the eigenvectors of this random J_{ij} matrix. When the system orders, this staggered magnetization also will have non-zero values, so we were looking at the distribution, and whether if you look at the largest eigenvalue, the staggered magnetization for that eigenfunction is extensive or not, and things like that. There is some numerical work on that: the simulation of the SK model and calculating the projections of the onsite magnetizations onto the eigenvectors of that J_{ij} matrix. So I got involved in some numerical work on spin glasses. The Monte Carlo simulations I did for the first time there. I didn't have a lot of computing facilities those days. I remember I went to the IBM research center in Yorktown Heights[41], where I knew Scott Kirkpatrick from this summer school. I actually spent some time to do that Monte Carlo there. Thus I got involved in some Monte Carlo work dealing with spin glass models.

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[36] R. A. Pelcovits, E. Pytte and J. Rudnick, "Spin-glass and ferromagnetic behavior induced by random uniaxial anisotropy," *Phys. Rev. Lett.* **40**, 476 (1978). https://doi.org/10.1103/PhysRevLett.40.476

[37] Jean-François Joanny: https://fr.wikipedia.org/wiki/Jean-Fran%C3%A7ois_Joanny

[38] See, *e.g.*, S. K. Nandi, R. Mandal, P. J. Bhuyan, C. Dasgupta, M. Rao, and N. S. Gov, "A random first-order transition theory for an active glass," *Proc. Nat. Acad. Sci. U. S. A.* **115**, 7688-7693 (2018). https://doi.org/10.1073/pnas.1721324115

[39] Amitabha Chakrabarty, *Statics and dynamics of a model for spin-glasses*, PhD Thesis, University of Minnesota (1987). https://primo.lib.umn.edu/permalink/f/1jg5c4a/UMN_ALMA21441377070001701

[40] A. Chakrabarti and C. Dasgupta, "Phase Transition in the Ruderman-Kittel-Kasuya-Yosida Model of Spin-Glass," *Phys. Rev. Lett.* **56**, 1404 (1986). https://doi.org/10.1103/PhysRevLett.56.1404

[41] Thomas J. Watson Research Center: https://en.wikipedia.org/wiki/Thomas_J._Watson_Research_Center

The other thing that you might find interesting was that when I went to IBM to do this, Peter Young[42] also was visiting. We ended up having a lot of discussions about timescales, in the sense that if you look at the SK model below the transition—the ordered state—then for small equilibrated systems, you want to know how does that time scale depend on the size N of the system. Because one of the things that was in this Parisi kind of discussion with a number of pure states is that there will be timescales that will diverge as the system size goes to infinity. I started some work which afterward I didn't follow up, but Peter did. There was a paper by Mackenzie and Young, where they showed that it grows as the exponential of some power of *N*[43]. So I got involved in doing numerical work on spin glass models.

When I took up the job at Minnesota, the first year I didn't do any research, because I was teaching for the first time. This took up all my time. That's one of the reasons I didn't follow up with the work that I started with Haim Sompolinsky or Peter Young. Then, at the end of second year this student joined me. Since I had been doing numerical work on spin glasses I thought it might be useful to look at realistic spin glasses and try to understand whether they exhibit phase transitions.

PC: Did you have better computational facilities at Minnesota at that point?

CD: [0:33:07] At Minnesota, we were lucky because Cray was based in Minnesota[44]. (Cray is the computer company who made these supercomputers.) So the University of Minnesota actually had a Cray supercomputer. Computing was much easier there. It's still not anywhere near what people can do now, but we did what we could at that time.

FZ: This was more or less the time when there was a kind of intense debate on the nature of the spin glass phase—between the droplet picture and the RSB picture. Were you involved in these discussions? And what is your impression about this discussion at the time?

CD: [0:34:03] I was not directly involved in this discussion. Although the model that we looked at is short-ranged, not of the fully-connected kind of models. So certainly whether the mean-field picture applies to these systems was a relevant question. I did read these papers—Fisher and

[42] 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
[43] N. D. Mackenzie and A. P. Young, "Lack of Ergodicity in the Infinite-Range Ising Spin-Glass," *Phys. Rev. Lett.* **49**, 301 (1982). https://doi.org/10.1103/PhysRevLett.49.301
[44] Cray: https://en.wikipedia.org/wiki/Cray

Huse[45]—about the droplet picture. The droplet picture was in some ways more familiar to me, because I had earlier done some work with Shang-keng Ma where you had these clusters which were two-level systems and so on and so forth[46]. I was aware of this controversy at that point, but I didn't do anything to try to resolve that controversy by looking at the ordered state in more detail to see what kind of signature of this way or that way one could get. It seems to me that the controversy is still not completely resolved, right? There are evidences in favor of both of the pictures.

FZ: What was your impression about the community at the time? How was it organized? For people working on spin glasses in the US in particular, what was the structure?

CD: [0:35:29] In the US, it seemed to me—apart of course of this Fisher and Huse work, which was done in the US—others, at least the ones that I talked to at that point, were more interested in some sense in the occurrence of phase transitions in realistic models. There was this special purpose machine that was built by Ogielski[47]. Peter Young was also involved. These people I knew very well. I had interactions with them. That was the main question, but of course one of the fundamental questions was about the nature of the ordered state. But for more realistic models, at least people doing numerical work were concerned with trying to understand whether they undergo phase transitions, whereas in Europe—I think because of Parisi—much more work was being done on this mean-field picture trying to check whether that works for short-ranged models.

PC: A different way of asking the question: how connected were the different groups working on spin glasses? Were there regular seminars? Would you invite each other? Would you travel? Would you exchange students or postdocs? Or, in yet another way, how isolated or connected were you in Minnesota?

CD: [0:37:04] I don't know. The internet wasn't there, so having connections was not so easy. You had to make phone calls, or to send letters. We used to discuss. Peter Young, I have known from my PhD days. As I said, there were occasions when I'd interact with him. That's one person I talked to

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[45] 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

[46] See Ref. 23.

[47] P. Charbonneau, *History of RSB Interview: Andrew T. Ogielski*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École Normale Supérieure, Paris, 2021, 23 p. https://doi.org/10.34847/nkl.86f6z55x

maybe on the phone sometimes, or correspondence by mail. David Huse[48] and Ravindra Bhatt, they were at that point at Bell Labs and I had connections with them. But you know the community was not very large. Because of all these difficulties in communicating on a daily basis it was not a very tightly knit group of people who worked on similar problems or anything like that. We met up occasionally. I visited some of these places, and those people came.

FZ: Did someone ever try to set up a network of people working in the US? Obtain some funding to work as a community and exchange?

CD: [0:38:30] In this area, in spin glasses, not that I know of. There was interest in other disordered systems, like superconducting systems, Josephson junction arrays[49], and some things like that. There was someone I knew at Ohio State University[50] who was working on that. So we did some work[51]. There was an experimentalist at Minnesota who was working on that[52]. Disordered systems were still quite popular, and one was trying to understand many properties of those, but it was more or less individual groups, as far as I remember.

PC: In 1987, you moved to India, to the IISc in Bangalore. What drove you to join the IISC at that point?

CD: [0:39:22] It was never my intention to stay permanently in the US. After two years in Minnesota, in 1984, I took a leave from there and came to IISc to spend one year as a visitor. At that point, I liked the IISc very much. At that point, however, there was no possibility of getting a permanent position there for various reasons. It depends on who is the director and whether he wants to have more positions in physics and things like that. So I went back to Minnesota, but then in ’86-’87, a position was offered so I came back to Bangalore.

PC: Shortly after joining IISc you left the field of spin glasses and you started to work on structural glasses in particular. How did that intellectual transition come about?

CD: [0:40:30] First of all, it came from the similarity of spin glasses and structural glasses I have commented on earlier, given the ’78 Les Houches

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[48] David Huse: https://en.wikipedia.org/wiki/David_A._Huse
[49] Josephson effect: https://en.wikipedia.org/wiki/Josephson_effect
[50] David Gordon Stroud: https://academictree.org/physics/peopleinfo.php?pid=431426
[51] A. Chakrabarti and C. Dasgupta, “Phase transition in positionally disordered Josephson-junction arrays in a transverse magnetic field,” *Phys. Rev. B* **37**, 7557 (1988). https://doi.org/10.1103/PhysRevB.37.7557
[52] Allen M. Goldman: https://history.aip.org/phn/11511023.html

school, where Phil Anderson gave talks on both spin glasses and structural glasses. He said that spin glasses were a simpler system because there you have disorder imposed on the systems, whereas in structural glasses the disorder is generated by the system. I was interested, but then when I came to IISC, the people that were there… In particular, the person who was instrumental in asking me to come and join IISc was T. V. Ramakrishan[53], who was a very well-known physicist who worked on localization and things like that and who also worked on the freezing of liquids into crystalline solids. There is a theory called the Ramakrishnan-Yussouff theory, which is sort of a mean-field theory of the freezing of a liquid into a crystalline solid[54]. He was there and I was interested in disordered systems so one of the things that we were discussing at that point was to see whether similar things can be done—whether the Ramakrishnan-Yussouff description can be used to study glasses—or whether this TAP kind of description can be extended to glasses, where you look at a local minimum of the free energy functional and see whether there are many minima and stuff like that[55]. After coming to IISc, I have not worked on spin glasses.

PC: Another group who suggested this pivot was Kirkpatrick, Thirumalai and Wolynes who made a direct analogy between spin glasses and structural glasses[56]. Were you following that work? What was your reaction to these ideas at the time?

CD: [0:42:48] I was following, and I actually thought that it was very nice. The way they connected this Potts glass to structural glasses is very interesting. By that time, replica symmetry breaking, its physical interpretation, was more well established. So that work didn't have some of the concerns that people used to express earlier—the concerns about the replica method and in particular about replica symmetry breaking—because now it was more or less established as a good technique. Also, what one means by replica symmetry breaking, that also was somewhat clear at that time. This

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[53] T. V. Ramakrishnan: https://en.wikipedia.org/wiki/T._V._Ramakrishnan ; See also: T. V. Ramakrishnan, "One Subject, Two Lands: My Journey in Condensed Matter Physics", *Annu. Rev. Condens. Matt. Phys.* **7**, 1-10 (2016). https://doi.org/10.1146/annurev-conmatphys-031115-011442

[54] See, *e.g.,* T. V. Ramakrishnan and M. Yussouff, "First-principles order-parameter theory of freezing," *Phys. Rev. B* **19**, 2775 (1979). https://doi.org/10.1103/PhysRevB.19.2775

[55] C. Dasgupta and S. Ramaswamy, "Search for a thermodynamic basis for the glass transition," *Physica A* **186**, 314-326 (1992). https://doi.org/10.1016/0378-4371(92)90386-5; C. Dasgupta, "Glass transition in the density functional theory of freezing," *Europhys. Lett.* **20**, 131 (1992). https://doi.org/10.1209/0295-5075/20/2/007

[56] See, *e.g.*, T. D. Kirkpatrick and P. G. Wolynes, "Stable and metastable states in mean-field Potts and structural glasses," *Phys. Rev. B* **36**, 8552 (1987). https://doi.org/10.1103/PhysRevB.36.8552; T. R. Kirkpatrick, D. Thirumalai, 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

made more sense. The first thing that I did on glasses was trying to find this multiplicity of minima. That was in some sense the TAP picture for glasses.

PC: Can you help us understand what was the structural glass community at that time? How was it structured? What were the different points of view that were present in the late-‘80s, early-’90s?

CD: [0:44:22] Again, I am not aware of any—apart from close-knit groups in Paris and in Rome in which several people were working on these things together—of any whatever structure in the community. In the US, as far as I know, there was no such large group. There were people like Peter Wolynes[57] who were doing this kind of work, but he was doing this mostly with people in his group. A few people were working on the mode-coupling theory. There were people who were doing simulations on glasses, some of them in chemistry departments in various universities, but no big group as far as I know. Neither in India. I did collaborate with many people in India, but that was more on a personal, one-on-one basis.

PC: One of your first papers on structural glasses adamantly concludes that there’s no growing static length scale in glasses[58]. What was your vision of the structural glass problem at that point?

CD: [0:45:35] Our vision at that point was that there is no static transition. What we said at that point was—whatever dynamic length scale one is talking about now I worked on that later—in that paper itself there are one or two lines that says that on very long timescales—in a purely static sense—it is difficult to get any length that is growing. But we missed looking at intermediate times. If we had looked at intermediate times, we would have of course seen that there is a growth of a dynamic time scale. Another thing that convinced us that there is no growing length scale was this finite-size scaling of the relaxation time with system size. There, we saw behavior which is quite opposite to what one finds in spin glasses in which time scales near the transition increases as a function of system size. In glass forming liquids, we found that the time scale is decreasing or remaining constant with system size. The title of that paper is of course misleading at this point.

There also, if you’re looking at a historical perspective, one thing [that] I should mention is that when I came back to IISc, in ’87 and until the early

[57] Peter Guy Wolynes: https://en.wikipedia.org/wiki/Peter_Guy_Wolynes

[58] C. Dasgupta, A. V. Indrani, S. Ramaswamy and M. K. Phani, “Is there a growing correlation length near the glass transition?” *Europhys. Lett.* **15**, 307 (1991). https://doi.org/10.1209/0295-5075/15/3/013

'90s, computation was almost impossible. The computers that we had, given the number of people who wanted to do computations, were very inadequate. One had to make a reservation in some sense to use the computers for a certain period. This work was actually done with a friend of mine who used to work in an aerospace development lab located in Bangalore[59]. They had fast computers, so whatever little computation we could do at that point was done over there. You can see from the paper that the simulation part is not really all that great, in the sense that small time scales, small system sizes and things like that are used.

PC: Simulations have come a long way for sure. At Minnesota, at IISc or elsewhere, did you ever get to teach around spin glasses and replica symmetry breaking? If yes, can you detail the context and the content?

CD: [0:48:21] I have taught a course on disordered systems, as a special topics course in statistical mechanics/condensed matter group at IISc. I taught it a few times. The last time I taught it with Srikanth Sastry[60], whom you know probably and who is also in Bangalore. We taught this course together. He did half of it.

PC: Was the first time in the mid-'90s? Could you just give us an idea of when that would have been?

CD: [0:49:03] No. Much later. The last one was maybe three, four years ago. Then the previous one was five years before that, and then again five years before that. There, we don't talk about only spin glasses but disordered magnets, percolation, and other disordered systems. Spin glass occupies a substantial part of that course and we do talk about replica symmetry breaking there. There are sometimes other places where I have taught about this. There are these summer schools which are organized once in a while, and there I have talked about replica symmetry breaking.

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

CD: [0:50:03] Not really. I just want to mention that I came back to replica symmetry breaking in the context of glasses, many years after thinking about that for spin glasses. That was done when we were looking at a liquid in the presence of pinning disorder. I got interested in that because we did

[59] Aeronautical Development Establishment: https://en.wikipedia.org/wiki/Aeronautical_Development_Establishment. Many of the early IISC papers also acknowledge computer time at the Minnesota Supercomputer Institute, because there were done in collaboration with Oriol Valls who is at the University of Minnesota.
[60] Srikanth Sastry: https://en.wikipedia.org/wiki/Srikanth_Sastry

some work on high-T_c superconductors, where you have a vortex lattice, and then pinning centers are very common there[61]. One wants to understand what effect this disorder has on the vortex lattice and the transition to the vortex lattice. There, this one-step replica symmetry breaking I looked at again, in collaboration with some people in France: Denis Feinberg and Fabrice Thalmann[62]. (Maybe Francesco knows him. Denis is at Laboratoire d'Etudes des Propriétes Electroniques des Solides, CNRS and Université Joseph Fourier, Grenoble, and Fabrice works at Institut Charles Sadron, Strasbourg.) We had some joint program with people in Grenoble to look at superconductivity, in particular this vertex lattice and things like that. This question came up in that context.

PC: So that's what reinterested you in ideas of replica symmetry breaking. Ideas of length scales came back in a more convincing way in your mind.

CD: [0:51:39] Right. This is somewhat different in the sense that there the replica symmetry breaking we talk about is in the equations of liquid state theory. In the liquid state case, one has this HNC or Percus-Yevick or this kind of liquid-state equation which tells you about the structure of the liquid and stuff like that. There, Mézard and Parisi introduced this notion of replica symmetry breaking in that context[63]. We used that to study pinning disorder in a hard-sphere liquid.

PC: I think from that point on until today you've remained interested in structural glasses and invested in that.

CD: [0:52:30] To a large extent, yes.

PC: 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?

CD: [0:52:47] It would be very difficult. Even if I had, if would be very difficult to find, because this would go back like 40 years. As I said, when we were working on these things then there were letters exchanged, because email was not available at that time. Those letters were there, but I would be

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[61] See, *e.g,* G. I. Menon and C. Dasgupta, "Effects of pinning disorder on the correlations and freezing of the flux liquid in layered superconductors. *Phys. Rev. Lett.* **73**, 1023 (1994). https://doi.org/10.1103/PhysRevLett.73.1023; C. Dasgupta and O. T. Valls, Two-step melting of the vortex solid in layered superconductors with random columnar pins, *Phys. Rev. Lett.* **91**, 127002 (2003). https://doi.org/10.1103/PhysRevLett.91.127002

[62] F. Thalmann, C. Dasgupta and D. Feinberg, "Phase diagram of a classical fluid in a quenched random potential, *Europhys. Lett.* **50**, 54 (2000). https://doi.org/10.1209/epl/i2000-00234-2

[63] 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

very surprised if I could find those letters. Letters with Haim, for example, or with Peter Young or with Ravin Bhatt. When I was in the US, and I was working on spin glasses, there were correspondences.

PC: So you brought them back with you to Bangalore, but you're not sure where they are. Is that right?

CD: [0:53:35] Yeah. You know, in those days one used to have some filing cabinets and to place old papers—at least I used to do—relevant to a particular publication in one file and keep there for posterity. But not anymore.

PC: Thank you very much for this conversation. It's been a real pleasure.

CD: [0:54:02] Ok. Thank you. I enjoyed reminiscing about those days, whatever I remember. Thanks for asking me to talk about this.