History of RSB Interview: Mehran Kardar

September 29, 2023, 10:00 to 11:00am (CET). Final revision: November 10, 2023

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Location:
Over Zoom, from Prof. Kardar's apartment while visiting Cambridge, UK.
How to cite:
P. Charbonneau, *History of RSB Interview: Mehran Kardar*, transcript of an oral history conducted 2023 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2023, 14 p.
https://doi.org/10.34847/nkl.cdf05i34

PC: Good morning, Professor Kardar. Thank you very much for joining us. As we've discussed ahead of this interview, the theme of these discussions is on the history of spin glasses and replica symmetry breaking in physics, which we roughly bound from 1975 to 1995. But before we dive into the subject, however, we have a few questions on background to ask you. Can you tell us a bit more about your family and your studies before starting university? In particular, how did you get interested in science?

MK: [0:00:42] I was born in Iran and continued through high school in Iran. I guess I was always interested in mathematics and science. I had all intentions to continue studying in Iran for university, but kind of unbeknownst to me my father had gotten me an application to go to the UK for university. I came to where I'm currently visiting, Cambridge University, and I got a degree in natural sciences. For the PhD, I went to MIT starting in 1979[1].

PC: You studied natural sciences with a focus on physics, I guess, at Cambridge. What drew you to this particular subfield, rather than mathematics or other sciences?

MK: [0:01:53] I guess if you're coming from a developing country such as Iran, the focus will be to go for something that is a high-earning position, so something like engineering. If you are inclined towards mathematics, science is a good medium in between to please family and allow you to continue the things that are more mathematical and theoretical.

[1] Mehran Kardar, *Ordering phenomena under competing interactions in adsorbed layers and in spin systems*, PhD Thesis, MIT (1983).
https://mit.primo.exlibrisgroup.com/permalink/01MIT_INST/ejdckj/alma990002076240106761 (Accessed October 10, 2023.)

Cambridge at that time—I don't know if it is also currently—did not have a specific degree in physics or chemistry. It was all natural sciences. So, I did indeed focus on physics, but along the way I took classes in chemistry, materials science, and such things also.

PC: What drew you to pursue graduate studies at MIT, then, and to work with Nihat Berker[2] on disordered materials in particular.

MK: [0:03:03] Actually, I am involved with graduate applications these days and I am kind of surprised that I did get to MIT in the first place, because the kinds of applicants that we get, already have very extensive experience in a particular area and kind of focus on continuing in that area. Somehow, coming from Cambridge University, I had not obtained any specific research experience. I was certainly good at the various topics that I had studied, and I had excellent grades in the exams, entrance exams, etc., but not having done research in any particular direction I just thought I might go toward things to study like general relativity or something very theoretical in high energy physics.

I had taken a risk in coming to MIT, because I didn't have any funding support from MIT. They just gave me admission without support. My family said: "Why don't you take the risk?" So, when I arrived at MIT without support, I sort of looked around for anybody who would give me some kind of financial support. In some sense, it is by accident that I landed in statistical physics, because the same year that I started as a graduate student Nihat Berker was hired as a faculty member. He was looking for students and had support. He offered me a RA position and I said: "Yes!" I'm very happy that accident occurred, and I did not end up pursuing general relativity or something else, but I can say that it was completely accidental that I ended up in this particular group. There was no underlying plan for that.

PC: Can you give us a general feel of what was the statistical physics community around MIT and Harvard, around that time? Were there group meetings or larger organizations or were you working solely with Prof. Berker?

MK: [0:05:50] I guess Professor Berker was to some extent hired at MIT in order to revive statistical mechanics there. Prior to him, Eugene Stanley[3] was at MIT, but he had left for Boston University several years earlier. And while MIT at that time was strong in various areas, statistical physics was not one

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[2] Nihat Berker: https://en.wikipedia.org/wiki/Nihat_Berker

[3] H. Eugene Stanley: https://en.wikipedia.org/wiki/H._Eugene_Stanley

of them. But then Nihat was very active. (I will refer to Professor Berker as Nihat, if you don't mind.) One of the first things that he did, was to arrange for Amnon Aharony[4] to come to MIT. Amnon was interacting with Bob Birgeneau[5], who was doing experiments on two-dimensional materials, and Litster[6], who was doing experiments on liquid crystals. So, the experimental component was certainly strong, and gradually the theory—with bringing visitors such as Amnon Aharony—became stronger and stronger. The first year that I was at MIT, Amnon Aharony gave a set of lectures on critical phenomena, and I remember that several people from Harvard and other places were also attending, so gradually that community grew. Later on, Henri Orland[7] also came as another visitor, and that was also an additional external person. So, I would say that through the efforts of Nihat Berker gradually the statistical physics theory at MIT became stronger and stronger.

PC: As you were just mentioning, Henri Orland was a visitor while you were there. From what we understand, he taught a class on disordered systems, which you took during your graduate years. If we're not mistaken, that was your first exposure to spin glasses and replica symmetry breaking. Is that the case? In any event, can you tell us a bit more about this course?

MK: [0:08:19] I would say that Henri Orland gave two courses at MIT that were very influential for me. The first one was a course that he presented jointly by John Negele[8] on quantum many-body systems. They later wrote a very nice textbook that was based on material they thought[9]. Maybe I will tell you later why that course was very important to me. The second course was indeed singly delivered by Henri Orland and was a very rapid and comprehensive scan through a lot of things that were related to polymers, disordered systems, random field systems, random bond systems, spin glasses. Indeed, he did cover replica symmetry breaking which was quite new at that time.

I guess one of the reasons that you're presently doing this historical overview is the role that Giorgio Parisi has played in replica symmetry

[4] Amnon Aharony: https://en.wikipedia.org/wiki/Amnon_Aharony
[5] Robert J. Birgeneau: https://en.wikipedia.org/wiki/Robert_J._Birgeneau
[6] See, *e.g.*, Interview of James David Litster by David Zierler on August 5, 2020, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, www.aip.org/history-programs/niels-bohr-library/oral-histories/47238 (Accessed October 10, 2023.)
[7] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Henri Orland*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 18 p. https://doi.org/10.34847/nkl.1d000dgs
[8] John W. Negele: https://en.wikipedia.org/wiki/John_W._Negele
[9] J. W. Negele and H. Orland, *Quantum many-particle systems* (Redwood City, CA: Adisson Wesley, 1988).

breaking etc. I should say that even before taking Henri Orland's course I was aware of Giorgio's contributions as a graduate student, not related to replica symmetry breaking, but his work on dimensional reduction in random fields via supersymmetry[10]. I guess as a second- or third-year graduate student we were very intrigued with this. With two other graduate students who were doing field theory, we studied this paper extensively, and even wrote a paper authored by these three students that was an extension of that work[11]. So, I guess my initial exposure to the work of Giorgio was more from the random field perspective, rather than the spin glass perspective.

PC: Had you come to that paper through the literature, or had you heard about it from colleagues or conferences?

MK: [0:11:10] Bob Birgeneau at MIT was doing experiments on systems that were mimicking the random field Ising model. There was always this discussion as to whether the three-dimensional or two-dimensional random field systems were ordered or not, because there was the tension between the Imry-Ma argument[12] and dimensional reduction. Through the presentation of Bob Birgeneau of the experiments and of the contrast of the theoretical possibilities, we had come across this paper of Parisi and Sourlas.

PC: You mentioned that quantum many-body course that was very influential to you. Would you mind elaborating as to why that was?

MK: [0:12:12] In your original email, you said that you are exploring the concept of replica symmetry breaking. My response was that I haven't really used replica symmetry breaking, but I have used replicas without symmetry breaking. The way that I came across that is essentially via interfaces and paths in random media and replicating them. For that, the replicated system becomes equivalent to a quantum mechanical set of particles with interactions[13]. The reason I knew how to deal with this system was because of the class that I had taken with Henri and Negele, where they had mentioned Bethe ansatz. So, my connection to the Bethe ansatz that I

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[10] G. Parisi and N. Sourlas, "Random magnetic fields, supersymmetry, and negative dimensions," *Phys. Rev. Lett.* **43**, 744 (1979). https://doi.org/10.1103/PhysRevLett.43.744

[11] M. Kardar, B. McClain and C. Taylor, "Dimensional reduction with correlated random fields. A superspace renormalization-group calculation," *Phys. Rev. B* **27**, 5875 (1983). https://doi.org/10.1103/PhysRevB.27.5875

[12] 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

[13] M. Kardar, "Replica Bethe ansatz studies of two-dimensional interfaces with quenched random impurities." *Nucl. Phys. B* **290**, 582-602 (1987). https://doi.org/10.1016/0550-3213(87)90203-3

used for this version of a replicated disordered paths came through exposure to the class that Henri and Negele were teaching.

PC: We'll get back to these papers in a moment, but I see the connection now. After completing your graduate studies, you were a junior fellow at Harvard[14], at which point you were essentially free to choose your own research problems. What generally drove your selection or research directions at that point in your career?

MK: [0:13:51] There is actually a continuity between what I was doing as a graduate student and later at Harvard as a junior fellow. Again, the experimental work of Bob Birgeneau was relevant because they had looked at the system of krypton adsorbed on graphite. Essentially, what happens is that graphite presents three possible sublattices for krypton atoms. They have to select one of the three. Because of their size, they can't sit next to each other on hexagons that are in sublattices A and B. They have to select one of the three sublattices, A, B or C, and so this is a three-state symmetry breaking in the Potts universality class. As you increase the pressure of krypton and more krypton wants to get on the surface, the way that it does so is that it condenses into domain walls—between domains that are in say A, B, or C—and then additional material can accumulate in the domain wall between A and B, say. A very important aspect of thinking about this commensurate-incommensurate transition was the statistics of these domain walls that form between domains. Already for my graduate work, for my PhD, I had studied a lot commensurate-incommensurate transitions and the role of these domain walls[15]. Then, when I was at Harvard, given that I had also looked at some disordered systems it was natural to think about what happens to these domain walls when the underlying system is disordered. So, it was sort of a natural switch to go from commensurate to incommensurate transitions in the absence of disorder to in the presence of disorder[16]. Again, these were one-dimensional defects. The other thing that I worked on at Harvard was two-dimensional manifolds, random surfaces etc[17]. This is kind of a theme that was with me from my PhD days to think about the statistics of lines, surfaces and then it became dynamics of surfaces and things like that.

[14] Harvard Society of Fellows: https://en.wikipedia.org/wiki/Harvard_Society_of_Fellows

[15] M. Kardar and A. N. Berker, "Commensurate-incommensurate phase diagrams for overlayers from a helical Potts model," *Phys. Rev. Lett.* **48**, 1552 (1982). https://doi.org/10.1103/PhysRevLett.48.1552

[16] M. Kardar and D. R. Nelson, "Commensurate-incommensurate transitions with quenched random impurities," *Phys. Rev. Lett.* **55**, 1157 (1985). https://doi.org/10.1103/PhysRevLett.55.1157

[17] See, *e.g.*, Y. Kantor, M. Kardar, D. R. Nelson, "Statistical mechanics of tethered surfaces," *Phys. Rev. Lett.* **57**, 791 (1986). https://doi.org/10.1103/PhysRevLett.57.791

PC: In two 1985 PRLs, you used the replica method[18]. In one of these, you mentioned that "since the $n \to 0$ limit may introduce some complications it is worthwhile to complement that the theoretical results with numerical simulations." You did not, however, elaborate or cite anything about these complications. I presume you were aware of the replica symmetry breaking scheme at that point. Was it so well known it didn't need a citation? Also, did you ever consider computing stability of the replication symmetric solution à la Thouless and de Almeida[19], which would have been another approach to validate the replica (symmetric) method?

MK: [0:17:39] Actually, the concern that I had—and the conflict that I had with some in the scientific community—was less regarding the domain of replica symmetry breaking, but the interpretation of even the replica symmetric solution. If you just look at the case of a single interface in a random environment and you replicate it, the corresponding replicated free energy has a term that is linear in *n* and a term that is n^3. So, if you sort of focus on the $n \to 0$ limit, from the linear term you can read off what the quenched average free energy is. But for the case of fluctuations, I wanted to rely on the absence of the n^2 term and the presence of an n^3 term to deduce 1/3 type of fluctuation for the exponent. Immediately, anybody who was familiar with various aspects of probability theory could tell you that in the large n limit it does not make sense to have something like the n^3 behavior. So, there was a lot of resistance to that particular interpretation. I'm not quite sure of something that I wrote many years ago, but if I were to guess what was behind that statement, it was not so much the usual replica symmetry breaking controversy, but just the ability to deduce something from the moments of a distribution.

PC: A couple years later, you did work a replicated Bethe ansatz study of a two-dimensional interface with quenched random impurities, as you mentioned earlier. In that work, you were much more careful in bringing up the spin glass literature. Had anything changed in between those two moments or was it just the idiosyncrasies of writing a paper?

MK: [0:20:26] I just don't remember. Sorry.

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[18] Ref. 16 and M. Kardar, "Depinning by quenched randomness," *Phys. Rev. Lett.* **55**, 2235 (1985). https://doi.org/10.1103/PhysRevLett.55.2235

[19] 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

PC: At about the same time as you wrote that paper on the replicated Bethe ansatz, David Thouless and collaborators at Cornell[20], Jim Sethna[21] and Jennifer[22] and Lincoln Chayes, who had been at Harvard at the time you were a junior fellow there, were also working on calculations on the Bethe lattice with disorder, but in the context of spin glasses. Were you following their work? Where was there any general awareness of people using these techniques in the community at the time?

MK: [0:21:05] I certainly remember Jennifer and Lincoln Chayes at Harvard, but my recollection is that with John Imbrie[23], who was also at Harvard at that time, the interest that they seemed to be having was more on the question of random field systems and approaching things from purely rigorous mathematical aspects rather than replica theory. So, that's part of their work at Harvard I was certainly not aware of.

PC: To be clear, they did that work at Cornell, not at Harvard, but you might have met them at Harvard. So, I understand that you had not kept in touch.

MK: [0:22:09] There was that group at Harvard. John Imbrie certainly had made a lot of progress on the random field problem. He was somewhat interested in the random bond but not that much.

PC: In 1986, and between those two works, you collaborated with Yi-Cheng Zhang and Giorgio Parisi on the dynamics of growing interfaces[24]. Can you tell us a bit more how that work came about? Also, did you discuss with Parisi about replica symmetry breaking at that time?

MK: [0:22:49] As a junior fellow at Harvard, I was pretty much independent in what I could do, but summers were particularly empty of stimulation in the Cambridge area. Most people would have left. There was an opportunity to spend summers at Brookhaven lab. Per Bak[25] was then the head of theory group there, and he had provided the opportunity to go and visit

[20] J. T. Chayes, L. Chayes, J. P. Sethna and D. J. Thouless, "A mean field spin glass with short-range interactions," *Commun. Math. Phys.* **106**, 41-89 (1986). https://doi.org/10.1007/BF01210926
[21] P. Charbonneau, *History of RSB Interview: James P. Sethna*, transcript of an oral history conducted 2022 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 16 p. https://doi.org/10.34847/nkl.7cbfsjjg
[22] P. Charbonneau, *History of RSB Interview: Jennifer Chayes*, transcript of an oral history conducted 2023 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2023, 17 p. https://doi.org/10.34847/nkl.151d08l1
[23] J. Z. Imbrie, "Lower critical dimension of the random-field Ising model," *Phys. Rev. Lett.* **53**, 1747 (1984). https://doi.org/10.1103/PhysRevLett.53.1747
[24] M. Kardar, G. Parisi and Y.-C. Zhang, "Dynamic scaling of growing interfaces," *Phys. Rev. Lett.* **56**, 889 (1986). https://doi.org/10.1103/PhysRevLett.56.889
[25] Per Bak: https://en.wikipedia.org/wiki/Per_Bak

Brookhaven. I spent two or three summers there. One of these summers was when Yi-Cheng Zhang had just been hired as a post-doc, coming fresh out of a PhD with Giorgio Parisi. One of the things that was nice about Brookhaven was that there wasn't much to do, except to sit around and talk about science and all kinds of things. Zhang had interesting things to say, and among the various discussions was a project that Giorgio had suggested to him, to look at an equation that described interfaces. We were not even quite sure what aspects of interfaces it was describing. So, that was around. I don't think immediately, but after I went back to Harvard, I at some point realized that what Zhang had described to me as the suggestion by Giorgio was very related to these problems of interfaces that I was looking at. Once that connection was apparent, Zhang and I sort of continued to work and figured out exactly what was going on, what the meaning of the question was, etc. Then, we constructed this paper. I think it was Zhang who was more in touch with Giorgio Parisi. I did not interact with him directly. It's somewhat interesting that there is this paper that establishes a connection between me, Zhang, and Giorgio Parisi, but I don't think I talked to Giorgio in person until 10-15 years later.

PC: Once you became faculty at MIT, you kept on using the replica method to study, in particular, paths in disordered systems[26]. Henri Orland was doing similar work at that same time as well. Had you kept in touch with him after the course? Were you at all in touch with him?

MK: [0:26:24] Certainly. Henri was a great influence both in terms of the classes that he taught and the opportunity to have somebody else interested in statistical physics. I recall that both of us met many times at the Institute for theoretical physics—whether it was KITP or before that as ITP[27]. We wrote papers also. His approach to disordered systems was more through variational approximations. We did have at least one paper that was related to an interface that we looked at by variational methods[28].

PC: Did you ever travel to Paris in those years and visit the groups working in statistical physics there?

MK: [0:27:35] I should say that I did not travel very much. The reason for that was [that] I'm originally from Iran and I was carrying a passport from Iran, which limited travel to a large extent. So, I had to be very careful when I

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[26] E.g., E. Medina, M. Kardar, Y, Shapir and W. R. Wang, "Interference of directed paths in disordered systems," *Phys. Rev. Lett.* **62**, 941 (1989). https://doi.org/10.1103/PhysRevLett.62.941

[27] Kavli Institute of Theoretical Physics: https://en.wikipedia.org/wiki/Kavli_Institute_for_Theoretical_Physics

[28] T. Garel, M. Kardar and H. Orland, "Adsorption of polymers on a fluctuating surface," *Europhys. Lett.* **29**, 303 (1995). https://doi.org/10.1209/0295-5075/29/4/006

wanted to make the investment of applying for a visa several months ahead of time to be able to visit some particular place or other. As I mentioned, it took me maybe ten years after coming to the US before international travel. After a while, I was a permanent resident, I had a green card, but my passport was still Iranian, so that limited where I could go. It was only around 2000 that I became a full US citizen, and I started traveling more frequently.

PC: In the early 90s, you did work on a spin glass model, the $\pm J$ model in 2D[29]. It's seemingly uncharacteristic compared to other work you had done. What drove you to be interested in this model?

MK: [0:29:01 One of the [reasons why] I was drawn to the problem of the directed polymer was that it was something that was possible to calculate realizations of, numerically in polynomial time. You were more certain of what was coming out of the numerics, because you could sort of get results that were almost exact in polynomial time. Then, teaching classes that were related to statistical physics, and teaching in particular Onsager solutions etc., it was clear to me that one could also do so for the 2D Ising model—calculate the partition function etc.—in polynomial time. I guess one thing that I did not fully appreciate was that sampling over many possible realizations was still a very difficult task to do. I had an excellent graduate student, Laurence Saul[30], who was able to write nice integer implementation of this method and to calculate partition functions in polynomial time. That was really the underlying reason, to sort of bring this algorithm to the attention of the community. In principle, we could have done more with it. After my student went on to other things, I did not follow up on that, but I understand that other people have, which is a positive thing that we wanted to germinate.

PC: In that work, you acknowledge conversations with David Huse and Daniel Fisher[31]. Were you following the spin glass conversation throughout these years? In what ways are you in touch with them otherwise?

[29] L. Saul and M. Kardar, "Exact integer algorithm for the two-dimensional ±J Ising spin glass," *Phys. Rev. E* **48**, R3221 (1993). https://doi.org/10.1103/PhysRevE.48.R3221; "The 2d ±J Ising spin glass: exact partition functions in polynomial time," *Nucl. Phys. B* **432**, 641-667 (1994). https://doi.org/10.1016/0550-3213(94)90037-X

[30] Lawrence Kevin Saul, *Exact computations in the statistical mechanics of disordered systems*, PhD Thesis, MIT (1994). https://mit.primo.exlibrisgroup.com/permalink/01MIT_INST/ejdckj/alma990006833880106761

[31] Daniel S. Fisher: https://en.wikipedia.org/wiki/Daniel_S._Fisher; David A. Huse: https://en.wikipedia.org/wiki/David_A._Huse

MK: [0:31:35] It was certainly through various meetings. For statistical physicists in the '80s and '90s, I think we should all acknowledge the importance of Joel Lebowitz's statistical physics meetings that would take place twice every year[32]. A lot of these debates about spin glass, replica symmetry breaking, the perspective of Huse and Fisher were constantly at the forefront of these meetings. So, if there was anything exciting and important in statistical physics, they would be represented at Joel Lebowitz's meeting. I and my students certainly owe a lot to these meetings, now in their fifth or sixth decade.

PC: You mentioned that others followed up on that work, but do you remember what was the initial reception to your results? How was it met?

MK: [0:32:57] I would say that, again, what I see as the important aspect of what we did was to introduce a technique and a method. I was myself not particularly satisfied with the results that we got, about what we could say from that method about the nature of the two-dimensional spin glass and its exponents, or the way that the heat capacity and other quantities became singular as you approach zero temperature. So, I think it was accepted as a method and something to follow up but there were no definite results in my perspective that emerged from that, and I think that was the perspective of the community.

PC: You mentioned that at the time a lot of the statistical physics community would center or at least be meeting at Rutgers. Over the 15-20 years that had elapsed since your arrival in Cambridge, had there been major changes in the statistical physics community in Cambridge proper?

MK: [0:34:25] There are constantly changes. The focus of the community is being modified as time goes on. Different people join in and go out. When I started, the statistical physics community was much closer to materials science and phase diagrams, and such things. People in metallurgy and material science would come to meetings and interact with us. At later times, it became interests shifted to disordered systems, granular materials, and then soft matter, biological physics, starting with topics that are related to proteins and now having now gone to the study of condensates etc. So, it's a constantly evolving community and it kind of branches. Some people that you interact a lot with at some point move out of your sphere. Just as an example, Barabási[33] and Stanley, who were

[32] P. Charbonneau, *History of RSB Interview: Joel L. Lebowitz*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 6 p. https://doi.org/10.34847/nkl.ad7a1tmg

[33] Albert-László Barabási: https://en.wikipedia.org/wiki/Albert-L%C3%A1szl%C3%B3_Barab%C3%A1si

working on interfacial problems, even [had] a book on that[34]. Then, later on, he has moved on to networks and network science. It is a totally different community that is looking at these complex systems that maybe has less interaction with, say, the physics of living systems community that we have at MIT that is interested in active matter, in biological systems in non-reciprocal interactions, etc. For example, in that particular case, abstraction of networks and reality of biological systems have completely fractured to communities that don't necessarily talk to each other. So, what I see as statistical physics is something that has been continuously growing and evolving. One of the things, however, that I'm a little bit worried about—this is true of MIT, and I think many other places—is that when we are looking to hiring, we don't say we want to hire somebody in statistical physics. It has to be something with either biological physics or some other branch that uses statistical physics as a tool, whereas when we are examining graduate students—our PhD requirements are the 4 courses: quantum mechanics, electrodynamics, classical mechanics and statistical mechanics—it's supposed to be at least one of four pillars of physics, yet we are not hiring necessarily people whose primary expertise is statistical physics.

PC: This naturally brings us to the next question. We know you teach statistical physics. Did you ever teach about spin glasses or replica symmetry breaking at MIT or elsewhere? If yes, can you detail?

MK: [0:38:09] Actually, no. I have not. I do teach topics that are related to critical phenomena, and it is possible that at the end of my classes I devote two or three lectures to either disordered systems or non-equilibrium dynamics, but it would be a rapid survey rather than something that is focused on giving the students the proper tools to deal with the subject.

PC: You did teach a bit about spin glasses at the Les Houches school in 1994[35]. Is that correct? Is that where you first lectured on the replica method?

MK: [0:39:05] Now, I remember. It was indeed on disordered systems with focus on directed paths. I don't know how much further it went, but probably those notes were the foundation of what appears as the final chapter of my book on *Statistical Theory of Fields*[36]. That, again, is the

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[34] A.- L. Barabási, H. E. Stanley, *Fractal Concepts in Surface Growth* (Cambridge: Cambridge University Press, 1995).
[35] M. Kardar, "Lectures on Directed Paths in Random Media," In: *Géométries fluctuantes en mécanique statistique et en théorie des champs*, F. David, P. Ginsparg and J. Zinn-Justin, eds. (Amsterdam: Elsevier, 1996).
[36] M. Kardar, *Statistical Physics of Fields* (Cambridge: Cambridge University Press, 2007); *Statistical Physics of Particles* (Cambridge: Cambridge University Press, 2007).

context that I mentioned that when I teach the class at MIT, that part, which grew out of the lectures from Les Houches would be condensed to one or two lectures at the end.

PC: So, this whole content did not initially emerge from the MIT lectures. We understand that before the pair of graduate textbooks in statistical physics we were just alluding to, which were published in 2007, there was a set of lecture notes that circulated broadly. Can you help us understand a bit what led to the genesis of these notes and the books? When did you start teaching this material at MIT?

MK: [0:40:28] I was hired at MIT in 1986. From that point onward, on and off, I have been teaching classes in statistical physics. In the initial years, Professor Nihat Berker was still around, and we would alternate teaching these classes. There are of course different perspectives. His class was more focused on position space renormalization group. But as I was there teaching my material, it was common for me to prepare by writing notes. Then, at some point, I would distribute these notes to the students. Then, there was a lot of encouragement from the students: "Why don't you make these notes, put them together into a book?" That's the origin of the books. They still, admittedly, have this character of lecture notes that have been put together. They are kind of terse. One of the strengths, however, is that, giving these classes every year, I had to develop new problems and problem sets. I think those problems are probably a strength of these books.

PC: In both these books and the original Les Houches lectures notes, you talk about the region of validity of the replica method in its provable sense, but you do not mention replica symmetry breaking itself. Is there any particular reason why the words or the concepts don't appear?

MK: [0:42:29] Maybe we should go back to Henri Orland's class, which was quite intimidating, I should say, because he was putting a lot of things together, thinking back about the facility with which he could present this material. My style of lecturing is that I should be able to go in front of the class and do the entire calculation, whatever it is, without resorting to notes. So, I should be able to keep everything in some logical sense in my mind. For whatever reason, maybe it was just because of lack of trying, I never developed that facility for presenting a replica symmetry breaking calculation from the beginning to the end. That's why I have not presented it in my lectures and hence it didn't make it into the books. But it is really a reflection of my weakness in grasping the entire picture in a manner that I would be able to present systemically within one class.

PC: From what we can tell, you've been one of the very few US-based physicists to use the replica method in your work up to the '90s, at least. Do you have any insight as to why that might be?

MK: [0:44:35] Not really. There are always conflicts between the characters of various people, and the way that they approach topics. There are trends. I can't really say why people would use one method or the other or avoid using something. It just doesn't make sense in my mind. There is some style of results and calculations that I was attracted to. For me, those results, it was interesting to look at moments of distributions and things like that, and to do things that are simple enough that I could also test them on the computer. Such, I could do with the replica method and looking at the moments. Somehow, I would say I was probably put off by the controversy regarding spin glasses and the different approaches that people had. Rather than to sort of weigh into that controversy and try to take one side or the other, I thought that there were other problems that were interesting enough that I could work on and close to my interests. To what extent that generalizes to other people in the community, I have no idea.

PC: We're nearing the end of this interview. Is there anything else you would like to share with us about this era that we may have missed or overlooked?

MK: [0:47:00] It was an exciting era, and it is continuing. I must say that I'm very impressed with all of the new results that are emerging, the work that you guys have done on the glass transition in high dimensions[37], being able to finally do somethings on the real glass as opposed to spin glass transitions. It's wonderful to see the progress that has been made in this direction. Maybe I'm close to the end of my teaching career, but one of the things I would like to do is add eventually that chapter on replica symmetry breaking to my books and the teaching, and maybe mention your works.

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

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[37] See, *e.g.*, G. Parisi, P. Urbani and F. Zamponi, *Theory of Simple Glasses: Exact Solutions in Infinite Dimensions* (Cambridge: Cambridge University Press, 2020); P. Charbonneau, J. Kurchan, G. Parisi, P. Urbani and F. Zamponi, "Glass and jamming transitions: From exact results to finite-dimensional descriptions," *Annu. Rev. Conden. Matter Phys.* **8**, 265-288 (2017). https://doi.org/10.1146/annurev-conmatphys-031016-025334

MK: [0:48:23] I may have, but I would have to go back and look at some filing cabinets that have not been opened in many years to see if there is something there or not.

PC: If ever you get around to it, I encourage you to consult the MIT libraries before discarding any of it. Thank you very much for this conversation.

MK: [0:48:50] My pleasure. Thank you for taking the time.