History of RSB Interview: Joseph A. Rudnick

February 11, 2021, 12:00pm-13:00pm (EST). Final revision: April 19, 2021

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Location:
Over Zoom, from Prof. Rudnick's home in Los Angeles, California, USA.
How to cite:
P. Charbonneau, *History of RSB Interview: Joseph A. Rudnick*, 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.ed19y09o

PC: Good morning, Prof. Rudnick. As we mentioned ahead of time, the purpose of this interview is to discuss the period during which replica symmetry breaking was developed, roughly from 1975 to 1995. To get us to that, there are a few questions on background that we would like to bring up. In your contribution to the Walter Kohn festschrift, you described how and why you chose to become a theoretical physicist[1]. Given that your father was himself a physicist[2], was it even an option for you not to become a physicist?

JR: [0:00:41] That's a very good question. Certainly, if I had discovered I was no good at it, I wouldn't have done it. There was this strong feeling: physics was almost a religion in my family. In fact, my brother once commented that I was the one who went into the family business. I actually remember that… I envy people who went into physics because they were so drawn to the subject. For me, there was a very strong feeling that, not that it was expected of me, but that it was a way of fulfilling some kind of destiny, or I don't know what.

PC: After your PhD, you had a number of postdocs: at the University of Washington, at Technion, and at Case Western Reserve. During those postdocs, you seem to have had a fairly high degree of autonomy in choosing your

[1] J. Rudnick, "It Started with Image Charges" in *Walter Kohn: Personal Stories and Anecdotes Told by Friends and Collaborators* eds. Matthias Scheffler and Peter Weinberger (Berlin: Springer-Verlag, 2003), 208-210.
[2] S. Putterman and S. Garrett, "Isadore Rudnick," *Phys. Today* **52**(2), 80 (1999). https://doi.org/10.1063/1.2802757

problems, in choosing your questions of interest. Can you tell us a bit what guided your problem selection at that time?

JR: [0:02:39] I kind of fell into some of them. Actually, my first postdoc was, to me, a tremendous disappointment. I figured it would end my career in physics. If you look at my publications, my publication rate was pretty sparse in my three years at the University of Washington. Part of it was that I and the person for whom I was a postdoc had different ideas about what was important. I worked with him specifically on something that obsessed him that I felt was not worth all that much attention. I was not that good at actually deciding what I should be doing myself. So I published one paper based on my dissertation[3]; I wrote another paper and referees criticized it, and I didn't have the self-confidence to revise and resubmit; and then I published a couple of papers with him[4].

The second [postdoc], at Technion, was with people I had met much earlier, in 1965, when my father was on sabbatical in Israel and worked at the Technion. They were kind of friends of the family. When I went there, they pretty much told me I could do whatever I was interested in. Actually, on my way to Israel [in] 1972, my father was also on sabbatical, in France—I think he was at [Orsay], he was wherever de Gennes[5] was—[and I stopped over]. When I got there, somebody—I can't remember who—pointed me towards this article by Kenneth Wilson that had just come out[6]. His first paper on the renormalization group. I remember trying to read it and finding it completely impenetrable. Then I went to Israel and they kind of told me I could do whatever I wanted, but one of them said: "Why don't you read that paper and tell us about it because this seems to be a hot new topic." So that's what I did. I seriously read the paper, and then I had some ideas of my own. That started me working on the renormalization group. I was fortunate enough to write a couple of papers that were noticed[7].

At the end of that I was looking for jobs back in the United States. I got two offers. One of them was a faculty position at Tufts University and the other

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[3] J. Rudnick, "Static screening by a bounded electron gas," *Phys. Rev. B* **5**, 2863 (1972). https://doi.org/10.1103/PhysRevB.5.2863

[4] J. Rudnick and E. A. Stern, "Second-harmonic radiation from metal surfaces," *Phys. Rev. B* **4**, 4274 (1971). https://doi.org/10.1103/PhysRevB.4.4274; "Self-Consistent Screening in a Simple Model," *Phys. Rev. B* **7**, 5062 (1973). https://doi.org/10.1103/PhysRevB.7.5062

[5] Pierre-Gilles de Gennes: https://en.wikipedia.org/wiki/Pierre-Gilles_de_Gennes

[6] K. G. Wilson, "Renormalization group and critical phenomena. I. Renormalization group and the Kadanoff scaling picture," *Phys. Rev. B* **4**, 3174 (1971). https://doi.org/10.1103/PhysRevB.4.3174

[7] See, *e.g.*, J. Rudnick, "ϵ expansion for the free energy of the continuous three-state Potts model: evidence for a first-order transition," *J. Phys. A* **8**, 1125 (1975). https://doi.org/10.1088/0305-4470/8/7/015; J. Rudnick, D. J. Bergman and Y. Imry, "Renormalization group analysis of a constrained Ising model," *Phys. Lett. A* **46**, 449-450 (1974). https://doi.org/10.1016/0375-9601(74)90959-1

one was a postdoc at Case Western. I was told—I can't remember if it was in writing or in a phone call—that [Case Western] would do their best to find me a faculty position. There wasn't an opening at the time, but they would try to find me one. Somebody I just talked to told me: “Oh, yes you should go to Case Western, because who has heard of Tufts?” I went to Case Western. That was not a smart decision. What had happened there is the departments at the Case Institute of Technology and Western Reserve College—they were institutions across the street from each other in Cleveland—merged. As it turned out, that involved the merger of two full-size physics departments. What followed was described to me by one of the 18 assistant professors at the time who actually survived the process that was known as “The Purge years". They basically lost, or didn't give tenure to, or fired 17 of the 18 assistant professors. There was a lot of pressure then not to grow, so I never got the promised offer.

I had already started working on RG, and that's what I continued doing there. I just fell into the interest in spin glasses. That actually came about when I took a small leave to UC San Diego and worked with Shang Ma[8]. Together, but he really drove that collaboration. We came up with a model for spin glass dynamics[9]. That's how I got into spin glasses.

PC: So that's the first time you heard about spin glasses?

JR: [0:08:19] Yeah, it’s the first time I heard about spin glasses. I read the papers of Edwards-Anderson [and] Sherrington-Kirkpatrick.[10]

PC: I think you then went on a summer leave to IBM as well. Is that possible?

JR: I was paid [only] for the academic year, so I didn't get paid during the summer. I was invited to just go to IBM. I did that for a few years. I worked there in the summer.

PC: And that’s where you met Erling Pytte[11]?

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[8] J. C. Y. Chen, J. Prentis and S. Schultz, “Shang-keng Ma,” *Phys. Today* **37**(4), 102 (1984). https://doi.org/10.1063/1.2916178

[9] S.-k. Ma and J. Rudnick, "Time-dependent Ginzburg-Landau model of the spin-glass phase," *Phys. Rev. Lett.* **40**, 589 (1978). https://doi.org/10.1103/PhysRevLett.40.589 See also: Sitges International School on Statistical Mechanics, June 1976, Sitges, Spain, directed by L. Garrido: S.-k. Ma, "Scale transformations in dynamic models," *Lecture Notes in Physics* **54,** 43-78 (1976). https://doi.org/10.1007/BFb0034505

[10] S. F. Edwards and P. W. Anderson, “Theory of spin glasses” *J. Phys. F* **5,** 965-74 (1975). https://doi.org/10.1088/0305-4608/5/5/017; 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] See, *e.g.*, Erling Pytte, “Spin phonon interactions in a Heisenberg ferromagnet,” PhD Thesis, Harvard University (1964). http://id.lib.harvard.edu/alma/990038987280203941/catalog

JR: [0:09:06] Yes. That's where we met.

PC: Can you tell us a bit about the genesis of that collaboration, of that meeting, and how you decided to work together?

JR: I had already started working with Shang Ma. To tell the truth, that's one of the cases where I don't know how it is that we got seriously involved in it together. It just started. I supposed we had mutual interests. I don't think he had done any work on spin glasses prior to that. He was kind of the manager of the research group. He was interested in a lot of things. I think that our first work was called "Scaling, equation of state, …"[12]. I think that's one of the works which I'm happy about, because we worked out some things.

PC: The first work is actually with Robert Pelcovits, a Harvard student[13].

JR: Oh, really? Is that the one on the random anisotropy model?

PC: Yes. The title is "Spin-glass and ferromagnetic behavior induced by random uniaxial anisotropy"[14].

JR: Ok. I didn't realize that this was the first one.

PC: Do you know where Shang Ma got his interest in spin glasses? Where did that emerge?

JR: Actually, I don't know whether it was he or I that first brought it up to tell you the truth. It's often hard to tell what starts a collaboration. It often starts with a cup of coffee.

PC: You told us a bit about the paper with Pytte, and how that came about. Did you have a particular intuition about the instability of the replica symmetry solution, or were you just applying RG, or applying the tools you already had? Do you know what led to this particular direction being chosen?

JR: [0:11:39] In the case of the random anisotropy model, I think that was in part me of the three of us. We were looking at random anisotropies. Of

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[12] E. Pytte and J. Rudnick "Scaling, equation of state, and the instability of the spin-glass phase," *Phys. Rev. B* **19**, 3603 (1979). https://doi.org/10.1103/PhysRevB.19.3603

[13] Bob Pelcovits as a graduate student with Bert Halperin. See, *e.g.*, Robert Alan Pelcovits, "Phase transitions in two-dimensional systems and disordered magnets," PhD Thesis, Harvard University (1978). http://id.lib.harvard.edu/alma/990038989480203941/catalog

[14] 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

course, at the time, one of the things that you naturally did when you were looking at statistical mechanical systems is to go to the $n \to \infty$ limit. Because it is well-established that in some cases the $O(n)$ model for which $n \to \infty$ limit was the spherical model. There was the famous paper by Stanley[15]. So there's sometimes maybe the hope that in that limit things would simplify. If they did, then maybe you could think of doing a $1/n$ expansion, and perturbatively build on that simplification. I can't remember how random… Random anisotropy was in the literature, and I think that I had looked at the $n \to \infty$ limit, and I realized that if you found the diagrams that dominate in the $n \to \infty$ limit, you would end up with a model in which there is no negative gap. You don't have the de Almeida-Thouless instability. So I think that's what gave rise to that collaboration. However, much of the key work in the paper was done by Bob Pelcovits, certainly the 4+ε expansion, possibly the 6-ε as well. We worked together to try to understand that better. The hope was, at least for me, that now that we can go to $1/n$ and somehow—that being controllable—we could maybe deal with it perturbatively. I have to admit that there came a point where I felt like: "No, this is not working out neatly the way I'd hoped." And I dropped it. Yadin Goldschmidt took it up, of course[16].

PC: Before we get to that, in some of your papers, especially the 1980 one, you mentioned special discussions with Cirano De Dominicis[17]. What was your relationship with the Paris group? How did you interact with the spin glass community in Europe in those years?

JR: [0:14:23] I'm trying to remember. So it was already '81 when I talked to him?

PC: No. Sorry. It's the 1980 paper "Mean-field theory of spin-glasses"[18].

JR: You have to remind me of that…

[15] H. E. Stanley, "Spherical model as the limit of infinite spin dimensionality," *Phys. Rev.* **176**, 718 (1968). https://doi.org/10.1103/PhysRev.176.718

[16] Y. Y. Goldschmidt, "Magnets with random uniaxial anisotropy: Thermodynamic properties in the large-*N* limit," *Nucl. Phys. B* **225**, 123-124 (1983). https://doi.org/10.1016/0550-3213(83)90015-9; "1/*N* expansion in the random anisotropy model: A solution with replica-symmetry breaking," *Phys. Rev. B* **30**, 1632 (1984). https://doi.org/10.1103/PhysRevB.30.1632

[17] C. De Dominicis: https://de.wikipedia.org/wiki/Cyrano_de_Dominicis

[18] J. Rudnick, "Mean-field theory of spin-glasses," *Phys. Rev. B* **22**, 3356 (1980). https://doi.org/10.1103/PhysRevB.22.3356

FZ: This is the paper in which you studied the Sherrington-Kirkpatrick model using a kind of diagrammatic expansion, and you reproduced the de Almeida-Thouless instability. Then you discussed the fact that you cannot [describe] the spin glass phase in terms of a single order parameter.

JR: Is that the one with Erling Pytte?

PC: It's you on your own.

FZ: It's just you.

JR: [0:15:19] Oh! I'm embarrassed. I don't even remember that paper.

FZ: It's a nice paper.

JR: Thank you. I'm trying to remember that paper… 1980, which means I was at Santa Cruz. Did I write that while I was at Yorktown Heights? Is there an affiliation there with IBM listed? I would have to go back and look at that paper. I'm very embarrassed that I don't remember what that was.

PC: Your affiliation is Santa Cruz on that paper and there's no other.

JR: Wow. If possible, can we defer conversation of that? I will look it up and refresh my memory.

PC: You can also annotate the transcription, in writing.

JR: Ok. I'll be happy to do that. I'll look it up, and I'll read it[19]. I'm trying to remember it. The one that I remember was with Erling, where we actually traced down the source of the instability to a single term in the partition function in terms of the *Q* matrix. We could verify that it all depended on the sign of a coefficient, and we knew what that sign had to be. The thing I also was pleased by is that there was discussion at the time that maybe this negative gap was a mathematical nuisance, but that it may not have signaled some kind of inherent pathology in the replica symmetric solution. The other thing we showed is that a negative value for that gap was inextricable from a negative value for the replica average of the square of a real quantity. So it was absolutely wrong. It was intolerable. Then, we tried tracing it back to the non-Gaussian statistics of these spins. I liked that because that was—at least for me—unassailable.

[19] **JR:** I've looked up the paper and am embarrassed that I forgot it. In retrospect I think it was a nice piece of work.

PC: When did you become aware of the Parisi solution? And how did you react to it?

JR: [0:18:24] Along with most people: when it was published, and then talked about broadly. I remember the form of the solution, the ultrametricity, how he structured his Q matrix. I thought it was very clever, but something at this point I didn't know… I think he did some very important work linking that to things that you could calculate without replicas. I never felt like I had enough of an intuitive grasp of it to work with it. I just said [to myself]: "This is very nice, and maybe it's the solution, but there's no way I could see myself contributing."

PC: Is that how you left the field? It roughly coincides with that reaction…

JR: [0:19:31] I did work afterwards. I did the work[20] with Anuradha Jagannathan[21] and a graduate student of mine, Sakkar Eva[22]. Anu and I [also] wrote a paper looking more closely at the structure of the spherical model spin glass[23]. Then we just noticed this funny little feature of the Bose-Einstein condensation that you could translate into Parisi-like structure of the Q matrix. I thought maybe you could look at that further. I wrote a subsequent paper many years later but, I don't think it really led anywhere useful[24].

PC: This work came about 10 years later. Were you following the discussion in around replica symmetry breaking throughout, even from a distance? Is that how you could see the connection?

JR: [0:20:28] No. At that point I tried but I just never felt like I… You know that Cirano and maybe [Imre Kondor]—I'm not quite sure who his collaborator was then—looked at the first fluctuation correction to the mean-field theory in the Parisi ansatz[25]. That work was impressively intimidating. Again, I

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[20] A. Jagannathan, S. Eva and J. Rudnick, "Parisi-like order parameter in the spherical model of a spin glass," *J. Phys. A* **24**, 2193 (1991). https://doi.org/10.1088/0305-4470/24/9/025

[21] Anuradha Jagannathan did her PhD with Michael Kosterlitz (1982-1985) at Brown and was later a post-doc with Ray Orbach (1987-1989) at UCLA. See, *e.g.*, Anuradha Jagannathan, "A 1/n Expansion for the Random Anisotropy Model," PhD Thesis, Brown University (1986). https://search.library.brown.edu/catalog/b1238221

[22] Sakkar Ara Eva, "Accomodation and void distribution in the random walk," PhD Thesis, UCLA (1990). https://catalog.library.ucla.edu/vwebv/holdingsInfo?bibId=1289783

[23] A. Jagannathan and J. Rudnick, "The spherical model for spin glasses revisited," *J. Phys. A* **22**, 5131 (1989). https://doi.org/10.1088/0305-4470/22/23/017

[24] S. Akhanjee and J. Rudnick, "Spherical Spin-Glass–Coulomb-Gas Duality: Solution beyond Mean-Field Theory," *Phys. Rev. Lett.* **105**, 047206 (2010). https://doi.org/10.1103/PhysRevLett.105.047206

[25] C. De Dominicis and I. Kondor, "Eigenvalues of the stability matrix for Parisi solution of the long-range spin-glass," *Phys. Rev. B* **27**, 606 (1983). https://doi.org/10.1103/PhysRevB.27.606

felt like I didn't have any insight, and maybe I was a bit too lazy to try to reproduce their calculations, which were quite elaborate.

PC: Absolutely. Did that experience working in spin glass-related topics influence the rest of your work in theoretical physics? Or is there a before and an after, and they are completely disjoint?

JR: [0:21:45] They're not completely disjoint, but I don't know that I can trace any connection. I think an experiment is useful even when it's not necessarily productive. I think one of the things I can say is that I discovered some of my limitations. I just didn't have either the mathematical background nor the intuitive insight to be able to work with something where I just didn't… Parisi, you know, is brilliant to the point of genius, and I knew I was not at that level.

PC: As you left UC Santa Cruz and moved to UCLA, Santa Cruz recruited Peter Young to come in.

JR: This was a brilliant hire.

PC: Were you at all related to his recruitment or to that effort?

JR: [0:22:55] No. I had gone to UCLA. They made an offer to Peter Young and he accepted, and I congratulated them on it. I think I told Mike Nauenberg[26], whom I still communicated with, that they were incredibly fortunate because I was aware of his work on two-dimensional melting. The fact is that he single-handedly was producing work of the same quality and impact as David Nelson and Bert Halperin working together. I was extraordinarily impressed by him.

PC: As you got to UCLA, you quickly became department chair, and then you served a second term later on, and then became dean. So you played a pretty significant administrative role there. Was there at any point a chance for you to influence the recruitment of spin glass or RSB-related physicists on the team?

JR: [0:24:06] Both as chair and… Well, I suppose I certainly played a role in the recruitment of Sudip Chakravarty[27]. He had done very important work using high-temperature expansions on the nature of spin glasses and on the

[26] Michael Nauenberg: https://en.wikipedia.org/wiki/Michael_Nauenberg
[27] Sudip Chakravorty: https://scholar.google.com/citations?user=yGDh0rIAAAAJ

existence of a spin glass transition[28]. I was very much involved in that [recruitment in 1987-1988], but I think in that case it was just that we were just looking to hire in that area and by very fortunate circumstances both Sudip and Steve Kivelson[29], who were then at Stony Brook applied for the position. It was a single position, but we had a very sympathetic administrator in Ray Orbach[30], who of course had experimental interest in spin glasses. He pretty much saw to it that not only were we allowed to make two offers, [but] he allowed us to make three offers. The third one to Dan Arovas didn't succeed, but both Sudip and Steve came. But I don't think that it was programmatically driven. We got Sudip and Steve, because C.N. Yang had an Institute for theoretical physics at Stony Brook[31], and for reasons that I cannot fathom Sudip and Steve were not offered membership in that Institute. I think with good reason they both felt that that was an unjustified snub on them. They were at least as good as the theorists that were in that institute. In fact, they were both offered membership at the point they had UCLA offers. I think at that point the damage has been done.

PC: You mentioned Bert Halperin[32] and David Nelson[33] earlier. During your work on spin glasses you were in relatively close contact with them. You had just collaborated with David Nelson, and Robert Pelcovits was a student of Bert Halperin. Were they encouraging of these efforts in spin glasses either for you or for Robert? Or were they largely indifferent? What was your impression?

JR: [0:26:43] I actually got to know Dave Nelson when he was still—I don't know if he was still a grad student of if he had just gotten his degree—at Cornell[34]. I was actually trying to reach Michael Fisher[35], because I had been in a kind of correspondence with Michael. I can't remember why I wanted to talk to him, but I called him up and Dave Nelson answered the phone. Apparently, Michael's secretary put David Nelson on. We just started to talk. I had never heard of him, and we started to talk. Our mutual

[28] See, *e.g.*, R. R. P. Singh and S. Chakravarty, "High-temperature series expansion for spin glasses. I. Derivation of the series," *Phys. Rev. B* **36**, 546 (1988). https://doi.org/10.1103/PhysRevB.36.546; "High-temperature series expansion for spin glasses. II. Analysis of the series," *Phys. Rev. B* **36**, 559 (1988). https://doi.org/10.1103/PhysRevB.36.559
[29] Steven Kivelson: https://en.wikipedia.org/wiki/Steven_Kivelson
[30] Raymond L. Orbach: https://en.wikipedia.org/wiki/Raymond_L._Orbach
[31] C. N. Yang Institute for Theoretical Physics: https://en.wikipedia.org/wiki/C._N._Yang_Institute_for_Theoretical_Physics
[32] Bertrand Halperin: https://en.wikipedia.org/wiki/Bertrand_Halperin
[33] David R. Nelson: https://en.wikipedia.org/wiki/David_Robert_Nelson
[34] David Robert Nelson, "Applications of the Renormalization Group to Critical Phenomena," PhD Thesis, Cornell University (1975). https://newcatalog.library.cornell.edu/catalog/44045
[35] Michael Fisher: https://en.wikipedia.org/wiki/Michael_Fisher

interest in RG just led to further discussions and collaboration. I do remember a couple of things about him. First, he had all this energy. For reasons of prejudice, I assumed that he was not very tall, because I associated that kind of energy with shorter people. Of course, I met him and he was quite tall. He was not at all thin, and I always associated that with thin people. This was a stupid prejudice on my part. The other thing I remember about Dave Nelson is that after spending time with him, my notion of who is good and who is not good was completely out of whack. I would then be asked to write letters of recommendation for people, and I would think of somebody: "Yeah, he's good, but he's not Dave Nelson." I would write this kind of measured letter for some incredibly good people because they were not David Nelson. My calibration was subsequently adjusted.

PC: Did Bert encourage his student to work on spin glasses with you, or was this completely an accident?

JR: [0:29:13] That I don't know. You would have to talk to him. I don't know what conversations he might have had with Bert.

PC: I'm asking because your paper with him thanks both Bert and David[36]. That's why I was curious.

JR: [0:29:41] I would have thanked David, and that would have probably been Bob talking to Bert.

PC: Could you help us contextualize the US response to replica symmetry breaking. I know it's a big ask, because there's not a uniform response, but what was your impression of the reception of those ideas in American physics?

JR: [0:30:15] I think they were incredibly influential. I think anybody who was working on spin glasses would have known about them and taken them into account. In the end, I think it was absolutely clear that it had to be broken, that the replica symmetric model had an inherent pathology. I can imagine if there was anybody who would have insisted on unbroken replica symmetry, certainly nobody that I knew of. I think the first people who introduced it were Bray and Moore[37], as I recall, the one step symmetry

[36] "Helpful discussions with TC Lubensky, S-K Ma, DR Nelson, M Wortis, S Kirkpatrick, R Alben, JMD Coey, S von Molnar are gratefully acknowledged." In: 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] A. J. Bray and M. A. Moore, "Replica-Symmetry Breaking in Spin-Glass Theories," *Phys. Rev. Lett.* **41**, 1068 (1978). https://doi.org/10.1103/PhysRevLett.41.1068

breaking. Then I think there were other versions, and then the Parisi ansatz seemed to be one you could settle on.

Ok, maybe not uniformly, because of the Fisher and Huse droplet approach[38]. Part of this whole notion of what was going on with the replica model is that there was this very elaborate multi-dimensional free energy landscape. If you're looking for ground states, it was something… I know that Scott Kirkpatrick did a lot of work on this with simulated annealing being invented to look for that[39]. They actually proposed an alternative model in which there was a single ground state. So I would say that it was not entirely universal. That was an alternate claim. I know that Peter Young did a lot of good simulations to try to see if you can find evidence for one or the other. Unfortunately, I can't tell you—you may know –what conclusion he came to. Can you tell me which was ultimately decided?

PC: We can discuss this after the interview. In the proceedings for Ray Orbach's inauguration, you wrote a [pedagogical] perspective on the replica trick[40]. Did you ever get to teach a class at Santa Cruz, UCLA, or elsewhere about the replica trick and replica symmetry breaking for spin glasses?

JR: [0:33:02] Yeah, I did. I taught a class. It was an advanced graduate course called many-body physics. I did teach a class and I did introduce that.

PC: At UCLA?

JR: At UCLA, yes.

PC: Could you give us details of what it included, and when this took place?

JR: [0:33:22] It would have been in my early years there. It would have been in the mid-to-late '80s or early '90s. Actually, it was a class where I used the book by Parisi[41]. I kind of talked about some of those things. I actually felt like he was like many overwhelmingly brilliant people. When he writes he assumes that you can make connections as fast as he did. I remember spending several lectures going over three pages of his book.

[38] 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
[39] S. Kirkpatrick, C. D. Gelatt and M. P. Vecchi, "Optimization by simulated annealing," *Science* **220**, 671-690 (1983). https://doi.org/10.1126/science.220.4598.671
[40] J. Rudnick, "Percolation and the Mysteries of Replication," in *Random Magnetism, High Temperature Superconductivity: Proceedings Of T Raymond L Orbach Inauguration Symposium* (Singapore: World Scientific, 1994), 59-76.
[41] M. Mézard, G. Parisi, M. A. Virasoro, *Spin Glass Theory and Beyond* (Singapore: World Scientific, 1987).

PC: So you did eventually work through some of the math of the replica trick yourself.

JR: [0:34:18] Oh, yes, I did. Of course I did. I just never felt that I had sufficient insight to do anything research-wise beyond reproduce what he had.

PC: Is there anything else about this epoch or these times that we may have skipped over and that you think we should be discussing?

JR: [0:35:06] I mean I do remember that there was a lecture by Mark Kac[42] that I listened to entitled: "When is an average not an average?" This was a time in which the kind of model building really based on Landau's approach—especially as elaborated by de Gennes—felt like if you came up with a good enough model you would have a straightforward path to understanding a system. It's kind of when we were brought up short that some of these tricks, especially these ways of averaging random systems with the use of replicas, that it was found you could have a model that seemed mathematically perfectly justified that just did not yield to analysis—at least the kind of which most are theoreticians are capable. I think that was an important lesson, again, just learning our limitations. Aside from that, I can't think of anything else that was a major issue of controversy or scandal. Maybe you can remind me of some.

PC: There need not be any. Francesco, is there anything else you would like to ask?

FZ: The Fisher-Huse droplet approach, I think, was based on the idea of real space renormalization group and of the infinite randomness fixed point. You worked on a different RG where you do an epsilon expansion around mean-field. Was there any discussion during the '80s between people in the US in trying to make contact between the two approaches and trying to understand a little bit better each other? There were two sides: the real space-strong disorder and the field theory epsilon expansion.

JR: [0:37:51] Ok. Now, I have to admit I never tried any calculations using that. I was never part of any discussion, so I can't comment on that.

FZ: So you never discussed directly with Daniel Fisher or David Huse about any of this?

JR: No, I never talked to them about it.

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[42] M. Niss, "A Mathematician Doing Physics: Mark Kac's Work on the Modeling of Phase Transitions," *Perspectives on Science* **26**, 186-212 (2018). https://doi.org/10.1162/POSC_a_00272

PC: As a final question, 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 in the future?

JR: [0:38:33] Actually, I think I do. Somewhere in the floor of a file cabinet there's a couple of folders worth. I never thought to do that. I don't think I was considered historically important enough. The American Physical Society, if they decide that somebody is significant enough, will send an archivist to that person's office at some point, go through all their papers and decide which ones should be kept and which ones can be [discarded].

PC: So can UCLA.

JR: [0:39:12] They haven't. I haven't heard from UCLA, if there were an archive that I could contact. Do you know of one?

PC: I encourage you to contact UCLA manuscripts and archives and initiate a conversation. Shang Ma's archives, for instance, are at UCSD[43]. He deposited his papers—or they were moved there—in the '80s. You having served as administrator as well, there are some university-related material that they might be happy to have. That might just get lumped into a larger ensemble.

JR: [0:39:55] Ok. I'll reach out. I'll see. It'll probably have to wait until the end of the pandemic because we'd have to get together in my office, but I'd be happy to do that.

PC: Thank you very much for your time.

JR: Thank you. It's been fun.

[43] Shang-keng Ma Papers (1966-1983), Special Collections & Archives, UC San Diego. https://oac.cdlib.org/findaid/ark:/13030/tf4p3006wn/entire_text/ (Accessed April 1, 2021).