History of RSB Interview: Kurt Binder

December 9, 2020, 8:15-9:30am (EST). Final revision: December 30, 2020

Interviewers:
Patrick Charbonneau, Duke University, patrick.charbonneau@duke.edu
Francesco Zamponi, ENS-Paris
Location:
Over Zoom, from Prof. Binder's home in Nieder-olm, Germany.
How to cite:
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

PC: First of all, thank you very much for agreeing to sit down with us for this interview. As we've discussed, the purpose of this interview is mostly to talk about the period during which replica symmetry breaking was formulated, from roughly 1975 to 1995. But to get to this, we have a few background questions that will get us to this epoch. In particular, I've read Michel Mareschal's recent profile about you[1]. From this piece, we understand that in the early seventies you were particularly interested in the interplay between renormalization group and numerical simulations in statistical physics. Actually, that was your main scientific thrust at that point. Can you describe how you then became aware and kept abreast of the theoretical developments? How these conversations and insights came about?

KB: You mean the critical phenomena and renormalization group?

PC: Yes.

KB: [0:01:21] I became aware [of it] in my thesis [years], already, when my thesis advisor—he was an experimentalist, Helmut Rauch[2]– made some experiments with polarized neutrons. He let a beam of polarized neutrons pass through a ferromagnet, and these polarized neutrons experienced a change of spin orientation. He heated the ferromagnet, and noticed that when the ferromagnet passes through the Curie temperature there was a strong anomaly in the signal. He asked that, in my thesis, I try to find out about what was a cause for this phenomenon. I then started to learn about

[1] M. Mareschal, "From Varenna (1970) to Como (1995): Kurt Binder's long walk in the land of criticality." *Eur. Phys. J. H* **44**, 161–179 (2019). https://doi.org/10.1140/epjh/e2019-100016-3
[2] Helmut Rauch : https://en.wikipedia.org/wiki/Helmut_Rauch

critical phenomena in 1967 from articles that had just appeared that year. There was a famous article by Kadanoff *et al*. in Reviews of Modern Physics[3] and by Michael Fisher in Reports on Progress in Physics[4]. [From those,] I found a lot of the original literature, and so I discovered that it was really a boiling field with very interesting questions. Then, I thought about how I could do something—finding out about spin correlations in a ferromagnet—which might be relevant for these experiments, at least when one thinks about the small angle scattering of neutrons. This is not the same as the measurement which was done, but it was related to it. My PhD advisor was quite happy about the idea that I should look into this problem in more detail. He was expecting that there would be something later for the experiments too. In the end, I contributed nothing to explain his particular experiment, which was in too small of a reactor anyway, because one needs a lot of intensity for neutron scattering, and in the direct beam, where we have the direct neutrons and the scattered neutrons intermix, you can hardly interpret [the results] in any quantitative manner. So the experiment did not really profit, but I had a nice topic for my PhD. I [first] tried to do some systematic high-temperature series expansion[5]. For real ferromagnets you have to do this with Heisenberg spins, and not with Ising spins, but I noticed then that it was extremely difficult to go to higher order and I looked for something else [to do]. I heard about the Monte Carlo method, and I started doing some of the Carlo simulations on spin correlations in Ising and Heisenberg classical ferromagnets. I was, at that time, the first to try to calculate such spin correlation functions with that method. We got the first publication already one year later, in 1968[6]. In 1969, I finished my PhD[7]. About the state-of-the-art, I actually learned about [it] in 1970, when I had the opportunity to attend this famous summer school at Lake Como[8]. These are the sort of things which are also talked about in this exposé which Michel Mareschal wrote.

Then I was working for a couple of years on phase transitions, most prominently in magnetic systems. In this context, I heard, for the first time in

[3] Leo P. Kadanoff *et al.*, "Static Phenomena Near Critical Points: Theory and Experiment," *Rev. Mod. Phys.* **39**, 395 (1967). https://doi.org/10.1103/RevModPhys.39.395

[4] Michael E. Fisher, "The theory of equilibrium critical phenomena." *Rep. Prog. Phys.* **30**, 615 (1967). https://doi.org/10.1088/0034-4885/30/2/306

[5] K. Binder, "High Temperature Expansions of Spin Correlation Functions for Ising and Heisenberg Ferromagnets," *Phys. Status Solidi B* **32**, 891-903 (1969). https://doi.org/10.1002/pssb.19690320243

[6] K. Binder and H. Rauch, "Calculation of spin-correlation functions in a ferromagnet with a Monte Carlo method," *Phys. Lett. A* **27**, 247-248 (1968). https://doi.org/10.1016/0375-9601(68)91119-5

[7] K. Binder. *Berechnung Der Spinkorrelationsfunktionen Von Ferromagnetika*. PhD Thesis, Technische Hochschule Wien (1969). https://catalogplus.tuwien.at/permalink/f/qknpf/UTW_alma2143892790003336

[8] Enrico Fermi International School of Physics , Course LI: Critical Phenomena, July 27-August 8, 1970. *Proceedings of the International School of Physics « Enrico Fermi», Course LI*, M. S. Green, editor (New York: Academic Press, 1971).

1972, about the strange experiments from the group of Mydosh and coworkers[9], on a cusp in the magnetic susceptibility in alloys such as copper with a few parts of a percent manganese and related materials. Also, at Jülich, they had done experiments on europium sulfide—a ferromagnet with nearest-neighbor ferromagnetic exchange and with next-nearest-neighbor antiferromagnetic exchange—and diluting this with non-magnetic strontium ions in order to produce a spin glass, as this was called[10]. Originally, I felt that this was a rather strange phenomenon. I did not start working on this immediately, but when I was six months…

PC: We will get there**.** I just wanted to take a step back. In the early 1970s, when you were trying to understand finite-size scaling, or starting to understand finite-size scaling, were you finding out about the work of Ken Wilson[11] mostly from reading the literature, or were you attending meetings?

KB: [0:07:42] No. The interest in finite-size scaling came about first in a very naïve way. The computers were very slow. We started on a lattice 6 x 6 x 6, and of course you find that everything in the vicinity of the Curie temperature is very much smeared out. Thus, the next thing to try was to do a 8 x 8 x 8 lattice and then you see it was already sharpening up somewhat. The largest data that we sent in this publication of 1969 is [for a] 10 x 10 x 10 [lattice]. This was really coarse, and then you read in the literature that the thermodynamics deals with the thermodynamic limit, by taking the number of spins to infinity. So it was immediately clear that one had to extrapolate to the thermodynamic limit. When I attended this Fermi summer school, Michael Fisher[12] was—in his lecture—talking about this effect in terms of what happened to the specific heat peak of Ising lattices in two dimensions. There you can, with the transfer matrix method, calculate the surrounding of the singularity exactly. So he made some phenomenological extension of how one could understand this. I don't know whether he really said this at the conference, but in his write-up one year later there was a statement that the linear dimension has to be compared with the correlation length.

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[9] V. Cannella and J. A. Mydosh. “Magnetic Ordering in Gold-Iron Alloys,” *Phys. Rev. B* **6**, 4220 (1972). https://doi.org/10.1103/PhysRevB.6.4220

[10] H. Maletta and P. Convert, “Onset of Ferromagnetism in $Eu_xSr_{1-x}S$ near x=0.5,” *Phys. Rev. Lett.* **42**, 108 (1979). https://doi.org/10.1103/PhysRevLett.42.108

[11] Ken Wilson: https://en.wikipedia.org/wiki/Kenneth_G._Wilson

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

At this Varenna summer school, there were some lectures on renormalization group methods given by Jona-Lasinio[13] from Rome, but this was extremely abstract. The connection of how this could help anything [having to do] with critical phenomena was completely unclear. In this conference, nobody—and there were very clever people, like Michael Fisher, Leo Kadanoff[14], Pierre Hohenberg, and so on—knew about Ken Wilson's work. This was at this time top secret. Ken Wilson knew why he didn't talk about it before he published something. This way his series of papers in Phys. Rev. B[15] really existed before he started to collaborate with Michael Fisher on this epsilon expansion, entitled "Critical exponents in 3.99 dimensions"[16]. This came really afterwards. So the finite-size scaling was completely independent and earlier than the renormalization group. Of course, when the renormalization group existed, this was a very nice way of interpreting why this should work. This came together confluently later.

PC: You mentioned earlier the work of Canella and Mydosh that was published 1972. In notes that you sent us, you wrote that you first heard of this work at a conference. Is it possible that it was the International Conference in Magnetism in Moscow in 1973[17]?

KB: [0:11:39] That is very likely. I was attending this conference. At this conference, I had also seen Michael Fisher again and had conversations with him about finite-size effects and so on. Spin glasses did not make a major role at this conference. There were some talks being held about spin glasses in other conferences at the time, but this was not such that it created an extreme excitement.

PC: So when you heard that talk it was just one of many talks. It didn't stand out as particularly interesting.

KB: [0:12:26] Yes. The thing which I found particularly interesting and which I later felt I would need to work on was the talk which was given by Phil Anderson during the stay which I had in 1974 at Bell Labs. He [then] made

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[13] Giovanni Jona-Lasinio : https://en.wikipedia.org/wiki/Giovanni_Jona-Lasinio

[14] Leo Kadanoff: https://en.wikipedia.org/wiki/Leo_Kadanoff

[15] 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; "Renormalization Group and Critical Phenomena. II. Phase-Space Cell Analysis of Critical Behavior," *Phys. Rev. B* **4**, 3184 (1971). https://doi.org/10.1103/PhysRevB.4.3184

[16] K. G. Wilson and M. E. Fisher. "Critical exponents in 3.99 dimensions," *Phys. Rev. Lett.* **28**, 240 (1972). https://doi.org/10.1103/PhysRevLett.28.240

[17] International Conference of Magnetism ICM-73, 22-28 August, 1973, Moscow, USSR: *Proceedings of the International Conference on Magnetism ICM-73,* 6 vols (Moscow : Nauka, 1974).

[the] approximation that is was not important in the treatment of the conduction electrons of the material, to take this Ruderman–Kittel interaction, which is oscillating and decaying with [the inverse of] the third power of distance[18]. He said that it was just important that there is competition between antiferromagnetic and ferromagnetic exchange, and you can assume that it's just a Gaussian distribution of interactions. For this, he made a mean-field approximation. When I heard this, I got the immediate impression: "Well, maybe what he predicts is an artifact of the mean-field approximation and that is a strange kind of phase transition which one could also study by Monte Carlo." So when I returned to Germany in the fall 1974, one of the first students which I found to work with me on a diploma thesis was Mister [Klaus] Schröder. I showed him my Monte Carlo code, and told him what he had to do in order to modify it to run a spin glass. He ran spin glass simulations. You know the physics of spin classes is one of frustration. This student got extremely frustrated, because he realized that when he repeated the run he got different results. One needed to repeat and repeat again, and increase the computing time, and still the results were not very convincing. But that is somehow the physics of the problem from the point of view of computer simulations. Of course, in 1974-75 the lattice sizes that one could simulate were still quite small and the length of the runs was still quite short. A lot of the results which we got were qualitatively reproduced in quite a surprising number of phenomenological findings of experimentalist. Quantitatively, the results were all quite unreliable, but at that time nobody realized that, really. The paper found extreme attention[19], but the student was frustrated and didn't want to stay for a PhD. He went off to doing business, while I got a lot of invited papers at various conferences.

PC: We will get there, but I want to get back quickly to your time at Bell Labs. You said that you saw a talk by Phil Anderson where we presented the early EA results. Did you talk to Phil Anderson more about this talk?

KB: [0:16:16] I asked a few questions, of course. He had a very distinctive way of putting off questions which he didn't like. I did not profit too much from the discussion, but of course other clever people which were in the audience, like my host, Pierre Hohenberg—he was a very brilliant scientist which I have held in extremely high regard and unfortunately died a few years ago—asked much more stringent questions about this and other

[18] Ruderman–Kittel–Kasuya–Yosida (RKKY) Interaction: https://en.wikipedia.org/wiki/RKKY_interaction

[19] K. Binder and K. Schröder, "Phase transitions of a nearest-neighbor Ising-model spin glass," *Phys. Rev. B* **14**, 2142 (1976). https://doi.org/10.1103/PhysRevB.14.2142

people also asked questions, so the whole discussion I found very stimulating. Although Phil Anderson is not a very well organized speaker, the talk was nonetheless extremely stimulating.

PC: Did he share with you a preprint before you left, or after you left, or was the talk alone enough for you to get going?

KB: [0:17:21] At the talk, I did take notes. I don't really know at which time I got a written version of this paper. I don’t remember. But the model to be used was relatively clear. I wanted to study this for the short-range case. You don't want to get misled by the mean-field approximation anyway. In critical phenomena, the task was always to find critical exponents beyond mean-field by simulations, and that's how you are not really guided much by knowing the mean-field theory when you are studying a model with nearest neighbor interactions.

PC: You mentioned that as soon as you got to Saarbrücken, that's a project you assigned to the first student you had, and that you passed on your code also. So, as you understood, it was a fairly simple modification of the code, which made it a good diploma project. Can you give us an idea of what was the technological at landscape at that point? What sort of a computational resources or sort of code did you have?

KB: [0:18:45] At that time, starting from my PhD work I was only using code which I wrote myself. For a Monte Carlo simulation, [it was a] straightforward implementation of the Metropolis algorithm for an Ising lattice or for other sort of nearest-neighbor model on a lattice. The code is really quite simple. Of course, it is important that the random number generator is not completely rubbish, so one always has to test that. This is good enough, but this was not an important limitation for that problem either. I don't know whether you have heard about the saying of Dietrich Stauffer[20], another Monte Carlo practitioner, somewhat later than that time. He said: “A good Monte Carlo code has no more lines of code then the age of the person who writes it.” The Monte Carlo code, in the most simple version is not a big deal. Of course, the real art about Monte Carlo simulations is really the judgement of what you do and how you analyze the data. Distinguishing what is a real result and what is just an artifact of an inappropriate choice of initial configuration or something like that. So it's not a big deal to do a quick Monte Carlo simulation. This was a good topic for a diploma student.

[20] Dietrich Stauffer: https://de.wikipedia.org/wiki/Dietrich_Stauffer

PC: As you mentioned earlier these results were very well received by the community. There was a lot of interest in those results, which led to you being invited to various conferences and also for you to write a brief review the theory of spin glasses already in 1977[21]. That work shows that you were very closely following the theoretical discussion on spin glasses that was going on at the same time. How were you following? Was this mostly through conferences, or were you an avid reader of the scientific literature? Or did you have personal connection?

KB: [0:21:22] At that time, you did not have the Internet, you were still following the literature by going to the library, reading the journals. This was a feasible task. The number of journals was much less than it is now, and, of course, when you find out who are the practitioners in the field, it was quite common at that time to send reprints around, and also to send a postcard to people from whom you heard they were working on something, and to send you some reprints. So there was a lot of preprints being exchanged. I remember that in 1979, I did also find out from preprints by Giorgio Parisi about his work[22], but I didn't understand it. So I didn't do anything with it, because it was so obscure to me. I got also at that time the preprints of Alan Bray and Michael Moore—I don't know if you came across those names—who were really at a lot at these conferences. Giorgio Parisi was not going on any condensed matter physics conferences at the time, so I didn't meet him in person at that time at all. I only met him much later. Of course, at conferences like to Statphys in Boston[23], he gave an invited talk, and then in 1992 at the Statphys conference in Berlin[24], where he got the Boltzmann medal, he gave an invited talk, also in Paris at the Statphys meeting he gave an invited talk. I met Parisi mostly at these big prominent conferences. At the small topical meetings which I attended on spin glasses, I think he was not there at the time. Most of these meetings were in the second half of the '70s and in the first years of the '80s. There, I had a lot of discussion with colleagues in the field. Therefore, the articles which I wrote at that time reflect more or less the state of the art at that time. Of course, when Parisi came, there was a revolution, so I had a hard time catching up. Mostly I succeeded in catching up through my

[21] Kurt Binder, "Theory of spin glasses: A brief review," In: Treusch J. (eds) *Festkörperprobleme* **17**, 55-84 (Berlin: Springer, 1977). https://doi.org/10.1007/BFb0107758

[22] G. Parisi, "Infinite Number of Order Parameters for Spin-Glasses," *Phys. Rev. Lett.* **43**, 1754 (1979). https://doi.org/10.1103/PhysRevLett.43.1754

[23] STATPHYS 16, the 16th International Conference on Thermodynamics and Statistical Mechanics, Boston University, August 11-15, 1986. Proceedings: *Statistical Physics*, H. E. Stanley ed. (Amsterdam, North-Holland, 1987); *Physica A* **140**(1–2) (1986). https://www.sciencedirect.com/journal/physica-a-statistical-mechanics-and-its-applications/vol/140/issue/1

[24] STATPHYS18, Berlin, Germany, August 2–8, 1992; STATPHYS20, Paris, France, July 20–24, 1998 : https://en.wikipedia.org/wiki/Statphys

interactions with Peter Young, who then did all the very careful finite-size scaling tests of the various predictions of the Parisi theory.

PC: We will get to that, but I wanted to step back again. In 1977 also, you wrote papers on spin glasses on your own[25]. Was this because you were particularly excited about the problem at that point, or because you couldn't find a student?

KB: [0:24:44] No. I was quite excited at the time, and also the field was rapidly developing. So, when you don't have a student, and I anyway felt I was young enough and I better don't forget how it is working by yourself. As you may have noticed, in 1985, on this Ising model in five dimensions and the ramifications of finite-size scaling above the upper critical dimension, I still published a paper just by myself[26]. It's only when you have a very large group—at Jülich I had a large group; at Mainz I had an even larger group with many students—that you stop doing work by yourself. That's not a very good thing, because working by yourself and just doing everything by yourself on a particular project is useful. It educates you and keeps you modest and your judgment sound. I try to be as close to basic work as possible.

PC: You just mentioned your work in higher dimensions. Actually, your first work in higher dimensions was a collaboration with your postdoc, Dietrich Stauffer, where you looked at spin glasses in dimensions 3, 4 and 5[27], and at the same time you also worked with your grad student, Ingo Morgenstern, on the two-dimensional version[28]. You were looking at the full spectrum of available data. Clearly, this was motivated by the ongoing theoretical discussions in the field about the dimensional dependence of the instability of the spin glass transition. Was this the first time that someone did numerical simulations in unphysical dimensions, four and five, for instance?

KB: [0:26:55] I'm not sure. It could be that on percolation this was also done by Dietrich Stauffer by himself[29]. I don't remember exactly when he

[25] K. Binder, "Effective field distribution and time-dependent order parameters of Ising and Heisenberg spin glasses," *Z. Phys. B* **26**, 339-349 (1977). https://doi.org/10.1007/BF01570744

[26] K. Binder, "Critical properties and finite-size effects of the five-dimensional Ising model," *Z. Phys. B* **61,** 13-23 (1985). https://doi.org/10.1007/BF01308937

[27] Dietrich Stauffer and K. Binder, "Comparative monte Carlo study of Ising spin glasses in two to five dimensions," *Z. Phys. B* **34**, 97-105 (1979). https://doi.org/10.1007/BF01362783

[28] Ingo Morgenstern and K. Binder, "Evidence Against Spin-Glass Order in the Two-Dimensional Random-Bond Ising Model," *Phys. Rev. Lett.* **43**, **16**15 (1979) https://doi.org/10.1103/PhysRevLett.43.1615 .

[29] It was in fact the work of Scott Kirkpatrick: Scott Kirkpatrick, "Percolation Phenomena in Higher Dimensions: Approach to the Mean-Field Limit," *Phys. Rev. Lett*. **36**, 69 (1976). https://doi.org/10.1103/PhysRevLett.36.69

worked on this, but at some point he worked on this. Since there was evidence that a renormalization group theory of spin glasses needed six as the marginal dimension, and if one makes an epsilon expansion for spin glasses, one would need to expand around six dimensions, so when this rumor about this fact spread around, then it was of course very interesting to try to compare different dimensions. From the Monte Carlo evidence, it came out clearly that the work which I had done with my student Schröder gave an estimate of where there was a transition that was clearly too high. When we checked this with more effort we got a lower number. Other people, like Bray and Moore[30], got a still lower number. Finally, the idea came about that maybe this was an effect of the limited observation time, if one ran an infinite time the transition would only be at zero temperature.

That motivated me too ask my PhD student, Ingo Morgenstern[31], to work on what is a kind of recursive transfer matrix method, by which one can, on two-dimensional lattices, do an exact static calculation. That is a brute force approach, but we could do it for quite a number of lattice sizes, small lattices and analysis, and from this work we got conclusive evidence that in two dimensions there were no transition, that the correlation length of the spin glass was diverging as a kind or power-law presumably as one approaches zero temperature. And the spin glass correlation function, although it was not converging to the square of an order parameter at zero temperature, but rather was decaying to zero as a power law. This brought a sort of evidence that two was below the so-called lower critical dimension. Actually, the discussion of the lower-critical dimension went on and on, even in the current time. There are still papers from this century, after the year 2000[32], which deal with this problem. I'm not sure whether this now really proven that it is at 5/2, or whether it is only a conjecture that it is at 5/2.

PC: Discussions are ongoing, as I understand as well. You've hinted at this already, but I'd like you to elaborate if you can. How was your work received by the theoretical groups in that early stage, before the Parisi solution?

[30] A. J. Bray and M. A. Moore, "Monte Carlo evidence for the absence of a phase transition in the two-dimensional Ising spin glass," *J. Phys. F* **7**, L333 (1977). https://doi.org/10.1088/0305-4608/7/12/004

[31] Ingo Morgenstern, *Ising-Systeme mit eingefrorener Unordnung in zwei Dimensionen*, PhD Thesis, University of Saarbrücken (1980). https://swb2.bsz-bw.de/DB=2.340/PPNSET?PPN=1144331579&PRS=HOL&HILN=888&INDEXSET=21

[32] *E.g.*, Stefan Boettcher, "Stiffness of the Edwards-Anderson Model in all Dimensions," *Phys. Rev. Lett.* **95**, 197205 (2005). https://doi.org/10.1103/PhysRevLett.95.197205; Andrea Maiorano and Giorgio Parisi, "Support for the value 5/2 for the spin glass lower critical dimension at zero magnetic field," *Proc. Nat. Acad. Sci. U.S.A*. **115** 5129-5134 (2018). https://doi.org/10.1073/pnas.1720832115; Valerio Astuti, Silvio Franz and Giorgio Parisi, "New analysis of the free energy cost of interfaces in spin glasses," *J. Phys. A* **52**, 294001 (2019). https://doi.org/10.1088/1751-8121/ab2744

And by the experimental groups? Who was the main audience for the works, and how were they received?

KB: [0:30:45] I think that it was received by most groups with interest. Experimentalists were happy because such Monte Carlo simulations could get the same decay of spin autocorrelation function with Monte Carlo time, which is similar to the experimental findings. One could also do Monte Carlo simulations of site-disorder models which were closer to the experiments, like the europium sulfide alloyed with strontium or related models. One could do relatively realistic simulations, so the experimentalists were very interested in getting some guidance in the interpretation of their experiments. Theorists were of course interested. First of all, to know what is really going on in spin glasses with short-range forces. Second, some persons started to simulate the infinite-range model as soon as the Sherrington-Kirkpatrick paper was around, which indicated the problems with the simple version of the replica method introduced by Edwards and Anderson. This Sherrington-Kirkpatrick was believed to be exactly solvable by statistical mechanics. You formulate a model for which the interactions between spins is independent of distances, you take the mean-squared interaction as to scale with the number of spins in a way to ensure a sensible thermodynamic limit, and then—apart from doing this replica trick—it is exact. Then, how could this give a wrong result, like the negative specific heat, which was already found by Sherrington and Kirkpatrick themselves[33]? (This shared paper appeared in parallel to my paper with Schröder. Their first paper appeared in 1975, and our paper was submitted in 1975 and it appeared in 1976, but it had been circulated around to most people [beforehand].) Scott Kirkpatrick was a simulation person also. He had done really pioneering work on simulations of the percolation problem, and so he started simulations on the infinite-range model. In the long paper by Sherrington and Kirkpatrick, which came out a few years after their letter[34], there was a comparison with simulations. As my simulations with Schröder had the problem of very sluggish observation time dependence, it was difficult to figure what was in equilibrium and what was not in equilibrium. This problem appeared in the infinite-range case also. Therefore, the theorists had seen those difficulties, and were quite interested. At these conferences, which I attended in the period from 1977 to 1983, Alan Bray and Michael Moore were quite regular attendees. I had a lot of discussion with them. They did both simulations and analytical calculation. Also, I had a lot of exchanges with David Sherrington. It was a very free

[33] David Sherrington and Scott Kirkpatrick, "Solvable Model of a Spin-Glass," *Phys. Rev. Lett.* **35**, 1792 (1975). https://doi.org/10.1103/PhysRevLett.35.1792

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

exchange of ideas and so on. I recall that this was a very pleasant way of dealing with each other at that time. And it was not yet this problem of counting citation numbers and things like that. This was not yet [a thing]. The Science Citation Index[35] was not yet a criterion for a person to get a permanent position.

PC: You describe all this network of people that were interacting at that point. Amongst all of them you are the one who wrote—I think—the most reviews of the field. You were essentially writing a status reports every couple of years. Were you seen as some sort of mediator or central figure, which gave you that role?

KB: [0:35:52] No. The first review, which I published in 1977, was just on the basis of an invited talk of a German Physical Society conference[36]. Then, I was also invited to a winter school in Norway[37] and to a summer school on fundamental problems in statistical physics[38]. All of these conferences wanted to write a book after the conference, so you were asked to supply an article. Different people have different attitude on this. Some people try to get away with some very lousy, short manuscript. When I was asked to write for these books, I felt it was useful to have a rather complete write up. So these were always more on the longer side and with complete references. They were then found useful by other people, so what way they found a bit more attention. This was probably also responsible for the fact that I was asked, together with Peter Young, to write a review article for Reviews of Modern Physics, which really is like a book, because it is almost 200 pages, with two columns, and many hundred references[39]. People still use it today, so it was a big effort, but it wasn't completely useless.

PC: We will get there also. It's a very interesting work, obviously. You've mentioned the Parisi solution. When it came out in 1979, you were sent a preprint, but you struggled to understand it. Was it mostly confusion, or was there skepticism with respect to the techniques? How would you describe your reception?

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[35] Science Citation Index : https://en.wikipedia.org/wiki/Science_Citation_Index

[36] Meeting of the German Physical Society, March 7–12, 1977, Münster, West Germany. https://link.springer.com/book/10.1007/BFb0107754

[37] NATO Advanced Study Institute on Strongly Fluctuating Condensed Matter Systems, Geilo, Norway, April 16-27, 1979. Proceedings: *Ordering in Strongly Fluctuating Condensed Matter Systems*, Tormod Riste, ed. (New York: Plenum Press, 1980).

[38] Fifth International Summer School on Fundamental Problems in Statistical Mechanics, June 23-July 5, 1980, Enschede, The Netherlands. Proceedings: *Fundamental Problems in Statistical Mechanics V*, E. D. G. Cohen, ed. (Amsterdam: North-Holland, 1980).

[39] K. Binder and A. P. Young, "Spin glasses: Experimental facts, theoretical concepts, and open questions," *Rev. Mod. Phys.* **58**, 801 (1986). https://doi.org/10.1103/RevModPhys.58.801

KB: [0:37:53] My original reception was that I was just confused, and I did not understand it at all. I was understanding that Sherrington-Kirkpatrick taking the order parameter as just a constant was inappropriate, and that one needed somewhere a broken symmetry. Even in the ferromagnet you have a broken symmetry, Ising ferromagnet up and down spins… Yeah, it had to be more complicated, but I was not understanding why this construction, which one could find in the Parisi papers was sensible and what was the reasoning behind it that took this and not something else. A lot of my colleagues had similar difficulties, so there were always some people questioning whether maybe one could do something else, which would still yield a solution with an even lower free energy. But these voices are now silent; they disappeared, and now I think the disagreement has ended. It's actually proven by these Talagrand works[40] that this is really the correct *ansatz*, and that's it! I have incredible respect for the incredible intuition which Giorgio Parisi had to find this. I don't know how he really did it. This is really a mystery to me. I don't know whether he explained it to anybody what let him to do it exactly that way. It's certainly one of the most intelligent and difficult achievements in theoretical physics of the second half of the 20th century.

PC: You had another series of papers that came out about spin glasses after the Parisi solution. The collaboration with Wolfgang Kinzel on field-cooled and zero field cooled simulations[41]. Can you tell us what inspired this work, and where did it fit?

KB: [0:40:29] This was inspired mostly by the interaction with experimentalists, who pointed out that there were these differences in the data. It was interesting to find out about it, whether this was also reproduced by the simulation, and whether from the simulation one could find some insight into why these differences did occur.

PC: I understand these works were very well received as well. Did you follow a bit the response to those papers?

KB: [0:41:08] Not really, because when it came… What I had found with Ingo Morgenstern, that in two dimensions there was no spin glass transition, and then trying to clarify what happens in three dimensions turned out to

[40] See, *e.g.*, Michel Talagrand. *Spin glasses: a challenge for mathematicians: cavity and mean field models*. (Berlin: Springer-Verlag, 2003).

[41] W. Kinzel and K. Binder, "Static and Dynamic Critical Magnetic Fields in Ising Spin-Glasses," *Phys. Rev. Lett.* **50**, 1509 (1983). https://doi.org/10.1103/PhysRevLett.50.1509; "Static and dynamic magnetic response of spin-glass models with short-range interactions," *Phys. Rev. B* **29**, 1300 (1984). https://doi.org/10.1103/PhysRevB.29.1300 .

be increasingly difficulty. Actually, my PhD student, Ingo Morgenstern, after he had made his PhD summa cum laude and so on, became a postdoc at the university of Heidelberg, in Heinz Horner's[42] group, also a spin glass theorist at that time, and then he had the opportunity to also spend that time at Bell Laboratories, [where] he worked with [Andy] Ogielski[43]. Ogielski had built a special purpose computer dedicated to do spin simulations only[44]. Then, that was the first believable results for the transition in the +/-*J* spin glass. Actually, it turns out that the estimate, which came out from their freezing temperature was only 8% too high, according to what one believes now is the correct answer, but the critical exponent nu, which they estimated is already a factor of two off. This shows that even with such an effort, at that time, you did not get sound results. One definitely wanted results that stand up to the criticism of time, for a long while. I felt I would not try to find somebody who builds a special purpose computer for me, so that I can stay in that race.

I felt that after I had suffered a lot from this review article with Peter Young—which we actually wrote in the years 1983-1984, and submitted in 1985, and it came out in 1986—and by then I was really tired to work on spin glasses. I felt I should do something else, and leave the competition to others who could do a better job. It turned out that they could. Giorgio Parisi was the mastermind behind this Janus collaboration and special purpose computer[45]. (I think it is physically located in Spain.) The papers which result from this collaboration have given the most precise numbers for the spin glass critical temperature and spin glass exponents to date, but when you look at the author list of the papers—24 authors on one paper—it's like a high-energy physics collaboration. This was not my style of working, so I didn't try to compete. Building a really good special purpose computer is a huge project in itself. It's the kind of engineering project for which you need a dedicated team who can do that, and I didn't see how I could compete on that count.

PC: Understandably. I'd like to go back to that *magnum opus*, the review paper with Peter Young. Can you tell us a bit how this idea came about, and how did you get to work? I don't think you ever collaborated with Peter Young before that, so this was new joint effort.

[42] Heinz Horner: https://de.wikipedia.org/wiki/Heinz_Horner

[43] Andrew T. Ogielski and Ingo Morgenstern, "Critical behavior of three-dimensional Ising spin-glass model," *Phys. Rev. Lett.* **54**, 928 (1985). https://doi.org/10.1103/PhysRevLett.54.928

[44] Andrew T. Ogielski, "Dynamics of three-dimensional Ising spin glasses in thermal equilibrium," *Phys. Rev. B* **32**, 7384 (1985). https://doi.org/10.1103/PhysRevB.32.7384; "Integer Optimization and Zero-Temperature Fixed Point in Ising Random-Field Systems," *Phys. Rev. Lett.* **57**, 1251 (1986). https://doi.org/10.1103/PhysRevLett.57.1251

[45] Janus Supercomputer : http://www.janus-computer.com/ (Last consulted December 22, 2020)

KB: [0:45:08] Peter Young was one of the persons who was regularly present at the same conferences as I, so we had a lot of discussions. The idea to do this together [came] when we found out that the two of us had both received invitations from the journal, and then we decided we better join forces. It was a lot of work anyway. A complete coverage of all experiments, simulations and analytical theories was such a big task. I didn't understand really how big the task was, until I started working. Then, I was trying to really look on all the papers which existed in the field, in order to make sure I am not unfair to anybody who had made an important contribution. Xerox copies or articles piled up, and piled up, and piled up, so distinguishing which was relevant, and which was only a kind of little decoration to something which was spelled out well enough and need not really be covered, really was a lot of effort. So it was nice to have a co-author to talk about this, and to exchange opinions. For a single person, giving such a judgment of contributions of many, many, many other authors is a delicate matter, and it's much better to do this jointly. Peter Young is a very nice character, a very nice person and a good friend, so we could do this very well together.

PC: This work is not only a work of synthesis and curation, but it also was a pedagogical effort. You mentioned that you had to teach yourself Parisi's works to then be able to explain it.

KB: [0:47:35] Yes. Just giving a list of who did what, that is not a good review, I think.

PC: Absolutely. So did you ever get to reuse that material after putting it together? Did you get to teach a course that used ideas of replica symmetry breaking, for instance, either at the university or in a summer school?

KB: [0:47:58] At the university, we later had research effort on amorphous materials and glasses, and I was the spokesman of this collaborative research effort between theorists, experimentalists and chemists[46]. In that context, I was giving a course on the statistical mechanics of disordered materials. That had a section on pair correlations in it, and a section on spin classes in it, and undercooled fluids and so on. This course became later the nucleus of the book, which I wrote together with Walter Kob[47]. (I'm not sure

[46] The Collaborative Research Centre (Sonderforschungsbereiche, SFB) 262, entitled 'Glaszustand und Glasübergang nicht metallischer amorpher Materialien' (The glass state and glass transition of non-metallic amorphous materials) was funded by the Deutsche Forschungsgemeinschaft (DFG) from July 1987 to December 2001.

[47] Kurt Binder and Walter Kob, *Glassy Materials And Disordered Solids: An Introduction To Their Statistical Mechanics,* (Singapore: World Scientific Publishing, 2005).

whether you are aware of that book.) This book, which is more recent, of course contains more recent spin glass literature, but it is not as complete as this article It rather tries to focus on things which are really important in retrospect, after some time. That is all that I did. I was not planning to write another review article or book on the subject of spin glasses on its own. The one with Peter Young was so much effort, and I didn't want to have this again.

PC: Just to situate us, when did this course started? Is this the mid-'90s or early-'90s?

KB: [0:49:42] This course was started in the early '90s, because this special research program started in 1987. In 1986, it was conceived and in 1987 the German National Science Foundation funded it. This meant that I got additional postdoc positions and more possibilities to hire PhD students. It was always a good idea, when you have such a research program to have some accompanying teaching activities. Of course, this course was not given only once, but it was given several times. After it had been given several times, Walter Kob—who is now in Montpellier, but at that time he was at Mainz—and I conceived the idea that we could have a book on this matter. This book, I was quite happy to do [it], although it still does not have such a high popularity. From my point of view, I find it quite complete and useful.

PC: As you said, as you left the world of spin glasses, you moved into the world of structural glasses with the creation of that program. I understand that part of your motivation was the development of Götze's mode-coupling theory[48]. Is that correct? If yes, could you elaborate?

KB: [0:51:28] Götze's mode-coupling theory existed at the time when we conceived this program, and it was not the main motivation for conceiving this program. This program was conceived mostly on the initiative of experimentalists who felt that it was important to have some coherent effort in this area. That theorists and experimentalists and chemists worked together and try to achieve some degree of cross-fertilization among our joint research effort, to avoid getting stuck in mediocre work. In fact, this research effort was quite successful. Also, over the 15 years this program ran, we had many conferences organized, and participants in this program played a major role at the glass conferences entitled *Relaxations in Complex Systems*, which was [first] held in Greece[49]. (Kia L. Ngai from the Naval

[48] Donal Mac Kernan, "Interview of Kurt Binder," *SIMU Challenges in Molecular Simulations: Bridging the Length- and Timescales gap,* **3**, 7-30 (2001). https://doi.org/10.13140/2.1.1252.0647

[49] First International Discussion Meeting on Relaxations in Complex Systems, 18-29 June 1990, Heraklion, Crete, Greece. Proceedings in *J. Non-Cryst. Sol.*, **131-133** (1991). https://www.sciencedirect.com/journal/journal-of-non-crystalline-solids/vol/131/

Research Laboratory in the United States, was the main organizer of these conferences.) We had there rich exchanges with an international community of people working on the structural glass transition. This structural glass transition is still rather controversial today. Because there are lots of experimentalists who don't like any of these spin-glass model related approaches, they don't like the Götze mode-coupling theory, they are still concerned with ideas from free volume distribution in the supercooled fluid. There are many of the old ideas from glass research, which date back decades ago and are still alive. Some people are still trying to work on that basis and analyze their data with it. This unfortunately affected more analytical work on theory of glasses, and has not contributed much to understanding and [to develop] a common agreement about what is going on, as [happened] in the spin glass field. In the spin glass field, even quite recently, there are *Physical Review Letters* papers where experiments on spin glasses are interpreted with concepts explaining the aging phenomenon, which one observes, and the theoretical concepts are really motivated by their analytical theory. This sort of close linkage between theory and experiment in the field of structural glass transition, I'm afraid, has not been reached yet. Whether it will be reached and when I don't know, but it's still an interesting field.

PC: Speaking of the connection between spin glasses and structural glasses: at about the same time as you started your program in Mainz, Kirkpatrick, Thirumalai and Wolynes used inspiration from Potts spin glasses to formulate a theory of structural glasses[50]. Did you follow these developments? Was this also a source of inspiration for you at the time or did it come later on?

KB: [0:55:33] This was definitely one of the concepts which I found very interesting. Is there something [by which] one can prove this, or disprove this? Of course, there was always in the glass transition the idea of looking for some glass correlation length. The attempts in which I was involved were trying to probe the correlation length from surface effects. Preparing voids with rough walls or with smooth walls for the same model, and then looking at how the dynamics was influenced as a function of temperature for

[50] See, *e.g.*, T. R. 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 and D. Thirumalai. "Mean-field soft-spin Potts glass model: Statics and dynamics," *Phys. Rev. B* **37**, 5342 (1988). https://doi.org/10.1103/PhysRevB.37.5342; D. Thirumalai and T. R. Kirkpatrick. "Mean-field Potts glass model: Initial-condition effects on dynamics and properties of metastable states," *Phys. Rev. B* **38**, 4881 (1988). https://doi.org/10.1103/PhysRevB.38.4881;

these different types of walls, and whether someone could extract a correlation length from this[51]. There were also these ideas, at about the same time, of looking for stringlike objects in glasses, so many of these ideas were there. But concerning the concept of Thirumalai and Wolynes, I didn't see that this was proving or disproving much of it. The problem is, of course, that always the simulation and the glassy models were not yet possible at low enough temperature. So that it wasn't really clear that one was deep enough in the glassy phase to still see something which relates to the free energy landscape of the glass. So as long as you are in the regime which is describable by the mode-coupling theory, it is clear that one is still far above the glass transition. There the relaxation times get already very long, so it's difficult to make sure that one interprets the simulations properly.

PC: But you had a lot of experience in that particular problem, right?

KB: [0:57:46] Yes, but I must say I don't claim to have solved the problem, so… (laughter).

PC: Clearly, there is a strong relationship between your work on spin glasses and your work on structural glasses. But did your work on spin glasses influence the rest of your research program in any other way that we have not talked about? Maybe in more subtle ways?

KB: [0:58:12] In the immediate aftermath, we did not consider standard structural glasses, but we went to what was called orientation glasses. [These] are dilute molecular crystals in which molecules have a quadrupolar moment, forming crystals like solid nitrogen; the quadrupolar moment of the nitrogen molecule is oriented and then you dilute such a nitrogen crystal with argon, which is just a spherical atom, with no quadrupolar moment. A sort of glass phenomenon [arises], and the phase diagram looks quite similar to the phase diagram of spin glass systems. We tried to study models for these orientational glasses. When I moved into this, there were not yet any other work around, so it was just possible to do some first exploratory studies, with relatively modest computer resources and not in need of starting already with a special purpose computer. I worked a couple of

[51] See, *e.g.*, P. Scheidler, W. Kob and K. Binder, "Cooperative motion and growing length scales in supercooled confined liquids," *Europhys. Lett.* **59**, 701 (2002). https://doi.org/10.1209/epl/i2002-00182-9; "The relaxation dynamics of a confined glassy simple liquid," *Eur. Phys. J. E* **12**, 5-9 (2003). https://doi.org/10.1140/epje/i2003-10041-7; "The Relaxation Dynamics of a Supercooled Liquid Confined by Rough Walls," *J. Phys. Chem. B* **108**, 6673–6686 (2004). https://doi.org/10.1021/jp036593s

years on this in the a late '80s and the beginning of the '90s on that problem[52].

PC: At the institutional level, did working on the spin glass problem change the statistical mechanics community in in Europe? Can you draw a difference before 1975 and after 1985, based on that intense collaboration that you've described? Or was it always like this?

KB: [1:00:03] I don't think the general structure of the community did change. Some of the prominent practitioners did get positions. Wolfgang Kinzel got a professorship first at Giessen, and then he worked in Würzburg, and Annette Zippelius[53]—I should have mentioned her name anyways, because her work with Sompolinsky on the dynamical approach to spin glass theory was really influential for a number of years[54]—got a position at the University of Göttingen, and so on. There were a number of people building up a community, but there were other fields getting popular: fractal growth and Kardar-Parisi-Zhang[55] was a very famous topic for a while, then soft matter came up, then active liquids and biology. So theoretical physics and statistical mechanics in Germany is a very diverse community which has many subfield and spin glass was [just] one of them.

PC: Speaking of the relationships between communities, one of the very close communities, at least from your standpoint, was that working on percolation at the same time. Dietrich Stauffer, maybe most prominently bridged the two. Was there a lot of crosstalk or were these understood to be completely different problems, with different methods and different ideas?

KB: [1:01:58] There was certainly a lot of crosstalk for several reasons: the percolation was also a problem there, the upper dimension being six, quenched disorder, one could also find many different variants of percolation which were mostly suitable to study by computer simulations. From a computer simulation perspective, this was very natural to have interaction on percolation.

[52] See, *e.g.*, K. Binder and J. D. Reger, "Theory of orientational glasses models, concepts, simulations," *Adv. Phys.* **41**, 547-627 (1992). https://doi.org/10.1080/00018739200101553
[53] Annette Zippelius : https://en.wikipedia.org/wiki/Annette_Zippelius
[54] Haim Sompolinsky and Annette 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
[55] Kardar-Parisi-Zhang equation : https://en.wikipedia.org/wiki/Kardar%E2%80%93Parisi%E2%80%93Zhang_equation

PC: But you didn't leverage that yourself. Your collaborators mostly used the convenience of the same Monte Carlo codes, I guess, and other resources.

KB: [1:02:53] Well, Dietrich Stauffer was for a while in my group and he pioneered this, and I found this extremely valuable, so I tried to support him as much as I could. He was quite a special character, so to say, so he did not have such an easy time as I had in finding a permanent position. So I certainly didn't try to steal from him any ideas or anything. I let him do what he wanted, and not get my name attached to what he was doing. I avoided it. He was a very nice collaborator on certain spin glass papers, but I tried to let the percolation field just for himself.

PC: We're nearing the end of the interview, so I wanted to give the chance to Francesco, in case there's any question he wanted to ask.

FZ: I'm following with much attention, but I don't have any question to ask.

PC: Is there anything else that you would like to share with us about this epoch that we may I missed or skipped over? Or some concluding thoughts?

KB: [1:04:30] I think we have touched a lot of issues. It would be very nice if the field of spin glasses and glasses were still flourishing in the near future, and if its practitioners continue to find great conclusions. Let's hope that this will be the case.

PC: We hope so too. Finally, have you kept notes, papers, correspondence from that epoch. If you did, do you have a plan to deposit them in an academic archive at some point?

KB: [1:05:17] I'm afraid not. Because the correspondence which I had was huge and on many different issues, and in quite a chaotic state. When I had to give up my office as a full professor in 2012 and move to a small office, then a lot of the paper which I felt was not of real interest was simply going to a waste paper container. It turned that it was not a single paper container, because I still had kept all these preprints, all of this reprints and preprints. I did keep [some] out of this material but not on spin glasses, because on that I did not plan to do any further work. You may find some recent work from myself on certain problems with semi-flexible polymers and stuff like that. So I kept a lot of papers on polymer simulations and theoretical polymer physics, but not on spin glasses. It's in the past. There was much less activity and much less paper. I did not think that anybody would be interested in the mess of papers which was left over from me at some point.

PC: I understand. Many people have done the same. Professor Binder thank you so much for your time and for your thoughts. It's been a real pleasure to hear about your work and that very special time in which you did it.

KB: [1:07:22] Yes, it was a very lucky time, really. At that time, in the mid-'70s, you could go to the library, you could scan all the important journals in an afternoon, and then you knew quite for a while what was going on. Now, it's completely impossible. With the preprint server and the journals, it's impossible. Simply the large number of scientists who work now make it very difficult to make visible contributions. It was much easier in the '60s and '70s of the last century. So I was a member of a still really lucky generation. Not as lucky as the people before the second world war, but lucky enough to do some interesting work.

PC: Thank you.

KB: You're welcome.