History of RSB Interview: Jürg Fröhlich

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PC: Good afternoon, Professor Fröhlich. As we were just discussing, we're going to converse about the history of replica symmetry breaking in spin glass physics, which took place between 1975 and 1995, roughly speaking. But before we delve into the heart of the topic, we have a few background questions to better situate your contributions. What originally drew you to the physics of phase transitions, in particular?

JF: [0:00:35] Let's phrase the question as follows: What drew me to statistical mechanics? When I was a PhD student—I had just started to work on my thesis[1]—there was a Les Houches School in 1970 that was devoted to statistical mechanics and quantum field theory[2]. Among the people in statistical mechanics, one of the lecturers was Jean Ginibre[3], whom you might know at least by name. He was a very brilliant, very intelligent and extremely decent colleague, who unfortunately passed away a few years ago. Then, there were Bob Griffiths[4] from Carnegie Mellon, Andrew Lenard[5] from Bloomington, Indiana, Elliott Lieb[6] then from MIT—he later

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[1] Jürg Fröhlich, *Über das Infrarot-Problem in einem Modell skalarer Elektronen und skalarer Bosonen der Ruhemasse 0*, PhD Thesis, ETH Zürich (1973). https://eth.swisscovery.slsp.ch/permalink/41SLSP_ETH/lshl64/alma99117248050105503 (Accessed June 18, 2023.)

[2] *Mathematical Physics*, Les Houches, France, July 5-August 29, 1970. See, e.g., *Statistical mechanics and quantum field theory*, C. DeWitt and R. Stora, eds. (New York: Gordon and Breach [c1971]). C DeWitt, "Les Houches Summer School," *Europhysics News* **1**(6), 6-7 (1969). https://doi.org/10.1051/epn/19690106006

[3] Jean Ginibre: https://en.wikipedia.org/wiki/Jean_Ginibre

[4] Robert Griffiths: https://en.wikipedia.org/wiki/Robert_Griffiths_(physicist)

[5] Andrew Lenard: https://de.wikipedia.org/wiki/Andrew_Lenard

[6] Elliott Lieb: https://en.wikipedia.org/wiki/Elliott_H._Lieb

moved to Princeton—and David Ruelle[7] from the IHES. They gave really wonderful lectures, which sparked my interest in matters of statistical mechanics. My PhD thesis was about quantum field theory; but during the defense, my advisor Klaus Hepp[8] asked questions about statistical mechanics, in particular about the Peierls argument[9], which deals with the phase transition in the Ising model.

Later on, I was a postdoctoral researcher, first at Harvard and then at Princeton (as an assistant professor in the math department). I arrived at Princeton at the same time when Elliot Lieb arrived as a professor. We had a lot of interactions, scientific and social ones. He gave a "seminar for friends of statistical mechanics" practically every semester. So, obviously I got interested in problems of statistical mechanics. In fact, I also gave a graduate course about statistical mechanics at Princeton. Then, in 1975, Barry Simon[10], who was one of my colleagues at Princeton, suggested that he, Tom Spencer[11] and I try to work on phase transitions. The obvious problem then was to try to show that there are phase transitions accompanied by continuous symmetry breaking and the emergence of Goldstone bosons. This looked like an extremely tough problem. In the end, we found a very elegant and technically rather simple solution[12]. Some of the ingredients we developed are still in heavy use nowadays: the infrared bounds (on two-point correlators), which are a statistical mechanics analogue of the Källén-Lehmann representation in quantum field theory[13]. So, that's how I got started on work in statistical mechanics.

Another piece of work, which I had actually carried out a little earlier, was about the statistical mechanics of two-dimensional classical two-component Coulomb gases[14]. These systems exhibit an interesting phase transition, namely the Kosterlitz-Thouless transition. Of course, I didn't

[7] See, *e.g.*, P. Charbonneau, *History of RSB Interview: David Ruelle*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 4 p. https://doi.org/10.34847/nkl.5330p51b
[8] Klaus Hepp: https://en.wikipedia.org/wiki/Klaus_Hepp
[9] It proves the existence of a phase transition in the two-dimensional Ising model. See, *e.g.*, https://en.wikipedia.org/wiki/Ising_model
[10] Barry Simon: https://en.wikipedia.org/wiki/Barry_Simon
[11] Thomas Spencer: https://en.wikipedia.org/wiki/Thomas_Spencer_(mathematical_physicist)
[12] J. Froehlich, B. Simon and T. Spencer, "Infrared bounds, phase transitions and continuous symmetry breaking," *Commun. Math. Phys.* **50**, 79-85 (1976). https://doi.org/10.1007/BF01608557
[13] Källén–Lehmann spectral representation: https://en.wikipedia.org/wiki/K%C3%A4ll%C3%A9n%E2%80%93Lehmann_spectral_representation
[14] J. Fröhlich, "Classical and quantum statistical mechanics in one and two dimensions: Two-component Yukawa—and Coulomb systems," *Commun. Math. Phys.* **47**, 233-268 (1976). https://doi.org/10.1007/BF01609843

understand this transition, yet. Then, in 1980, Tom Spencer and I rigorously established the existence of the Kosterlitz-Thouless transition[15].
So, that's roughly how my trajectory in statistical mechanics began.

PC: In your selecta book, which came out about 13 years ago, you described mathematical physics as a very useful endeavour in at least two different situations[16]. The first one being when progress in discovering new laws of nature is stagnating, and the second one when deep conceptual problems turn up that call for a radical rethinking of the foundations of physical theory. In which of these two categories, would you generally situate your contributions to statistical mechanics and phase transitions more specifically?

JF: [0:04:55] Obviously, in the first category. I was just a little too young to participate in the big revolution that happened in particle physics, in connection with the discovery of the standard model and asymptotic freedom, etc. So, my feeling was that not too much was happening there; but there were still lots of interesting problems for mathematical physicists to try their teeth on. One circle of problems was phase transitions. However, I believe I have also done work on problems belonging to the second category, namely rethinking foundations of physical theories. But that came much later. It's mostly connected to quantum theory, not to phase transitions.

PC: You described a bit how you came to work on phase transitions and statistical mechanics. But, more generally, how did you find problems to work on? What drove your interests in jumping from one problem to another?

JF: [0:06:04] The word "jumping" is well chosen. Of course, you know Freeman Dyson[17]. He often complained that his attention span was very short, and therefore he had to keep moving from one problem to another one. I'm somewhat similar in this respect. I never worked on any particular problem for a very long period of time; at least not before I retired. Now,

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[15] J. Fröhlich and T. Spencer, "Kosterlitz-Thouless transition in the two-dimensional plane rotator and Coulomb gas," *Phys. Rev. Lett.* **46**, 1006 (1981). https://doi.org/10.1103/PhysRevLett.46.1006; "The Kosterlitz-Thouless transition in two-dimensional abelian spin systems and the Coulomb gas," *Commun. Math Phys.* **81**, 527-602 (1981). https://doi.org/10.1007/BF01208273; "On the statistical mechanics of classical Coulomb and dipole gases," *J. Stat. Phys.* **24**, 617-701 (1981). https://doi.org/10.1007/978-1-4899-6762-6_2

[16] J. Fröhlich, "Introduction," In: *A Journey Through Statistical Physics: From Foundations to the Quantum Hall Effect – Selecta of Jürg Fröhlich*, G. Felder, G. M. Graf and K. Hell, eds. (Dordrecht: Springer, 2009): xv-xlvi.

[17] Freeman Dyson: https://en.wikipedia.org/wiki/Freeman_Dyson

during the past 10 years, I've been fairly steadily focussed on certain problems. But earlier in my career, I liked to move from one area to another one. That's simply how I proceeded in my work. This was still possible during my time as an active researcher. I didn't have to specialise very much. Nowadays, I think that people like me are considered to be somewhat unprofessional; but in my time this was tolerated. In fact, it was perhaps even a certain asset.

PC: Was it largely through chance encounters with other scientists or through steady reading of the literature? By what mechanism would you be choosing problems to work on?

JF: [0:07:19] Actually, I'm a fairly slow reader. I have never read very much. Of course, sometimes it's unavoidable. You have to learn something; then you have to read up. But I'm really a slow reader. I got my inspiration more from attending talks, attending summer schools. For example, Les Houches, I always thought, was a wonderful place to get inspiration — but also in colloquia and in seminars, and so on. Besides that, I was very lucky in meeting many people I really liked to talk and listen to. For example, when I was at Princeton as an assistant professor, that was during a period when Erhard Seiler*,* of the Heisenberg Institute in Munich, was a visitor at the Institute for Advanced Study[18], and we had absolutely wonderful discussions that I found extremely stimulating. For example, it is certainly thanks to Erhard that I got started on the two-dimensional Coulomb gas. Actually, this was originally an attempt to understand Sine-Gordon theory[19]; but it was explained to me by Erhard that this theory is connected to the two-dimensional Coulomb gas. This triggered my interest in the two-dimensional Coulomb gas. Afterwards, I got to know Tom Spencer, and we had an immensely successful and harmonious collaboration for several years.

PC: Your first paper on spin glasses proper was a collaboration with Aernout van Enter[20], whom I think you met at the Les Houches School in 1984[21].

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[18] "Erhard Seiler," *Institute for Advanced Studies,* 9/1973 – 6/1975. https://www.ias.edu/scholars/erhard-seiler (Consulted June 5, 2023.)

[19] Sine-Gordon Equation: https://en.wikipedia.org/wiki/Sine-Gordon_equation

[20] A. C. D. Van Enter and J. Fröhlich, "Absence of symmetry breaking for N-vector spin glass models in two dimensions," *Commun. Math. Phys.* **98**, 425-432 (1985). https://doi.org/10.1007/BF01205791

[21] *Critical Phenomena, Random Systems, Gauge Theories*, Les Houches, France, 1 August—7 September 1984. Proceedings: *Critical Phenomena, Random Systems, Gauge Theories*, K. Osterwalder and R. Stora, eds. (Amsterdam: Elsevier, 1986).

However, at that school you were already lecturing on spin glasses[22]. When did spin glasses enter your field of vision and in what context?

JF: [0:09:26] I spent four and a half years at IHES[23] as a *professeur permanent*, but then I got pulled back towards my home institution, which is the ETH in Zürich. At ETH I supervised a lot of PhD students and I needed to generate good problems for PhD theses. Spin glasses were in the air, and I had one student, Anton Bovier[24], who developed an interest in working on them. That's how we got started. But I have to confess that I never found spin glasses incredibly interesting or important, to be honest. But it was a circle of problems that seemed to be interesting to work on together with PhD students or postdocs.

PC: Did you just say to your student: "Go study this field"? Or had you already absorbed part of the literature?

JF: [0:10:28] I had some background knowledge. At the beginning of my time at ETH, I [was running] a weekly working seminar. We would present interesting problems to one another, and my students would report on the progress they were making in solving some of them. That's how our work on spin glasses got started.

PC: Do you remember which papers in particular? Were these the works of Parisi or earlier works?

JF: [0:11:00] I'm not sure anymore, but I think I already heard about spin glasses when I was at Princeton. The Edwards-Anderson spin glass was something that was discussed. So, I had some idea about what these spin glasses were about and what some of the interesting problems were. The Parisi solution, I'm not sure anymore when I first heard about it. Maybe at a Cargèse school. I think Parisi mentioned that at Cargèse[25], if I remember correctly. Originally, my first attempts were directed towards understanding spin glasses in finitely many dimensions; Ising spin glasses with short-range couplings, that was my main interest, initially. These

[22] J. Fröhlich, "Mathematical Aspects of the Physics of Disordered Systems," In: *Critical Phenomena, Random Systems, Gauge Theories*, K. Osterwalder and R. Stora, eds. (Amsterdam: Elsevier, 1986), 725-893.

[23] Institut des Hautes Études Scientifiques: https://en.wikipedia.org/wiki/Institut_des_Hautes_%C3%89tudes_Scientifiques

[24] Anton Bovier, *Disordered systems and random geometry: polymers, spin glasses, interfaces*, Doctoral Dissertation, Swiss Federal Institute of Technology (ETH) Zürich (1986).

[25] Possibly: Cargèse Summer Institute: Recent Developments in Gauge Theories, 26 August-8 September 1979, Cargèse, France. Proceedings: *Recent Developments in Gauge Theories*, G. 't Hooft, C. Itzykson, A. Jaffe, H. Lehmann, P. K. Mitter, I. M. Singer, R. Stora eds. (New York: Plenum Press, 1980). https://doi.org/10.1007/978-1-4684-7571-5

models have similarities with certain lattice gauge theories. And lattice gauge theory was very much on my mind during that period.

PC: Could you elaborate a bit on that point? What similarities were you seeing?

JF: [0:12:19] You can view the random exchange couplings in a spin glass as frozen configurations of a lattice gauge field. There is a gauge invariance: You can gauge transform the couplings and simultaneously gauge transform the spins on the sites and that leaves the equilibrium measure of the spin glass invariant. Of course, in lattice gauge theory, you integrate over the gauge fields, which means you look at annealed models, while in the spin glass problem the gauge fields are quenched. So, obviously these are two different ball games. Nevertheless, some of the formal aspects of these two problems are somewhat similar, and this played a certain role in my getting interested in the spin glass problem. In fact, in my work with Bovier[26], the idea of gauge invariance played a fairly significant role.

PC: As you mentioned yourself, you moved from finite-dimensional or short-range spin glasses to long-range spin glasses when you started working with a postdoc at ETH, Bogusław Zegarliński[27]. Can you explain that transition to us?

JF: [0:13:57] I do not remember the details, but we had lots of discussions, and, at some point, Zegarliński told me he was interested in spin glasses, and he had an idea of how to deal with long-range spin glasses. So, we started to work on it. I think we found some fairly nice results on long-range spin glasses[28]. Then, at some point, we noticed that some of our ideas would also apply to the Sherrington-Kirkpatrick model, so we wrote a paper on our insights[29]. In fact, our paper was perhaps the first verification that there really is a phase transition in the Sherrington-Kirkpatrick model. However, our methods did not reveal the very tantalizing low-temperature structure of the equilibrium states. It just showed that there *is* a phase transition. It gave insight into the high-temperature regime, of course; but that's much easier than the low-temperature regime.

[26] See, *e.g.*, A. Bovier and J. Fröhlich. "A heuristic theory of the spin glass phase." J. Stat. Phys. **44**, 347-391 (1986). https://doi.org/10.1007/BF01011303

[27] "Bogusław Zegarliński," *Mathematics Genealogy Project* (n.d.). https://www.mathgenealogy.org/id.php?id=218063 (Accessed June 6, 2023.)

[28] J. Fröhlich and B. Zegarlinski, "The disordered phase of long-range Ising spin glasses," *Europhys. Lett.* **2**, 53 (1986). https://doi.org/10.1209/0295-5075/2/1/008; "The high-temperature phase of long-range spin glasses," *Commun. Math. Phys.* **110**, 121–155 (1987). https://doi.org/10.1007/BF01209020

[29] J. Fröhlich and B. Zegarlinski, "Some comments on the Sherrington-Kirkpatrick model of spin glasses," *Commun. Math. Phys.* **112**, 553-566 (1987). https://doi.org/10.1007/BF01225372

PC: Is there a specific way in which your expertise that you had built up to that point helped you to attack this problem? Or was this a new set of tools altogether?

JF: [0:15:19] I was familiar with many methods in statistical mechanics, in particular cluster expansions, high-temperature cluster expansions, low-temperature expansions, the Peierls argument, the Pirogov-Sinai [theory][30], and so on. I also knew about correlation inequalities. I had a decent background in analysis and a somewhat less decent background in probability theory. But, in any event, I had enough tools in my bag to think that we could successfully work on such problems. I have to say [that] very often in my professional life I came up with problems and with some ideas of how to attack them; but I was fortunate to have good collaborators who would then tighten the screws and come up with technical ideas that I would probably never have found. Zegarliński was one of these younger collaborators. He is a very strong mathematician, and when he was in my group at ETH, he had lots of good technical ideas about our projects that played a pivotal role in our joint work.

PC: As you mentioned earlier, that work proved the existence of a phase transition but mainly provided information about the high-temperature phase. Was the Parisi solution of any help or intuition in devising your approach? Or was this completely independent?

JF: [0:16:53] This was, if I remember correctly, completely independent. Although we had heard about Parisi's very imaginative ideas, which were sort of mysterious and quite fascinating, we really didn't understand what they meant. Our work had essentially nothing to do with Parisi's work. The first time I had an extended discussion with Parisi was in '83, namely after a conference at ETH commemorating the 25th anniversary of Wolfgang Pauli[31]'s death. I suggested to invite Parisi as a lecturer at that conference. Thus, he got invited and attended. He stayed for an extra day or two, and then the main subject of the discussion was his ideas about the Sherrington-Kirkpatrick model and how they could possibly be made precise with the help of concepts borrowed from operator algebras with continuous dimensions (type-II von Neumann algebras). Unfortunately, I think, I was not sufficiently intelligent and not sufficiently motivated to

[30] See, *e.g.*, S. Friedli and Y. Velenik, "Pirogov–Sinai Theory," In: *Statistical Mechanics of Lattice Systems: A Concrete Mathematical Introduction* (Cambridge: Cambridge University Press, 2007), 346-408. https://doi.org/10.1017/9781316882603.008 https://www.unige.ch/math/folks/velenik/smbook/Pirogov-Sinai_Theory.pdf (Accessed June 6, 2023.)

[31] Wolfgang Pauli: https://en.wikipedia.org/wiki/Wolfgang_Pauli

actually really look into the details of the Parisi solution. That never changed, unfortunately.

PC: How was your work received at the time?

JF: [0:18:28] You see, there was a competing paper. To the best of my knowledge, our efforts were completely independent. The competing paper was written by Michael Aizenman, Joel Lebowitz and David Ruelle[32]. They also showed that there was a phase transition in the Sherrington-Kirkpatrick model. I tend to think we got a somewhat more detailed result than they did, but the main result in both papers was the existence of this phase transition. Since Aizenman, Lebowitz[33] and Ruelle[34] were big shots, I think their paper got much more widely known than ours. In fact, it's a little ironic: I have a colleague here in Zürich, Erwin Bolthausen[35], who also did quite a lot of work and spin glasses. He wrote a review wherein he didn't quote us[36]. Not very long ago, I told him that I thought it was a little strange that he didn't quote us, since, after all, we were almost next-door scientists in Zürich. He then said that he had never heard about our paper. So, I think it didn't become very well known.

PC: You knew David Ruelle from IHES, and you knew Michael Aizenman[37] because you had worked with him. Were you completely unaware of each other's work?

JF: [0:20:01] To the best of my knowledge, absolutely unaware; for, I was here in Zürich, and Zegarliński was with me, and we didn't hear about their

[32] M. Aizenman, J. L. Lebowitz and D. Ruelle, "Some rigorous results on the Sherrington-Kirkpatrick spin glass model," *Commun. Math. Phys.* **112**, 3–20 (1987). https://doi.org/10.1007/BF01217677

[33] 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

[34] P. Charbonneau, *History of RSB Interview: David Ruelle*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 4 p. https://doi.org/10.34847/nkl.5330p51b

[35] P. Charbonneau, *History of RSB Interview: Erwin Bolthausen*, transcript of an oral history conducted 2022 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 14 p. https://doi.org/10.34847/nkl.21be1l67

[36] See, *e.g.*, E. Bolthausen, "On the proof of the Parisi formula by Guerra and Talagrand, dans Séminaire Bourbaki,": **2004/2005**, exposés 938-951, Astérisque, no. 307 (2006), Exposé no. 948, pp. 349-377. http://www.numdam.org/item/SB_2004-2005__47__349_0/ Contrast with: E. Bolthausen, "Random Media and Spin Glasses: An Introduction into Some Mathematical Results, and Problems" In: *Spin Glasses*, E. Bolthausen and A. Bovier, eds. (Berlin: Springer, 2007). https://doi.org/10.1007/978-3-540-40908-3_1

[37] P. Charbonneau, History of RSB Interview: Michael Aizenman, transcript of an oral his- tory conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 16 p. https://doi.org/10.34847/nkl.dfd42521

attempt until their paper came out, which was when our paper was already essentially finished. So, our efforts were really totally independent.

PC: Afterwards, you largely left the field of spin glasses. What other problems drew you away?

JF: [0:20:33] Yes, but I actually maintained an interest in the replica method and the related "supersymmetry method." In the mid-'80s, I got interested in braid statistics[38], anyons[39], etc. My interest was motivated by my desire to understand the quantum Hall effect. I developed some pretty good ideas about braid statistics. In the spring of 1987, I had a sabbatical which I spent at IHES. Luckily Vaughan Jones[40] was there. I knew him from my time when I was a permanent professor at IHES. He was then a PhD student of André Haefliger[41] in Geneva. André told him he should occasionally visit the IHES and talk to Alain Connes[42] about mathematics. When I met him again at the IHES in 1987, I discovered that he knew a great deal about braids and Yang-Baxter matrices, and so on. He was a wonderfully generous person. He gave me all his seminar notes, and that helped me a lot to work out my ideas about braid statistics. Of course, I knew that this would also be relevant for two-dimensional conformal field theory. Thus, during the next several years, I mostly spent working on braid statistics, super-selection sectors in two- and three-dimensional quantum field theories[43], and, well, applications to constructing invariants for knots and links. If I may say — and this is probably not so well known, but it's a fact — I was most probably first in having the idea that Chern-Simons theory would yield invariants for knots and links[44]. When I started to calculate, unfortunately, I encountered some problems in my calculation; and then Edward Witten's paper about Chern-Simons theory and invariants for knots and links appeared, and it sort of stole me the show[45]. But that can happen, and it is not a tragedy. In any event, I then went on with my calculations and, again, thanks to meeting somebody who was technically

[38] Braid statistics: https://en.wikipedia.org/wiki/Braid_statistics
[39] See, *e.g.*, J. Fröhlich and P.-A. Marchetti, "Quantum field theory of anyons," *Lett. Math. Phys.* **16**, 347-358 (1988). https://doi.org/10.1007/BF00402043
[40] Vaughan Jones: https://en.wikipedia.org/wiki/Vaughan_Jones
[41] André Haefliger: https://en.wikipedia.org/wiki/Andr%C3%A9_Haefliger
[42] Alain Connes: https://en.wikipedia.org/wiki/Alain_Connes
[43] See, *e.g.*, J. Fröhlich, F. Gabbiani, P.-A. Marchetti, "Superselection structure and statistics in three-dimensional local quantum theory," In: *Knots, Topology, and Quantum Field Theories*, L. Lusanna, ed. (Singapore: World Scientific, 1989). https://doi.org/10.1142/9789814540742
[44] J. Fröhlich and C. King, "The Chern-Simons theory and knot polynomials," *Commun. Math. Phys.* **126**, 167-199 (1989). https://doi.org/10.1007/BF02124336
[45] E. Witten, "Quantum field theory and the Jones polynomial," *Commun. Math. Phys.* 121, 351-399 (1989). https://doi.org/10.1007/BF01217730

stronger than I am, Christopher King[46] of Northeastern, we managed to push to the end the strategy I had envisaged to solve this problem. So, that's really what happened.

For some time, I left statistical mechanics in favour of fundamental quantum theory, conformal field theory and the like.

PC: Did you not have a program on spin glasses to pursue that went beyond the analysis of the high-temperature phase of the Sherrington-Kirkpatrick model?

JF: [0:24:25] Not really, no — except that my work with Bovier really was about low-temperature properties of short-range spin glasses (!). The idea that there can be systems in statistical mechanics that exhibit a very complicated landscape of equilibrium states and ground states, that was an idea that was already circulating in the community, and I found it interesting, but I did not pursue it. First, I did not think that the spin glass problem was particularly important. I simply did not find it terribly interesting. And second, it seemed to me that I didn't have a chance to make a really useful contribution, so I didn't pursue it. You probably know Marc Mézard[47]. He was approached when Parisi got the Nobel Prize. One of his comments was that the SK model is largely uninteresting, but that, in his opinion, the *methods* developed to solve and understand it, are unbelievably interesting. That also summarizes my own feelings.

PC: Did you nevertheless keep abreast of the advances in the field after you left, or was this a sharp cut?

JF: [0:25:43] I still occasionally listened to people. I'm sure I must have heard Parisi's story quite a few times, and in different places. At a meeting in Bielefeld (in honour of my friend Philippe Blanchard[48]), I heard Francesco Guerra[49] talk about his progress in understanding the mathematics of the Parisi solution. Then I heard Michel Talagrand at a meeting on the Monte Verità near Ascona, where there is a conference centre. Talagrand was

[46] R. Shorten, "In Memoriam: Professor Christopher King," *Imperial College London* (22 March 2023). https://www.imperial.ac.uk/news/243874/in-memoriam-professor-christopher-king/ (Consulted June 6, 2023.)

[47] Marc Mézard: https://en.wikipedia.org/wiki/Marc_M%C3%A9zard

[48] Philippe Blanchard: https://en.wikipedia.org/wiki/Philippe_Blanchard

[49] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Francesco Guerra*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 27 p. https://doi.org/10.34847/nkl.05bd6npc

invited to give some lectures there[50]. It was certainly very interesting to listen to him, although it seemed to me that his work was technically well above what I usually do.

PC: In your selecta, you wrote that, if you could, you would rather "exploit the huge potential of the methods of Parisi and his followers […] for applications to problems in other fields" rather than work again on various versions of the SK model, or the Edwards-Anderson model[51]. Can you please expand a bit on your reasoning for that impression?

JF: [0:26:46] I didn't remember that I wrote such a thing, but you apparently remind me of something I forgot. My opinion goes in the same direction as Mézard's remark. Replica symmetry breaking and the cavity method and the ultra-metric structure of the space of equilibrium states of the SK model and so on, these are ideas which have apparently found or will find lots of interesting applications to other problems. I'm glad that people have worked this out and are pushing this further, but I'm not one of them.

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

JF: [0:27:37] No, I have not given a single talk about replica symmetry breaking. I've been interested in the replica method[52] and also in a cousin of it, namely, to introduce Grassman variables and make the model under scrutiny supersymmetric, in order to calculate quenched averages. That has really been of interest to me, and, in some other work, I have applied such ideas, but at a technical level much more modest than what happened in the SK model.

PC: Did you give lectures, for instance, about your work on the high-temperature phase before you left, or was this just a paper?

JF: [0:28:26] Did I give lectures? I would not exclude that I have given some seminars about my work with Zegarlinski, in particular. Actually, I gave a

[50] International Conference on Equilibrium and dynamics of spin glasses, A. Bovier and E. Bolthausen, 18-23 April 2004, Centro Stefano Franscini, Monte Verità, Ascona, Switzerland. See, e.g., Annual Research Report 2004 of Weierstraß-Institut für Angewandte Analysis und Stochastik (Berlin: WIAS, 2005). https://www.wias-berlin.de/annual_report/2004/fb04wias.html (Consulted October 26, 2021)

[51] J. Fröhlich, "Introduction," In: *A Journey Through Statistical Physics: From Foundations to the Quantum Hall Effect – Selecta of Jürg Fröhlich*, G. Felder, G. M. Graf and K. Hell, eds. (Dordrecht: Springer, 2009): xv-xliii.

[52] See, *e.g.*, F. Constantinescu, J. Fröhlich and T. Spencer, "Analyticity of the density of states and replica method for random Schrödinger operators on a lattice," J. Stat. Phys. **34**, 571-596 (1984). https://doi.org/10.1007/BF01018559

seminar about my work with Bovier at Bell labs. There was a young guy in the audience whose name is David Huse[53], and after my talk he told me: "You know, whenever I hear about spin glasses, it seems to me nobody understands anything about them." But not long afterwards he started to work, with Daniel Fisher[54], on the spin glass problem. Two people who made some really relevant contributions that went beyond what Bovier and I did were Chuck Newman and Dan Stein[55]. They did some very nice work. In fact, I have a lot of respect for Chuck Newman. I don't know Stein, but I know Chuck Newman quite well. I know he also did a lot of very nice work on SLE[56] in recent years.

PC: Is there anything else you would like to share with us about that era which we may have missed or overlooked?

JF: [0:30:00] The era of [my] work on spin glasses? No! All I could add is [that], for a while, I was at a research institute, namely the IHES, working primarily with Tom Spencer, but also just by myself, without collaborators. If I had stayed there, I would never have touched spin glasses. It was just the need to have nice problems for PhD students that made me recommend some of these problems to my younger friends and work on some of these problems myself. But I confess that I did not find these questions particularly profound or fundamental for physics. I still think that's the right view. I think the methods invented to understand spin glasses are more interesting for use in fields that are not directly related to physics, for example to solve certain optimization problems, operations research and so on. And, at heart, I am a physicist. I like to work on deep problems of physics, and spin glasses did not look like a particularly profound problem of physics.

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

JF: [0:31:40] If I move the camera with my computer a little, you may see what large number of notebooks and how much literature is on the bookshelves in my home office. I stock thousands of transparencies (slides) and thousands of pages of notes. I have notes of all my lectures at ETH,

[53] David A. Huse: https://en.wikipedia.org/wiki/David_A._Huse
[54] See, *e.g.*, D. S. Fisher and D. A. Huse, "Ordered phase of short-range Ising spin-glasses," *Phys. Rev. Lett.* **56**, 1601 (1986). https://doi.org/10.1103/PhysRevLett.56.1601
[55] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Charles M. Newman and Daniel L. Stein*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 35 p. https://doi.org/10.34847/nkl.3dbc3ja3
[56] Schramm–Loewner evolution: https://en.wikipedia.org/wiki/Schramm%E2%80%93Loewner_evolution

probably 4000 pages or more, everything unpublished. I'm pretty sure if I went through all the stuff that is on my bookshelves, I might find some old notes on spin glasses, but I am not able to locate them right now. I did not have plans to make them public. I sometimes played with the idea of perhaps scanning the notes of my lectures at ETH. I think I have some rather nice notes on quantum field theory, quantum mechanics, statistical mechanics and so on. But so far, it's all in the private domain—not public—and with former students.

PC: Will ETH take your archives, your papers, or not?

JF: [0:33:04] I have no idea. I have never asked them. Nowadays, I could scan them. I have been a little too lazy to stand at the scanning machine and scan all the thousands of pages, but maybe I should do it at some point. But I'm not sure that they will attract much interest. There is too much literature circulating through the Internet nowadays, and much of it is garbage; so, I don't want to contribute to that.

PC: Prof. Fröhlich, thank you very much for this conversation.

JF: [0:33:40] It was a pleasure to discuss with you. I hope you have heard what little I can contribute to this particular subject.