# History of RSB Interview: Erwin Bolthausen

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**Interviewers:**
Patrick Charbonneau, Duke University, patrick.charbonneau@duke.edu
Francesco Zamponi, ENS-Paris

**Location:**
Over Zoom, from Prof. Bolthausen's home in Elsau, Switzerland.

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**PC:** Good morning, Prof. Bolthausen. Thank you very much for joining us. As we've discussed ahead of this interview we'll mainly be going over ideas surrounding spin glasses and replica symmetry breaking, which we bound roughly from 1975 to 1995, but with a bit of a flexibility on these dates. Before we dive into this matter, and in order to help situate your contributions, can you tell us what first interested you in mathematics and what led you to pursue graduate studies in probability and statistics?

**EB:** [0:00:37] Really, originally, I wanted to study physics at ETH. But at ETH physics and mathematics, for the first year, are completely parallel, and so I switched to mathematics. At that time, when you studied mathematics at ETH in Zürich, there was a lot of physics to take. You'd take a lot of physics courses. So I took quite a number of physics courses during my math studies. Originally, I was more interested into abstract mathematics—algebra, logic—so that was my focus. It depends also on the teacher you have. My main teacher at ETH in mathematics was Beno Eckmann[1], the father of Jean-Pierre Eckmann[2]. I also made my thesis with him on relatively abstract algebra[3], but then I wanted to switch to something more applied. Originally, I wanted to do some really applied statistics, but then I was hooked with probability theory. That was my

[1] Beno Eckmann: https://en.wikipedia.org/wiki/Beno_Eckmann
[2] Jean-Pierre Eckmann: https://en.wikipedia.org/wiki/Jean-Pierre_Eckmann
[3] Erwin Bolthausen, *Einfache Isomorphietypen in lokalisierten Kategorien und einfache Homotopietypen von Polyeder* [Simple isomorphy in localized categories and simple homotopy types of polyhedra], PhD Thesis, Eidgenössische Technische Hochschule Zürich (1974). https://doi.org/10.3929/ethz-a-000153723

starting point in probability theory. That was a postdoc position at the university In Konstanz.

**PC:** Before we move along, where did your interest in physics come from? Why did you first want to do physics?

**EB:** [0:02:24] That dates back [to] high school. It's difficult to say. In high school[4], originally, I wanted to study chemistry. Then, this high school had fantastic labs and I actually had a very good chemistry teacher. We could, in the free days, go into the lab and just make experiments ourselves. (I think today it would be totally forbidden to do this.) But then I realized that I'm not so good at experiments. Then I became more interested in physics. We also had an excellent physics teacher.

**PC:** It was mainly through your exposure at school, then.

**EB:** [0:03:20] Yeah.

**PC:** This may be an overly crude picture, but from what I could tell over the first couple of decades of your career you seem to have moved to an ever greater engagement with problems in mathematical physics. What drew you in that direction, in general?

**EB:** [0:03:43] Ok. I was working more on classical limit theorems, and had some papers in more classical stuff[5]. Then I became interested in large deviation theory[6]. That was a time when Donsker[7] and Varadhan[8] were publishing their papers on large deviations. I started to read them and got hooked to large deviation. Obviously, large deviation is very much connected with mathematical physics, in particular with mean-field—type models. I was totally hooked. There is this polaron problem. (I don't know if you're familiar with that.) Donsker and Varadhan had this fantastic paper applying large-deviation theory to this polaron problem[9]. So I got interested in this

[4] Alte Kantonsschule Aarau: https://en.wikipedia.org/wiki/Old_Cantonal_School_Aarau

[5] See, *e.g.,* E. Bolthausen, "On a functional central limit theorem for random walks conditioned to stay positive," *Annals Prob.* **4**, 480-485 (1976). https://www.jstor.org/stable/2959252; "On the central limit theorem for stationary mixing random fields," *Annals Prob.* **10**, 1047-1050 (1982). https://www.jstor.org/stable/2243560

[6] See, *e.g.*, E. Bolthausen, "Laplace approximations for sums of independent random vectors," *Prob. Theo. Relat. Fields* **72**, 305-318 (1986). https://doi.org/10.1007/BF00699109; "Markov process large deviations in τ-topology." *Stoch. Process. Their Appl.* **25**, 95-108 (1987). https://doi.org/10.1016/0304-4149(87)90192-X

[7] Monroe D. Donsker: https://en.wikipedia.org/wiki/Monroe_D._Donsker

[8] S. R. Srinivasa Varadhan: https://en.wikipedia.org/wiki/S._R._Srinivasa_Varadhan

[9] M. D. Donsker and S. R. S. Varadhan, "Asymptotics for the polaron," *Comm. Pure and Appl. Math.* **36**, 505-528 (1983). https://doi.org/10.1002/cpa.3160360408

kind of things more and more. Also, their paper on what is called the Wiener sausage[10], which is also strongly connected with this topic in mathematical physics. So I got more and more driven in this direction. It was not dependent on a particular teacher I had in Konstanz.

Konstanz is not very far from Zürich. We also regularly went to Zürich for seminars. The activities in Zürich were more intensive than in Konstanz. It's just one hour drive to Zürich. At the time Hans Föllmer[11] was in Zürich, and we went to his seminars. He was also interested in statistical physics at that time. So it developed in this way.

A bit later, I got interested also in random media. (That was quite [a bit] later.) I wrote this paper on directed polymers in random environments[12]. I was just driving in this direction. It was totally natural that I became interested in spin glasses and the Sherrington-Kirkpatrick model. That was unavoidable when I came into contact with that.

**PC:** When did you actually first hear about spin glasses? What drew you to that problem at that point?

**EB:** [0:06:34] That is difficult to say. I don't know exactly. After Konstanz, I had for a short time a position in Frankfurt. In Frankfurt, I had also close contact with Hermann Rost[13], who was also very much interested in mathematical physics. After that, I had a position in Berlin. In Berlin, I had close contact with some people really from theoretical physics, mathematical physics, like Robert Schrader[14], for instance. (I guess he's very well known in theoretical physics. He died a couple of years ago.) He also gave me papers. He was more knowledgeable about this background. He showed me results, for instance, by John Imbrie and Thomas Spencer about directed polymers[15]. I was able to make a contribution to that [problem] after looking at that [work].

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[10] M. D. Donsker and S. R. S. Varadhan. "Asymptotics for the Wiener sausage," *Comm. Pure and Appl. Math.* **28**, 525-565 (1975). https://doi.org/10.1002/cpa.3160280406; E. Bolthausen, "On the volume of the Wiener sausage," *Annals Prob.* **18**, 1576-1582 (1990). https://www.jstor.org/stable/2244335

[11] Hans Föllmer: https://en.wikipedia.org/wiki/Hans_F%C3%B6llmer

[12] E. Bolthausen, "A note on the diffusion of directed polymers in a random environment," *Comm. Math. Phys.* **123**, 529-534 (1989). https://doi.org/10.1007/BF01218584

[13] "Hermann Rost," *Mathematics Genealogy Project* (s.d.). https://www.genealogy.math.ndsu.nodak.edu/id.php?id=24634 (Accessed June 2, 2022.)

[14] "Robert Schader," *Mathematics Genealogy Project* (s.d.). https://www.genealogy.math.ndsu.nodak.edu/id.php?id=93917 (Accessed July 18, 2022.)

[15] J. Z. Imbrie and T. Spencer, "Diffusion of directed polymers in a random environment," *J. Stat. Phys.* **52**, 609-626 (1988). https://doi.org/10.1007/BF01019720

I don't really know where I first came into contact with spin glasses, but it was totally natural because I was interested in mean-field models from my interest in large deviations and I became more and more interested in random media and also random walks in random media. It was somehow evident that I should fall one day on the Sherrington-Kirkpatrick [model]. But I cannot exactly fix the date when I first came into contact.

**PC:** Michael Aizenman mentioned that he might have steered you in this direction[16]. Do you recall that discussion?

**EB:** [0:08:36] Yes. That could well have been Michael Aizenman. After Berlin, I was in Zürich. I came to Zürich at the end of the '80s, and then I met Michael Aizenman several times. It could well be that it was he who brought me to that. I read his paper. This was one of the earliest rigorous papers on spin glass theory, this Aizenman-Lebowitz-Ruelle [paper][17]. That, I studied carefully, but I'm not sure that Michael showed me that or that I came across [it] before. You see, when I was in Zürich, Alain Snitzman[18] was interested in it. And I had very close contacts with Alain Snitzman, so we got interested in spin glass theory. It was particularly attractive for me, as there wasn't much mathematical theory around at that time. That is related with the fact that I am typically not so good with studying complicated theory topics on which there is a huge amount of existing literature and you first have to read that before you can do anything. Probably, I had the impression maybe one can do something without spending two years of learning theory before[hand]. That might have been a motivation also. But then I was very much interested in the paper by Aizenman-Lebowitz-Ruelle. And there was a parallel paper by Fröhlich-Zegarlinski at that time with [a] somehow similar flavor, [but] a little bit in another direction[19]. That was it.

**PC:** Michel Talagrand told us that he learned about spin glasses from you, at a meeting in 1993[20]. Why did you think that the problem would be appropriate or interesting to him? Or was it a topic that you were discussing with many other people at that time?

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[16] P. Charbonneau, *History of RSB Interview: Michael Aizenman*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 21 p. https://doi.org/10.34847/nkl.dfd42521

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

[18] Alain-Sol Szitman: https://en.wikipedia.org/wiki/Alain-Sol_Sznitman

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

[20] P. Charbonneau, *History of RSB Interview: Michel Talagrand*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 20 p. https://doi.org/10.34847/nkl.daafy5aj

**EB:** [0:11:25] No, no! Actually, I listened to the interview with Michel, and to what he said about that. Michel probably was not aware that I had a very good reason for asking him about it. Of course, he was already well known for theories about general Gaussian processes. One of the key results he had – he solved a relatively well-known problem, this dominating measure criterion for Gaussian processes[21] – that was initially done by Fermique[22]. Fermique gave a criterion. It was on several issues of Gaussian processes: a criterion for continuity, characterization of the maximum in terms of an entropy condition. It had a long history, the whole topic, but Fermique gave this criterion, which is called dominating measure criterion. It was proved by Fermique to be sufficient, for instance, to prove that certain processes are bounded. It was a difficult thing to prove that it was also necessary—I think many people thought it was not true, but finally Talagrand proved that it was also necessary. It was actually equivalent to certain properties of Gaussian processes. This proof by Talagrand, I had studied [it] carefully.

This proof is about the same problems as you are facing in the Sherrington-Kirkpatrick because it's a criterion for finiteness of the maximum after some normalization. Then, I realized that the proof was very much based on ultrametricity. It is very general, but it was really based on a proof that you can approximate things by ultrametricity. Then, I looked at it for Sherrington-Kirkpatrick, and for Sherrington-Kirkpatrick it was useless. The theory is very general, but you couldn't characterize constants. For instance, you could give the maximum only up to constants, so you could never prove anything like the Parisi formula for the maximum [free energy] of Sherrington-Kirkpatrick. It was evident after I looked carefully at it. But still I thought maybe one can do some refinement of this ultrametric approximation.

I tried it a little bit myself, of course, without success, but then I thought it was evident that Talagrand would be the right person to ask about that. Then I met him at this conference in Arhus, so I explained it to him. He immediately got hooked. He contacted me about the background literature and things like that. That was the way I came to that, to ask Talagrand. Maybe it was the most influential 20 minutes of *his* life.

**PC:** In the preface to your book on spin glasses you mentioned the 1996 workshop in Berlin that brought together the mathematicians interested

[21] M. Talagrand, "Regularity of Gaussian processes," *Acta Mathematica* **159**, 99-149 (1987). https://doi.org/10.1007/BF02392556

[22] Xavier Fermique: https://en.wikipedia.org/wiki/Xavier_Fernique

in spin glasses[23]. Were you present at that meeting[24]? If yes, can you tell us a bit more about the importance of that meeting?

**EB:** [0:16:03] You said 1996 in Berlin?

**PC:** Yes.

**EB:** [0:16:12] I never organized a workshop on spin glasses.

**PC:** You were not an organizer. It was organized by Bovier with Picco.

**EB:** [0:16:23] No. I was not present there. I was not present at this conference. With Bovier, I organized one meeting in Ascona[25], but that was considerably later.

**PC:** What was your involvement, then, with the community after having talked to Michel Talagrand? Did you regularly read the literature and attend meetings?

**EB:** [0:16:55] I followed. I have written a couple of papers even more recently about topics in spin glasses. I followed it very closely. I was very much interested in it. I organized the meeting with Bovier in Ascona. That was exactly at the time when Talagrand just had a rigorous proof of the Parisi formula. We had a meeting… You know Monte Verità?

**PC:** It was the 2004 meeting, I think.

**EB:** [0:17:49] Yes. That was a 200[4] meeting. We essentially got (not everybody but) many of the important actors in spin glass theory. We were able to get to Ascona: Talagrand, Guerra, Parisi, Aizenmann, Fröhlich[26]. I believe it had some influence on the cooperation between physicists and mathematicians because both sides were present there. It was exactly at the time when there was big progress. The paper by Guerra had already appeared[27], and Talagrand had announced his results but the paper was

[23] " In 1996 a workshop in Berlin brought together the leading experts in the field." Erwin Bolthausen and Anton Bovier, "Preface," In: *Spin Glasses*, Erwin Bolthausen and Anton Bovier, eds. (Berlin: Springer-Verlag, 2007), iii-iv.

[24] *Workshop 'Mathematics of spin systems with random interactions,'* A. Bovier and P. Picco, Weierstrass Institute for Applied Analysis and Stochastics, Berlin, Germany, August, 20-24, 1996.

[25] *Equilibrium and dynamics of spin glasses*, E. Bolthausen and A. Bovier, Centro Stefano Franscini, Monte Verità, Ascona, Switzerland , April 18-24, 2004.

[26] Jürg Fröhlich: https://en.wikipedia.org/wiki/J%C3%BCrg_Fr%C3%B6hlich

[27] F. Guerra, "Broken replica symmetry bounds in the mean field spin glass model," *Comm. Math. Phys.* **233**, 1-12 (2003). https://doi.org/10.1007/s00220-002-0773-5

not yet available[28]. It was just at that time. I think it was somehow an influential meeting.

**PC:** It was timely, for sure. Can you describe a bit more about the excitement that was emerging from this moment? Was there really communication between the two groups? Was there a common language?

**EB:** [0:19:11] Yes. I think so. I think Talagrand had contacts with Parisi. He had a lot of discussions with Michael Aizenmann at this meeting. Michael was really skeptical about the way Talagrand was attacking this problem, I would say. Michael told me [so], but Talagrand was also a little annoyed by the skepticism [of] Michael Aizenmann. The paper was not yet out, so there was a little bit of tension between the main actors. But I think they communicated quite closely. Michael Aizenmann with Guerra[29]… Jürg Fröhlich was also there and he communicated a lot with other people, so I think it had some influence on the cooperation between these players in this field.

At that time, my only contribution was this paper with Alain Sznitman on this abstract cavity method[30]. That's a little bit a strange paper—the paper with Alain Sznitzman—in the sense that is extremely highly cited but actually not for [its] rigorous contribution to spin glass, but because we invented what is called now Bolthausen-Sznitman coalescent, which came out from a close investigation of the Ruelle cascades. That's the reason the paper had a lot of influence, but not in spin glass theory. The main point of this paper is that we tried to understand the mathematical structure exactly behind this cavity method as it appears in the Sherrington-Kirkpatrick model. That was the main aim. In this sense, it was successful. We really set up exactly the mathematical structure of this cavity method. That was the main aim of this paper. At that time, that was my only contribution to spin glass theory.

**PC:** In your role as editor of *Probability Theory and Related Fields,* from 1994 to 2000[31], you did let a number of papers on the topic appear. Were you

[28] M. Talagrand, "The Parisi formula," *Annals Math.* (2006): 221-263. https://www.jstor.org/stable/20159953 **PC:** The manuscript was submitted to the journal on May 13, 2003. It was not posted to arXiv, but might have nevertheless been circulating.

[29] 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

[30] E. Bolthausen and A.-S. Sznitman, "On Ruelle's probability cascades and an abstract cavity method," *Comm. Math. Phys.* **197**, 247-276 (1998). https://doi.org/10.1007/s002200050450

[31] Probability Theory and Related Fields (PTRF): https://en.wikipedia.org/wiki/Probability_Theory_and_Related_Fields

actively encouraging these contributions? If yes, can you detail how this went about?

**EB:** [0:22:52] I had close contact with Michel Talagrand then. Also, I encouraged him to publish some of his papers in PTRF. Of course, Talagrand had his first papers then written, which was still everything in high temperatures. These were just, at the beginning, in high temperatures. It's probably not so much known that Talagrand… Because the topic of this interview is mainly about replica symmetry breaking… It's not so much known—because the paper is largely forgotten to some extent—that the first rigorous result by Talagrand on replica symmetry breaking is an early paper he published in PTRF[32]. He was able to do something not for the Sherrington-Kirkpatrick, but *p*-spin models, which have one level of symmetry breaking. The paper had some shortcomings in the sense that it was not totally unconditional. He couldn't really fully prove what he wanted to prove, but it depended on some assumptions which were plausible. He could remove that a little later, but the paper he published first in PTRF contains an abstract version of how to construct these pure states. Then he had proof really that these pure states exist and he gave the complete characterization of the distribution in terms of these Ruelle cascades. Of course, it was only one level of symmetry breaking. He gave the Gibbs states etc., but he had to assume a condition, which was relatively weak. The referees didn't vote to have the paper [published]. They recommended not to take it because it was only a conditional result. I then read it myself to a large extent and I took it despite the negative comments by the referees. I think this was really the first result of replica symmetry breaking in a real spin glass. In that sense, I [exerted] some influence. I certainly encouraged Michel to publish many of his papers in PTRF.

**PC:** In the book *Ten Lectures on Random Media*[33], which appeared in 2001, you wrote that "most of mean-field spin glass theory is still very far from a mathematically rigorous understanding". At that time, were you hopeful that such an understanding would be forthcoming? And why did you think that it was important for graduate students and postdocs nevertheless to hear about this problem?

**EB:** [0:26:25] That's a difficult question. Probably among mathematicians nobody was really hopeful that the Parisi formula could be proven. That

[32] M. Talagrand, "Rigorous low-temperature results for the mean field p-spins interaction model," *Probab. Theo. Rel. Fields* **117**, 303-360 (2000). https://doi.org/10.1007/s004400050009
[33] Erwin Bolthausen and Alain-Sol Sznitman, *Ten lectures on random media* (Basel: Birkhäuser Verlag, 2002).

was probably the issue. Even Talagrand told me that. Just as an anecdote. Talagrand said to me—that was before he proved it, and before Guerra had his results, maybe one year before: "The only reason that I have some success in spin glass theory is that I've never tried to prove the Parisi formula." He thought that this is something so fantastic—and also these results with replica symmetry breaking matrices by Parisi—that he didn't have much hope that it could go into this direction. But it's always happening like that. You expect that some problems are too difficult, and then somebody finds a way out. There were actually more modest problems I also know that Talagrand was [working on]. Actually, one of the so-called modest problems is still a mathematically open problem. One needs to prove that for the Sherrington-Kirkpatrick the de Almeida-Thouless is a phase separation line. That is still mathematically open. Of course, after the Parisi formula it's just an analytic problem. There are good results in this direction but it is not fully proven that the AT line is a phase separation line. That is something I had myself tried to prove but I knew that Talagrand was working on that. But that, of course, is quite modest now compared with the other developments we just discussed. Ultrametriciy was also an issue. Everybody—at least I—thought that progress in the direction to understand replica symmetry breaking would come by mathematically understanding ultrametricity. In the end, the first proof by Talagrand didn't use ultrametricity. That was quite a surprise to me actually, that it would go in that direction, that one could prove the Parisi formula before first proving ultrametricity. For that reason, even if at that time I thought that the Parisi formula would be a hopeless thing, I thought that it would be an interesting topic to go on.

**PC:** So graduate students and postdocs should go because of that.. Can you expand a bit on why young people should have learned about this field? Did you see that as a program for the next 30 years and therefore people should get engaged on it?

**EB:** [0:30:32] At that time I had a PhD student, Nicola Kistler[34]. He has done other works in other directions than spin glasses but also related to spin glasses. At that time—that was before the Parisi formula was proven— I had an idea on how to go on with this AT line that was based on this TAP equation approach. (Much later, just six years ago, I published paper on this TAP equation which appeared in CMP which became very influential in other directions[35]). At that time, I had the idea one could use this TAP

[34] "Nicolas Kistler," *Mathematics Genealogy Project* (s.d.). https://www.mathgenealogy.org/id.php?id=107575 (Accessed June 15, 2022.)

[35] E. Bolthausen, "An iterative construction of solutions of the TAP equations for the Sherrington–Kirkpatrick model," *Comm. Math. Phys.* **325**, 333-366 (2014). https://doi.org/10.1007/s00220-013-1862-3

approach to compute the free energy up to the AT line also. Then, I proposed this problem to Nicola Kistler, and he tried to work on it. This AT thing, we couldn't prove. Nicola tried for a long time. Nicola was totally fascinated by spin glasses also. After the first one or two years where we didn't succeed with this AT problem, I proposed to do something else not related to spin glasses. Then, he looked at it and said he could not give up spin glasses. "It's so interesting." Finally, we did something in the direction of ultrametricity[36], which was partly successful, but they were just toy models. I think they are largely forgotten now my papers on ultrametricity, because Panchenko finally did it with the Ghirlanda-Guerra identities[37]. We had some toy models which were related to Derrida's random energy model, but which were not ultrametric from the start, for finite *N*. We could prove ultrametricity in the [thermodynamic] limit for just combinatorial reasons. That was the way I was thinking at that time about it. In a sense it was successful, but after a full proof of ultrametricity it was maybe no longer so interesting. I don't know. Maybe it could still become one day.

**PC:** It feels that there was a certain degree of competition in reaching a proof of the Parisi formula. Was it palpable in the community?

**EB:** [0:34:04] Yes. Very clearly. You could feel it in Ascona, actually. There was some competition about that. You could feel it. That, you could feel between the main players. Certainly, Michael Aizenman had different ideas from the ones of Talagrand. You could feel it in the discussions. Also, everybody was talking with me. I got [in the middle]. Talagrand was commenting about Michael Aizenman. Michael Aizenman was commenting about Talagrand. Guerra was commenting etc. It was relaxed, but you could feel the competition very clearly.

**PC:** From your viewpoint, how important was the physics literature in reaching the mathematical physics proof?

**EB:** [0:35:14] People couldn't do very much with these replica and this replica symmetry breaking ansatz. I know that some people are very much interested to turn it into something rigorous. For instance, Gérard Ben

[36] E. Bolthausen and N. Kistler, "On a nonhierarchical version of the generalized random energy model," *Annals Appl. Probab.* **16**, 1-14 (2006). https://www.jstor.org/stable/25442724; "On a nonhierarchical version of the generalized random energy model, II: ultrametricity," *Stoch. Proc. Appl.* **119**, 2357-2386 (2009). https://doi.org/10.1016/j.spa.2008.12.002

[37] D. Panchenko, "The Parisi ultrametricity conjecture," *Annals. Math.* **177**, 383-393 (2013). https://doi.org/10.4007/annals.2013.177.1.8; S. Ghirlanda and F. Guerra, "General properties of overlap probability distributions in disordered spin systems. Towards Parisi ultrametricity," *J. Phys. A* **31**, 9149 (1998). https://doi.org/10.1088/0305-4470/31/46/006

Arous[38] had some ideas about it. I think it never substantiated. These replica symmetry breaking matrices didn't play a big role in mathematics up to now. People were just looked at it as some kind of miracle. I was once giving a short course in Vancouver on spin glasses[39]. I spent two hours explaining Parisi's derivation of the Parisi formula. After[wards], somebody told me that this is the most non-rigorous thing he had ever heard about. But the cavity approach was of course very important. That was also extremely important for what we had been doing and also [for] Talagrand. You see, Talagrand's first paper on high temperature used a version of the cavity method. He had a huge amount of work on different models where he applied this later. He had papers on the perceptron, he had papers on the *k*-SAT problem, he had papers on the assignment problem. It's all contained in his book also[40]. Many of the things are still open. Talagrand had an enormously complicated derivation of the replica symmetry formula for the perceptron. It takes three chapters in his book. That's essentially all based on the cavity method, on his version of the cavity method. Just recently I have a paper on the perceptron together with Nike Sun, Shuta Nakajima and Changji Xu**,** where we have a different proof that is slight more general that is based on the TAP approach[41]. That's of course very recent that people got more interested in this TAP business. The original paper by Thouless-Anderson-Palmer[42] is just nowadays playing an enormous role in mathematical development. There are very interesting developments on this TAP approach by a number of people in mathematics. Eliran Subag has recent papers on this TAP business which are very interesting and develop insights[43].

**PC:** You mentioned that course in Vancouver that you gave, but at the university of Zürich or elsewhere did you ever teach about spin glasses or

[38] Gérard Ben Arous: https://en.wikipedia.org/wiki/G%C3%A9rard_Ben_Arous

[39] **EB:** The course I gave in Vancouver, in 2004, was during the first summer school in probability there. I had spent a sabbatical in Vancouver, and they asked me to give a short course—about five lectures—but it was announced just locally, and not in the program of the school. See, *Summer School in Probability*, Martin Barlow, Pacific Institute for the Mathematical Sciences, University of British Columbia, Vancouver, Canada, May 25-June 25, 2004. https://www.pims.math.ca/science/2004/ssprob/ (Accessed July 18, 2022.)

[40] Michel Talagrand, *Spin Glasses: A Challenge for Mathematicians: Cavity and Mean Field Models* (Berlin: Springer-Verlag, 2003); *Mean Field Models for Spin Glasses* (Berlin: Springer-Verlag, 2010-2011).

[41] E. Bolthausen, S. Nakajima, N. Sun and C. Xu, “Gardner formula for Ising perceptron models at small densities,” arXiv:2111.02855.

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

[43] See, *e.g.*, E. Subag, “The free energy of spherical pure *p*-spin models--computation from the TAP approach.” arXiv:2101.04352; “TAP approach for multi-species spherical spin glasses I: general theory,” arXiv:2111.07132; “TAP approach for multi-species spherical spin glasses II: the free energy of the pure models.” arXiv:2111.07134.

replica symmetry breaking? If yes, can you detail the context and the content?

**EB:** [0:39:46] Not in Zürich. The idea was when Panchenko's work on ultrametricity came out we made a seminar on that[44], which was quite influential. For instance, David Belius[45] told me that that this seminar we had in Zürich influenced him to go into spin glass theory. He's currently at the University of Basel. He's a young researcher. But I was never teaching in Zürich on spin glasses. I gave a course in Kyoto once[46], and I know at least of one young Japanese researcher who became influenced by this course. That's Shuta Nakajima[47], who just got a permanent position in Japan.

**PC:** Was this recently or in the early 2000s?

**EB:** [0:40:47] It was more recently. I have always difficulty remembering the dates. It was around 2015 [that] I was giving a course. There, I could also present my results on the TAP business and also give the results by Panchenko: how Panchenko proved ultrametricity, and how it can be used to prove the Parisi formula.

**PC:** In the long run, over your career… Some people have told us that the prestige of probably has increased in the math community. Is that your impression as well, and did spin glasses play any role in that change in perception of the subfield?

**EB:** [0:42:00] It's certainly the case. That is clear. You see that there are now Fields medals in probability. Most of the topics—I don't know if I can say that—the mathematicians were most impressed by are somehow related to mathematical physics. These Fields medals[48] went to people working on two-dimensional models which are related to this conformal field theory[49]. (This Schramm–Loewner evolution equations[50] and things like that are

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[44] **EB:** It was announced as "student seminar" for PhD students and postdocs, and we always informed on short notice what we are doing in a particular semester. I couldn't find out exactly in which year it was.
[45] "David Belius," Mathematics Genealogy Project (n.d.). https://www.mathgenealogy.org/id.php?id=169747 (Accessed June 15, 2022.)
[46] *Course on spin glasses*, Erwin Bolthausen, Department of Mathematics, Kyoto University, Kyoto, Japan. Feb. 24, March 11, 19, 23, 25, 2015. https://www.math.kyoto-u.ac.jp/en/event/seminar/2588 (Accessed June 15, 2022.)
[47] "Shuta Nakajima," ResearchGate (s.d.) https://www.researchgate.net/profile/Shuta-Nakajima-2 (Accessed June 15, 2022.)
[48] Fields Medal: https://en.wikipedia.org/wiki/Fields_Medal
[49] See, *e.g.*, Wendelin Werner (Fields Medal 2006): https://en.wikipedia.org/wiki/Wendelin_Werner and Stanislav Smirnov (Fields Medal 2010): https://en.wikipedia.org/wiki/Stanislav_Smirnov
[50] Schramm–Loewner Evolution : https://en.wikipedia.org/wiki/Schramm%E2%80%93Loewner_evolution

related to conformal field theory.) Spin glass theories I think too, [but] maybe to a lesser extent received attention. Maybe if somebody below 40 would have proven the Parisi formula it would have gotten the Fields Medal. That could well be. But it was by Guerra and Talagrand. It's actually even growing to some extent. People in applied statistics became very much interested in spin glass theory, particularly the Stanford school, so [Andrea] Montanari and his students. It's partly influenced by my paper on the TAP equation[51]. It's just [a] high temperature [scheme], but it goes up to the AT line for the Sherrington-Kirkpatrick. I published it, but essentially it was a failure, because I wanted to compute the free energy up to the AT line. Finally, I published this paper just on this TAP iteration which converges up to the AT line. Fortunately, I hadn't read anything in the literature. It was a good thing that I hadn't read anything, because it was well known among people like Parisi that the TAP equation is very unstable even at high temperature. Then, I found the right way to look at it. I published it in CMP. I thought: "Ok. Send it to CMP. Probably, they are not taking it." I was not aware that that solved a long-standing open problem. That somehow came as a surprise to me. Even more surprising was that it had a lot of influence on the development of algorithms and this compressed sensing business. I don't mention it because it is my paper, but just to mention how influential ideas from spin glass theory have become in totally different fields. For instance, in theoretical computer science, I know that [Amin] Coja-Oghlan started to become interested just from theoretical computer [science] problems. He was not coming from mathematical physics. I remember he was giving a talk, and then he didn't yet work on it, but he was totally fascinated by predictions coming from spin glass theory. In the meantime I think he has very good results just influenced by spin glass theory and this whole Parisi approach[52].

**PC:** Is there anything else you'd like to share with us about this era that we may have missed or overlooked?

**EB:** [0:47:08] Nothing really important, I guess. I had made some notes a little bit on this, but I think no.

**PC:** In closing, do you have notes, papers or correspondence from that epoch? If yes, do you intend to deposit them in an academy archive at some point?

[51] See Ref. 35.

[52] See, *e.g.*, D. Achlioptas and A. Coja-Oghlan, "Algorithmic barriers from phase transitions," *2008 49th Annual IEEE Symposium on Foundations of Computer Science*, 793-802 (2008). https://doi.org/10.1109/FOCS.2008.11

**EB:** [0:47:45] Probably I don't have much. For some time I was exchanging things with Michel Talagrand, but probably I don't have anything which could be of interest. These were just email exchanges. I don't really think, no.

**PC:** If ever you do find some interesting emails, please consider…

**EB:** [0:48:25] Yes. If something… With Talagrand, we had a number of exchanges. He was mainly asking me about literature at the beginning. And then a little bit of exchanges about what was going on. But we didn't really have very close contacts about scientific matters. I don't think that I have anything.

**PC:** Fair enough. Prof. Bolthausen, thank you very much for your time and for this conversation. It's been a real pleasure.

**EB:** [0:49:09] Thank you very much for inviting me. It was a pleasure to talk about this development also. You're welcome.