# History of RSB Interview: Michael Aizenman

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**Location:**
Over Zoom, from Prof. Aizenman's home in Princeton, New Jersey, USA.

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**PC:** Good morning, Prof. Aizenman. Thank you very much for joining us. As we discussed ahead of this interview, the theme of our discussion will be the development of replica symmetry breaking in physics, which roughly covers 1975 to 1995. But before we get to that, I have a few preliminary questions. How did you first get interested in physics and then to pursue research in mathematical physics?

**MA:** Thank you for the invitation.

I was drawn to mathematics at about the age of 12, finding delight in logic puzzles, then algebra as a language useful in problem solution, and Euclidean geometry which I found astonishing and delightful. Physics in high school was a curiosity, but my fascination with it was ignited later, in college. First through analytical mechanics and then quantum and statistical physics. I was deeply struck seeing that Newtonian mechanics, in which the key concepts are the forces, through which accelerations are explained, can be equivalently presented through variational principles. The two approaches suggest vastly different conceptualization of the mechanism by which the physical world evolves, but the resulting dynamics coincide. In quantum mechanics the perspective changes even further: relevant trajectories are not only those which optimize the action, but ``anything can go''. Yet these different theories are consistent with each other, some exactly and the other in certain limits. In my senior year I had the good chance to attend a short series of lectures by Herbert Callan, who focused on ``entropy'', as the quantifier of irreversibility through which thermodynamics makes sense. That too made an impression. Since then, I find mathematics to be particularly enjoyable and satisfying when lucid mathematical arguments are found to project on interesting physics, nature, information theory, or find some other manifestation. At the same time, I never lost the pleasure of seeing a well-crafted mathematical interplay of abstracted mathematical notions.

Throughout my career I insisted on holding joint appointments, in physics and mathematics. The two fields have somewhat different cultures. In physics, it is the discovery of interesting phenomena that counts. If a discovery is initially attributed to what was later found to be not the right mechanism, the is acceptable. What is remembered are not the errors, but the achievements. In mathematics higher emphasis is placed on a proper understanding of the structures involved, along with appreciation of their elegance and complexity, and there is longer memory for erroneous claims. I personally like the combination of the two perspectives. One also finds that often the more mathematical understanding of a phenomenon ``gives it legs'' – enabling the recognition of similar effects in very different fields.

Having a joint appointment exposes you to pressure and criticism from both directions. I find that stimulating, and have been in such an environment since my studies and early appointments.

**PC:** You did the undergraduate degree at the Hebrew University, and then you moved to the US to do your PhD. What drew you or drove you to the US?

**MA:** After the undergraduate studies, and an introduction to research through an M.Sc. project (with Hanoch Gutfreund[1], concerning bosonization[2]) it became clear that I would like to engage in mathematical physics. I set to find a program which would provide a compelling combination of the two. I was particularly drawn to the foundations of quantum mechanics. Among the graduate programs in the US, I learned that the one at the Belfer Graduate School of Science, has a number of people who are focused on fundamental questions concerning the meaning of this strange theory and

[1] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Hanoch Gutfreund*, transcript of an oral his- tory conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 16 p. https://doi.org/10.34847/nkl.1adb9r42 ,

[2] M. Aizenman and H. Gutfreund, "Momentum distribution in the Tomonaga model at finite temperatures," J. Math. Phys. **15,** 643-647 (1974). https://doi.org/10.1063/1.1666700

where else can it lead to[3]. I was intrigued by the work of David Finkelstein[4], but was also open minded, appreciating that there is much that I do not know.

The Belfer Graduate School was (and still is) a secular institution linked with Yeshiva University, in New York. The university aims at providing its students exposure to both Jewish traditional scholarship and college education. Perhaps taking cue from the success of the Albert Einstein School of Medicine[5] (in NYC), which is affiliated with this college, at some point (perhaps early '60s) the idea emerged: "If YU can have such an excellent school of medicine, why not also a school of science?" The budding Belfer Graduate School of Science has attracted a collection of interesting individuals. Freeman Dyson[6] gave lectures and spend some time there. Eliott Lieb[7] passed through for a year or more (both before my time). Among the theoretical physicists present there when I arrived were Yakir Aharonov[8], Joel Lebowitz[9], Dan Mattis[10], Aage Petersen[11], Leonard Susskind[12]. David Finkelstein's work was guided by the idea that quantum physics should make us realize that the world is weaved through a different logic than the one we are used to. His hope was to codify quantum logic, adapt its language, and then see the very space-time emerge out of elementary quantum constructs. These were high dreams and they captured my imagination.

After about a year I came to recognize that while this pursuit has been very exciting, I somehow did not manage to formulate along these lines a project with recognizable goals, or prospects. I also started to appreciate other things in mathematical physics. After some exploration I pivoted towards

[3] The Belfer Graduate School of Science notably collaborated in publishing: Aage Petersen, *Quantum Physics and the Philosophical Tradition* (Cambridge, Mass.: MIT Press, 1968). See also, Aage Petersen collection of reprints and manuscripts, 1922-1976. American Institute of Physics, Niels Bohr Library & Archives, College Park, MD 20740, USA. https://history.aip.org/ead/20090236.html (Consulted August 8, 2021.)
[4] David Finkelstein: https://en.wikipedia.org/wiki/David_Finkelstein
[5] Albert Einstein College of Medicine: https://en.wikipedia.org/wiki/Albert_Einstein_College_of_Medicine
[6] Freeman Dyson: https://en.wikipedia.org/wiki/Freeman_Dyson
[7] Eliott Lieb: https://en.wikipedia.org/wiki/Elliott_H._Lieb
[8] Yakir Aharonov: https://en.wikipedia.org/wiki/Yakir_Aharonov
[9] See, *e.g.*, 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
[10] "Daniel Charles Mattis," *Mathematics Genealogy Project* (s.d.). https://www.mathgenealogy.org/id.php?id=258843 Consulted September 5, 2022.)
[11] See, *e.g.*, Lara D'Agaro, "Aage Petersen collection of reprints and manuscripts, 1922-1976," *American Institute of Physics, Niels Bohr Library & Archives* (2007) https://history.aip.org/ead/20090236.html (Consulted September 5, 2022.)
[12] Leonard Susskind: https://en.wikipedia.org/wiki/Leonard_Susskind

research under the guidance of Joel Lebowitz. In retrospect, it seems possible that the goals of Finkelstein's project found partial expression in developments which occurred later, *e.g.*, Alain Connes' noncommutative geometry[13], and perhaps also SYK[14]. His vision may not have been misguided, but in addition to the ambitious, creative, and somewhat loose thinking it may have required deep mathematical skills.

Joel Lebowitz's office at that time was a hub through which were passing interesting people. Among repeat visitors I recall David Ruelle, Oscar Lanford[15], and Oliver Penrose[16]. Graduate course would regularly spill into free discussions. As a high-power physicist or mathematician would pass by Joel would say: "David, tell us about the Asano contraction[17]; or "Oscar, tell us about your work on the Boltzmann equation." Jointly with Joel and Shelly Goldstein[18] we dove into ergodic theory, identifying a physical model with the "T/T-inverse" mapping as a candidate for an ergodic system without the K property[19]. Field theory was taught by Lenny Susskind[20],… The experience was a bit like studying for PhD at an Institute for Advanced Study[21]. Do not expect solidly organized courses, but if you are sufficiently curious, open-minded, and dare to join a discussion, you get exposed to very interesting stuff, and to events happening around you (among which were Joel's remarkable *Yeshiva meetings*[22]).

As it happens, shortly after I got my degree the school was scaled down, at least temporarily, and its PhD program was interrupted[23]. With the termination of the Vietnam war, and the reorientation of the national efforts, funds were redirected. There may perhaps have also been some local hes-

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[13] Alain Connes: https://en.wikipedia.org/wiki/Alain_Connes
[14] Sachdev–Ye–Kitaev model: https://en.wikipedia.org/wiki/Sachdev%E2%80%93Ye%E2%80%93Kitaev_model
[15] Oscar Lanford: https://en.wikipedia.org/wiki/Oscar_Lanford
[16] Oliver Penrose: https://en.wikipedia.org/wiki/Oliver_Penrose
[17] Asano Contraction: https://en.wikipedia.org/wiki/Asano_contraction
[18] Sheldon Goldstein, *Ergodic Theory and Infinite Systems,* PhD Thesis, Belfer Graduate School of Science Yeshiva University (1974). https://hdl.handle.net/20.500.12202/2181 (Consulted September 5, 2022.)
[19] See, *e.g.*, M. Aizenman, S. Goldstein and J. L. Lebowitz, "Ergodic properties of an infinite one dimensional hard rod system," *Comm. Math. Phys.* **39**, 289-301 (1975). https://doi.org/10.1007/BF01705376
[20] Leonard Susskind: https://en.wikipedia.org/wiki/Leonard_Susskind
[21] Institute of Advanced Study: https://en.wikipedia.org/wiki/Institute_for_Advanced_Study
[22] T. Feder, "One man, one hundred meetings, and a physics subfield," *Physics Today* **61**, 10, 30 (2008). https://doi.org/10.1063/1.3001861
[23] See, *e.g.*, R. Wines and A. M. Halpern, "Academic Freedom and Tenure: Yeshiva University," *Academe* **67**, 186-195 (1981). https://doi.org/10.2307/40253365; G. Klaperman, "Yeshiva University: Seventy-Five Years in Retrospect," *American Jewish Historical Quarterly* **54**, 5 (1964). https://www.jstor.org/stable/23874788

itation about the program. The research faculty has scattered: Joel Lebowitz made his home at Rutgers, Lenny Susskind at Stanford, David Finkelstein at Georgia Tech, etc.

As my first postdoc appointment I was offered a visiting membership at Courant institute, and that was followed up by postdoctoral position at Princeton University with Elliott Lieb. Upon arriving at Princeton, I met also Barry Simon[24] who, though not older than me, was already a tenured professor giving lectures on his upcoming books on mathematical physics[25].

During the postdoctoral appointments I got to supplement my formal education. I also benefited from mentorship by Eliott Lieb.

**PC:** Could you describe how you were selecting problems, as a postdoc and then as a junior faculty?

**MA:** As a postdoc I was encouraged to seek interesting goals and chart my path. But I was also continuously exposed to questions worth of long-term concentration, and to techniques which seemed worth to study. As you know, physics is a source of enduring challenges and also a spring of new and surprising phenomena. Once you engage in it, you may run into unexpected questions, or develop a new perspective which draws you deeper.

You are affected by what you are exposed to. Hearing about interesting challenges you try to develop you own thoughts, and start coming up with ideas. Most of them are wrong, but you keep thinking. I like to take a well-motivated problem, turn over in my mind, and am ready to think about it for a while before seeing a breakthrough. But I have also been open to serendipitous encounters.

An example of the latter was a seminar by Edward Nelson, in which he presented a conjecture of potential relevance to long challenging flow problems, which from a formal analytical perspective made sense. Since it has stubbornly resisted his efforts, Nelson posed it as a question which could earn one a degree in mathematics. I found the challenge appealing. A day or two later I felt that I see a path to a proof and set up an appointment. But preparing for it I noted a small gap in the argument and asked to postpone the presentation. I then spent a month trying to fix the proof, but each time a gap was closed another one appeared. Eventually, I decided to

[24] Barry Simon: https://en.wikipedia.org/wiki/Barry_Simon

[25] Michael Reed and Barry Simon, *Methods of Modern Mathematical Physics*, *Vol. I: Functional Analysis* (San Diego: Academic Press, 1972); *Vol. II: Fourier Analysis, Self-Adjointness* (San Diego: Academic Press, 1975); *Vol. III: Scattering Theory* (San Diego: Academic Press, 1978); *Vol. IV: Analysis of Operators* (San Diego: Academic Press, 1977).

give the other side a chance, and challenged myself to come up with a mechanism through which the conjecture could fail. Having heard of fractal sets, though not yet having much experience with such, I asked myself whether a fractal crack could lead to a counterexample. And indeed, once the question was conceptualized properly it was not difficult to produce a counterexample. I may add that Ed was gracious, and a good sport, and my paper on the subject was accepted to the *Annals of Mathematics*[26]. After a follow-up article, limiting the effect which I discovered, I returned to topics of my previous and future longer-term interest.

Another excursion tripped by a serendipitous encounter, this time it was a doubt expressed by Thomas Hoffmann-Ostenhoff[27], led to a joint work with Barry Simon in which we showed how the Harnack inequality for Schrödinger operators can be derived through the Feynmann-Kac functional integral[28].

You learn from each such experience. Through the work on the counterexample I got to meet fractal geometry. Random fractal structures have resurfaced later in my works on the stochastic geometry of critical percolation and other models of statistical mechanics (in particular, in the joint work with Almut Burchard[29], which had some further implications[30]).

Returning to your question: I do not enjoy working in a crowded field. In my student days the message for students of physics was that high-energy physics and the structure of elementary particles is the interesting frontier, and the best should rush there. It was not the path I followed.

When I arrived at Princeton, there was a loud call for mathematical physicists *worth their salt* to work on the constructive field theory program[31]. Its architects laid the case, which was generally accepted by both physicists and mathematicians, that there's a crisis in physics. We are missing a firm understanding of what quantum field theory is, and how to deal with its

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[26] M. Aizenman, "On vector fields as generators of flows: a counterexample to Nelson's conjecture," *Ann. Math.* **107**, 287-296 (1978). https://doi.org/10.2307/1971145

[27] Thomas Hoffmann-Ostenhof: https://de.wikipedia.org/wiki/Thomas_Hoffmann-Ostenhof

[28] M. Aizenman and B. Simon, "Brownian motion and Harnack inequality for Schrödinger operators," *Comm. Pure Appl. Math.* **35**, 209-273 (1982). https://doi.org/10.1002/cpa.3160350206

[29] M. Aizenman and A. Burchard, "Hölder regularity and dimension bounds for random curves," *Duke Math. J.* **99**, 419-453 (1999). https://doi.org/10.1215/S0012-7094-99-09914-3

[30] See, *e.g.*, J. Ding and X. Jiaming. "Exponential decay of correlations in the two-dimensional random field Ising model," *Inventiones Mathematicae* **224**, 999-1045 (2019). https://doi.org/10.1007/s00222-020-01024-y

[31] Constructive quantum field theory: https://en.wikipedia.org/wiki/Constructive_quantum_field_theory

singularities. To point the direction, axioms were charted under the guidance of R. Haag[32] and A.S. Wightman[33]. Again, although I appreciated the talent and energy which I saw directed towards this challenge, I was not drawn into it.

In a somewhat different direction, Elliott Lieb as also Joel Lebowitz have exemplified for me of the value of a quest to shed light on fundamental questions, such as: the stability of matter, the roots of ferromagnetism (hint: Fermi statistics play an essential role for each), dynamics in driven systems and out of equilibrium. Though the goal is to shed light on basics questions of physics, the techniques developed in the process lead to new mathematical insights, spurring also that field

At Princeton, in addition to the regular seminar, there was a weekly mathematical physics *brown bag* [meeting]. All were invited to come, Thursday noon, if I recall correctly, with their choice of lunch (hence the brown bag). Among the regular participants were Eliott Lieb, Barry Simon, Freeman Dyson[34], Jürg Fröhlich[35], along with postdocs, graduate students and visitors. In a lively and freewheeling discussion people would share with others interesting ideas, observations, and challenges. It was marvelous and stimulating.

Initially, the topics to which I was drawn concerned critical phenomena, the structure of Gibbs states[36], the critical behavior in Ising – type non-solvable models, and related mathematical questions. Then, somewhat unexpectedly, through the study of the scaling limits of statistic mechanical models (critical Ising and the like) I came upon analysis with implications for the constructive field theory program[37]. This culminated with arguments indicating that in four dimensions, which was the constructive program's long goal, the scaling limits of the critical models in a class which has included the path charted towards $\phi^4{}_d$ field theory, would lead to only

[32] Rudolf Haag: https://en.wikipedia.org/wiki/Rudolf_Haag
[33] Arthur Wightman: https://en.wikipedia.org/wiki/Arthur_Wightman
[34] Freeman Dyson: https://en.wikipedia.org/wiki/Freeman_Dyson
[35] Jürg Fröhlich: https://en.wikipedia.org/wiki/J%C3%BCrg_Fr%C3%B6hlich
[36] See, *e.g.*, M. Aizenman, "Instability of phase coexistence and translation invariance in two dimensions," *Phys. Rev. Lett.* **43**, 407 (1979). https://doi.org/10.1103/PhysRevLett.43.407; "Translation invariance and instability of phase coexistence in the two dimensional Ising system," *Comm. Math. Phys.* **73**, 83-94 (1980). https://doi.org/10.1007/BF01942696
[37] See, e.g., M. Aizenman, "Proof of the triviality of $\varphi^4$ field theory and some mean-field features of Ising models for $d$>4," *Phys. Rev. Lett.* **47**, 1 (1981). https://doi.org/10.1103/PhysRevLett.47.1; "Geometric analysis of $\phi^4$ fields and Ising models. Parts I and II," *Comm. Math. Phys.* **86**, 1-48 (1982). https://doi.org/10.1007/BF01205659

gaussian fields (a task which, incidentally, was completed only recently[38]). Alan Sokal, at the time Arthur Wightman's graduate student, referred to this and the parallel work of J. Froehlich[39] as *destructive field theory*.

Later, a theme which I found fascinating and which has guided some further progress is the existance of strict relations, and further reaching similarities, between structurally different models, like percolation and Ising systems.

A theme which started to play a significant role in my thinking about critical phenomena is their relation to emergent stochastic geometric structures. For example: the formation of long-range order in Ising-type models can be attributed to the percolation of clusters which are fully correlated, as is nicely expressed in random cluster models.

Such analogies have led to a sequence of results (with C. M. Newman, D. Barski, R. Fernandez[40]) in which progress was made in a manner of climbing a rope ladder with two feet. Reaching a new result about the Ising model, one would ask: Is there an echo of that in the stochastic geometric models, say percolation? This led to new results about percolation models (including the so-called sharpness of the phase transition), which reached even beyond what was previously known for Ising. Learning from that you ask yourself whether a corresponding statement could be developed for Ising spin systems. In such a manner the above work on the upper critical dimension for Ising models was followed by one on percolation (for which the critical dimension is different). The synergy was found particularly effective in our analysis of the Thouless phenomenon in one-dimensional systems with 1/|x-y|^2 interaction[41] – in joint works with Charles Newman and Jennifer and Lincoln Chayes.

[38] N. Aizenman and H. Duminil-Copin, "Marginal triviality of the scaling limits of critical 4D Ising and $\phi^4_4$ models," *Ann. Math.* **194**, 163-235 (2021). https://doi.org/10.4007/annals.2021.194.1.3

[39] J. Fröhlich, "On the triviality of $\lambda\phi^4_d$ theories and the approach to the critical point in d $>_{(=)}$ 4 dimensions," *Nucl. Phys. B* **200**, 281-296 (1982). https://doi.org/10.1016/0550-3213(82)90088-8

[40] See, *e.g.*, M. Aizenman, D. J. Barsky and R. Fernández, "The phase transition in a general class of Ising-type models is sharp," *J. Stat. Phys.* **47**, 343-374 (1987). https://doi.org/10.1007/BF01007515; M. Aizenman, and D. J. Barsky, "Sharpness of the phase transition in percolation models," *Comm. Math. Phys.* **108**, 489-526 (1987). https://doi.org/10.1007/BF01212322; M. Aizenman and C. M. Newman, "Tree graph inequalities and critical behavior in percolation models," *J. Stat. Phys.* **36**, 107-143 (1984). https://doi.org/10.1007/BF01015729

[41] M. Aizenman, J. T. Chayes, L. Chayes and C. M. Newman, "Discontinuity of the magnetization in one-dimensional $1/|x-y|^2$ Ising and Potts models," *J. Stat. Phys.* **50**, 1–40 (1988). https://doi.org/10.1007/BF01022985

Eventually such cross considerations reached also quantum spin models, as was showed in our recent joint work with Hugo Dominil-Copin and Simone Warzel[42]. In it, different systems—one classical and two quantum—are found to share a common mathematical scaffolding, which is simultaneously behind the discontinuity of the phase transition of classical 2D *q*-state Potts models at *q*>4, and behind the dimerization and Neel order in two different of quantum spin chains. An appealing explanation of these phenomena is enabled through the combination of the different perspectives associated with the quantum and classical projections of a common random loop system.

An earlier example of a linkage between classical and quantum worlds can be found in the two-dimensional Ising model, which was solved by Lars Onsager[43]. From the works by Bruria Kaufman[44], Lieb-Schultz-Mattis[45], and Leo Kadanoff[46] we learn that this very classical system is best understood in terms of emergent quantum degrees of freedom (including fermions and spinors). I find that intriguing, and enticing.

That's a long-winding answer to some of your early questions. Perhaps we should return to questions now.

**PC:** By curiosity, while you were at Princeton, did you get to know and interact much with Francesco Guerra[47], who was then involved with the Wightman group?

**MA:** As I recall, Francesco Guerra and I briefly overlapped in Princeton, but we did not make much contact on that occasion. (We connected more later.) I remember seeing Francesco in a mathematical physics seminar at which another visitor was presenting results on field theory, getting questioned by him and grilled by Barry. Guerra returned to Rome a short time after, having made a very important contribution to the constructive field theory

[42] M. Aizenman, H. Duminil-Copin and S. Warzel, "Dimerization and Néel Order in Different Quantum Spin Chains Through a Shared Loop Representation," *Ann. Henri Poincaré* **21**, 2737–2774 (2020). https://doi.org/10.1007/s00023-020-00924-2

[43] Lars Onsager: https://en.wikipedia.org/wiki/Lars_Onsager

[44] Bruria Kaufman: https://en.wikipedia.org/wiki/Bruria_Kaufman

[45] See, *e.g.*, T. D. Schultz, D. C. Mattis and E. H. Lieb, "Two-dimensional Ising model as a soluble problem of many fermions," *Rev. Mod. Phys.* **36**, 856 (1964). https://doi.org/10.1103/RevModPhys.36.856

[46] L. P. Kadanoff, "Scaling laws for Ising models near $T_c$," *Physics Physique Fizika* **2**, 263 (1966). https://doi.org/10.1103/PhysicsPhysiqueFizika.2.263

[47] 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

program. At the time, ground-breaking results were derived through cluster expansions and hard analytical methods. Francesco Guerra[48], in a work related also to that of Edward Nelson[49], brought in a new perspective. He pointed out that looking at the functional integral from a statistic mechanical point of view, you can see in a non-perturbative manner relations which lead to simpler methods for the proof of what was sought after.

Years later Francesco Guerra had a similar impact on spin glass theory. In both cases his observations seemed to arrive unexpected, were quickly grasped, and right away progress accelerated.

**PC:** Let's talk about you contribution to spin glasses, then. When did you first hear about this family of models, and ideas of replica symmetry breaking? Then, what led you to work on formalizing certain aspects of these results?

**MA:** I may have first heard in depth about Parisi's solution of the Sherrington-Kirkpatrick spin glass models from a series of lectures which David Ruelle gave at Rutgers[50], around 1986. David was a frequent visitor, always willing to share his wisdom. On that occasion he made an intriguing presentation on Parisi's solution. In it, after briefly mentioning the original replica calculation, he laid down his version of the infinite-volume limit of Derrida's REM and GREM (generalized random energy model) calculations[51].

I found the structure which he outlined very intriguing and would later spend time digesting and bring up its remarkable properties.

Meanwhile, starting with the more elementary questions about the SK model, jointly with Joel Lebowitz and David Ruelle we wrote a short paper[52] with some basic results. One of these was the surprising fact that in this random system at any temperature above its critical point (and at vanishing external field) the system's total free energy has drastically small fluctuations – just of order one (!). We also presented a rigorous upper bound on the Ising spin glass' ground state energy. In hindsight it should not come as a surprise but could still be an-eye opening observation that

[48] F. Guerra, L. Rosen and B. Simon, "Nelson's symmetry and the infinite volume behavior of the vacuum in $P(\phi)_2$," *Comm. Math. Phys.* **27**, 10–22 (1972). https://doi.org/10.1007/BF01649655

[49] Edward Nelson: https://en.wikipedia.org/wiki/Edward_Nelson

[50] 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.

[51] P. Charbonneau, *History of RSB Interview: Bernard Derrida*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 23 p. https://doi.org/10.34847/nkl.3e183b0o

[52] 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

the ground state energy of this Ising mean-field model is much harder to determine than the ground state energy of a random matrix. We showed how to use the latter's Wigner-Dyson-Mehta semi-circle law[53] for some explicit bounds. The algorithm we used also indicates that there is a rich collection of states at the spectrum's low range. We did not say much about the model's fascinating structure below the critical temperature, but did present results concerning the thermodynamic manifestation of the non-vanishing of its order parameter. The latter does imply the existence of replica symmetry breaking but does not yet spell its structure.

As a general comment on my work as a mathematical physicist: to understand the math underlying an observation which was initially arrived at through a brilliant physicist's guess, or deduction, it is typically not fruitful to spend the time trying to dot the i's and cross the t's, in the previously provided explanation. Often (though with exceptions) in non-trivial problems this does not work, and the effort is also not appreciated by the discoverer of the phenomenon in question. The way we progress is take the question apart and try to construct inroads through steps which are expressed in clear mathematical terms. At the end one may reconnect with the intuition which has guided the physical argument, sometimes recognizing also the limitations or occasionally errors in its implicit assumptions.

The SK model's Parisi's solution can be explained by assuming that the model's random pattern of the replicas' overlaps falls into a *universal class* of overlap distributions, which is parametrized by an order parameter of novel type, which Parisi has brilliantly identified. Assuming that to be the case, the system's free energy, and its ground state energy, are reduced to a variational calculation of Parisi's integral expression.

Motivated by this observation I set myself the goal of shedding light on the properties of the random overlap structure which Ruelle modestly described as *Derrida's GREM*. I have been presenting the questions and developing ideas to colleagues, some of whom found ways to improve their formulation, and to younger collaborators with whom we wrote joint works on the subject.

Perhaps we should pause here now, because otherwise I'll glide into a more detailed discussion of spin glass models and the Parisi solution.

**PC:** We can come back to that. I wanted to ask questions about the more immediate aftermath of your first work. I saw that you wrote an NSF grant

[53] Wigner semicircle law: https://en.wikipedia.org/wiki/Wigner_semicircle_distribution

right after that paper[54], where you said you wanted to keep on working on spin glasses in the following three years. I don't think you did at that point, at least not in the publication record. Can you tell us what happened or why?

**MA:** Can you remind me of the year?

**PC:** The grant which mentioned spin glasses was from 1989 to 1991.

**MA:** Yes. To be more precise, the grant's abstract reads:

"Research is in statistical mechanics with emphasis on systems with quenched disorder. Topics include fundamental issues in the theory of random field and spin-glass models, localization effects and extended states for random operators, and other questions concerning time evolution and critical behaviour."

Bundled there are three (or potentially more) different physically interesting phenomena caused by quenched disorder. During the duration of this grant, I was quite successful with two out of the three, and the focus of research has shifted accordingly. In fact, around and during that time, in two different collaborations I have worked on two breakthrough results.

The first topic, referred to as *random field*, concerns the then active question under what conditions would the introduction of weak disorder in homogeneous system's parameters eliminate a first order phase transition. Coincidentally, this was another area in which Giorgio Parisi, with Nicolas Sourlas[55], have made a bold prediction, in this case based on a field-theoretic *dimensional reduction* argument. An alternative proposal was advocated by Yosef Imry and Shang-keng Ma[56]. This was a rare case in which rigorous analysis by mathematical physicists has answered a question before physicists reached consensus through other means.[57] (It was also a

[54] M. Aizenman, "Critical Behavior and Disorder Effects in Statistical Mechanics and in Quantum Systems (Physics), NSF-PHY #8912067 (1989-1991). https://www.nsf.gov/awardsearch/showAward?AWD_ID=8912067 (Consulted August 11, 2021.)

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

[56] Y. Imry and S.-k. Ma, "Random-field instability of the ordered state of continuous symmetry," *Phys. Rev. Lett.* **35**, 1399 (1975). https://doi.org/10.1103/PhysRevLett.35.1399

[57] J. Z. Imbrie, "Lower critical dimension of the random-field Ising model," *Phys. Rev. Lett.* **53**, 1747 (1984). https://doi.org/10.1103/PhysRevLett.53.1747; "The ground state of the three-dimensional random-field Ising model," *Commun. Math. Phys.* **98**, 145 (1985). https://doi.org/10.1007/BF01220505; J. Bricmont and A. Kupiainen, "Lower critical dimension for the random-field Ising model," *Phys. Rev. Lett.* **59**, 1829 (1987). https://doi.org/10.1103/PhysRevLett.59.1829; "Phase transition in the 3d random field Ising model," *Commun. Math. Phys.* **116**, 539 (1988). https://doi.org/10.1007/BF01224901

rare case in which one of Parisi's predictions needed to be corrected.) My contribution to the random-field theory, carried jointly with Jan Wehr, was presented in papers published during the period which you asked about.[58]

The other topic in which I got immersed during this grant's duration, and to some extent was quite successful, concerned Anderson localization and delocalization[59].

At the beginning of this period [in 1989] I was at the Courant Institute, but about to make a transition to Princeton University. Through Barry Simon's initiative I received an invitation to spend a term at Caltech as a Fairchild Scholar[60], and I found the conditions ideal for diving into this subject, which was mostly new to me. What came out was the fractional moment approach to Anderson localization[61]. It has provided a relatively easy and transparent proof, and a tool for handling related properties, such as dynamical localization, which were not within reach before. In short time this approach was found useful also for other results, obtained in different collaborations and some by others: derivation of Poisson spectral statistics, application to QHE, and eventually new, though modest, results on delocalization.

Returning to spin glasses: I did continue to think about the subject, and also to talk about its challenges, and opportunities.

An aspect of Ruelle's presentation which I found fascinating, was that the presumed distribution of the random cascade of the extremal states' overlaps and free energies is a structure of universal relevance. It is remarkably quasi-stationary, in a sense which should make it indeed relevant for cavity dynamics (the addition of a site to an already huge system), and many other dynamical processes. (To convey the point, I was referring to the *Indy 500*[62] and other more complicated races. But a range of other applications may easily come to mind.)

[58] M. Aizenman and J. Wehr, "Rounding of first-order phase transitions in systems with quenched disorder,'' *Phys. Rev. Lett.* **62**, 2503 (1989). https://doi.org/10.1103/PhysRevLett.62.2503; "Rounding effects of quenched randomness on first-order phase transitions," *Comm. Math. Phys.* **130**, 489-528 (1990). https://doi.org/10.1007/BF02096933

[59] Anderson Localization: https://en.wikipedia.org/wiki/Anderson_localization

[60] See, *e.g.*, "The Fairchild Scholars Program," *Engineering and Science* **44**, 20-23 (1981). https://resolver.caltech.edu/CaltechES:44.4.Fairchild (Consulted August 11, 2021.)

[61] M. Aizenman and S. Molchanov, "Localization at large disorder and at extreme energies: An elementary derivations," *Comm. Math. Phys.* **157**, 245-278 (1993). https://doi.org/10.1007/BF02099760

[62] Indiana 500: https://en.wikipedia.org/wiki/Indianapolis_500

Guided by this observation I suggested to develop an approach based on: 1) a more explicit mathematical description of the cascades for which the overlap distribution is a natural order parameter (as in Parisi's solution), 2) establish a general result to the effect that under certain conditions, still to be properly spelled out, such a structure would emerge in a broad class of dynamical processes of competing particles (*e.g.*, cavity dynamics, or renormalized versions of it).

On my periodic visits to ETH-Zurich, I outlined the project to Erwin Bolthausen[63] and Alain-Sol Sznitman[64], stressing that Ruelle's work was fundamental, but there was still room for better mathematical grasp of the process. This had the positive effect of steering the interest of the two superb probabilists towards the subject. The results were a beautiful mathematical formulation of Ruelle's cascade process, presented properly from a probabilistic perspective[65].

The second part of this project has been to prove that under broad conditions a dynamical process, which is quasi-stationary in the sense indicated above, should gravitate towards a state described by a distribution in this class—up to accidental degeneracies which may perhaps be resolved through an arbitrarily small jiggle in the model's parameters.

This vision has motivated our works with Anastasia Ruzmaikina[66] and, later, Louis-Pierre Arguin[67] (which relate to REM and GREM correspondingly). A different aspect of the theory was explored in a work with Pierluiggi Contucci[68].

**PC:** Were you following what other mathematical physicists were doing and that time, say Leonid Pastur[69] or Francesco Guerra[70], around those years?

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[63] See, *e.g.*, 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

[64] Alain-Sol Sznitman: https://en.wikipedia.org/wiki/Alain-Sol_Sznitman

[65] 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

[66] A. Ruzmaikina and M. Aizenman, "Characterization of invariant measures at the leading edge for competing particle systems," *Ann. Probab.* **33**, 82-113 (2005). https://doi.org/10.1214/009117904000000865

[67] L.-P. Arguin and M. Aizenman, "On the structure of quasi-stationary competing particle systems," *Ann. Probab.* **37**, 1080-1113 (2009). https://doi.org/10.1214/08-AOP429

[68] M. Aizenman and P. Contucci, "On the Stability of the Quenched State in Mean-Field Spin-Glass Models," *J. Stat. Phys.* **92**, 765–783 (1998). https://doi.org/10.1023/A:1023080223894

[69] Leonid Pastur: https://en.wikipedia.org/wiki/Leonid_Pastur

[70] See, *e.g.*, L. A. Pastur and M. V. Shcherbina, "Absence of self-averaging of the order parameter in the Sherrington- Kirkpatrick model," *J. Stat. Phys.* **62**, 1-19 (1991). https://doi.org/10.1007/BF01020856; *Stochastic processes, physics and geometry II*, eds. S. Albeverio, U. Cattaneo and D. Merlini (Singapore: World Scientific, 1995). The preprint of the chapter on spin glasses included in this book is dated April 1992:

**MA:** In general, I tend to be somewhat informed of what is happening in fields of my interest, but only up to a point, especially when I am busy with number of different topics. In this case I have periodically had opportunities to meet Leonid and Francesco. We are friends and I feel deep respect towards their work. On occasions we exchanged thoughts and observations, but I was not following very closely.

However, I did learn of Francesco's breakthrough work with Fabio Toninelli, and was very impressed by it. A window was opened on the subject and fresh air rushed in. I felt that technically this pointed to a short-cut advancing one closer to the goal. Trying to digest its implications, and link with the general perspective outlined above, we formulated the variational principle which was presented in the joint work with Robert Sims and Shannon Starr[71].

The proof of the validity of Parisi's solution was subsequently presented by Michel Talagrand[72], drawing on Guerra's previous insights combined with with Talagrand's deep results on *concentration of measure* and related properties of functional integrals. This was a very impressive accomplishment, well deserving the accolades which it has received. But one is still left with the feeling that we have not yet arrived at sufficient clarity in the conceptual grasp of the relevant principles. Perhaps the ideas outlined above may one day still shine through.

As I hear, steady progress is still being made in this direction, *e.g.*, in the works of Dmitry Panchenko, Andrea Montanari, Antonio Auffinger[73], Eliran Subag, and others.

**PC:** Would it be fair to say that the mathematical physics community working on spin glasses was atomized, at least in those years? Every one of you was loosely aware of each other's work, but not deeply entangled…

**MA:** To some extent that was true, but as I indicated only partly so. However, I agree that the exchange of news and recognition of the value of results could be improved.

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Francesco Guerra, "Fluctuations and thermodynamic variables in mean field spin glass models," arXiv:1212.2905.

[71] M. Aizenman, R. Sims and S. Starr, "An extended variational principle for the SK spin-glass model," *Phys. Rev. B* **68**, 214403 (2003). https://doi.org/10.1103/PhysRevB.68.214403

[72] See, *e.g.*, 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

[73] Antonio Auffinger: https://en.wikipedia.org/wiki/Antonio_Auffinger

That may also apply to the larger body of researchers with interest in spin glasses. I have the impression that the mathematical works, and the perspective, which are mentioned here would ring unfamiliar to many within the spin glass community. You should be congratulated and thanked for undertaking this project.

**PC:** We're nearing the end of the questions I've prepared. It is there anything else you would like to share with us that we may have overlooked or missed?

**MA:** Let me close by noting that this circle of observations, ideas and lessons which emerged from the ENS - centered group, marvelously presented in the book by Parisi-Mézard-Virasoro[74], and explored in myriad of directions also with Nicolas Sourlas[75], is still a very active subject, including among mathematicians. I was delighted to see the way it has affected the thinking of, among others, Lenka Zdeborová[76]. Recently I heard a talk by Nike Sun[77], who, along with Alan Sly[78], has been proving fascinating results concerning threshold phenomena in *k*-SAT problems[79], that are of interest for computer scientists. All that is inspired by the spin glass perspective which now shows up in the work of physicists and mathematicians on a host of other topics. The continuing interest in the subject attests to the value of the ideas and tools which have emerged from the studies of the metaphorical spin glass models.

**PC:** Francesco, is there anything that you would like to ask?

**FZ:** No thank you.

**PC:** Have you kept papers, notes, correspondence from epoch? If yes, do you have any plans to deposit them in an academic archive at some point?

**MA:** I did not think of depositing correspondence in archives. But yes. It is an interesting idea.

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[74] M. Mézard, G. Parisi and M. A. Virasoro, *Spin Glass Theory and Beyond* (Singapore: World Scientific, 1987).
[75] See, *e.g.*, N. Sourlas, "Spin-glass models as error-correcting codes," *Nature* **339**, 693-695 (1989). https://doi.org/10.1038/339693a0
[76] Lenka Zdeborová: https://en.wikipedia.org/wiki/Lenka_Zdeborov%C3%A1
[77] Nike Sun: https://en.wikipedia.org/wiki/Nike_Sun
[78] Allan Sly: https://en.wikipedia.org/wiki/Allan_Sly_(mathematician)
[79] See, *e.g.*, J. Ding, A. Sly and N. Sun, "Proof of the satisfiability conjecture for large *k*" *Proceedings of the forty-seventh annual ACM symposium on Theory of computing*, 59-68 (2015). https://doi.org/10.1145/2746539.2746619

**PC:** I encourage you to consider it, one day, when you have time. There are certainly some interests for the history of the fields and for history of science more generally. Thank you very much for your time. It's been a very nice and enlightening conversation.

**MA:** Thank you!