History of RSB Interview: Hidetoshi Nishimori

January 11, 2023, 20:30 to 22:00 (ET). Final revision: March 14, 2023

Interviewer:
Patrick Charbonneau, Duke University, patrick.charbonneau@duke.edu

Location:
Over Zoom, from Prof. Nishimori's office at The Tokyo Institute of Technology, Tokyo, Japan.

How to cite:
P. Charbonneau, *History of RSB Interview: Hidetoshi Nishimori*, transcript of an oral history conducted 2023 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2023, 16 p. https://doi.org/10.34847/nkl.ff43wl37

PC: Good morning, Professor Nishimori. Thank you very much for joining me. As we were just discussing, the goal of this series of interviews is to go through the history of the ideas and the context surrounding the formulation of replica symmetry breaking and spin glasses as well as their applications. We roughly bound this topic in time from 1975 to 1995, but we will deviate from this timeframe a bit. Before we dive into the heart of this topic, I'd like to ask you a few questions about your own background. First, can you tell us a few things about your youth and about how you became interested in science?

HN: [0:00:50] In my younger days—at high school and before—I was very much interested in mathematics rather than physics or chemistry. I was very fascinated by computations, calculations. Honestly, I was not very much interested in physics itself. But after entering university and learning what real mathematics was, my interest deviated from mathematics with [its] rigorous logic toward theoretical physics with lots of computations. That's how my interest in studying physics [emerged].

PC: Why did you attend the University of Tokyo in physics? What drew you to Tokyo.

HN: [0:01:48] I was raised in a remote area of Japan, on the island of Shikoku[1], in the western part. It didn't have, honestly, very active universities in the field of science and technology, so I was looking for a place to learn more rigorously about science and mathematics, which naturally drew me to Tokyo.

[1] Shikoku: https://en.wikipedia.org/wiki/Shikoku

PC: Then, what led you to pursue graduate studies in statistical mechanics with Professor Suzuki[2], coming from that background?

HN: [0:02:29] I took courses of physics, and Professor Masuo Suzuki taught the course of statistical physics. He taught very enthusiastically. We learned that he was very much fascinated by and loved statistical physics. That attitude attracted me to the field, rather than the subject itself. I was interested very much in a broad spectrum of physics, including particle physics. Professor Suzuki's attitude was the main motivation that I entered the field.

PC: Can you describe a bit the University of Tokyo at that time? Professor Suzuki himself was part of the Kubo[3] group, I think.

HN: [0:03:20] Yes. Kubo led a very large group. He was very famous, and he attracted a lot of students. He, Professor Suzuki and Professor Yasushi Wada were the three professors of the group of statistical physics. Almost 20 or 30 students belonged to that group. It was a very big group. The group was very active. Students were discussing with each other very frequently at lunch, at dinner. We had sports event. I enjoyed those days very much.

PC: Once you joined Professor Suzuki's research group, how did you first become aware of spin glasses? Did you know about them beforehand?

HN: [0:04:12] I don't think I had known the subject of spin glasses before I joined the group. But after joining the group I soon realized that everyone was interested in spin glasses, because the topic was very actively studied and very fashionable at that time, as probably everybody in this field knows today. So, naturally I became acquainted with that topic of spin glasses.

PC: Do you know where Professor Suzuki's own interest in the topic came from?

HN: [0:04:55] He was generally interested in phase transitions and critical phenomena[4]. He had been working on that topic for many years. I don't

---

[2] See, *e.g.*, "Masuo Suzuki," *Prabook* (n.d.). https://prabook.com/web/masuo.suzuki/202571 (Consulted November 25, 2022.)

[3] Ryogo Kubo: https://en.wikipedia.org/wiki/Ryogo_Kubo

[4] See, *e.g.*, M. Suzuki, "Relationship among exactly soluble models of critical phenomena. I: 2D Ising model, dimer problem and the generalized XY-model," *Prog. Theo. Phys.* **46**, 1337-1359 (1971). https://doi.org/10.1143/PTP.46.1337; "Relationship between *d*-dimensional quantal spin systems and (*d*+

know how he became acquainted with spin glass theory itself, but he was studying spin glass theory at that time, using Landau-type mean-field analysis[5]. I learned from him that topic, and I studied very many papers on that problem.

PC: For your thesis[6], where did the idea of using gauge transformations to solve spin glass models originate?

HN: [0:05:39] It's a bit of long story. I was generally fascinated by the duality transformation of the Kramers-Wannier type[7]. That was a very clever trick to predict the exact value of the critical point of non-random systems in two dimensions. It was a very simple theory, but it had a very powerful aspect to predict the exact value of the transition point of the Ising model, which was later confirmed by Lars Onsager by his well-known exact solution[8]. I learned it and I was very fascinated by the topic. I tried to generalize it to random systems which had not be done, I thought. I first applied it to random ferromagnetic systems. The sign of the interaction was ferromagnetic, but the amplitude was random. That topic was also pursued at that time. I tried to apply the method of duality transformation to that random system, and I achieved something using the replica trick. But the replica trick didn't work very well for that system. It worked to some extent, but it didn't yield the exact solution. I thought it was the exact solution, but it was pointed out by someone later that it was not exact [only] approximately good. So, I proceeded to apply the method to the spin glass case with both signs of interactions, plus and minus, the so-called Edwards-Anderson model in two dimensions. But there was a difficulty in analyzing that model by duality because of the negative sign for antiferromagnetic interactions in the original model. Negative signs appear in the spin glass system naturally. With a negative sign, the dual variable, the coupling constant, becomes imaginary. So, it was not very physically meaningful. But I managed to transform that imaginary variable,

---

1)-dimensional ising systems: Equivalence, critical exponents and systematic approximants of the partition function and spin correlations." *Prog. Theo. Phys.* **56**, 1454-1469 (1976). https://doi.org/10.1143/PTP.56.1454;

[5] See, *e.g.*, M. Suzuki, "Phenomenological theory of spin-glasses and some rigorous results," *Prog. Theo. Phys.* **58**, 1151-1165 (1977). https://doi.org/10.1143/PTP.58.1151

[6] Hidetoshi Nishimori, *Rigorous Results on Random Spin Systems with Competing Interactions*, Doctor of Science Thesis, University of Tokyo (1982). http://gazo.dl.itc.u-tokyo.ac.jp/gakui/cgi-bin/gazo.cgi?no=105704; http://q-annealing.org/papers/Thesis1981.pdf (Accessed March 14, 2023.) **HN:** In retrospect, the title should have been *Exact Results…* instead of *Rigorous Results….* The corresponding Japanese 厳密 does not distinguish these two terms, and I did not understand the difference between the two very well at that time.

[7] Kramers-Wannier duality: https://en.wikipedia.org/wiki/Kramers%E2%80%93Wannier_duality

[8] L. Onsager, "Crystal statistics. I. A two-dimensional model with an order-disorder transition," *Phys. Rev.* **65** 117–149 (1944). https://doi.org/10.1103/PhysRev.65.117

the coupling constant, to a real positive ferromagnetic coupling by using an auxiliary variable which corresponds to the correlation function of a non-random ferromagnetic system. It was a non-trivial step. That procedure had been known for years, in retrospect, but I didn't know it. So, I developed that I idea. In short, I transformed the partition function of the spin glass system to the correlation function of a non-random ferromagnetic system in two dimensions by the duality transformation. In that process, I found that a class of spin glass systems with some properties correspond to the same correlation function in the pure system. I analyzed the properties of this class of random spin systems, and I recognized that those spin glass systems share the same distribution of frustration. I then came up with the idea to connect those random spin systems, [with] systems with the same distribution of frustration. [This] turned out to be the gauge transformation. This way, I found a gauge transformation to connect various distributions of randomness with the same distribution of frustration. So, the concept of gauge transformations I didn't know [by name], but I came up with the idea naturally. After that, I recognized that the same idea had been reached by a group of very famous particle physicists, Fradkin-Huberman-Shenker[9]. I was very disappointed, of course, as a student. I can't compete with those very famous professors. But I recognized by reading their paper in *Physical Review B*, that they formulated the gauge transformation, but they didn't derive any exact solutions. I went beyond their results by deriving the exact solution for the spin glass system. So, I could publish the paper[10].

PC: How was that paper received when it came out?

HN: [0:10:45] When I first presented my result at a JPS[11] meeting, I announced the result. Immediately after my talk, the chairman stood up and said: “Your result contradicts all the known results.” He meant that the fact that the internal energy that I derived didn't have any singularity at all. It was a very simple hyperbolic tangent of the inverse temperature. It's something that even a high school student knows that it has no singularity at all. But the condition that I derived [for] that exact result involved a point of phase transition. Across a phase transition, I showed that the internal energy had no singularity at all, which was very curious. He pointed it out. (It was natural for him to point it out that way.) But it was the exact result. I declared I could say nothing more, so it was not accepted. I also wrote a manuscript of that result and submitted it to a journal published

---

[9] E. Fradkin, B. A. Huberman and S. H. Shenker, “Gauge symmetries in random magnetic systems,” *Phys. Rev. B* **18**, 4789 (1978). https://doi.org/10.1103/PhysRevB.18.4789

[10] H. Nishimori, "Internal Energy, Specific Heat and Correlation Function of the Bond-Random Ising Model,” *Prog. Theo. Phys.* **66**, 1169–1181 (1981). https://doi.org/10.1143/PTP.66.1169

[11] Physical Society of Japan (JPS): https://en.wikipedia.org/wiki/Physical_Society_of_Japan

somewhere in the United Kingdom. The referee responded saying—I clearly remember what the referee said: "The model is artificial, and the result is thin to warrant publication." Just a single line. A piece of paper of this size: 1 cm [by] 10cm. It was enclosed in an envelope. I was shocked. In retrospect, my paper was not very well written. So, I revised the manuscript over and over again, several times. Finally, I could convince the referee that the paper was worth a publication. I was very discouraged by those first responses, but Professor Suzuki encouraged me saying that exact result results will stand the test of time. I was very much relieved by his remark.

PC: Those first few papers on spin glasses do not actually make any reference to replica symmetry breaking. Were you aware of these ideas at the time when you published your works?

HN: [0:13:28] Yes. I read the papers by Edwards and Anderson[12] and Sherrington and Kirkpatrick[13].

PC: Parisi[14]'s as well?

HN: [0:13:38] No. I don't think so. I read those papers by Sherrington-Kirkpatrick and Edwards-Anderson, but I didn't follow the development of replica symmetry breaking in late '70s, because I was more interested in gauge transformation rather than replica symmetry or replica symmetry breaking.

PC: When did the awareness of ideas of replica symmetry breaking make it to Professor Suzuki's group and to you and your colleagues in Tokyo?

HN: [0:14:24] I, myself, vaguely knew the idea of replica symmetry breaking, the fact that the ideas were developing among European scientists mainly. Professor Suzuki one time attended a conference in Europe[15]. I don't know where it was, but he returned home and said to us that Giorgio Parisi proposed a very complicated mathematical way to avoid the problem of

[12] S. F. Edwards and P. W. Anderson, "Theory of spin glasses," *J. Phys. F* **5**, 965 (1975). https://doi.org/10.1088/0305-4608/5/5/017

[13] D. Sherrington and S. Kirkpatrick, "Solvable model of a spin-glass," *Phys. Rev. Lett.* **35**, 1792 (1975). https://doi.org/10.1103/PhysRevLett.35.1792

[14] See, *e.g.*, G. Parisi, "Toward a mean field theory for spin glasses," *Phys. Lett. A* **73**, 203-205 (1979). https://doi.org/10.1016/0375-9601(79)90708-4; G. Parisi, "Infinite number of order parameters for spin-glasses," *Phys. Rev. Lett.* **43**, 1754 (1979). https://doi.org/10.1103/PhysRevLett.43.1754

[15] Most likely : Disordered Systems and Localization, Rome, Italy, May 1981. See M. Suzuki and S. Miyashita, "Static and dynamic properties of spin glasses," In: *Disordered Systems and Localization*, C. Castellani, C. Di Castro and L. Peliti, eds. (Berlin: Springer-Verlag 1981), 151-155. https://doi.org/10.1007/BFb0012552

negative entropy in the ground state of the Sherrington-Kirkpatrick model. We were very impressed, but he also added that that trick, his method, was too mathematical and artificial. He didn't believe it, he said. I therefore forgot about it afterwards.

PC: Professor Suzuki had contacts in Europe but were you in touch with European scientists at the time in France, Italy, and the UK?

HN: [0:15:30] No. I was a student. It was very difficult for Japanese to visit foreign countries at that time, because of the geographic isolation and it was very expensive. But some of them visited Japan for conferences. I should have met some of them, but I don't remember who they were.

PC: After your thesis you went to the United States for postdocs, first with Robert Griffiths[16]. How did this idea come about? What were you hoping to gain or work on during your postdoc?

HN: [0:16:15] Professor Bob Griffiths from Carnegie Mellon University was very famous for his work on mathematical physics, of course. I used several of his works. He sent a call for a postdoctoral position to many people in the world, including to Professor Suzuki. Professor Suzuki posted that call on the billboard of the group and I came across it. One evening, I went home on the subway, and I met Professor Suzuki on that train. I talked about it, and Professor Suzuki strongly suggested that I applied for it, and I did. Professor Bob Griffiths accepted this way. I went to the United States, but the contract was just for a year because Professor Griffiths was planning to leave for Paris for a sabbatical after one year. So, I started applying for the next position soon after I arrived at Pittsburgh. Professor Michael Stephen[17], from Rutgers University, accepted my application.

PC: In that second postdoc, you worked with Professor Stephen on the Potts glass model[18]. What interested you in this particular model?

HN: [0:17:46] Professor Michael Stephen was an expert of the Potts model. He wrote several papers on the Potts model but without randomness[19]. He was interested in the spin glass analysis using gauge transformation,

---

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

[17] See, *e.g.*, M. O. Scully, E. Abrahams and D. M. Lee, “Obituary of Michael J. Stephen,” *Physics Today*, 1945-0699 (16 May 2007). https://doi.org/10.1063/PT.4.2114

[18] H. Nishimori and M. J. Stephen, “Gauge-invariant frustrated Potts spin-glass,” *Phys. Rev. B* **27**, 5644 (1983). https://doi.org/10.1103/PhysRevB.27.5644

[19] See, *e.g.*, L. Mittag and M. J. Stephen, “Dual Transformations in Many-Component Ising Models,” *J. Math. Phys.* **12**, 441-450 (1971). https://doi.org/10.1063/1.1665606; “Mean-field theory of the many component Potts model,” *J. Phys. A* **7**, L109 (1974). https://doi.org/10.1088/0305-4470/7/9/003

through my talk at Rutgers, at the statistical mechanics meeting organized by Joel Lebowitz there in the math department[20]. So, he accepted my application, and he soon proposed me to work on the Potts model with randomness. So, we started working on it.

PC: One of the findings of that model is that there's a first order transition for certain values of q. That later turned out to be quite important. What were you making of that finding at the time? Was this surprising? Was this interesting?

HN: [0:18:48] I knew that the non-random Potts model with long-ranged, mean-field type interactions, went through a first-order transition for large number of components of q. We generalized it to the random case. But I was not very much surprised because we knew that the Potts model without randomness underwent a first-order transition. It was natural generalization, I thought. What we did was not very much complicated using the replica method in the way that Sherrington-Kirkpatrick did. We generalized it to the Potts case, and we used just another **type** of argument to judge the nature of the transition as first order. I should say I was not very much surprised. It was natural, I thought.

PC: Was Professor Stephen also familiar with the replica trick, or was this your contribution there as well?

HN: [0:20:00] He should have known generally, but I don't think he had any explicit experience of using the replica method to analyze random systems. He was apparently interested in that trick after listening to my talk at the Rutgers meeting.

PC: After that one year at Rutgers, you went back to Japan to take up a faculty position. Then, you worked with a student, Yoshihiro Taguchi, and Takehiko Oguchi, who was his supervisor, on quantum spin glasses[21]. Where did this idea of introducing quantum effects in spin glasses come from, and how did this collaboration come about?

HN: [0:20:52] I hadn't worked on quantum spin systems very much until I returned to Tokyo after my postdoc days in the United States. Professor Oguchi[22] had been working on many topics of statistical mechanics, of spin

---

[20] 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

[21] H. Nishimori, Y. Taguchi and T. Oguchi, "Ground state of quantum spin glass with infinite range interactions," *J. Phys. Soc. Japan* **55**, 656-659 (1986). https://doi.org/10.1143/JPSJ.55.656

[22] Takehiko Oguchi: https://ja.wikipedia.org/wiki/%E5%B0%8F%E5%8F%88%E6%AD%A6%E5%BD%A6

systems, including quantum spin systems, such as the Heisenberg model and the XY model. Professor Oguchi had a very large group. He very frequently invited nearby faculty members and students for lunch, and I joined in those occasions. We had many chats over lunch, which included about quantum spin systems. He was working on the properties of the ground state of non-random frustrated quantum systems, such as the antiferromagnetic Heisenberg model on the triangular lattice, which was a very fashionable topic at that time, around the middle of the 1980s. He was working on it by various methods, including numerical diagonalization of large matrices representing those quantum spin systems. I learned that topic that way, by conversation over lunch. Professor Oguchi suggested me to work on it as well if I was interested in numerical computations. I didn't have experience in matrix diagonalization of quantum spin systems, But Professor Oguchi was a very big name at that time in Japan in the community of statistical physics, so I said vaguely yes. I started working on it, but soon I became very fascinated by that method because computers work as programmed. I just write the program and the computer spits out the answer correctly, unlike analytical calculations, which made a lot of errors when I worked on [them]. Of course, there were bugs in the computer programs, but they were simple to fix. So, I was so fascinated by that method. I wrote very many versions of the program. I became very familiar with that method. At one time, I made a world record of matrix diagonalization of quantum systems spin systems with 27 spins. This was a world record at the time, in the late 1980s. Professor Oguchi was very impressed by the result and encouraged me to go further. That attitude was very relatively rare as an old established figure in Japan at the time. In retrospect, he was very fair. Yoshiro Taguchi[23], as a student, joined the project. We very energetically studied a number of ground state problems of quantum spin systems numerically and analytically[24]. We naturally extended that method to the random spin systems with quantum randomness because computer programs didn't care if the interactions were random or not.

[23] Yoshihiro Taguchi, "規則格子, 不規則格子およびフラクタル格子上の相転移 [Phase transitions on regular, irregular and fractal lattices]," PhD Thesis, Tokyo Institute of Technology (1988). https://ci.nii.ac.jp/naid/500000040655/ (Accessed March 7, 2023.)

[24] See, *e.g.*, T. Oguchi, H. Nishimori and Y. Taguchi, "The spin wave theory in antiferromagnetic Heisenberg model on face centered cubic lattice," *J. Phys. Soc. Japan* **54**, 4494-4497 (1985). https://doi.org/10.1143/JPSJ.54.4494; "Ground state of antiferromagnetic quantum spin systems on the triangular lattice," *J. Phys. Soc. Japan* **55**, 323-330 (1986). https://doi.org/10.1143/JPSJ.55.323

PC: Not long after that, you worked on the perceptron, the Hopfield model, and neural networks more generally[25]. How did you get interested in this class of problems?

HN: [0:27:27] That topic of neural networks, in particular the Hopfield model, was very popular at that time, around 1990, mainly because of the famous paper by John Hopfield proposing the model under his name[26]. Also, the topic became very active by the famous papers by three Israeli scientists: Daniel Amit, Hanoch Gutfreund and Haim Sompolinsky[27]. Those papers represented direct applications of the spin glass theory to a neural network model, the Hopfield model. They were very beautiful papers, although it involved a lot of complicated computations using the replica method and its symmetry breaking. Frankly, I didn't very much care about the physics of the neural network problem, but I was very strongly attracted by the technical aspects of those developments, including replica symmetry and replica symmetry breaking. I became aware of that topic this way.

PC: Did you become aware of it mostly through reading papers, or were you traveling to Israel or traveling to Europe, where these communities were established?

HN: [0:27:05] No. As I said, it was very difficult for young researchers to go abroad from Japan to visit.

PC: Even as a faculty, that remained a challenge?

HN: [0:27:17] Yes. I didn't have a lot of funding at a time.

PC: What was your impression of the community from such a distance?

HN: [0:27:34] As I said, I didn't have direct communications with that community of neural networks, or error correcting codes, or spin glasses, but I respected those physicists working on those topics of neural network

---

[25] See, *e.g.*, M. Shiino, H. Nishimori and M. Ono, "Nonlinear Master Equation Approach to Asymmertrical Neural Networks of the Hopfield-Hemmen Type," *J. Phys. Soc. Japan* **58**, 763-766 (1989). https://doi.org/10.1143/JPSJ.58.763; H. Nishimori, T. Nakamura and M. Shiino, "Retrieval of spatio-temporal sequence in asynchronous neural network," *Phys. Rev. A* **41**, 3346 (1990). https://doi.org/10.1103/PhysRevA.41.3346

[26] John J. Hopfield, "Neural networks and physical systems with emergent collective computational abilities," *Proc. Natl Acad. Sci. U.S.A* **79**, 2554-2558 (1982). https://doi.ord/10.1073/pnas.79.8.2554

[27] See, e.g., Daniel J. Amit, Hanoch Gutfreund and Haim Sompolinsky, "Storing infinite numbers of patterns in a spin-glass model of neural networks," *Phys. Rev. Lett.* **55**, 1530 (1985). https://doi.org/10.1103/PhysRevLett.55.1530

theory or error correcting codes. They found very interesting problems outside of physics yet solved [them] by physics-based approaches.

PC: A couple years later, you tried in a direction you were just mentioning, error correcting codes[28]. How did you get interested in that problem and its connection to spin glasses? Was it also through the literature?

HN: [0:28:22] Yes, through the literature. In fact, Professor Pal Rujan, from [Oldenburg,] Germany sent me a preprint—he knew my work on spin glass theory—by post, of course at that time. His manuscript, which was later published in *Physical Review Letters*[29], showed by numerical methods that decoding of an error correcting code becomes optimal, in some sense, at a finite temperature that corresponds to the so-called Nishimori line in the phase diagram—of the spin glass theory. I didn't have any knowledge about error-correcting codes at that time, but I was attracted by his statements. So, I started to learn that topic very vigorously. I understood what it meant, and started to work to prove that his numerical result was indeed correct. I could prove it analytically. During that process, I learned that Nicolas Sourlas, from Paris, opened that field for physicists by his famous paper [that] appeared in Nature[30]. I just wondered why it was published in *Nature* that mathematical topic.

PC: You later collaborated with a researcher in Hong Kong, Michael Wong, on the statistical mechanics of both error-correcting codes and image restoration[31]. Was there a natural sequence of papers from your original work to that one or was it fairly discontinuous? How did this come about?

HN: [0:30:10] I didn't know the topic of image restoration from the perspective of statistical mechanics. Michael Wong, I met him several times at conferences. We knew each other, and he invited me to apply for a visiting position at his university, Hong Kong University of Science and Technology. I applied and I was accepted to stay there for several months. He told me about the topic of image restoration, and he proposed to work on it by using mean-field type theory corresponding to spin glass physics. We started working on it this way.

---

[28] See, *e.g.*, H. Nishimori, "Optimum decoding temperature for error-correcting codes," *J. Phys. Soc. Japan* **62**, 2973-2975 (1993). https://doi.org/10.1143/JPSJ.62.2973
[29] P. Rulján, "Finite temperature error-correcting codes," *Phys. Rev. Lett.* **70**, 2968 (1993). https://doi.org/10.1103/PhysRevLett.70.2968
[30] N. Sourlas, "Spin-glass models as error-correcting codes," *Nature* **339**, 693-695 (1989). https://doi.org/10.1038/339693a0
[31] See, *e.g.*, H. Nishimori and K. Y. M. Wong, "Statistical mechanics of image restoration and error-correcting codes," *Phys. Rev. E* **60**, 132 (1999). https://doi.org/10.1103/PhysRevE.60.132

PC: These topics are more typically appear in computer science or electrical engineering departments. Was there anyone in these communities with whom you could be in touch in Japan or beyond? Or was this purely a physics treatment in your experience?

HN: [0:31:14] The latter, the physics experience. Pal Rujan and Michael Wong taught those topics with their papers and [through] direct communications in the case of Michael Wong. I didn't know what was the perception of those developments from the perspective of the electrical engineering community. I was just involved in the physics community.

PC: So, you never went to engineering meetings to present this work?

HN: [0:31:55] Not at all, just at JPS meetings and via papers.

PC: Shortly after that, you started to work on quantum annealing, which I guess drew you away from these error-correcting codes and alike. Was there any connection between this new direction and the work you had done on spin glasses and complex systems before then?

HN: [0:32:23] Yes, in some sense. I started to work on the quantum version of the Hopfield model of neural networks with a postdoc, Yoshihiko Nonomura, in the middle of the 1990s[32]. The Hopfield model was sort of exactly what Amit Gutfruend and Haim Sompolinsky had worked on about 10 years [prior]. I came up with the idea to introduce quantum effects in the Hopfield model in the form of a transverse field, the *x*-component of the Pauli matrix. I worked on it very vigorously and we came up with a result that surprised us, in the sense that the quantum effects in the form of the transverse field played almost the same role as the classical thermal effects [in] the case of the classical Hopfield model. So, I started work on quantum problems very vigorously.

PC: Was the work that had been done in the early '80s on classical annealing in complex systems a parallel or a source of inspiration for quantum annealing[33]?

[32] See, *e.g.*, Y. Nonomura and H. Nishimori, "Quantum Hopfield Model," arXiv:cond-mat/9512142 (1995). https://doi.org/10.48550/arXiv.cond-mat/9512142; H. Nishimori and Y. Nonomura, "Quantum effects in neural networks," *J. Phys. Soc. Japan* **65**, 3780-3796 (1996). https://doi.org/10.1143/jpsj.65.3780

[33] See, *e.g.*, T. Kadowaki and H. Nishimori, "Quantum annealing in the transverse Ising model," *Phys. Rev. E* **58**, 5355 (1998). https://doi.org/10.1103/PhysRevE.58.5355

HN: [0:34:04] Yes. Of course, everyone knew the work of Scott Kirkpatrick and others on simulated annealing[34]. I, of course, knew that work. As I said, I found that quantum effects in the Hopfield model played almost the same role as classical thermal effects do. So, I combined those two ideas to try to find the ground state of the classical spin glass using quantum effects, instead of classical thermal effects, which naturally led to the idea of quantum annealing.

PC: A few years later you started using quantum annealing to study quantum spin glass models[35]. What was the motivation for studying this class of model this way?

HN: [0:35:12] In our first paper on quantum annealing in 1998 with Tadashi Kadowaki, we studied spin glass problem itself because I had studied the spin glass problem for a long time, and I of course knew that the identification of the ground state properties was a very difficult but important problem. Quantum annealing was designed to find that state of the Ising model with general interactions, including the random case, so it was very natural for me to apply the idea of quantum annealing to spin glass systems. We found, very surprisingly at that time, that quantum effects were better at finding the ground state than classical thermal effects. Better in the sense that the probability of finding the ground state was much higher in the quantum case than in the classical case. We didn't know why, but the numerical results showed that. This way, I entered the field.

PC: In parallel, over the last 20 years, there was a revival of interest in the so-called Nishimori line and its importance in mathematical physics circles. Had you paid attention to the use of mathematical physics to study spin glasses up until that point?

HN: [0:36:54] Not so much. I knew that my results were used under some context in mathematical physics[36], in information related problems, but I didn't follow those papers very seriously. Partly because I was working on quantum annealing very vigorously and I didn't have any capacity to work

[34] S. Kirkpatrick, C. D. Gelatt Jr. and M. P. Vecchi, "Optimization by simulated annealing," *Science* **220**, 671-680 (1982). https://doi.org/10.1126/science.220.4598.671

[35] See, *e.g.*, K. Nishimura, H. Nishimori, A. J. Ochoa and H. G. Katzgraber, "Retrieving the ground state of spin glasses using thermal noise: Performance of quantum annealing at finite temperatures," *Phys. Rev. E* **94**, 032105 (2016). https://doi.org/10.1103/PhysRevE.94.032105

[36] See, *e.g.*, P. Le Doussal and A. B. Harris, "Location of the Ising spin-glass multicritical point on Nishimori's line," *Phys. Rev. Lett.* **61**, 625 (1988). https://doi.org/10.1103/PhysRevLett.61.625; Y. Iba, "The Nishimori line and Bayesian statistics," *J. Phys. A* **32**, 3875 (1999). https://doi.org/10.1088/0305-4470/32/21/302

on the spin glass theory or its applications; Partly because I didn't think I had any idea to work on it beyond my old method in my thesis.

PC: How did your collaboration with Pierluigi Contucci and your student Satoshi Morita then arise[37]?

HN: [0:37:48] Pierluigi Contucci was very interested in my method to derive the exact solution for the spin glass problem. He contacted me by email already at the time, and he visited me. He naturally proposed the idea to use our method to various problems that I didn't know.

PC: After that collaboration, you didn't pursue much in the mathematical physics of spin glasses, but it you keep abreast of what happened in the years since?

HN: [0:38:42] Frankly, not so much again. I paid attention from time to time, but I didn't work very vigorously, unless I was invited to do so like Pierluigi did.

PC: One of the collaborators you worked with for many years is David Sherrington[38], from the mid-90s and over a decade afterwards[39]. How did you get to know David and got to spend a sabbatical at Oxford in 1994?

HN: [0:39:22] He was very famous, and we met at a few conferences. He visited Japan once or twice and we met. Probably, I met him at some conference in Europe. (I don't remember.) He was very friendly, as we all know, and he was very active. I was very attracted by his character. I was looking for somewhere to spend a sabbatical, so I applied. I asked him to accept me, and he did.

PC: You kept working for many more years after that sabbatical. Where were the ideas for problems coming from?

---

[37] See, *e.g.*, S. Morita, H. Nishimori and P. Contucci, "Griffiths inequalities for the Gaussian spin glass," *J. Phys. A* **37**, L203 (2004). https://doi.org/10.1088/0305-4470/37/18/L03

[38] See, *e.g.*, P. Charbonneau, *History of RSB Interview: David Sherrington*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 39 p. https://doi.org/10.34847/nkl.072dc5a6

[39] See, *e.g.*, H. Nishimori, W. Whyte and D. Sherrington, "Finite-dimensional neural networks storing structured patterns," *Phys. Rev. E* **51**, 3628 (1995). https://doi.org/10.1103/PhysRevE.51.3628; T. Obuchi, H. Nishimori and D. Sherrington, "Phase diagram of the p-spin-interacting spin glass with ferromagnetic bias and a transverse field in the infinite-*p* limit," *J. Phys. Soc. Japan* **76**, 054002 (2007). https://doi.org/10.1143/JPSJ.76.054002

HN: [0:40:21] Maybe from communications with David Sherrington. I don't remember very clearly, but David Sherrington proposed the idea for some problem, and we started to talk about [those to] each other. I'm sorry I don't remember very clearly but probably it was David Sherrington.

PC: Did you keep visiting Oxford regularly, or just was this largely through email?

HN: [0:40:57] Largely through email. I think I visited once or twice, or more than twice, but not very frequently.

PC: You did co-organize his 65th birthday party and the conference associated with it[40]. How did that come about?

HN: [0:41:21] This is a very impressive. Ton Coolen, who was probably a postdoc with David Sherrington when I spent a sabbatical there, we [became] friend and we communicated from time to time. Ton Coolen invited me to join the organization of the birthday conference. It was a very impressive conference.

PC: It looked like a pretty big event.

HN: [0:41:50] Yes. Many famous people gathered and talked about very recent and interesting topics.

PC: During your time at the Tokyo Institute of Technology or elsewhere, did you ever teach about spin glasses or and or replica symmetry breaking? If yes, can you detail what those classes might have been?

HN: [0:42:16] I taught a graduate course of statistical physics at Tokyo Institute of Technology many times over two decades or more[41]. I taught about very many topics, including spin glasses and the replica symmetric solution, but I didn't give details of replica symmetry breaking because it was too advanced for general physics students. I taught the same topics at several other universities in Japan as guest lecturer, but that's all. I should say that

---

[40] *Viewing The World Through Spin Glasses*, *in honour of David Sherrington on the occasion of his 65th birthday*, Ton Coolen, Hidetoshi Nishimori, Nicolas Sourlas and Michael Wong, Oxford, UK, 31 August-1 September 2007. Proceedings: T. Coolen, H. Nishimori, N. Sourlas and M. Wong, "Viewing the world through spin glasses," *J. Phys. A* **41**, 320301 (2008). https://doi.org/10.1088/1751-8121/41/32/320301

[41] **HN:** A textbook emerged from this course: H. Nishimori, 相転移･臨界現象の統計物理学 [Statistical Physics of Phase Transitions and Critical Phenomena] (Tokyo, 培風館 [Baifukan], 2005]. Translated and updated version: H. Nishimori and G. Ortiz, *Elements of phase transitions and critical phenomena* (New York: Oxford University Press, 2011).

I didn't teach the replica symmetry breaking to students, but the replica symmetric solution.

PC: When would this have started for the first time approximately?

HN: [0:43:22] Probably around 1990.

PC: You eventually wrote a book in Japanese called the *Statistical Physics of Spin Glasses and Information Processing*[42]. Was this at all related to the class you were teaching?

HN: [0:43:44] Yes. The book was a collection of my lecture notes on the subject at Tokyo Tech and at other universities. But the contents were greatly expanded by including many advanced topics, such as the replica symmetry breaking, neural networks. I thought the topic was too advanced for the reader of books in Japan, but the editor of the book from Iwanami publisher, Mr. Yoshida, encouraged me. He said: "Write everything you want to write. We don't care if it doesn't sell." But it sold a lot.

PC: Have you ever taught using that book as a textbook?

HN: [0:44:35] I used that book in my course. I used part of the book in the course.

PC: But never the replica symmetry breaking part.

HN: [0:44:46] Never. There's the concept of symmetry breaking, which was very useful and very beautiful, but I said: "If you are interested, you can read the book."

PC: This book quickly got translated to English at Oxford University Press. How did that happen?

HN: [0:45:10] Professor David Sherrington was on the governing board of Oxford University Press. He was looking for someone to write a book for Oxford University Press. He asked me to do so, so I [followed] his suggestion.

PC: As you said, the Japanese book was very well received. It sold well. How was the Oxford book received? Did it also have a good reception?

---

[42] H. Nishimori, スピングラス理論と情報統計力学 [Spin glass theory and information statistical mechanics] (Tokyo: 岩波書店 [Iwanami Shoten], 1999). H. Nishimori, *Statistical physics of spin glasses and information processing: an introduction* (Oxford: Oxford University Press, 2001).

HN: [0:45:43] Generally, I think so, but I vaguely remember that a book review appeared in some journal about my book on the spin glass theory[43]. I don't remember the statement very clearly, but that review was not very friendly. It was too mathematical, too complicated for the audience. It vaguely said that way, I remember. But it's widely read recently, partly because of the developments that you mentioned, the information theory related developments. Since the book had been on the market for more than 20 years, I decided to make it open access, so that students can read it more easily. I already made an arrangement with Oxford University Press and the open access version will appear in several months.

PC: Thank you! We went broadly over this period. Is there anything else about this era that we may have missed or skipped over that you think would be germane to this discussion?

HN: [0:47:23] No. I don't think so.

PC: In closing, do you still have notes, papers or correspondence that concerns this topic? If yes, do you intend to deposit them in an academic archive at some point?

HN: [0:47:46] Unfortunately, no. I retired officially as a professor about three years ago. At that time, I discarded almost all those materials because I thought I wouldn't need them. The only ones that I still keep are the computation notes about the quantum Hopfield model, which involved a lot of computations, more than 100 pages. I couldn't discard those notes because I thought I may have to follow them once again, to do something beyond them. If you are interested, I can show them to you, but that's the only [thing]. [Another one] that I remember, but I don't have that copied, is the statement by the referee to my first paper on the exact solution, which said—this is the correct statement; this is a statement that I correctly remember because it was very impressive—"The model is artificial, and the result is thin to warrant publication."

PC: Professor Nishimori, thank you very much for this discussion.

HN: [0:49:14] You're welcome.

---

[43] R. Podgornik, "Book Review: Statistical Physics of Spin Glasses and Information Processing. Hidetoshi Nishimori, Oxford Science Publications, Oxford University Press, 2001," *J. Stat. Phys.* **109**, 335-337 (2002). https://doi.org/10.1023/A:1020091132740