History of RSB Interview: Fumihiko Tanaka

October 27-November 22, 2022. Final revision: December 30, 2022

Interviewers:
Patrick Charbonneau, Duke University, patrick.charbonneau@duke.edu
Location:
Assembled from a series of email exchanges over the interview period.
How to cite:
P. Charbonneau, *History of RSB Interview: Fumihiko Tanaka*, transcript of an oral history conducted 2022 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 11 p. https://doi.org/10.34847/nkl.adfcm02v

PC: Can you tell us a bit more about your family and studies before starting university?

FT: My father was the vice-president of a publishing company specialized in a wide range of educational books for primary school as well as for junior- and senior-high school students. He wanted me to become a medical doctor or an architect. But when I was in junior high school—at age 14—I realized that I had a strong attachment to mathematics and physics. I was particularly inspired when I saw some of Euler's formulae on infinite power series in a math dictionary in the school library.

In the second year of senior-high school in Nara, Naoki Onishi[1]—then a graduate student at Osaka University, later a famous professor of nuclear physics—was in charge of our class as a part-time lecturer of a physics course. I was deeply impressed by his lecture on mechanics and had a dream of becoming a physicist.

PC: How did you get interested in physics, and later to pursue a PhD in theoretical physics?

FT: In 1966, I entered the Department of Architecture in the Faculty of Engineering of the University of Tokyo at my father's request. I studied design a couple of years. But occasionally, I felt strong attachment to my old dream. I finally changed my specialty and moved to the Department of Physics in the Faculty of Science. Doing this, I wasted two years.

---

[1] See, *e.g.*, "Naoki Onishi," *inspire HEP* (2021). https://inspirehep.net/authors/994868 (Consulted November 23, 2022.); "Naoki Onishi," *The University of Tokyo* (n.d.) http://www7b.biglobe.ne.jp/~ftanaka/member/onishi/index.html (Consulted November 28, 2022.)

PC: You did not work on spin glasses during your graduate studies, but you did start working on systems with quenched disorder shortly afterwards, notably using the replica trick. How did you learn about the replica trick and spin glasses? And how did you get interested in disordered systems?

FT: I was a physics graduate student at the University of Tokyo (1971-1976), where many world-famous professors of physics, especially elementary particle physics and statistical mechanics, were working. During the first two years of the graduate course, I studied under the supervision of Professor Taro Kihara (1917-2001) the chemical physics of the liquid state for materials such as electrolytes, anisotropic liquids, polymer solutions, etc. Because Kihara retired, I then moved to Professor Ryogo Kubo[2]'s group. My direct supervisor was Professor Masuo Suzuki[3], who was actively working on phase transitions and critical phenomena. Many students in the group had a strong interest in the theoretical study of phase transitions.

Under the supervision of Professor Suzuki, I first learned about Wilson's renormalization group theory of critical phenomena. The topic of my Ph.D. thesis[4] was its application to quantum phase transitions—mainly Bose condensation—to investigate the effect of quantum physics on critical phenomena[5]. Because Wilson and Fisher solved the main problem of critical phenomena, most of us—the students in Kubo's group—were looking for other fundamental but unsolved problems in statistical mechanics.

In the middle of 1970s, there were many interesting problems: superfluid ^{3}He[6] (1972), Kosterlitz-Thouless transition[7] (1973), scaling theory of polymer solutions[8] (1973), spin glasses[9] (1975). And later: volume transition of

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

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

[4] F. Tanaka, *Renormalization Group Theory of Critical Dynamics in Bose Systems*, PhD Thesis, University of Tokyo (1976). http://gazo.dl.itc.u-tokyo.ac.jp/gakui/cgi-bin/gazo.cgi?no=103843 (Consulted December 30, 2022.)

[5] See, e.g., M. Suzuki and F. Tanaka, "Critical Dynamics in Bose System," *Prog. Theor. Phys.* **52**, 344-346 (1974). https://doi.org/10.1143/PTP.52.344; F. Tanaka, "Coherent representation of dynamical renormalization group in Bose systems," *Prog. Theor. Phys.* **54**, 1679-1692 (1975). https://doi.org/10.1143/PTP.54.1679

[6] D. D. Osheroff, W. J. Gully, R. C. Richardson and D. M. Lee, "New magnetic phenomena in liquid He 3 below 3 mK," *Phys. Rev. Lett.* **29**, 920 (1972). https://doi.org/10.1103/PhysRevLett.29.920

[7] Kosterlitz-Thouless transition: https://en.wikipedia.org/wiki/Kosterlitz%E2%80%93Thouless_transition

[8] See, *e.g.*, P.-G. de Gennes, "Exponents for the excluded volume problem as derived by the Wilson method," *Phys. Lett. A* **38**, 339-340 (1972). https://doi.org/10.1016/0375-9601(72)90149-1; *Scaling Concepts in Polymer Physics* (Ithaca, NY: Cornell University Press, 1979).

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

gels[10] (1978), scaling theory of Anderson localization[11] (1979), microphase separation of block copolymers[12] (1980), etc. Professor Suzuki was so highly interested in spin glasses that some of his students, Seiji Miyashita[13], Hidetoshi Nishimori[14] and me, intensively worked on the problem. We learned quite a lot about the replica method from the paper by Edwards and Anderson.

First, I tried to apply it to phase transitions under random fields[15], which is a bit easier than for spin glasses, by combining replicas with the renormalization group. That is the start of my work on disordered systems. Then, I attempted to extend the replica technique to dynamical problems. I took the time-dependent Ginzburg-Landau equation for spins with random interaction as a typical example and applied replicas to its path integral form[16].

I finished my Ph.D. in 1976 and was employed by Tokyo university as a research associate of Professor Suzuki starting April 1976.

PC: Do you know what got Professor Suzuki interested in spin glasses in the first place?

---

[10] T. Tanaka, "Collapse of gels and the critical endpoint," *Phys. Rev. Lett.* **40**, 820 (1978). https://doi.org/10.1103/PhysRevLett.40.820

[11] E. Abrahams, P. W. Anderson, D. C. Licciardello and T. V. Ramakrishnan, "Scaling theory of localization: Absence of quantum diffusion in two dimensions," *Phys. Rev. Lett.* **42**, 673 (1979). https://doi.org/10.1103/PhysRevLett.42.673

[12] L. Leibler, "Theory of microphase separation in block copolymers," *Macromolecules* **13**, 1602-1617 (1980). https://doi.org/10.1021/ma60078a047

[13] See, *e.g.*, S. Miyashita and M. Suzuki, "Frustration and Clusters in Spin Glasses–Study on the Low Temperature Phase of the ±*J* Model," *J. Phys. Soc. Jpn.* **50**, 1840-1845 (1981). https://doi.org/10.1143/JPSJ.50.1840; M. Suzuki and S. Miyashita, "Order parameters and nonlinear susceptibilities in spin glasses," Physica A **106**, 344-354 (1981). https://doi.org/10.1016/0378-4371(81)90231-4

[14] H. Nishimori, "Exact results and critical properties of the Ising model with competing interactions," *J. Phys. C* **13**, 4071 (1980). https://doi.org/10.1088/0022-3719/13/21/012; Nishimori, Hidetoshi. "Internal energy, specific heat and correlation function of the bond-random Ising model," *Prog. Theor. Phys.* **66**, 1169-1181 (1981). https://doi.org/10.1143/PTP.66.1169

[15] F. Tanaka, "Phase Transitions in Gaussian Random Magnetic Fields," *Prog. Theor. Phys.* **58**, 1166-1176 (1977) https://doi.org/10.1143/PTP.58.1166; "Phase Transitions in Gaussian Random Fields. II: Ferromagnets in d=4+ε Space Dimensions," *Prog. Theor. Phys.* **59**, 1483-1492 (1978). https://doi.org/10.1143/PTP.59.1483

[16] F. Tanaka, "Dynamical Replica for Quenched Random Spin Systems" *Prog. Theor. Phys.* **59**, 304-305 (1978). https://doi.org/10.1143/PTP.59.304; "Critical Dynamics under Quenched Random Magnetic Fields. I: —1/*m* Expansion of the Dynamic Critical Exponent," *Prog. Theor. Phys.* **60**, 380-392 (1978). https://doi.org/10.1143/PTP.60.380; "Critical Dynamics under Quenched Random Magnetic Fields. II: -ε= 6-*d* Expansion of the Dynamic Critical Exponent," *Prog. Theor. Phys.* **60**, 1686-1691 (1978). https://doi.org/10.1143/PTP.60.16861; "Critical Dynamics under Quenched Random Magnetic Fields," *Prog. Theor. Phys. Supp.* **64**, 442-451 (1978). https://doi.org/10.1143/PTPS.64.442

FT: No, I don't know. He had long been interested in phase transitions and critical phenomena. After the paper by Edwards-Anderson appeared, many Japanese researchers got interested in the problem, because it was a new fundamental unsolved problem in statistical mechanics. This trend was a kind of fashion. Later, Suzuki published a couple of papers with Miyashita on the Monte Carlo simulation of spin glasses[17], which I didn't read.

PC: In 1978, you moved to Cambridge to work with Professor Sam F. Edwards[18]. How did that opportunity come about, and what drew you there? What project were you pursuing?

FT: Professor Kubo and Professor Sam F. Edwards had been old friends. Edwards asked Kubo for a candidate postdoc researcher for the Cavendish Lab. Kubo recommended me. I had the great opportunity to spend my sabbatical leave at the Cavendish. Of course, the great history of the Cavendish laboratory attracted me there. I had no hesitation to go.

One day (19 September 1976), I received a letter from Professor Michael Fisher[19], in which he encouraged me to go to the Cavendish. (He had come to Tokyo University to give a series of lectures on the renormalization group theory. Graduate students of Kubo's group took care of him while he stayed in Japan. He stayed in Japan the whole summer of 1976 and traveled to many places.) In the letter he also referred to de Gennes[20]' work on polymers and encouraged me to visit him while I was in Europe. This letter was another good fortune for my decision.

Given the great reputation of the Edwards-Anderson paper on spin glasses, I expected to work on the problem with many collaborators in Edwards' group.

PC: At that time, were you following the discussion about replica symmetry breaking? Do you know to what extent Professor Edwards followed that discussion?

FT: I started at the Cavendish laboratory[21] in October 1978. Quite surprisingly, I found no one working on spin glasses in Edwards' group of TCM (Theory

[17] See, *e.g.*, S. Miyashita and M. Suzuki, "Effects of frustration on freezing transition" *J. Magnet. Magnet. Mater.* **31**, 1113-1114 (1983). https://doi.org/10.1016/0304-8853(83)90820-X
[18] Sam F. Edwards: https://en.wikipedia.org/wiki/Sam_Edwards_(physicist)
[19] Michael Fisher: https://en.wikipedia.org/wiki/Michael_Fisher
[20] Pierre-Gilles de Gennes: https://en.wikipedia.org/wiki/Pierre-Gilles_de_Gennes
[21] Cavendish laboratory: https://en.wikipedia.org/wiki/Cavendish_Laboratory

of Condensed Matter[22]). Robin Ball[23] and Mark Warner[24] were working as university staffs, Pooran Singh, Richard Rennie and I joined as postdoc. Professor Edwards himself and most of the group members focused their research efforts on polymers. I was totally isolated, but through repeatedly listening to their talks, I was gradually attracted by polymer science.

I learned that replica technique had its genesis in the study of rubber elasticity[25]. Sam invented the method when he looked into the mechanical properties of rubber, which are randomly cross-linked polymer networks, and hence have a frozen topology. People might think that the idea of replica suddenly occurred to him when Anderson talked to him about the problem of spin glasses in the famous "Saturday morning chat". But for him replicas seemed to be a common routine for the study of disordered systems, such as electron localization in disordered media, randomly cross-linked polymer networks, spin glass, random matrices[26] etc.

In the summer of 1978, just before I joined in his group, Sam sent me in Tokyo his paper on rubber elasticity[27] to introduce me to the activity of his group. It treated the deformation of rubber with random cross-links by the replica method. According to this paper, the idea of replicas goes back to talks he gave at conferences in 1970-1971. It was a very tough paper for me, and it still is. Through the struggle to solve its riddle, I fell into polymer science.

Sam was highly enthusiastic about his "tube model" of polymer chain motion[28]. Besides, he was very busy with his work as the former chairman of SRC. I could see him only once a week for a very short time. He first suggested me the problem of Anderson localization, and then of spin glass. The Edwards-Anderson paper on spin glasses caused a great sensation. Its citation was breathtaking. But Sam was not quite happy with the negative

---

[22] "TCM History," *TCM Group* (n.d.). https://www.tcm.phy.cam.ac.uk/about/history/ (Consulted November 25, 2022.)

[23] Robin Christopher Ball, *Replica Theory of Polymer Networks*, PhD Thesis, University of Cambridge (1980). https://idiscover.lib.cam.ac.uk/permalink/f/t9gok8/44CAM_ALMA21428181470003606

[24] Mark Warner, *Molecular motion of polymeric systems*, PhD Thesis, Imperial College London (1977); E. Terentjev, "Professor Mark Warner, FRS," *Liquid Crystals Today* **30**, 56-58 (2022), https://doi.org/10.1080/1358314X.2022.2048970

[25] See, *e.g.*, P. Charbonneau, "From the replica trick to the replica symmetry breaking technique," *IAMP News Bulletin* **2022**(October), 5-25 (2022); arXiv:2211.01802 [physics.hist-ph] https://doi.org/10.48550/arXiv.2211.01802

[26] S. F. Edwards and R. C. Jones, "The eigenvalue spectrum of a large symmetric random matrix," *J. Phys. A* **9**, 1595 (1976). https://doi.org/10.1088/0305-4470/9/10/011

[27] R. T. Deam and S. F. Edwards, "The theory of rubber elasticity," *Philo. Trans. Royal Soc. Lond. A* **280**, 317-353 (1976). https://doi.org/10.1098/rsta.1976.0001

[28] Reptation: https://en.wikipedia.org/wiki/Reptation

entropy in their solution at zero temperature. Therefore, we decided to study the ground state of EA model. People often talk about the degeneracy, but at that time no one actually calculated the residual entropy. I tried to find the solution by many theoretical methods without finding the final answer. I had wasted almost eight months before Sam agreed to my answer in the summer of 1979. Sam loved the result. The discussion we had there was a kind of "peak experience" in my life. I have clear memory when he enthusiastically proved my result [about the ground state energy] to be correct on the blackboard (not on a usual paper pad) in his room in the southeast corner on the 3rd floor of the Cavendish laboratory. It was a very hot summer. A vast swarm of greenflies broke out in the grass of the college courts. They flew into the room through the open windows. Sam's face and mine were totally covered with them when we finished the discussion.

During these days, Sam and I never talked about replica symmetry breaking (RSB). One day (29 June 1979), Professor Michael Moore[29] came to the Cavendish to give a talk on spin glasses. To rescue the negative entropy, he suggested a two-group RSB in his talk. This is the first time I encountered RSB. Then, Professor John A. Hertz[30] came for a talk (11 November 1979), in which he referred to Parisi's work on RSB.

During my stay, famous polymer scientists came to Cavendish for talks, lectures, and discussions. For instance, P.-G. de Gennes gave a very impressive talk in the TCM seminar (November 1978), in which he discussed the excluded-volume effects and microphase separation of polymer networks. Later, Paul J. Flory[31] came (2, 4 and 9 May 1979) to give a series of lectures on polymer solutions and rubbers. I was deeply moved by them. Besides, Murugappan Muthukumar joined us as a postdoc in April 1979, just after finishing his Ph.D. in Chicago[32]. He worked with Sam on polymer solutions in the room next to me in the TCM[33]. We—Muthu and I—often talked at lunchtime about various hot topics in physics of the era, such as renormalization group, scaling law of polymer solutions, spin glasses, etc. Also, in

---

[29] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Michael Moore*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 26 p. https://doi.org/10.34847/nkl.997eiv27

[30] See, *e.g.*, P. Charbonneau, *History of RSB Interview: John A. Hertz*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 18 p. https://doi.org/10.34847/nkl.cad347wh

[31] Paul Flory: https://en.wikipedia.org/wiki/Paul_Flory

[32] Murugappan Muthukumar, *Polymer dynamics and hydrodynamics of suspensions at nonzero concentrations*, PhD Thesis, University of Chicago (1979). http://pi.lib.uchicago.edu/1001/cat/bib/283040

[33] See, *e.g.*, M. Muthukumar and S. F. Edwards, "Extrapolation formulas for polymer solution properties," *J. Chem. Phys.* **76**, 2720-2730 (1982). https://doi.org/10.1063/1.443257; "Screening concepts in polymer solution dynamics," *Polymer* **23**, 345-348 (1982). https://doi.org/10.1016/0032-3861(82)90333-0

June 1979, Professor Josef Jäckle from the University of Konstanz joined us as a visiting professor. He was a specialist of structural glasses[34]. He helped me acquire the general background knowledge about glasses. These experiences proved to be a major turning point in my academic career.

PC: In your work "Analytic theory of the ground state properties of a spin glass"[35], you estimated the ground state of the infinite-range Ising spin glass. What was the reaction to your work?

FT: Immediately after we wrote the papers, we received several quick responses. Sam sent the copy to several places and seemed to have discussed our results with them on the phone. We submitted the [pair of] papers on 12 March 1980 to *J. Phys. F*. Then, the work by Bray and Moore on the counting of the number of TAP solutions came out[36] (submission date is 17 April 1980). I also received a letter from De Dominicis[37], from the Saclay group, in which he mentioned that they reached a conclusion on the existence of many solutions of the TAP equations in 1979[38], although their full paper appeared later[39].

I was back in Tokyo at the end of March 1980. I talked about our work in several places in Japan, in particular, in the seminar of Kubo's group. Professor Kubo liked it and introduced me to the famous work by Wannier[40] on the ground state entropy of the frustrated antiferromagnetic Ising model on a triangular lattice[41]: 0.33831, which is about 1.5 times larger than 0.19923 for the SK model. Some of his graduate students, including

[34] See, *e.g.*, J. Jäckle, "On the glass transition and the residual entropy of glasses," *Philo. Mag. B* **44**, 533-545 (1981). https://doi.org/10.1080/01418638108224037

[35] S. F. Edwards and F. Tanaka, "The ground state of a spin glass," *J. Phys. F* **10**, 2471 (1980). https://doi.org/10.1088/0305-4608/10/11/019; F. Tanaka and S. F. Edwards, "Analytic theory of the ground state properties of a spin glass. I. Ising spin glass," *J. Phys. F* **10**, 2769 (1980). https://doi.org/10.1088/0305-4608/10/12/017; "Analytic theory of the ground state properties of a spin glass. II. XY spin glass," *J. Phys. F* **10**, 2779 (1980). https://doi.org/10.1088/0305-4608/10/12/018

[36] A. J. Bray and M. A. Moore, "Metastable states in spin glasses," *J. Phys. C* **13**, L469 (1980). https://doi.org/10.1088/0022-3719/13/19/002

[37] Cirano De Dominicis: https://de.wikipedia.org/wiki/Cyrano_de_Dominicis

[38] C. De Dominicis, "Solution des équations de Thouless-Anderson-Palmer pour le modèle de verre de spin de Sherrington-Kirkpatrick," *C. R. Acad. Sc. Paris B* **289**, 281-284 (1979). https://gallica.bnf.fr/ark:/12148/bpt6k54906950/f91.item (Consulted November 23, 2022.)

[39] C. De Dominicis, M. Gabay, T. Garel and H. Orland, "White and weighted averages over solutions of Thouless Anderson Palmer equations for the Sherrington Kirkpatrick spin glass," *J. Physique* **41**, 923-930 (1980). https://doi.org/10.1051/jphys:01980004109092300. The manuscript has a submission date of 19 March 1980.

[40] Gregory Wannier: https://en.wikipedia.org/wiki/Gregory_Wannier

[41] G. Wannier, "Antiferromagnetism. The triangular Ising net," *Phys. Rev.* **79**, 357 (1950). https://doi.org/10.1103/PhysRev.79.357

H. Nishimori in the Suzuki group, started a similar counting work on frustrated systems. Later, a famous Japanese mathematician taught me that the ground state degeneracy corresponds to the memory capacity of the Hopfield model of neural networks.

PC: Were you then aware of Giorgio Parisi's parallel efforts to obtain the ground state energy through RSB?

FT: No, I actually didn't. I knew his work very indirectly, only through the talk by John A. Hertz in Cavendish Lab. (11 November 1979).

PC: What was your reaction to RSB when you did? What about Prof. Edwards'?

FT: Parisi's first paper appeared in December[42]. I read it. Coincidentally, it was submitted to PRL in June, when Sam and I found the zero-point entropy. My first impression on his RSB work was a bit passive. It gave me too much of an artificial impression. The physical reality behind the mathematics was hardly recognizable to me. Unfortunately, Sam was so busy—as usual—that I could not find the chance to discuss Parisi's RSB with him.

Much later, when Sam was invited to Japan in 1996 as an eminent professor of the Japan Society for the Promotion of Science (JSPS)[43]. He then stayed in Tokyo and gave lectures at several places, one of which was the Tokyo University of Agriculture and Technology, where I was. Sam and I had a plenty of time to discuss. He gave me a handwritten note about a new idea for spin glasses, in which he referred to Parisi's RSB[44] and proposed a new method to study RSB—Fourier transforming the replica space. (I would like to deposit his note in an academic archive because some might be curious to know more about this idea.)

PC: After leaving Cambridge, you published a paper on spin glasses with Professor Nishimori[45]. How did this collaboration come about?

---

[42] G. Parisi, "Infinite number of order parameters for spin-glasses," *Phys. Rev. Lett.* **43**, 1754 (1979). https://doi.org/10.1103/PhysRevLett.43.1754. An article appeared earlier, but was not as complete: 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

[43] Japan Society for the Promotion of Science: https://en.wikipedia.org/wiki/Japan_Society_for_the_Promotion_of_Science

[44] S. F. Edwards, "New Ideas on Spin Glasses," *handwritten notes* (17 September 1996), p. 17-18.

[45] F. Tanaka and H. Nishimori, "Distributions of the metastable energy levels and the internal magnetic fields in spin glasses," *J. Phys. F* **11**, 1237 (1981). https://doi.org/10.1088/0305-4608/11/6/010

FT: H. Nishimori was a graduate student in the Kubo-Suzuki group when I was back. He suggested that we evaluate the ground state degeneracy, together with the energy distribution, for the planar Potts model, i.e., a simple extension of Ising model. It was straightforward work, and we quickly found the answer. Nishimori then moved on to quantum spins, and I left spin glasses.

PC: What new research opportunities drew you away?

FT: I had been interested in polymer science before I started research on spin glasses, because for some reason Professor Kubo did many theoretical works on the rubber elasticity in the early stage of his career (1940s). He wrote a book[46], and it attracted me.

During my stay at the Cavendish, Sam introduced me to his ideas on several unsolved problems in polymer physics using his characteristic field-theoretical forms. Most of them were very tough, difficult to understand but exciting. Through the struggle to understand them, I fell into polymer science. Besides, many world-famous polymer scientists, including P. J. Flory and P.-G. de Gennes came for talks. I was deeply moved.

In 1981, I moved to TUAT (Tokyo University of Agriculture and Technology) as an assistant professor. In the research group, there were a couple of famous theoretical physicists working on polymers. (The university had formerly been a college for silk related industry.) I took this chance to start my new career, and completely left spin glasses. I thought that decision was right, and it turned out to be so, because during the rest of my life I was able to accomplish two major works on thermoreversible gels: the rheology of transient networks with Sam Edwards (1992)[47], and the thermal properties of reversible polymer networks with Walter H. Stockmayer[48] (1994)[49]. I have enjoyed working in this field, and in fact I still am[50].

---

[46] R. Kubo, ゴム弾性 [Gomu dansei, Rubber Elasticity] (Tokyo: Kawade Shobō, 1947). https://catalog.umd.edu/docno=002264341

[47] F. Tanaka and S. F. Edwards, “Viscoelastic properties of physically crosslinked networks. 1. Transient network theory,” *Macromolecules* **25**, 1516-1523 (1992). https://doi.org/10.1021/ma00031a024; “Viscoelastic properties of physically crosslinked networks: Part 2. Dynamic mechanical moduli,” J Non-Newton. Fluid Mech. **43**, 273-288 (1992). https://doi.org/10.1016/0377-0257(92)80028-V

[48] Walter H. Stockmayer: https://en.wikipedia.org/wiki/Walter_H._Stockmayer

[49] F. Tanaka and W. H. Stockmayer, "Thermoreversible gelation with junctions of variable multiplicity," *Macromolecules* **27**, 3943-3954 (1994). https://doi.org/10.1021/ma00092a039

[50] See, *e.g.*, F. Tanaka, “Thermoreversible Gelation of Associating Polymers in Hydrogen-Bonding Mixed Solvents," *Langmuir* **38**, 5098-5110 (2021). https://doi.org/10.1021/acs.langmuir.1c02040; F. Tanaka, “Thermoreversible Gelation Interfering with Phase Separation in Multicomponent Mixtures of Associating Polymers,” *Macromolecules* **55**, 5233-5248 (2022). https://doi.org/10.1021/acs.macromol.2c00921

PC: Did you nevertheless keep track on the research on spin glasses and RSB?

FT: No, I didn't. I have been completely occupied by polymer science.

PC: Did ideas from spin glasses and RSB nevertheless find any use in your subsequent research career?

FT: Yes. Replicas seemed to be a very efficient way to study elastic properties of randomly cross-linked polymer networks, and also conformational changes and phase separation of random copolymers. Cross-linking reactions take place randomly. The topological structure of the products is not controllable. Therefore, one must take a weighted average of deformed rubbers over all topologies to compare with experiments. Hence, we need replicas. In fact, Deam and Edwards did this in the very early era of science of random complex systems. However, I am still not sure how to break replica symmetry in rubbers. Copolymerization of many component monomers (21 for proteins) also takes place randomly because polymerization reactions are statistical. Therefore, for the study of random copolymers in solutions, one has to take a weighted average over all random sequences. Thus, we also need replicas. But again, I don't know how to break replica symmetry.

PC: During your time at Cambridge, the Tokyo University of Agriculture and Technology, the University of Kyoto or elsewhere, did you ever teach or lecture about RSB or spin glasses? If yes, can you detail?

FT: No, I was not able to teach RSB. My understanding of RSB still remains incomplete. I can't find how to find the condition as to whether RSB is required, or RSB is not necessary (also known as *innocent replicas*). For instance, the calculation of the SK ground state degeneracy and its energy distribution without RSB turned out to be correct. As for polymer science, I can't find how to break symmetry for rubbers and random copolymers.

Thermoreversible polymer gels have cross-links that lie somewhere in between quenched randomness (with n=0 replicas) and annealed randomness (with n=1 replicas). They are neither completely frozen nor completely equilibrated. Experimental data depend on the time scale of observation. For these systems, I think we need a new type of RSB.

PC: Do you still have notes, papers, or correspondence from that epoch? If yes, do you intend to deposit them in an academic archive?

FT: I have the following documents:

1) Sam wrote many ideas and math formulae on paper pads during our discussions. Most of them were improvised, and not well organized. They were difficult to read. I lost them. Later, when he came to Tokyo in 1996, he gave me a handwritten note about several new theoretical ideas for the study of disordered systems. This one is well organized. I would like to deposit a copy of it.
2) I have a letter from Professor De Dominicis from Saclay, in which he introduced me to his earlier work published in *Comptes Rendus de l'Académie des Sciences de Paris*. I would like to deposit it.
3) I had to numerically solve a simple equation to get the final answer. There was no personal computer available at that time, and it was a bit difficult for a visiting fellow like me to access the university computer. Therefore, I first looked into a numerical table of error functions. I immediately found the line where the sign of the equation changes. It was a fun. I would like to deposit the table as a commemoration of the old pastoral days.