History of RSB Interview: Lennart Sjögren

October 12, 2021, 9:00 to 10:00am (EDT). Final revision: February 26, 2022

Interviewers:
Patrick Charbonneau, Duke University, patrick.charbonneau@duke.edu
Location:
Over Zoom, from Prof. Sjögren's home in Alingsås, Sweden.
How to cite:
P. Charbonneau, *History of RSB Interview: Lennard Sjögren*, transcript of an oral history conducted 2021 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 19 p. https://doi.org/10.34847/nkl.382d6bmv

PC: Good morning, Prof. Sjögren. Thank you very much for joining us. The theme of this series of interviews is the ideas that surrounded spin glasses and replica symmetry breaking broadly defined, over a period that we roughly bound from 1975 to 1995. Before we dive into that subject, can you tell us a bit about yourself, and in particular about your family and your studies before attending University?

LS: [0:00:43] I was born in 1947 in Grängesberg, a small village in a region of Sweden called Dalarna**.** Grängesberg is built around an iron mine owned by a private company called Gränges[1]. This company was the dominant employer in Grängesberg and also had a steelwork in Oxelösund, 120 km south of Stockholm, and a railway, TGOJ, for the transport of the iron ore to the steelwork but also with passenger traffic. For a young person growing up in Grängesberg, the alternatives were to work in the mine, in a dynamite factory in the village, or on the railway. There was also a brewery, Spendrups, now one of the largest in Sweden. The iron mine is now closed but the company Gränges founded in 1896 still exists as a manufacturer of aluminum products.

My father chose to work with the railway as an engine-driver transporting passengers. My mother was a housewife but she had worked as a ladies' hairdresser before she married. Due to a reorganization within TGOJ our family moved in 1952 to a small or medium sized industrial town called Eskilstuna halfway between Grängesberg and Oxelösund. There I grew up and went to elementary school and later high school. After my exam from high school in 1966 I did my military service, and then I left to study at

[1] Gränges: https://sv.wikipedia.org/wiki/Gr%C3%A4nges

Chalmers University in Göteborg. I started my studies at the university in 1967, and my PhD studies in 1971[2].

PC: What interested you to physics in the first place?

LS: [0:01:40] Roughly when I was 15, more or less, I decided to be a researcher in atomic physics for some reason. I don't remember precisely why. It was interesting, I thought. I was interested in physics and mathematics, and at that time—in the '60s—there were much ado about nuclear power and these kinds of things. I suppose this was one reason for the interest. At Chalmers I started studying at a program called technical physics. That education contained mathematics and much modern physics with quantum mechanics, atomic and molecular physics, solid state physics, nuclear physics etc. It was mainly invented for the nuclear industry at that time.

PC: What then led you to pursue graduate studies in theoretical physics, in particular?

LS: [0:02:53] I don't now remember how I reasoned, but I suppose I thought it was most interesting. During my undergraduate studies I was interested in both experimental and theoretical courses, but in the last year I studied mostly theoretical courses, like nuclear physics, quantum mechanics etc. and also courses in mathematics, like group theory and functional analysis. The departments of experimental and theoretical physics invited students interested to continue with PhD studies for meetings, and I visited these meetings and discussed with some people, but eventually my interest in theory and mathematics made the choice. At the time I got a scholarship from Chalmers university which financed my PhD studies for four years, so I was free to choose what I wanted to study. Otherwise, a PhD student must find a supervisor who have sufficient research money to finance a student.

PC: What drew you then to work with Alf Sjölander[3] on the kinetic theory of classical liquids?

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[2] Lennart Sjögren, *Theoretical Studies of Atomic Motions in Classical Liquids*, PhD Thesis, Chalmers tekniska högskola (1977). https://search.ebscohost.com/login.aspx?direct=true&db=cat07470a&AN=clc.16cc33ca.adb8.45f6.9937.873d883f0c83&site=eds-live&scope=site&authtype=guest&custid=s3911979&groupid=main&profile=eds

[3] Alf Sjölander: https://sv.wikipedia.org/wiki/Alf_Sj%C3%B6lander

LS: [0:03:30] I talked to many people at the Institute of Theoretical Physics[4]. I talked with Stig Lundqvist[5], Olle Brander[6] and others. (Maybe you've heard about Stig Lundqvist because he was a very influential person in Sweden; he was the chairman of the Nobel committee for a long period[7].) When I talked with Stig Lundqvist I mentioned that I was interested in magnetism, but he did not recommend that since it was a very difficult problem. He suggested that I talk with Alf Sjölander, who already had a student in this field. So, I went to Alf and discussed with him. He was then interested to take up the study of the dynamics of classical liquids which he had done before together with Singwi and others[8], and he suggested that I should work with him on this problem. Alf Sjölander had a very strong reputation, so I thought I should choose him. He was, so to speak, the best, so I thought it was a challenge to see if I could make it.

PC: After your PhD, except for a postdoc of one year in Paris and a brief stint in industry, you remained at the Institute of Theoretical Physics at Chalmers as a research assistant—*radassistant*. Can you describe the environment and the functioning of the statistical physics group at the institute?

LS: [0:04:43] At the department there was a total of six research groups, three in condensed matter physics and three in more fundamental physics like high energy physics and mathematical physics. One group in high-energy physics later changed to "physical resource theory" oriented toward questions of environment and sustainability. In condensed matter physics there was, except for the statistical physics group, the solid state theory

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[4] The institute of Theoretical Physics was a joint institute for research and education in theoretical physics, serving Chalmers University of Technology and the University of Göteborg. It was organized in five research divisions: elementary particle physics, statistical physics, solid state physics, surface physics and chemistry, and mathematical physics. See, e.g., *Annual Report 1983* (Göteborg: Institute of Theoretical Physics, 1983). https://inis.iaea.org/collection/NCLCollectionStore/_Public/16/072/16072611.pdf (Consulted November 27, 2021.)

[5] Stig Lundqvist (1925-2000). See, *e.g.*, Philip W. Anderson, "Stig Olov Lundquist, 9 August 1925—6 April 2000," *Proc. Amer. Philo. Soc.* **147**, 287-291 (2003). https://www.jstor.org/stable/1558217; Peter Apell, Tord Claeson, Göran Grimvall, Lars Hedin, Mats Jonson, Bengt Lundqvist, Yvonne Steen, Göran Wendin, Stellan Östlund, "In memorian: Stig Lundqvist," *Chalmers University Physics Department* (2000). http://fy.chalmers.se/~tfybil/StigLEng.html (Consulted November 19, 2021.)

[6] Olle Brander (1933-2020). See, *e.g.*, https://www.minnesrummet.se/system/funeral_notices/pdfs/001/144/756/original/2342399-1.pdf?1599275354 (Consulted November 19, 2021)

[7] Nobel Committee for Physics: https://en.wikipedia.org/wiki/Nobel_Committee_for_Physics

[8] See, *e.g.*, K. S. Singwi and Alf Sjölander, "Diffusive motions in water and cold neutron scattering," *Phys. Rev.* **119**, 863 (1960). https://doi.org/10.1103/PhysRev.119.863; A. Rahman, K. S. Singwi and A. Sjölander, "Theory of slow neutron scattering by liquids. I," *Phys. Rev.* **126**, 986 (1962). https://doi.org/10.1103/PhysRev.126.986

group with Stig Lundqvist and a little bit later there was Bengt Lundqvist[9] in surface physics. The statistical physics was a rather small group, I think. Alf Sjölander did not have many students[10]. Before me he had three students and after me three or four more. He didn't take many students, but he was very active himself. He really sat down and did calculations himself. As a student, you got a very good help, so to speak. He could instruct you very much. He was interested in everything you did, which at that time was very good. Later, maybe not. He interfered too much when you wanted to become independent.

PC: In notes you sent us, you mentioned that you started working on spin glasses in 1983[11]. From where did this interest emerge? How did you first hear about spin glasses?

LS: [0:06:46] As I wrote in these notes[12], we studied liquids, in particular the long-time tails in the memory function[13]. Then we started to discuss glasses internally. At the time, spin glasses was really a hot topic, and as I mentioned above I was for some reasons interested in the problem of

[9] Bengt I. Lundqvist. See, *e.g.*, https://academictree.org/physics/peopleinfo.php?pid=85009 (Consulted November 21, 2021.)

[10] We note, *e.g.*, Göran Niklasson, *Unified theory of transport properties of an anharmonic crystal*, PhD Thesis, Chalmers tekniska högskola (1970).
https://search.ebscohost.com/login.aspx?direct=true&db=cat07470a&AN=clc.9b2e38f9.b22a.43d1.8152.d82bb5d33d36&site=eds-live&scope=site&authtype=guest&custid=s3911979&groupid=main&profile=eds
Magnus Månson, *On the dynamics of Heisenberg paramagnets*, PhD Thesis, Chalmers tekniska högskola (1974).
https://search.ebscohost.com/login.aspx?direct=true&db=cat07470a&AN=clc.1875b2d9.6ee6.4365.8257.9ee802363ba4&site=eds-live&scope=site&authtype=guest&custid=s3911979&groupid=main&profile=eds
Göran Wahnström, *Theoretical investigation of the motion of an adsorbed atom*, PhD Thesis, Chalmers tekniska högskola (1985).
https://search.ebscohost.com/login.aspx?direct=true&db=cat07470a&AN=clc.bf2bc164.8adf.4c88.b0c3.34bed4a1156e&site=eds-live&scope=site&authtype=guest&custid=s3911979&groupid=main&profile=eds
Ulf Bengtzelius, *Dynamics of supercooled liquids and of liquid-glass transitions*, PhD Thesis, Chalmers tekniska högskola (1986).
https://search.ebscohost.com/login.aspx?direct=true&db=cat07470a&AN=clc.9dd1198c.e1a8.46bd.a300.78d419e64d22&site=eds-live&scope=site&authtype=guest&custid=s3911979&groupid=main&profile=eds

[11] **LS:** "I left the department for a job in the industry since it was very difficult to get a position at the university. After a year or so I decided to make an effort for an academic future, and around 1983 I returned to the university and started to work on spin-glasses."

[12] **LS:** "Under my supervisor, Prof. Alf Sjölander, I had studied the kinetic theory of classical liquids up to my PhD in 1977. Thereafter while I was looking for a job in the industry, I started to look at the problem of the memory function $\Gamma(t)$ for the velocity correlation function $\Phi(t)$ of a single particle in a simple classical liquid. […] The reason for the interest in this problem at the time was the appearance of several molecular dynamics studies of this function. In particular it was found by Levesque and Verlet, who performed simulations for liquid argon at several densities and temperatures, that the memory function $\Gamma(t)$ was given very accurately by the expression $\Gamma(t) = A \exp(-at^2) + Bt^4 \exp(-bt)$."

[13] L. Sjögren and A. Sjölander, "Kinetic theory of self-motion in monatomic liquids," *J. Phys. C* **12**, 4369 (1979). https://doi.org/10.1088/0022-3719/12/21/005

magnetism already early during my undergraduate studies. I know that Alf Sjölander himself wanted to study supercooled liquids and regular glasses. I thought that if you want to study glasses, spin glasses is a good subject.

PC: Would you have heard about the topic from the literature or conferences or seminar speakers?

LS: [0:07:39] First in the literature, then we had certain guests who were interested in spin glasses, such as Marek Cieplak[14] from Poland. I had some discussions with him. Later, of course, we also talked with John Hertz[15] from Nordita, but that was from '84 or '85, something like that. There was a lot of activity in these years. This is one reason.

PC: In 1983, Professor Götze[16] came to Chalmers on a sabbatical[17]. Do you know what drew him to Chalmers in particular?

LS: [0:08:42] He simply wanted to study glasses. We had contact with Götze. Maybe in '73, he visited us for a talk. He had since long studied liquids and then he started to study the metal-insulator transition. Then he went to the classical Lorentz model, where you have a particle in a fixed array [of obstacles]. He developed—together with Leutheusser and Yip—the theory for the Lorentz gas[18]. Then he wanted to study supercooled liquids and therefore Alf simply invited him.

PC: So Alf and Prof. Götze knew each other?

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[14] Marek Cieplak did a four-month visit to Chalmers Institute of Technology in 1978-1979. See, e.g., http://info.ifpan.edu.pl/SL-4/index.php?group=cieplak (Consulted November 27, 2021.)

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

[16] Wolfgang Götze: https://en.wikipedia.org/wiki/Wolfgang_G%C3%B6tze

[17] **LS:** During our collaboration Götze visited our department in 1983, 1988 and 1993. The final visit was financed by *Stiftelsen Riksbankens jubileumsfond* [Bank of Sweden Tercentenary Foundation]—this fund also pays for the so-called Nobel prize in economics—and within a scientific collaboration between Sweden and Germany via a Humboldt prize. The prize was given to scientist in several areas, and there was a formal ceremony in the Swedish parliament where the Swedish minister of education and the German ambassador in Sweden participated. At the ceremony Götze gave a short seminar telling the audience what we were doing, and to prepare for this Götze took an intensive course in Swedish so that he learned to say something in Swedish.

[18] W. Götze, E. Leutheusser and S. Yip, "Dynamical theory of diffusion and localization in a random, static field," *Phys. Rev. A* **23**, 2634 (1981). https://doi.org/10.1103/PhysRevA.23.2634; "Correlation functions of the hard-sphere Lorentz model," *Phys. Rev. A* **24**, 1008 (1981). https://doi.org/10.1103/PhysRevA.24.1008; "Diffusion and localization in the two-dimensional Lorentz model," *Phys. Rev. A* **25**, 533 (1982). https://doi.org/10.1103/PhysRevA.25.533

LS: [0:10:13] As I said, Götze had been at our institute for some seminar or for shorter periods. In '73 there was a Nobel symposium in Göteborg[19], and while visiting Alf he was at that symposium as guest for a few days.

PC: During his stay, the two of you co-authored an article on the mode-coupling theory (dynamics) of spin glasses[20]. Can you walk us through the genesis of that work?

LS: [0:10:52] The idea was to try to understand the neutron scattering data which existed by Mezei and co-worker[21]. We already then thought that maybe it's the same cause as structural glasses, so it's a dynamical transition. There was also a paper by Edwards and Anderson[22], where they started from the dynamical description to study spin glasses. John Hertz papers were also along similar ideas. We thought along these lines. Later we understood that spin glasses are more complicated than that. That there are more complex interactions than in structural glasses.

PC: Were you aware at the time you started of the work of Sompolinsky and Zippelius, who were also trying to obtain a dynamical theory of spin glasses[23]? If yes, how influential was their work on your thinking and in the field at that time?

LS: [0:12:24] Annette Zippelius was a student of Götze[24], so of course we knew these results. But I don't know if they influenced us so much, except that we had several discussions about their work. In particular we wanted to understand the origin of a multitude of order parameters in their model. We thought that this should lead to a hierarchy of relaxation times, and we did not understand how this could happen. There were also some

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[19] Collective properties of physical systems, 24th Nobel Symposium, Lars Hedin, Lamek Hulthén, Bengt Lundqvist and Stig Lundqvist, June 12-16, 1973, Aspenäsgården, Lerum, Sweden. Proceedings: *Collective properties of physical systems*, B. Lundqvist and S. Lundqvist, eds. (New York: Academic Press, 1973).

[20] W. Götze and L. Sjogren, "A dynamical treatment of the spin glass transition," *J. Phys. C* **17**, 5759 (1984). https://doi.org/10.1088/0022-3719/17/32/011

[21] F. Mezei and A. P. Murani, "Combined three-dimensional polarization analysis and spin echo study of spin glass dynamics," *J. Magn. Magn. Mater.* **14**, 211-214 (1979). https://doi.org/10.1016/0304-8853(79)90120-3

[22] 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

[23] See, *e.g.*, H. Sompolinsky and A. Zippelius, "Dynamic theory of the spin-glass phase," *Phys. Rev. Lett.* **47**, 359 (1981). https://doi.org/10.1103/PhysRevLett.47.359; "Relaxational dynamics of the Edwards-Anderson model and the mean-field theory of spin-glasses," *Phys. Rev. B* **25**, 6860 (1982). https://doi.org/10.1103/PhysRevB.25.6860

[24] Annette Zippelius, *Eine kinetische Theorie klassischer Flüssigkeiten* [A kinetic theory of classical fluids], PhD Thesis, Technische Universität München (1976). https://opac.ub.tum.de/TouchPoint/perma.do?q=+1035%3D%22BV002034864%22+IN+%5B2%5D&v=tum&l=en

works by Hertz which were similar to our work in a certain sense[25]. We discussed all of these works.

PC: You said you discussed with John Hertz a little later, right?

LS: [0:13:02] That was later. We had then finished our own paper, but we invited him to discuss with us. He was then at Nordita, so it was simple for him to travel to Göteborg to give a seminar and have discussions.

PC: During his stay, Prof. Götze also worked with Prof. Sjölander and a PhD student, Ulf Bengzelius, on the mode-coupling theory of liquids[26]. In notes you sent us and just before, you mentioned that Sjölander and you had been thinking about supercooled liquids for some time already, almost five years[27]. What was the conceptual hurdle that prevented progress upon that interaction? What did getting together unlock?

LS: [0:14:00] What do you mean?

PC: You had been interested in this project for five years, but it seems that it's only with the visit of Prof. Götze that significant advances were made. What was enabled by this visit? Or what new idea came about?

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[25] J. A. Hertz, "Dynamical mean-field theory for spin glasses. I. Short-time properties in zero field," *J. Phys. C* **16**, 1219 (1983). https://doi.org/10.1088/0022-3719/16/7/009; "Dynamical mean-field theory for spin glasses. II. Approach to equilibrium in a small field," *J. Phys. C* **16**, 1233 (1983). https://doi.org/10.1088/0022-3719/16/7/010

[26] U. Bengtzelius, W. Götze, and A. Sjölander, "Dynamics of supercooled liquids and the glass transition," *J. Phys. C* **17**, 5915 (1984). https://doi.org/10.1088/0022-3719/17/33/005

[27] **LS:** At the time when our results on the long-time tail were worked out, in 1977, we discussed the implications on supercooled liquids. This lead us to the following observations:

1. On supercooling a liquid, the tail in $\Gamma(t)$ must increase, i.e., the contribution from $m(t)$ will become larger, and the contributions from the currents will probably decrease. This effect must come from the density fluctuations in the system, i.e., the cage effect must become more efficient, and any particle will be trapped by its surrounding for a longer and longer time.
2. When the area under $m(t)$ increases the diffusion constant will decrease, and if the area tends to infinity diffusivity will tend to zero.
3. Eventually, however, the contributions from the currents by way of δ must act as a cutoff of this tendency, and the dynamics will be dominated by activated hopping processes over potential barriers.
4. If we simply set $\delta = 0$ in our model there may be an ideal transition to a disordered solid which will occur due to a constant tail in $m(t)$, i.e., $m(z) \propto 1/z$, $z \to 0$.

It was clear that to answer these questions we had to know the input into the various mode coupling terms above, i.e., we needed corresponding theories for the density correlation function $F(qt)$ and the transverse current correlations $C^T(qt)$. In 1978, I then developed such theories. L. Sjögren, "Kinetic theory of current fluctuations in simple classical liquids," *Phys. Rev. A* **22**, 2866 (1980). https://doi.org/10.1103/PhysRevA.22.2866

LS: [0:14:22] Personally, after my PhD, I went to Paris and worked with Jean-Pierre Hansen on plasmas[28]. After I returned to Sweden, I was in Göteborg for one year working on molten salts and such problems, which was in a certain way a continuation of the plasma work[29]. Then, I left for the industry. Then, of course, Prof. Sjölander made some work together with Turski from Poland[30], but in a very phenomenological way with some hopping model[31].

The real work started when Alf got his student, Ulf Bengtzelius, who started by going back to these mode-coupling equations. They, Alf and Ulf, had been working for one year or so before Götze joined. In a certain sense, they had some understanding; they had for instance arrived at the F_2-model which they started to analyze, and found a discontinuous transition for the non-ergodicity parameter. Actually, when Götze joined us he thought that the glass transition was like in the Lorentz model, a continuous transition. But Alf realized that it was a discontinuous transition. He had to convince Götze that it was a discontinuous transition. Then Götze was very clever, so by his experience from the Lorentz model, he was able to make a lot of progress on the F_2-model with Alf and Ulf. In particular Götze rather soon arrived at the exponents *a* and *b* in the solution. I remember that he came to my room very excited about this.

PC: A paper on the mode-coupling theory of glasses came out at roughly the same time as the paper by Alf and Ulf, written by Eberhard Leutheusser[32]. Could we say that there was a race to the mode-coupling theory of glasses at the time, or were these two completely independent events?

LS: [0:17:09] Of course, Leutheusser was a student of Götze[33], and they had worked on the Lorentz model together with Yip. Then Leutheusser went to

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[28] M Baus, J.-P. Hansen and L. Sjögren, "Electrical conductivity of a strongly coupled hydrogen plasma," *Phys. Lett. A* **82**, 180-182 (1981). https://doi.org/10.1016/0375-9601(81)90115-8; L. Sjögren, J.-P. Hansen, and E. L. Pollock, "Self-diffusion, conductivity, and long-wavelength plasma oscillations in strongly coupled two-component plasmas," *Phys. Rev. A* **24**, 1544 (1981). https://doi.org/10.1103/PhysRevA.24.1544; J.-P. Hansen and L. Sjögren, "Plasma oscillations and sound waves in collision-dominated two-component plasmas," *Phys. Fluids* **25**, 617-628 (1982). https://doi.org/10.1063/1.863808

[29] L. Sjögren and F. Yoshida, "Self-consistent calculation of excitation spectra in simple molten salts," *J. Chem. Phys.* **77**, 3703-3713 (1982). https://doi.org/10.1063/1.444273

[30] Łukasz Turski: https://pl.wikipedia.org/wiki/%C5%81ukasz_Turski

[31] A. Sjölander and L. A. Turski, "On the properties of supercooled classical liquids," *J. Phys. C* **11**, 1973 (1978). https://doi.org/10.1088/0022-3719/11/10/007

[32] E. Leutheusser, "Dynamical model of the liquid-glass transition," *Phys. Rev. A* **29**, 2765 (1984). https://doi.org/10.1103/PhysRevA.29.2765

[33] Eberhard Leutheusser, *Die Dynamik von klassischen Flüssigkeiten harter Kugeln* [The dynamics of classical hard sphere liquids], PhD Thesis, Technische Universität München (1979). https://opac.ub.tum.de/TouchPoint/perma.do?q=+1035%3D%22BV002076884%22+IN+%5B2%5D&v=tum&l=en

the United States and worked with Yip. While the three were working on the Lorentz model, they also discussed what applied to real glasses and tried to understand it. I don't know precisely what ideas came up then, but as I understand it Götze thought it was a continuous transition when he came to Göteborg. I suppose it was independent and that Leutheusser understood the problem when he was in the United States, and Alf and Ulf Bentzelius had started in Sweden. They both came to the same conclusion that it must be a discontinuous transition. I don't know if Leutheusser had these idea already when he was in Götze's group or to what extent Yip was involved. Later it was understood that more general models contain both continuous (type A) and discontinuous (type B) bifurcation scenarios.

PC: After that visit, you went to TU München for some time to work on the mode-coupling theory of glasses with Prof. Götze. How did this visit come about? What was the idea?

LS: [0:18:48] He asked me if I wanted to come to Munich, so he applied for money. Meanwhile, he was offered, I think, some position at some other university, but this was not realized. It took some time before he got some money, but eventually he succeeded to get some money and invited me. I came down in '85 and stayed one year.

PC: Was there a particular project you two wanted to work on? Was it continuing the spin-glass work? What was the idea behind your visit?

LS: [0:19:40] The spin-glass work, we left. He discussed with Fischer[34], who was also an expert, and we realized the spin glasses are more complicated systems[35].

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[34] Konrad H. Fischer (1929-2016). See, *e.g.*, Alex Braginski, "Konrad H. Fischer," *IEEE CSC Superconductivity News Forum* (2016). https://snf.ieeecsc.org/obituary/konrad-h-fischer (Consulted November 28, 2021.)

[35] **LS:** Götze had a long discussion with K. H. Fischer when he visited München, in July 1984. At that time Fischer had the following comments about our work:

1. All known results are for the Sherrington-Kirkpatrick (SK) model. Since we are discussing a different model nothing can be said against or in favor of our paper.
2. The results on the SK model are now established. In particular, there is a spontaneous symmetry breaking and hence a phase transition in the sense that $\langle S_i \rangle^2$ does not tend to zero for vanishing field if $T < T_c$.
3. The most relevant result for the SK model is the de Almeida-Thouless line and the result for the nonlinear susceptibility χ_2, defined as $S/B = \chi + \chi_2 B^2 + O(B^4)$ for $B \rightarrow 0$. One gets $\chi_2 \propto 1/(T-T_c)$ for $T > T_c$. For $T = T_c$ one gets $S/B = \chi + a|B|$. These results find good support in experiment. As long as we could not make statements about these properties, experts will ignore our paper.
4. There is another critical line, the Gabay-Toulouse line, which one has to explain.

We decided to work on structural glasses, and try to develop this theory further. We started to study the α relaxation[36]. We did some numerical work on the simplified model. Then came this work by Das and Mazenko[37], and I realized their model was precisely what we had in liquids, i.e., coupling to the currents. So, we simply took more or less this theory, which I had developed, and worked out the consequences of this additional term, this so-called delta term. We found a possible scenario for the glass transition. This was essential to understand what the glass transition is all about[38]. There is some underlying singularity which is never reached. Depending on how close you come to this singularity you can see different effects. Then there are those non-trivial dynamical scenarios in this region. We also made some calculations where we included this so-called hopping term to see if it could work numerically, but that was still on the simplified models[39].

PC: Can you describe how your collaboration functioned? In particular, how were your two expertise complementary in this effort?

LS: [0:22:31] Götze had this very strong mathematical background, obviously. But we were both working independently and we discussed more or less every day. He came to some conclusions and I came to some conclusions. This is how it worked.

PC: And once you agreed, you published?

LS: [0:23:13] Yeah. But I remember especially two occasions, one in '85 and the other in '88, which I wrote about in the notes[40].

In the occasion in '85, when I was in Munich, we did not understand this two-parameter scaling law. It was a very complicated scenario, with a window which opened around the singularity. Götze was at home,

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[36] W. Götze and L. Sjögren, "α-relaxation near the liquid-glass transition," *J. Phys. C* **20**, 879 (1987). https://doi.org/10.1088/0022-3719/20/7/005

[37] S. P. Das, G. F. Mazenko, S. Ramaswamy and J. J. Toner, "Hydrodynamic theory of the glass transition," *Phys. Rev. Lett.* **54**, 118 (1985). https://doi.org/10.1103/PhysRevLett.54.118; S. P. Das and G. F. Mazenko, "Fluctuating nonlinear hydrodynamics and the liquid-glass transition," *Phys. Rev. A* **34**, 2265 (1986). https://doi.org/10.1103/PhysRevA.34.2265

[38] W. Götze and L. Sjögren, "The glass transition singularity," *Z. Phys. B* **65**, 415-427 (1987). https://doi.org/10.1007/BF01303763

[39] W. Götze and L. Sjögren, "Scaling properties in supercooled liquids near the glass transition," *J. Phys. C* **21**, 3407 (1988). https://doi.org/10.1088/0022-3719/21/18/007

[40] **LS:** In the F_{13} model the decay of $\phi(t)$ has a characteristic log t dependence for mesoscopic times. Such behavior has been observed in spin glasses but is also present in many polymeric systems. In the susceptibility this logarithmic decay show up as a plateau in $\chi''(\omega)$ and in the corresponding spectrum $\phi'(\omega)$ as a $1/f$ noise.

thinking, and he presented more or less this result. We then worked out all details of this thing[41].

I remember a similar occasion, in ’88, when we were discussing the higher-order singularities, the cusps. We did not understand their dynamics. We saw from the numerical results that there were logarithmic terms, but we did not understand how you could get logarithmic terms from our equation. After all, we start from Newton’s equations, and it is a similar problem with these power laws that we get. There are no power laws in Newton’s equations. They are regular, so to speak. So how do you get these power laws? It was even more difficult to understand how you can get this logarithmic term. Then, we guessed it’s maybe powers of log t. Maybe the critical spectrum is $1/(\log t)$ to some power. We checked it with the spin glass data which we had from Mezei and co-workers**.** We found that it could be fitted perfectly with a $1/(\log t)^2$. There was a certain breakthrough. Then I went home and tried some ansatz. I tried a function of log t, and tried to determine this unknown function. In this case an elliptic function is the solution[42]. Götze showed later together with a student[43], that the solution can be given as a polynomial in log t.

PC: You mentioned numerical work as inspiring a lot of your analytical developments. Can you describe what sort of techniques and computational resources you had at your disposal to do this numerical work?

LS: [0:26:53] I had done a lot of numerical work in my thesis work. I solved the mode-coupling equations for liquids and compared with neutron scattering and molecular dynamics simulations. I simply used the same kind of programs and applied it to the schematic models. I worked mostly with schematic models. (Ulf Bengtzelius got my programs and solved for the full equation[44].) But the schematic models were very rewarding because they were simple and you could easily get out results. Götze also

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[41] See Ref. 38.

[42] W. Götze and L. Sjögren, "β relaxation near glass transition singularities," *J. Phys.: Condens. Matter* **1**, 4183 (1989). https://doi.org/10.1088/0953-8984/1/26/014; "Logarithmic decay laws in glassy systems," *J. Phys.: Condens. Matter* **1**, 4203 (1989). https://doi.org/10.1088/0953-8984/1/26/015

[43] W. Götze and M. Sperl, “Logarithmic relaxation in glass-forming systems,” *Phys. Rev. E* **66**, 011405 (2002). https://doi.org/10.1103/PhysRevE-66-011405

[44] See, *e.g.*, U. Bengtzelius and L. Sjögren, “Changes of thermodynamic quantities at the glass transition,” *J. Chem. Phys.* **84**, 1744-1751 (1986). https://doi.org/10.1063/1.450473; U. Bengtzelius, “Theoretical calculations on liquid-glass transitions in Lennard-Jones systems,” *Phys. Rev. A* **33**, 3433 (1986). https://doi.org/10.1103/PhysRevA.33.3433

developed numerical techniques which we then implemented[45]. That was later, I think. What I did was that I Laplace-transformed the equations and solved them in Laplace-transformed space, or in Fourier space. Then I inverted it, and then I iterated, so to speak. You start with some exponential function and then you iterate until you get convergence.

PC: Were these desktop or university computers you could use to do that?

LS: [0:28:36] That was university computers at that time. At that time, desktop computers were not so effective. In Göteborg I had a pile of cards, but in Munich I could use a terminal. In my PhD work, I punched cards and you had to wait one day before you got results. My advisor, Alf Sjölander, also made a lot of numerical work back in the time when there were no commercial computers. There were two computers in Sweden, BARK[46] and BESK[47], which had been built by some fellow who had been at IBM in the United States during the war, and then returned. At that time, they didn't even have cards. They had some roll of paper, which they punched with holes. He was one of the first actually to do numerical computations using a computer in Sweden. His computations are in the book by Lovesey and Marshall about neutron scattering[48].

PC: What was the initial reaction of the community to the mode-coupling theory of glasses?

LS: [0:30:31] There were two kinds of reactions. Many theoreticians were very skeptical. In particular, in the United States, but also some in Europe. Meanwhile the experimentalists were very pleased because they got a theory which they could use to analyze their experiments. As I write in my notes[49], even some experimentalist met a lot of resistance and had problems to get their papers accepted. There was strong opposition in the

[45] See, *e.g.*, M. Fuchs, W. Götze, I. Hofacker and A. Latz, "Comments on the α-peak shapes for relaxation in supercooled liquids," *J. Phys.: Condens. Matter* **3** 5047 (1991). https://doi.org/10.1088/0953-8984/3/26/022

[46] BARK: https://en.wikipedia.org/wiki/BARK_(computer)

[47] BESK: https://en.wikipedia.org/wiki/BESK

[48] W. Marshall and S. W. Lovesey, *Theory of Thermal Neutron Scattering: The Use of Neutrons for the Investigation of Condensed Matter* (Oxford: Clarendon Press, 1971).

[49] **LS:** The detailed predictions of the theory, attracted many experimentalists in the field who started to use the theory to analyze their experiments. Sidney Yip took the initiative to summarize the experimental situation up to 1995 which was published in the volume "Relaxation Kinetics in Supercooled Liquids–Mode Coupling Theory and its Experimental Tests". See J.-P. Hansen and S. Yip, "Molecular dynamics investigations of slow relaxations in supercooled liquids," *Trans. Theo. Stat. Phys.* **24**, 1149-1178 (1995). https://doi.org/10.1080/00411459508203948

early years. I remember this conference in Crete[50], where some people were very upset and shouted at us[51]. But, as I said, some experimentalists also faced some opposition when they used our theory to analyze their work. Some got very strange referee reports[52].

PC: In the notes you sent us, you mentioned that one of the criticisms of the mode-coupling theory singularity is that it could not exist because hopping processes were neglected. Was this obvious from the start? If not, when did the realization sink in?

LS: [0:32:19] This was obvious from the start, as I also write in my notes, because we already had these currents. It was just that our argument was: "Let's forget about this from the start. We can study the ideal case first." But we realized that there were of course hopping. It's known from solid state also that you have this kind of hopping processes in solids. You have diffusion in solids. In particular, Alf Sjölander, who had worked on solids, he knew all of this. Of course, Götze also knew all of this. It's true that this was an argument against the theory from the start. In particular, there was a paper by Siggia[53], which was very influential in the United States.

PC: If we now fast forward a few years in time, at some point colloidal hard spheres eventually became an important theme for mode-coupling theory research. Especially after the work of van Megen and coworkers. Can you

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[50] International Discussion Meeting on Relaxations in Complex Systems, Heraklion, Crete, Greece, June 18-29, 1990. *Proceedings of the International Discussion Meeting on Relaxations in Complex Systems*, K. L. Ngai and G. B. Wright, eds., *J. Non-Cryst. Sol.* **131-133**, 1-536 (1991). https://doi.org/10.1016/0022-3093(91)90262-5

[51] **LS:** At a conference around 1990 on Crete we were invited and presented our results. I remember that in a following discussion some people were very upset and very aggressive against our theory. Alf Sjölander made some general comment on our theory whereby a person really shouted at him for no reason at all. At the time this reaction was very surprising to us.

[52] **LS:** When I visited München, during 1985-86, an experimentalist showed me a referee report on a paper which he sent to PRL. He had used the MCT to analyze his experiments on complex liquids. The referee had some remarks on his experimental work, and regarding his analysis using MCT he had asked a theoretician in his department which was a Nobel laureate and who called the theory "mumbo jumbo". **PC:** The group of Wolfgang Gläser at TU München led a series of neutron-scattering studies of complex liquids that relied on the mode-coupling theory at that time. https://de.wikipedia.org/wiki/Wolfgang_Gl%C3%A4ser

[53] **LS:** A plausible reason for this skepticism from scientists in USA was an early paper by Eric Siggia, where he argued that the singularity found by Leutheusser "was an artifact of the approximation". Siggia's statement was of course correct, since the ideal transition studied by Leutheusser and Bengtzelius *et al.* neglected the hopping processes which enter in δ. However, the point of studying the ideal transition was to understand the plateau region which develops when a liquid is supercooled to a glass. The whole bifurcation scenario with its detailed predictions for the dynamics will exist at least for systems with a small δ. The whole scenario with the "fast" β-process and its nontrivial dynamics was missed by Siggia. See E. Siggia, "Comment on dynamical theories of the liquid-glass transition," *Phys. Rev. A* **32**, 3135 (1985). https://doi.org/10.1103/PhysRevA.32.3135

walk us through how that interaction between mode-coupling theory and colloidal hard spheres came about? What motivated what, and how did the discussion take place?

LS: [0:34:11] We started to look for experiments. Up to '88, there were mostly computations. We solved the schematic model on the computer, but there were no real experiments known by us to test the theory. Somehow, Götze got in contact with Pusey[54]. He probably met Pusey at some conference[55]. Götze, I suppose, suggested that we should analyze their experiments[56]. We had some cooperation. They also analyzed their experiments, and we had some meetings with Pusey and van Megen. They learned what the theory was about, and we made a joint analysis of their experiment. They published one experimental paper, we published one theoretical paper, but there were two actually joint papers, which appeared in the same issue, after each other[57]. Of course, we offered Pusey and van Megen to also have their names on our paper, but they thought it was better to have two papers.

The point was also that since they essentially studied a hard-sphere system, and for hard spheres you know the structure factor and you could evaluate the mode-coupling integrals, therefore it was possible to actually make comparisons with no adjustable parameters. This was the big interest. Here, you have experiments and there are no parameters in the theory, except for this overall timescale. It was a real success. It was in a certain sense a benchmark experiment for the theory.

PC: Would it be fair to say that this was a breakthrough for that theory? Did people look at it differently after that work? What was the reaction afterwards?

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[54] Peter Pusey: https://en.wikipedia.org/wiki/Peter_Pusey

[55] Although Pusey and Götze might have met earlier, they were at least both lecturers at the summer school *Liquides, cristallisation et transition vitreuse*, Session LI, 3-28 Juillet 1989, École d'été de physique théorique, Les Houches, France. *Liquids, Freezing and the Glass Transition, Les Houches. Session LI, 1989*, J.-P. Hansen, D. Levesque and J. Zinn-Justin, eds. (Amsterdam: North Holland, 1991).

[56] See, *e.g.*, P. N. Pusey and W. van Megen, "Observation of a glass transition in suspensions of spherical colloidal particles," *Phys. Rev. Lett.* **59**, 2083 (1987). https://doi.org/10.1103/PhysRevLett.59.2083; W. van Megen, S. M. Underwood and P. N. Pusey, "Nonergodicity parameters of colloidal glasses," *Phys. Rev. Lett.* **67**, 1586 (1991). https://doi.org/10.1103/PhysRevLett.67.1586

[57] W. van Megen and P. N. Pusey, "Dynamic light-scattering study of the glass transition in a colloidal suspension," *Phys. Rev. A* **43**, 5429 (1991). https://doi.org/10.1103/PhysRevA.43.5429; W. Götze and L. Sjögren, "β relaxation at the glass transition of hard-spherical colloids," *Phys. Rev. A* **43**, 5442 (1991). https://doi.org/10.1103/PhysRevA.43.5442

LS: [0:37:25] For some experimentalists it was probably a breakthrough. We got in contact with Cummins[58] at CUNY, in New York, and Loidl and Lunkenheimer from Darmstadt in Germany, and this lead to a very fruitful collaboration. There were also other interested experimentalists like Sillescu[59] from Mainz and Mezei[60], Richter[61] and Petry performing neutron scattering experiments to test the theory. In Sweden, Lena Torell and her group also made some contributions[62].

I remember I was at a ski conference in Switzerland. (A meeting where you ski in day time and you have a conference in the evening.) That was in a small village in Switzerland. Then I didn't like to ski, or I was not a very good skier, at least in downhill. I thought I would go to the nearby village by bus, and on the bus I met some other guy also visiting the conference, Franz Fujara. We went to this village, but it was not so good weather, so we went to some café, had some coffee, and I explained the theory to him. Later, I met him at some other conference. He told me that after we met the first time he applied for money and started to do experiments[63]. He did indeed a lot of experiments on the mode-coupling theory. He came from Mainz, where there were several strong experimental groups, so he probably interacted with other people. It somehow spread around that this was an interesting theory.

PC: In 1992, you co-authored a major review entitled "Relaxation processes in supercooled liquids"[64]. Why was this a good time to write a review of the work in your perspective?

LS: [0:40:01] Somehow we felt that we had turned to some point where we understood the problem. Götze also got some suggestion to write a

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[58] See, *e.g.*, A. Li, W. M. Du, X. K. Chen, H. Z. Cummins and N. J. Tao, "Testing mode-coupling predictions for α and β relaxation in $Ca_{0.4}K_{0.6}(NO_3)_{1.4}$ near the liquid-glass transition by light scattering," *Phys. Rev. A* **45**, 3867 (1992). https://doi.org/10.1103/PhysRevA.45.3867

[59] See, *e.g.*, E. Bartsch, F. Fujara, M. Kiebel, H. Sillescu and W. Petry, "Inelastic Neutron Scattering Experiments on Van der Waals Glasses—A Test of Recent Microscopic Theories of the Glass Transition," *Ber. Bunsenges. Phys. Chem.* **93**, 1252-1259 (1989). https://doi.org/10.1002/bbpc.19890931121

[60] See, *e.g.*, W. Knaak, F. Mezei and B. Farago, "Observation of scaling behaviour of dynamic correlations near liquid-glass transition," *Europhys. Lett.* **7**, 529 (1988). https://doi.org/10.1209/0295-5075/7/6/009

[61] See, *e.g.*, D. Richter, B. Frick and B. Farago, "Neutron-spin-echo investigation on the dynamics of polybutadiene near the glass transition," *Phys. Rev. Lett.* **61**, 2465 (1988). https://doi.org/10.1103/PhysRevLett.61.2465

[62] See, *e.g.*, D. Sidebottom, R. Bergman, L. Börjesson and L. M. Torell, "Two-step relaxation decay in a strong glass former," *Phys. Rev. Lett.* **71**, 2260 (1993). https://doi.org/10.1103/PhysRevLett.71.2260

[63] See, *e.g.*, J. Wuttke, M. Kiebel, E. Bartsch, F. Fujara, W. Petry and H. Sillescu, "Relaxation and phonons in viscous and glassy orthoterphenyl by neutron scattering," *Z. Phys. B* **91**, 357-365 (1993). https://doi.org/10.1007/BF01344065

[64] W. Götze and L. Sjögren, "Relaxation processes in supercooled liquids," *Rep. Prog. Phys.* **55**, 241 (1992). https://doi.org/10.1088/0034-4885/55/3/001

review, I think, from another journal. We got an offer from two journals and we thought that this was the best journal. We thought there was still this opposition. Opposition is still present, even today. To write a review is a good thing if you want to spread the ideas, or explain the theory in more simple ways, to make it more readable.

PC: In that review, you mentioned the work of Kirkpatrick and Thirumalai[65], who had proposed a unification of the mode-coupling theory of glasses with the thermodynamic description of disordered solids, under a theory that was derived from (spin-like) Potts glasses. This relationship was also mentioned in Prof. Götze's notes his in Les Houches lectures[66]. How much attention did you pay to that proposal at the time? Can you describe its impact, if it had any?

LS: [0:42:09] I don't know if I have any direct memory. I think they had some interesting ideas. They got several transition temperatures, I remember, which we discussed. Kirkpatrick also did some real work on ordinary glasses[67], so we probably discussed this more than the spin glasses. I don't remember this very well I must say.

PC: Did you interact with them, or with Kirkpatrick, in those years at all or was this completely disconnected?

LS: [0:43:06] We met Kirkpatrick in Fukuoka, in Japan. Kawasaki organized a conference in Fukuoka and there was Kirkpatrick[68] invited. Then, we discussed with him. We went out, having dinner and so on. At that conference, he also discussed this spin glass, Potts model.

PC: A few years later, you essentially left the field of structural glasses, if I understood correctly. Was there a drive for leaving at that time? And what impact did your work on the mode-coupling theory of glasses have on your subsequent research?

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[65] See, *e.g.*, T. R. Kirkpatrick and D. Thirumalai, "Comparison between dynamical theories and metastable states in regular and glassy mean-field spin models with underlying first-order-like phase transitions," *Phys. Rev. A* **37**, 4439 (1988). https://doi.org/10.1103/PhysRevA.37.4439; "Mean-field soft-spin Potts glass model: Statics and dynamics," *Phys. Rev. B* **37**, 5342 (1988). https://doi.org/10.1103/PhysRevB.37.5342

[66] W. Götze, "Course 5. Aspects of Structural Glass Transitions," in: *Liquids, Freezing and the Glass Transition, Les Houches. Session LI, 1989*, J.-P. Hansen, D. Levesque and J. Zinn-Justin, eds. (Amsterdam: North Holland, 1991). p. 287

[67] T. R. Kirkpatrick, "Mode-coupling theory of the glass transition," *Phys. Rev. A* **31**, 939 (1985). https://doi.org/10.1103/PhysRevA.31.939

[68] Slow dynamics in condensed matter, Fukuoka, Japan, 4−8 Nov 1991. Proceedings: *AIP Conf. Proc.* **256**, Eds. K. Kawasaki, M. Tokuyama and T. Kawakatsu (1992). https://aip.scitation.org/toc/apc/256/

LS: [0:44:23] I left mainly because I could not find sufficient financing for my research. I was therefore involved in a lot of administration and teaching, and I had very little time for research, especially after 2000. Most of my time was spent on teaching. I was vice-dean for some years and head of department for some years. I would say that after 2000, I could not really follow the development. But I found your paper here, with Reichman, from 2005[69]. Was this for some summer school?

PC: Yes. These were notes that Reichman used at a summer school[70]. I helped him flesh them out into an article.

LS: [0:45:47] Do you know that you taught them mumbo jumbo?

PC: I was a graduate student. I did not do the teaching. I helped with the write up.

LS: Ok!

PC: At Chalmers or elsewhere did you ever teach about the mode-coupling theory of glasses? If yes, can you detail your experience?

LS: [0:46:16] I had a master course in stochastic processes in physics, chemistry and biology. I had a chapter on colloids, so I started with the Fokker-Planck equation and I used the mode-coupling approximation and went through these experiments in colloids. This was where I taught mode-coupling I can say.

PC: What year would this have been roughly?

LS: [0:47:12] It could have been from 2000 or something. There was this Bologna structure[71]. You first have a bachelor of science and then a master of science. So, we started these master programs around 2000. Then I had this course on stochastic processes until my retirement in 2012. This course was about Langevin equation, Fokker-Planck equation, Brownian motion and this kind of things.

PC: Before my lecture notes, which you brought up, there were lecture notes published in 1991 by Prof. Götze for the 1989 Les Houches School. How

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[69] D. R. Reichman and P. Charbonneau, "Mode-coupling theory," *J. Stat. Mech.* P05013 (2005).
[70] School for "Unifying Concepts in Glass Physics III", Silvio Franz and Srikanth Sastry, Bangalore, India, June 25-26, 2004.
[71] Bologna Process: https://en.wikipedia.org/wiki/Bologna_Process

impactful was the Les Houches school and the lecture notes for the pedagogy of the mode-coupling theory?

LS: [0:48:55] These were very detailed. Götze is a kind of traditional German intellectual, very detailed. Many people prefer to read our review article, because they think it's simpler. If you read Götze's impressive book[72], you understand that he put some effort to make the theory rigorous, so to speak. I think this was also his ambition with these Les Houches lectures. In this sense, they are very helpful if you want to know the detailed structure of the theory.

PC: You mentioned in your notes and here again in your response, the importance of mathematical rigor. Was this mostly self-driven, in the sense that this was what Prof. Götze was trying to achieve? Or was this in response to criticism? Or was this a general goal of the community?

LS: [0:50:22] This was our common interest. I brought it up that we should write a rigorous mathematical paper, proving our theorems, of what we could prove, and Götze had similar ideas. When I was in München in 1985, we proved that the solutions to our equations were monotonic functions. We hoped: "Maybe it's even a completely monotonic function; the superposition of exponentials" which is observed in all experiments. We wanted to prove this kind of things. A proof that the solution exists follow a conventional path and is rather straightforward. Then, we tried to obtain some other rigorous properties, for instance, that it is a completely monotonic function. We understand that there is this bifurcation scenario. Irrespective of whether you like it or not it just falls out of the self-consistent equations. This is what people who are critical to the theory did not understand, that there is a bifurcation in the equations, and this bifurcation gives this nontrivial relaxation. It was more our personal interest, but it's always good to have exact results.

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

LS: [0:52:38] No, I don't.

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

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[72] W. Götze, *Complex Dynamics of Glass-Forming Liquids: A Mode-Coupling Theory* (Oxford: Oxford University Press, 2009).

LS: [0:52:59] If I have correspondence?

PC: Do you still have papers, notes, correspondence about these ideas from that epoch?

LS: [0:53:13] Yes. I have a file with letters from Götze and other people. Do you think that anybody is interested?

PC: I think so. We can discuss this more after the recording, but one solution could be that if Prof. Götze has an archival fonds in Munich, you could deposit these letters along it.

LS: [0:54:00] Where do you find such archives?

PC: Usually, it would be at the university libraries at TU München that would have established such a depository. In short, I understand the material you have, and we'll carry on discussing after the end of the recording. Thank you for this interview.

LS: Ok. Thank you.