History of RSB Interview: Haim Sompolinsky

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Location:
Over Zoom, from Prof. Sompolinsky's Hebrew University of Jerusalem office in Jerusalem, Israel.

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PC: Hello, Prof. Sompolinsky. Thank you very much for joining us. As we discussed before this interview, the theme of our conversation will mostly be the ideas surrounding spin glasses and replica symmetry breaking, which we bound in time roughly from 1975 to 1995. But we will diverge a bit and ask a few questions on background to begin with, which will help situate your contributions to that field. Can you tell us a bit more about your family and your studies before starting university, and what got you to pursue studies in physics afterwards?

HS: [0:00:58] That's a long story. My father was a scientist, a microbiologist[1]. He was working on a clinical ward, but also at university, so it was not unnatural for me to think about a scientific career. Before joining the university, I studied at a yeshiva—a religious academia for Talmudic and religious studies—and then I joined the Israeli army for compulsory service, as you know. I did my army service alongside my graduate studies. I first joined Bar-Ilan university for the bachelor's degree and then continued with the master's and PhD[2]. When I decided on an academic career, I was actually torn between mathematics and physics. I also did some psychology. So, I studied three subjects in parallel. Then, when I had to decide about graduate studies, I decided that despite the beauty of

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[1] A. Spiro, "Microbiologist and Holocaust hero David Sompolinsky dies at 100," *The Times of Israel*, October 20, 2021. https://www.timesofisrael.com/microbiologist-and-holocaust-hero-david-sompolinsky-dies-at-100/ (Accessed May 17, 2022.)

[2] Haim Yitzhak Sompolinsky, תכונות דיאלקטריות של גבישים פרואלקטריים מטיפוס KDP [Dielectric properties of KDP-type pro-electric crystals], PhD Thesis, Bar-Ilan University (1979).
https://biu.primo.exlibrisgroup.com/permalink/972BIU_INST/1b2mrro/alma990000155180205776

mathematics, the beautiful meeting of mathematics and physics was kind of a good compromise between psychology and mathematics. Psychology was too qualitative and non-quantitative, not grounded neither in theory nor in experiment the way I saw it. But physics was the beautiful meeting of theory and mathematics and nature. That captured my intellect.

PC: What then drew you specifically to theoretical solid-state physics within the mathematical physics ideas?

HS: [0:03:10] As you can imagine, having a strong affinity to mathematics and also poor technical skills, experimental science was not part of my agenda. Within physics, I found that working on problems which are closer to the spatial and temporal scales of everyday life, like condensed matter physics, made more sense than working on astrophysics or particle physics.

PC: You then went on for postdoctoral studies at Harvard, in the group of Bert Halperin[3]. What drew you there? Why with him and why there, in particular?

HS: [0:04:15] I finished my army service alongside with my PhD at Bar-Ilan University, and it was clear to me that I wanted to do a postdoc abroad. I don't know. Harvard, you know, is a school that everybody look toward, and Professor Bert Halperin was one of the figures that everybody knew.

PC: So, you just contacted him? You didn't have any prior relationships or particular exchanges with him before applying?

HS: [0:05:06] No. I wrote to him. I applied. I got some fellowship from Israel to partially support my postdoc[4], and I was fortunate to be accepted in his group.

PC: Can you describe how the group was functioning at that time? What did it mean to be part of the Halperin group?

HS: [0:05:26] It was quite an experience. We were basically seven—or around that number—postdocs in one big room; [there was] one big office for his group. It was quite international. We had [people] from Israel [and], obviously, from the US. We had a Dutch postdoc, we had one from China, and we had one from Germany. So, it was quite a lively group. We were

[3] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Bertrand I. Halperin*, transcript of an oral history conducted 2021 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 14 p. https://doi.org/10.34847/nkl.7ac326ng

[4] Rothschild Fellowship for Postdoctoral Research: https://en.wikipedia.org/wiki/Rothschild_Fellowship

socially interacting, of course. But in terms of the science, I would say most of the group—and perhaps most of the condensed matter groups in theory at Harvard—were focusing on critical phenomena.

This was the time of the peak of the renormalization group. I would say everybody did an epsilon expansion of one sort or another. At that time, critical dynamical phenomena—Bert Halperin was one of the founders of that field—was also a very big topic. I joined the group and the first thing I realized is that I didn't understand what people were talking about. I came from more classical condensed matter, mean-field [theory] and some perturbation expansion, and some self-consistent mean-field stuff and so on. That was for me a first exposure to modern critical phenomena and renormalization group. The first thing I decided is that I would buy Shang-keng Ma's—I don't know if you know it—beautiful book on critical phenomena[5]. I locked myself in for a couple of weeks—this was in the summer, at home—and I went from A to Z of that book. Then, I felt ok. I felt comfortable. I began to understand what people were talking about.

But although I was new to the field, I also realized that it is a field that is already mature, and people are doing… I mean the framework is already there. I found not so exciting [the idea] to join that work, to just do another epsilon expansion on another model. It didn't appeal to me. In Bert Halperin's group, if you do… Sometimes he or some other professor would come and say: "Why don't you calculate this or that?" But if that's not what appeals to you, then you are left to yourself. I was left to myself for quite a while, searching my way, reading.

I did some calculation with Shang-keng Ma[6]. (He visited.) I worked myself through the crowd, so to speak, but I didn't find a topic that would engage me intellectually. I was fortunate to overlap at the same time with professor Aharony,[7]—you may know him—an Israeli distinguished physicist, who came for a sabbatical. I discussed with him those problems and he told me: "There's a problem. There's a spin glass something." He gave me a few papers by Phil Anderson. "Why don't you read it?" It caught my mind. Here's a field, something very new. It was different from what people did, and it was something that was still open. (I like open problems. I still like to work on open problem.) So, I started to work on that. I started to do something about that. I came to Bert Halperin, looking for his advice about that. He said: "I don't recommend it. This is a problem that the biggest minds in condensed matter physics have tried and failed: Phil

[5] Shang-keng Ma, *Modern theory of critical phenomena* (Reading: W.A. Benjamin, 1976).
[6] Shang-keng Ma: https://en.wikipedia.org/wiki/Shang-keng_Ma
[7] Amnon Aharony: https://en.wikipedia.org/wiki/Amnon_Aharony

Anderson, Thouless[8], all these big names. It's stuck." He was very discouraging. But I was stubborn enough that I said: "OK. I'll continue." As they say, the rest is history. When I came to him with some results, he of course realized that. "Keep working on it. That's good!"

In general, Professor Bert Halperin, as a human being, [is] very warm, very kind, [a] special person. But I also immediately—we all—realized that as a scientist, as a mentor, he can be cold-blooded, merciless, critical of your work. We had a ritual. Every one of us met him once a week, and that was the pattern. The person after the meeting came back to the room with tears in his (or her) eyes, because Bert Halperin would just shred the work. That's what happened to me at the beginning. I came up with all kinds of ideas, but… I was amazed, first of all, [by] how fast he knew what I was doing. Not only the problem, but also what I was doing. And how fast he shut it down mercilessly. He didn't raise his voice, but he would put a knife into this, and that's it. You go back.

It was, for me, really life changing. It has really shaped my mind as a scientist. You cannot fool yourself. For several weeks you can work, and you think you know, you understand this and that, you come… That was a very important education for all of us there. It sharpened our minds. And the clarity! Even to this day you find scientists—physicists or not—that have some fuzzy minds. They kind of have some intuition. They see some experiment, they see something, they tell a story. You know, it's on shaky ground, but it's kind of fuzzy. Whenever I am in that fuzzy mode, Bert Halperin is in front of me. "No, don't." If something is not clear, there is a mistake potentially that is looming there. I'm telling my students: "When you have some doubts, don't ignore it, because it may mean that you're doing some serious mistake. So, don't fool yourself." That's the worst thing for a scientist, fooling yourself. Anyway, that is part of what we learnt from Bert Halperin, aside from great physics.

Unbelievable, [his] intuition! You can save so much work and so much agony and so much frustration by following some intuition. It's amazing! I must say that mathematics… I don't know if there is some of that in mathematics, but I must say that [it's remarkable in] physics to see the great minds, how much mileage they can gain before doing a calculation, just by thinking about scales, about underlying mechanisms or potential. That's fantastic! Condensed matter physics is really the battlefield of great minds.

[8] David J. Thouless: https://en.wikipedia.org/wiki/David_J._Thouless

PC: As you just said, you started working on spin glasses, but more specifically you were using a dynamical approach to spin glasses[9]. Where did the particular approach to the problem come to you?

HS: [0:15:03] I actually don't know. It's a good question I don't have an answer to. I remember the first thing I worked on in spin glasses is trying to write down dynamical mean-field equations for the spin glass order parameter. I think – but I am not sure – that at that time the replica approach was bizarre, so not clear, so uninterpretable. That was before the replica symmetry breaking interpretation came, Parisi's work[10]. So, it was something that naturally didn't attract me. I was looking for an alternative, more interpretable theory. That's what led me to dynamics. So, early on, I started looking at a dynamic approach.

PC: You also quickly started to collaborate with another postdoc in the group, Annette Zippelius[11], on these problems. Can you explain how that collaboration came about?

HS: [0:16:25] Ok. First of all, she worked on dynamical problems, if I remember correctly, on critical dynamical problems at that time[12], not spin glasses. Second, we were all sitting in one big room. We all shared ideas and problems, so it was very natural for us to join forces.

PC: So, she didn't know spin glasses before? You brought this to the conversation?

HS: [0:16:51] Yeah. That's right. Annette is a great scientist. It wasn't hard to get her into the problem, and also to engage her into it. So, it was a very fruitful, but also very natural collaboration for two members in this group.

PC: What was the intermediate reaction to those works? Were people excited, or did it take a while for them to be appreciated?

HS: [0:17:29] I don't know who "they" is. I was very excited.

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[9] H. Sompolinsky, "Time-dependent order parameters in spin-glasses," *Phys. Rev. Lett.* **47**, 935 (1981). https://doi.org/10.1103/PhysRevLett.47.935; 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

[10] See, *e.g.*, P. Charbonneau and F. Zamponi, *History of RSB Interview: Giorgio Parisi*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 77 p. https://doi.org/XXXXXXXX

[11] Annette Zippelius: https://en.wikipedia.org/wiki/Annette_Zippelius

[12] See, *e.g.*, A. Zippelius, B. I. Halperin and D. R. Nelson, "Dynamics of two-dimensional melting," *Phys. Rev. B* **22**, 2514 (1980). https://doi.org/10.1103/PhysRevB.22.2514

PC: The community, say, Bert Halperin, the reviewers, then people who might have invited you for talks.

HS: [0:17:40] I must say that the papers were received very well by the reviewers. I think it was a different time. (That's a topic for a different conversation about the review process then and today. I must say that today to publish is a nightmare. It really discourages great young minds. Some of the brightest students that I had left science largely because they were frustrated by this agony of publishing their work, but at that time it wasn't like that.) Good science wasn't hard to get through the system. *Physical Review Letters*, you didn't have to claim that this is the discovery of the century. You did some top science, and it would go through. My experience was that. At that time, as a young postdoc, for me what was important was Bert Halperin, David Nelson[13], other colleagues in the group or at the university. [There,] it was very well received. Bert Halperin told me: "Half a year ago, I would discourage you from doing work on spin glasses, but now go ahead!" I remember the summer of the first year of my postdoc, there was a Gordon conference. I was very honored that Bert Halperin in his talk reported on my work[14], so I was very encouraged. It's not hard. I knew that I [was] doing something different, something new, particularly when I found out the dynamical analogue of replica symmetry breaking. I must say, to me, it was rather a miracle because there was no systematic path to it. I was looking at these dynamical self-consistent equations, and it just came… There was no kind of mathematical or perturbation theory or some calculation that would lead to it. It was like a jump. Sometimes you have [that] in science. It just jumps to you that's it's the way to do it. I felt: "This is something new; this is something nice." That was enough for me.

PC: Was it not your plan from the start to make this connection? Or did it emerge as you were working?

HS: [0:20:46] It wasn't. It emerged.

PC: While at Harvard, you collaborated on a couple of other projects with Bert Halperin and with Chris Henley as well as with Chandan Dasgupta[15]. Can you tell us a bit about the genesis of these other projects?

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[13] David Robert Nelson: https://en.wikipedia.org/wiki/David_Robert_Nelson

[14] *Quantum Solids and Fluids,* Plymouth State College, A. M. Goldman and C. M. Varma, June 29-July 3, 1981. See, *e.g.*, A. M. Cruickshank, "Gordon Research Conferences," *Science* **211**(4487), 1191-1227 (1981). https://www.jstor.org/stable/1685252

[15] C. L. Henley, H. Sompolinsky and B. I. Halperin, "Spin-resonance frequencies in spin-glasses with random anisotropies," *Phys. Rev. B* **25**, 5849 (1982). https://doi.org/10.1103/PhysRevB.25.5849; C. Dasgupta and

HS: [0:21:07] These were related to spin glasses. With Bert Halperin and Chris Henley, it was initiated by Bert. He was motivated by NMR experiments at that time. It was very much Bert's style to look in from experiments to try to understand the physics. He came up with the idea of local order parameters which are not scalars or vectors but kind of matrices which represent the local order. Chris Henley[16], with his beautiful abstract geometric mind, was terrific. It was natural to work. I was very happy to have some work together with Bert Halperin, in particular.

Chandan Dasgupta[17], once again, was a postdoc at that time. Again, these are all bright scientists, and it was not unnatural to work together and to come up with joint work. There was another paper from my postdoc, which was not on spin glasses. It was on localization in phonons with Michael Stephen[18]. Again, it was an open environment, and I was exposed at that time. I was thinking about localization, [which] was an important topic at that time. Thinking about other phenomena in disordered systems that can exhibit that phenomenon…

PC: After your time at Harvard, you moved to Bar-Ilan, but you kept strong collaborations in the US. You notably wrote papers with Gabriel Kotliar[19], with Daniel Fisher[20]. Can you describe what was your relationship with Bell labs and how these two sets of works came about?

HS: [0:26:45] I got a position at Bar-Ilan. It was my plan, for myself and my family, to go back to Israel, but I felt that I wanted to spend more time in the US and experience working with more people there. I had an opportunity with Patrick Lee[21]. He invited me to come to Bell Labs. The deal with Bar-Ilan was that I would come to Bar-Ilan, spend a year at Bar-

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H. Sompolinsky, "Equivalence of statistical-mechanical and dynamic descriptions of the infinite-range Ising spin-glass," *Phys. Rev. B* **27**, 4511 (1983). https://doi.org/10.1103/PhysRevB.27.4511

[16] V. Elser and N. D. Mermin, "Christopher L. Henley," *Physics Today* (2015). https://doi.org/10.1063/PT.5.6160

[17] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Chandan Dasgupta*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 19 p. https://doi.org/10.34847/nkl.c4dc2us6

[18] S. John, H. Sompolinsky and M. J. Stephen, "Localization in a disordered elastic medium near two dimensions," *Phys. Rev. B* **27**, 5592 (1983). https://doi.org/10.1103/PhysRevB.27.5592

[19] See, *e.g.*, H. Sompolinsky, G. Kotliar and A. Zippelius, "Exchange stiffness and macroscopic anisotropy in Heisenberg spin-glasses," *Phys. Rev. Lett.* **52**, 392 (1984). https://doi.org/10.1103/PhysRevLett.52.392; G. Kotliar and H. Sompolinsky, "Phase transition in a Dzyaloshinsky-Moriya spin-glass," *Phys. Rev. Lett.* **53**, 1751 (1984). https://doi.org/10.1103/PhysRevLett.53.1751

[20] D. S. Fisher and H. Sompolinsky, "Scaling in spin-glasses," *Phys. Rev. Lett.* **54**, 1063 (1985). https://doi.org/10.1103/PhysRevLett.54.1063

[21] Patrick A. Lee: https://en.wikipedia.org/wiki/Patrick_A._Lee

Ilan, and then they would let me spend a year at Bell Labs. Since then, for many years, I was a visiting scholar at Bell Labs every summer. I was interacting with Phil Anderson, Daniel Fisher, and, later on, with John Hopfield[22], biophysicists and so on. The story of neural networks emerged from that. But the connection was Patrick Lee. Actually, I did with him some work which was not published, related to the localization transition and ϕ^4 theory. It was a great time. Bell Labs, at the time, was at the peak of its glorious days in condensed matter physics. Again, the experience of meeting Phil Anderson in the morning, over a cup of coffee, and on the blackboard spontaneously discussing this or that. Phil Anderson took me somewhat under his wing. Because he was Phil Anderson, he had his own department at Bell Labs: Department of Fundamental Physics. Only one member was there, which was Phil Anderson. He was the head of the department, but there was nobody else. That was the idea: to make him his own department. There was a secretary. Phil Anderson brought me to this department, so I was very spoiled. I had this department. I had the secretary for myself, and I had the occasion to interact with him informally, to discuss with him. It was great.

FZ: I wanted to ask you a question about that. Since you interacted so closely with Anderson, what was his feeling at that time about what was going on in the spin glass field, in particular the replica solution, the dynamics that you had developed, the relation between the two and how they could be applied to other systems? I think it was a bit before he came up with the paper on optimization[23], where he was proposing to apply spin glass ideas to optimization.

HS: [0:27:13] I don't remember any particular discussion with him on replica symmetry breaking. Obviously, it was too formal for him. It's like [for] Bert Halperin. They were reluctant to think in a kind of formal framework that they couldn't directly relate to in physical terms, at least at that time. But later on, the ultrametricity did grab attention and imagination, including Phil Anderson's. I don't know exactly what year it was he came up with a model of prebiotic model of evolution along spin glasses[24], but, you know, spin glass was always his favorite, so it was very natural for us to discuss spin glasses. Ultrametricity was something which resonated, but this was

[22] P. Charbonneau, *History of RSB Interview: John J. Hopfield*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2020, 21 p. https://doi.org/11280/5fd45598

[23] Y. Fu and P. W. Anderson, "Application of statistical mechanics to NP-complete problems in combinatorial optimization," *J. Phys. A* **19**, 1605 (1986). https://doi.org/10.1088/0305-4470/19/9/033

[24] P. W. Anderson, "Suggested model for prebiotic evolution: The use of chaos," Proc. Nat. Acad. Sci. **80**, 3386-3390 (1983). https://doi.org/10.1073/pnas.80.11.3386

later on, when this ultrametric structure emerged through the work of Parisi and others.

FZ: This was the 1984 paper with Toulouse, Sourlas, Mézard and Virasoro[25]. Had Anderson seen that paper at the time?

HS: [0.29:05] I don't remember. At that time, there were preprints, there were seminars. I don't remember which paper he read or not. But ideas like that came out and they were topics of discussion.

PC: In Bar-Ilan proper, you had another collaboration on spin glasses with David Gross and your first graduate student, Ido Kanter, on the instability of the RSB solution in certain spin glass models[26]. I think that David Gross was on sabbatical then.

HS: [0:29:41] With David Gross[27], it was about the Potts glass. He spent summers in Israel; he had family in Israel. We met in Israel, and we started a collaboration. I was lucky to interact with him at the time when—he will forgive me if I say that—he was unemployed, more or less. It was after the standard model was fully developed – including his contributions and so on – and before the string theory. Those theorists didn't have a big problem, a big project in their agenda. So, he was looking around, he was talking. Spin glasses were one of the fashionable and hot topics at that time. Anyway, that's what led to our collaboration. I was again lucky to work with those great minds. Unfortunately for me, and for the condensed matter community, when string theory came up, he was drummed back into fundamental physics and high energy physics.

FZ: What was the motivation for that study. Why did you choose to study the Potts glass together at the time?

HS: [0:31:28] I can't remember exactly. I may be wrong, but I think there was a paper on three state glass by Sherrington[28]. It was an approximate [treatment] and there was some ambiguity whether this transition is first-order [or] second-order [or] what are the order parameters. It looked like an interesting problem, but again a problem which is yet to be cleared. We

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[25] M. Mézard, G. Parisi, N. Sourlas, G. Toulouse and M. Virasoro, "Nature of the spin-glass phase," *Phys. Rev. Lett.* **52**, 1156 (1984). https://doi.org/10.1103/PhysRevLett.52.1156

[26] D. J. Gross, I. Kanter and H. Sompolinsky, "Mean-field theory of the Potts glass," *Phys. Rev. Lett.* **55**, 304 (1985). https://doi.org/10.1103/PhysRevLett.55.304

[27] David Gross: https://en.wikipedia.org/wiki/David_Gross

[28] D. Elderfield and D. Sherrington, "The curious case of the Potts spin glass," *J. Phys. C* **16**, L497 (1983). https://doi.org/10.1088/0022-3719/16/15/003

were influenced, obviously, by the work of Derrida[29] on the random energy models or *p*-spin [models], where the first-order replica symmetry breaking is exact. We were looking for a limit where you can basically solve the problem and clarify [the physics] to some degree. That was the story.

PC: Were you aware of the work that followed shortly afterwards by Elizabeth Gardner[30] who found a similar phenomenology but in a different model[31]? Or did this realization come much later?

HS: [0:33:03] I don't remember. I mostly got to know Elizabeth Gardner from her work on neural networks and the perceptron[32], of course. I don't remember. At that time, I guess I knew every substantial theory work on spin glasses. We were a relatively small community. We had rather frequent meetings in Orsay, in Heidelberg. For several years, the spin glass community met and discussed and debated. I'm sure I knew everything significant that came out from Derrida and Gardner, but I don't remember now that particular paper.

PC: You then authored a series of seminal papers on neural networks with Daniel Amit and Hanoch Gutfreund[33]. How did this work come about? How did this collaboration come about?

HS: [0:34:22] The collaboration started before the work on neural network. I was at Bar-Ilan, and I made some connection with Daniel Amit[34] and Hanoch Gutfreund[35]. I started to spend some time at the Hebrew University. Daniel Amit was particularly keen to learn [about] spin glasses. I remember the first project we had, which wasn't published, was trying to use supersymmetry on the spin glass problem. I think we made some

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

[30] Elizabeth Gardner: https://en.wikipedia.org/wiki/Elizabeth_Gardner_(physicist)

[31] E. Gardner, "Spin glasses with *p*-spin interactions," *Nucl. Phys. B* **257**, 747-765 (1985). https://doi.org/10.1016/0550-3213(85)90374-8

[32] See, *e.g.*, E. Gardner, "The space of interactions in neural network models," *J. Phys. A* **21**, 257 (1988). https://doi.org/10.1088/0305-4470/21/1/030; E. Gardner and B. Derrida, "Optimal storage properties of neural network models," *J. Phys. A* **21**, 271 (1988). https://doi.org/10.1088/0305-4470/21/1/031

[33] D. J. Amit, H. Gutfreund and H. 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; "Spin-glass models of neural networks," *Phys. Rev. A* **32**, 1007 (1985). https://doi.org/10.1103/PhysRevA.32.1007; "Statistical mechanics of neural networks near saturation," *Ann. Phys.* **173**, 30-67 (1987). https://doi.org/10.1016/0003-4916(87)90092-3

[34] Daniel Amit: https://en.wikipedia.org/wiki/Daniel_Amit

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

progress, but it never was mature enough to publish something. That was our first attempt.

I was first exposed to John Hopfield's seminal paper in PNAS at the end of my postdoc. I was attending a colloquium that Phil Anderson gave at MIT, in physics. It was called, if I remember correctly: "Seeing the world through spin glasses." Or some version of that. It was on spin glasses, but also on related fields outside physics, the optimization problem, and his interest in prebiotic evolution and so on. He mentioned John Hopfield's work applying the spin glass problem to associative memory. It barely registered in my mind. I had never thought about neural networks. It was a curiosity, but then when I worked with a young collaborator of Daniel Amit and Hanoch Gutfreund, we worked on physics and spin glasses, I remember that Hanoch Gutfreund came back from a visit in the US. He actually visited Berkeley with Little. (They had a long collaboration on superconductivity.) Then, he stopped on his way back in Paris, meeting his old friend, Gérard Toulouse[36]. He told us, Daniel Amit and myself: "Look, I visited Berkeley, and I talked to Bill Little, and he was talking to me about neural networks. Then, I come to Paris, and I talked to Gérard Toulouse, and all I see on his desk is the papers of John Hopfield on neural networks. So, something is there." So, we decided maybe it's time to get into it. That's what triggered our interest and curiosity about neural networks.

PC: How were your skills complementary in this? What were you each bringing to the problem?

HS: [0:38:11] Obviously, I brought the spin glass part. Daniel Amit, his insight. Also, he was much better than any of us on what was numerics at that time. I must say it was very low-level numerics, but he was much more modern than us at that time in doing simulations and stuff like that. He liked computers. Hanoch Gutfreund brought old-fashioned physics intuition into the field. He is coming from a very different field, but [has] a good old solid condensed matter physics insight. It was a very good synergy and complementary approach. Daniel Amit was also a lot more formal in his work in critical phenomena, [bringing] a field-theoretic perspective. And Hanoch Gutfreund [has a] much more basic physics intuition.

PC: So, you brought the replica symmetry breaking toolkit?

HS: [0:39:41] Spin glasses, yes. Not only replica symmetry breaking, but replica [period]. You know replica symmetry breaking was important, but it wasn't playing a major role. I would say it is also true not only for the early papers,

[36] Gérard Toulouse: https://en.wikipedia.org/wiki/G%C3%A9rard_Toulouse

but also later on, if you look back at the influence of spin glasses on the entire field of neural networks or theoretical neuroscience, computational science. Replica symmetry breaking appears here and there, but it's not a major play. That was what we found interesting about neural networks. It's kind of in-between the kind of simple magnetic systems—ordered systems in general—and spin glasses. It was situated in a place where [on the one hand] you could interpret at least a part of the spectrum of the ground state, or the low-energy states, and you actually control them by learning, design them by learning; [and] on the other hand there are looming things around it which you have to know how to handle. These are all the pure states and the spin glass phase, where symmetry breaking appears. But it's kind of symmetry breaking… When there is symmetry breaking either it has a small effect in correcting capacity from 0.138 to 0.114—it was not a big thing—or it was evidence that you are in forbidden territory, you are in a spin glass phase. There was early on, and maybe in some minds even today a hope or a dream that the full-blown spectrum of ultrametricity and full-blown spin glass metastable states could be recruited for high-capacity computation. I don't know. I don't think this dream has materialized so far. And I think that the field as a whole tended to rely on more controllable and more robust states as the states for doing computation.

PC: What was the reaction of the community to these results? Was the enthusiasm immediate?

HS: [0:42:40] Yeah. It was a bomb shell. Not one of my colleagues told me: "I was doing it and I saw your paper." It was a time where the entire community of spin glass experts like me, Alan Bray[37], John Hertz[38], van Hemmen[39], Cirano De Dominicis[40], and Derrida, we had this great toolbox. Ok, so spin glasses, you know, they fight: three dimensions here and there; there is a transition, there's no transition; the American scientists vs the European scientists. As much as it was… I was agnostic. It's great science, and there are applications to this outside physics. Whether the good old spin glasses in three dimensions had replica symmetry breaking or a

[37] 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

[38] 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, 2021, 18 p. https://doi.org/10.34847/nkl.cad347wh

[39] See, *e.g.*, P. Charbonneau, *History of RSB Interview: J. Leo van Hemmen*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 22 p. https://doi.org/10.34847/nkl.16e5m0oj

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

different transition didn't really prevent me from sleeping at night. Mostly, we were interested in finding other areas where this can be applied. People did all kinds of things, and now here comes neural networks. This was a big development.

PC: In that context, the three of you co-organized a workshop at the Institute for Advanced Studies in Jerusalem[41]. What led to this gathering? And what in your mind were the highlights of that year-long program?

HS: [0:44:35] What led [to it]? It was a natural candidate for a program in the Institute of Advanced Studies. It was an emerging topic. Daniel Amit and Hanoch Gutfreund were both very well embedded within the university leadership to be able to propose it and to get it approved. And it was very timely, because we were a rather dispersed group of scientists, working on kind of a new field. We had different ideas. Not all ideas were the same, and also, we were at that time beginning to interact with biologists, neurobiologists. At the beginning, it was just stat mech, but then we were naïve enough to say: "Well, maybe this is serious. Maybe it has to do with the brain." So, we started to engage in bi-weekly (or regular) seminars with local neurobiologists at the Hebrew University. For the first year or so we were just wasting time to fight about culture. Is a neuron +/- 1 or zero/one? This kind of debate. Who cares? You shift! But biologists couldn't handle it. They had intuition. And by the way, they were right. For some phenomena it makes a difference. Anyway, we were fighting about that. What is exciting to you saying negative or positive? It's not exciting. There's the biological definition and all kinds of fights of arguments about that. Point neuron. What is this point neuron? It's not a neuron. But we were stubborn enough.

You know, Israelis and Israeli scientists [in particular], that's the way they discuss, by fighting, by disagreeing. That's the way our engine works. Sometimes, I started a discussion, a debate with a foreign scientist and he got insulted. I don't know what. This is… You should be honored if I'm fighting with you. That's the way Israelis are, for good or for [bad].

So, we started arguing, but we never gave up. Professor Moshe Abeles[42] and the late Itzchak Parnas[43], they were patient, and they were insistent. They knew that there was something here and they kept engaging us. And it was fantastic. This was maybe the first barriers between physics and

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[41] D. Amit, H. Gutfreund, H. Sompolinsky, Israel Institute of Advanced Studies, Jerusalem, Israel, 1987.
[42] Moshe Abeles: https://en.wikipedia.org/wiki/Moshe_Abeles
[43] Itzchak Parnas: https://he.wikipedia.org/wiki/%D7%99%D7%A6%D7%97%D7%A7_%D7%A4%D7%A8%D7%A0%D7%A1

neurobiology—at the time I could say even biology, because there was no system biology—which were crumbling. This was part of the goal of that, and part of the success of that workshop.

Moshe Abeles sat next door. (He wasn't at the medical school, which is on a different campus.) He was next door to me, and Daniel Amit and Hanoch, and Valentino Breitenberg[44]. He's not only an established scientist, but an intellectual biologist, [with] culture and music. He was like from the Renaissance, from the movies about Renaissance scientists and intellectuals. He was there, and then, of course, Gérard Toulouse, Elizabeth Gardner. Alessandro Treves[45] was a student at that time. It was a great group of scientists. Peretto[46] and Derrida came for some period of time. That was one of the foundational events in developing our trajectory. I think it was very successful.

By the way, there was also a meeting associated with it. I don't remember if it was at the beginning or the end, but I think at the beginning. Anyways, there was a meeting which was also very important, and gave us an opportunity to meet with neuroscientists that were thinking a little bit different from us about the neural circuit and gave us perspective. You know, the importance of dendrites and the details of nonlinearity of propagation of signal in a neuron and in the network. We began to see a field which has some commonality. We can be with each other, we can talk about the same problems, but with different approaches. That type of interdisciplinary enterprise started to emerge through those early meetings.

I remember [something], by the way. I gave a talk in one of these meetings and Itzak Parnos came to me and said: "Haim, you mentioned excitatory postsynaptic potential and inhibitory postsynaptic potential. I'm very happy. Finally, you started talking in the language of biology, not + J or -J, J_{ij}. No! You started to talk in terms of biological, biophysical terms." He was very happy.

PC: Speaking of that divide, we've had a recent email exchange with you in a different context—about a book[47]—where a distinction between artificial and biological neural networks was proposed. How grounded in this epoch in this tension about the language?

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[44] Valentino Breitenberg: https://en.wikipedia.org/wiki/Valentino_Braitenberg

[45] Alessandro Treves: https://neurotree.org/beta/peopleinfo.php?pid=3712 (Accessed May 17, 2022.)

[46] See, *e.g.*, the preface in Pierre Peretto, *An Introduction to the Modeling of Neural Networks* (Cambridge: Cambridge University Press, 1992).

[47] *Spin Glass Theory and Far Beyond*, P. Charbonneau, E. Marinari, M. Mézard, G. Parisi, F. Ricci-Tersenghi, G. Sicuro, F. Zamponi, eds. (Singapore: World Scientific, 2023).

HS: [0:51:31] I don't know. I didn't have a lot of time to engage with my colleagues about it. But I did think, and I do think it's the wrong terminology. One of the earliest models or theories in computational neuroscience—[that] of David Marr[48] for the cerebellum, and later on applied to the neocortex—is a feed-forward neural network. It was very influential and very important in understanding the cerebellum as an information processing system and a learning system. So, the dichotomy between feed-forward networks as artificial and recurrent networks as biological to me doesn't make sense.

PC: About the same time, or right after the workshop, you moved from Bar-Ilan to the Hebrew University. From that point on, you did not work again with Amit nor with Gutfreund. Can you describe us, then, how the group was working at the Hebrew University?

HS: [0:52:45] Can you repeat the question?

PC: You moved from Bar-Ilan to the Hebrew University. But you also stopped publishing with Daniel Amit and with Hanoch Gutfreund at that same time. So, I was curious how the group worked together, or did their interests diverge at that point?

HS: [0:53:02] I think it was a natural development, because we realized how much work there is to be done in the field, on the one [hand], and on the other [hand], I – at least myself – became more senior and had my own students. I had students in Bar-Ilan like Ido Kanter[49] and **Eli Barkai(?),[50]** and I had students at the Hebrew University. This is naturally what happened. When you start having your group, you start to have problems that you want your group to work on.

Also, Daniel Amit started to write a book on it[51]. Again, knowing Daniel Amit, it was very natural. He was a writer in his soul, which was fine with us, but it consumed his energy. Actually, he generously invited us to join him. Hanoch was rector then president of Hebrew University, so I guess it

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[48] David Marr: https://en.wikipedia.org/wiki/David_Marr_(neuroscientist)

[49] Ido Kanter, תאוריה של זכוכיות ספין והשלכותיה למערכות מרכבות במתמטיקה ובביולוגיה [The theory of spin glass and its implications for complex systems in mathematics and biology], PhD Thesis, Bar-Ilan University (1987). https://biu.primo.exlibrisgroup.com/permalink/972BIU_INST/1b2mrro/alma990002337750205776

[50] See, *e.g.*, E. Barkai, I. Kanter, and H. Sompolinsky, "Properties of sparsely connected excitatory neural networks," *Phys. Rev. A* **41**, 590 (1990). https://doi.org/10.1103/PhysRevA.41.590

[51] Daniel J. Amit, *Modeling Brain Function: The World of Attractor Neural Networks* (Cambridge: Cambridge University Press, 1989).

was part of the reason why he was less interested in that, or even less active to some degree. I was too busy working. I felt I didn't want to spend [then] time to write a book. I just wanted to produce more. Each one of us had a different path.

PC: By that point, you had largely left the field of traditional spin glasses. Did you keep abreast of what was happening, of the discussions that were going on?

HS: [0:55:15] To some degree. I was still working on problems which were influenced by spin glasses, like random matrix theory and chaos and dynamics in random neural networks. You can think [of their dynamics] as an analog of spin glasses but in asymmetric non-equilibrium systems. But I stopped working on spin glasses, per se, largely because I was too occupied with neural networks and those problems. Also, I didn't feel—but that may be not fair—that there was something too exciting that is still yet to be discovered. That maybe an unfair judgement, but I was busy working on the field. At the same time, it may be not reflected in papers, but I was reading more about the brain and about biology and neurobiology. I started a collaboration with David Kleinfeld[52], whom I met at Bell Labs, about sequence memory but also applying it to animal models and to pattern generators[53]. There is [only] that much that one person can do at a given time.

PC: Can you describe what were the general questions you were pursuing at that point in neural networks? You said that it felt like there was something to be discovered.

HS: [0:57:16] I had, at that time, a love and hate relationship with the cortex. Obviously, what motivated us was memory, learning, big questions. We were all thinking about trying to decipher the enigma of the human brain, or maybe cognitive functions. But we also were frustrated—and I must say even today to some degree—by the lack of sufficient progress in validating those models in experiment for various reasons. One big reason is that designing and carrying out experiments with animals takes orders of magnitude more time than in spin glasses. I remember when I was working in Bar-Ilan on spin glasses or even on high-T_c superconductivity at this time with my friend Yosi Yeshurun[54], who was an experimentalist working on

[52] "David Kleinfeld," Neurotree (undated). https://neurotree.org/beta/peopleinfo.php?pid=1773 (Accessed May 20, 2022.)

[53] See, e.g., D. Kleinfeld and H. Sompolinsky, "Associative neural network model for the generation of temporal patterns. Theory and application to central pattern generators," *Biophys. J.* **54**, 1039-1051 (1988). https://doi.org/10.1016/S0006-3495(88)83041-8

[54] See, *e.g.*, Y. Yeshurun and H. Sompolinsky, "Transverse ordering in anisotropic spin glasses,"

the same problem, I would suggest: "Why don't you measure this?" And a week after that, he would measure this, and we could compare.

Now, this is not going to be like that in biology. So, I was oscillating. At some point, I said: "Well, maybe you should focus on small animals, like slugs and worms and so on." I worked on that a time, but I realized that these are too specialized circuits. So, I went back to the cortex, but the challenge which fascinated me then—it still [does] today—is to really understand high-level information processing in the brain. What would fascinate me in this challenge is the combination of a physical, chemical, biological systems, where you can study, as a physicist… You have electric currents, dynamics, all kinds of phenomena. There are people who spend their career studying rhythms and non-linear asynchrony between rhythms. The brain is a dynamical physical system like any other physical systems. But to me the fact that the same physical system generates cognition, consciousness, memory, perception, this combination intrigues me all the time, and still keeps intriguing me. The dream is to contribute to the emergence of a theory in the same way that we talk about theory in physics, a theory that has a qualitative nature to it, but implemented in a quantitative set of equations that can actually engage with experiments and in refinement, revisions and so on. Today, we see more of it in neuroscience, but at that time it was something that didn't exist.

PC: Was this your vision already in the early '90s?

HS: [1:01:00] That was the vision all along.

PC: One of your early graduate students at the Hebrew University was Andrea Crisanti[55]. How did your connection to Rome emerge?

HS: [1:01:13] It was through Daniel Amit. Daniel Amit, as you know, had a long-time connection with Rome. Crisanti came [to Israel] and at that time I don't know why [but] Daniel Amit was busy. (He was maybe the chair of the institute.) So, Andrea was looking for something. He met me down the hall, and we started interacting. I asked Dany if it's ok with him. He said: "Fine. Take care of him. I don't have the patience, basically." I was lucky to

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Phys. Rev. B **31**, 3191 (1985). https://doi.org/10.1103/PhysRevB.31.3191; Y. Yeshurun, I. Felner and H. Sompolinsky, "Magnetic properties of a high-T c superconductor $YBa_2Cu_3O_7$: Reentry-like features, paramagnetism, and glassy behavior," *Phys. Rev. B* **36**, 840 (1987). https://doi.org/10.1103/PhysRevB.36.840

[55] Andrea Crisanti, *Static and dynamic properties of neural networks*, PhD Thesis, Hebrew University of Jerusalem (1988). https://www.nli.org.il/en/dissertations/NNL_ALEPH001067766/NLI

have met Andrea Crisanti. Both he and Sommers[56], we were collaborating together. Unbelievably bright mind and skills. Very fast. Anyway, he was a young student. He didn't really have something to do at that time, so we started collaborating.

I don't know if you know, but in the paper on chaos in random neural network—in that *Physical Review Letters*—we said: "The details are to be published." Or something like that. We never published the actual calculation. Every time people ask me: "Where is the calculation of that paper?" Now, we've had a ready draft—I still have it—with Andrea Crisanti and Sommers. It was the end of his PhD at Hebrew university. He got a position at Rome. He was transitioning to Rome. He didn't have patience or interest to get it published. I have had halfway through comments, corrections, changing and so on left in my drawer for—I don't know—thirty years or something. Then, only two years ago, finally, we published it[57]. Our duty, our debt to the community was a long paper describing the derivation of that short letter.

PC: Another interesting recruitment you made was that of Sebastian Seung[58], as a postdoc in the early '90s. In a 2005 New York Times interview, he is described as having fallen "under the spell of a gregarious Israeli" in joining your group[59]. What was so compelling to physicists to jump into this problem at that point, which made these recruitments possible more generally?

HS: [1:04:41] I don't think it is hard to understand that. Physicists, many of them are driven by big questions. Look, I came to physics, not to do condensed matter theory. I started studying physics because I was dreaming about Einstein, relativity, astrophysics. Many of the students that come into physics think about quantum mechanics, relativity. Then, they go through the PhD, they do beautiful work, but they are attracted, seduced by big problems, where they see: "Wow! What physics can continue doing." That's what happened then and that's what happens today. There are fantastic students doing string theory or other fields of

[56] See, *e.g.*, H. Sompolinsky, A. Crisanti and H. J. Sommers, "Chaos in random neural networks," *Phys. Rev. Lett.* **61**, 259 (1988). https://doi.org/10.1103/PhysRevLett.61.259; H. J. Sommers, A. Crisanti, H. Sompolinsky and Y. Stein, "Spectrum of large random asymmetric matrices," *Phys. Rev. Lett.* **60**, 1895 (1988). https://doi.org/10.1103/PhysRevLett.60.1895

[57] A. Crisanti and H. Sompolinsky, "Path integral approach to random neural networks," *Phys. Rev. E* **98**, 062120 (2018). https://doi.org/10.1103/PhysRevE.98.062120

[58] Sebastian Seung: https://en.wikipedia.org/wiki/Sebastian_Seung

[59] G. Cook, "Mind Games," *New York Times Sunday Magazine* (Jan. 11, 2015), 27. https://www.nytimes.com/2015/01/11/magazine/sebastian-seungs-quest-to-map-the-human-brain.html (Accessed May 21, 2022.)

fundamental physics and they are lured by the seduction of a physics contribution to this big question. It's very natural. It's hard to resist it if you have sufficient confidence in your skills, and you are not deterred by the immensity of the problem. Because you know that you're not going to solve it. It's silly. It will take generations to solve this problem. But if you are bold enough to say: "Well, I can contribute, maybe," and you are willing to take the risk, then, of course, why not? That was Sebastian. He was a bright young scientist, but also, he has a romantic [view] about science that we all had. He came to Bell Labs one summer and we started to discuss neural networks and yeah!

PC: In 1992, you co-founded the Hebrew University Interdisciplinary Center for Neural Computation (ICNC)[60]. What made this center timely? And what led to its foundation?

HS: [1:07:10] As I discussed earlier, we had by that time established good connections, a good rapport, with neuroscientists at the Hebrew University, and also with a couple of other researchers in psychology and in mathematics and computer science. So, we were already building a lively community coming from many departments, and we realized two things. One is that we had a lot of common interests that we would like to build on to establish a community. The other one was that there are too many barriers, particularly in teaching and students. Every student had to be in one department, with one mentor from this department, and so on. We wanted to break those barriers.

At the Hebrew University, one way at that time to build new things was building a center. So, we were one of the first interdisciplinary centers at the Hebrew University. We had almost zero funding—very little funding—and most of the work that was done was voluntary. Our teaching… We started a PhD program, and the Hebrew University was one of the pioneering academic institutions worldwide that had the vision: "We don't know what it is computational neuroscience, but here is a group of scientists. They are doing very well. They are very enthusiastic. Let them go!" So, they let us, and they formally accepted a PhD program into this interdisciplinary field, which was very important, because we can then recruit students and give them a diploma, give them a PhD curriculum. We had weekly seminars, we had visitors, but with very little funding. I had to teach extra for this program, so had most others, but we said: "Ok. It's worthwhile." It was a great time.

[60] Interdisciplinary Center for Neural Computation: https://en.wikipedia.org/wiki/Interdisciplinary_Center_for_Neural_Computation

Prof. Moshe Abeles, who was the head of that for many years—since the establishment—and was a great leader, he was one of those… In that time, it wasn't so common a neuroscientist, an experimentalist, with a very quantitative mind. Also, he did modeling at the system level. He wasn't obsessed with the microbiophysical details. [He thought] about many neurons in a network, information processing, representations and so on. He has his own view on presentation, which we didn't necessarily all the time agreed with or shared, but nevertheless he was very receptive, and he was a leader in this institute, at ICNC. It was a great place to host visitors, and it was a facilitating institution, or center, for building this new discipline.

It was one of the first PhD programs dedicated to this. We didn't say [to] a student: "Take two courses in physics, take two courses in biology, take four courses of your own choice and that's it." Many schools, even today, that's what they do. We insisted on a program, a curriculum dedicated to this field. We developed our own curriculum, our own courses. To this day, that's what we do. So, this is very special, very unique, and it was recognized internationally as a unique place to get trained in this new field.

PC: The study of neural networks appeared to have fallen a bit out of fashion by the mid to late 1990s. Did you experience that change? If yes, how did that impact or not your work?

HS: [1:12:34] Neural networks, as models of computation from a machine learning, computer science perspective fell out of fashion for reasons that are well-known today. I would say, largely because they didn't deliver the technological promises, but also because there were some well-established theoretical weaknesses: local minima, lack of data, and overfitting. Those problems are well-known. So, machine learning and computer science and engineering moved away from this field.

It did affect us to a large degree, in the sense that some of my work and some of the work of my community—theorists—were on more abstract questions such as the statistical mechanics of learning, following the pioneering work of Elizabeth Gardner. At the same time, I, myself, was much more involved in modeling neural circuits in the brain, working on visual processing, on population coding with Sebastian Seung[61], on the balance of excitatory to inhibitory with Carl von Vreesviijk[62]. So, the

[61] See, *e.g.*, H. S. Seung and H. Sompolinsky, "Simple models for reading neuronal population codes," Proc. Nat. Acad. Sci. U.S.A. **90**, 10749-10753 (1993). https://doi.org/10.1073/pnas.90.22.10749

[62] See, *e.g.*, C. van Vreeswijk and H. Sompolinsky, "Chaos in neuronal networks with balanced excitatory and inhibitory activity," *Science* **274**, 1724-1726 (1996). https://doi.org/10.1126/science.274.5293.1724;

shifting of focus, at least for my group, was on questions more related to neural circuits in the brain: how they process information, and how the information processing is shaped by both the architecture and the dynamics. This was sort of the dynamics of the field at the time.

But I must say we never left neural networks as an abstract field. One of the works—this was already in there in the 20th century—that was very important for me was the work with Robert Gütig[63] about tempotron[64]. This is an example of what I said. There was the established work on perceptron, but the brain is talking with spikes. So, we're trying to understand how we can take a very basic foundational model in machine learning of the perceptron and make it more relevant to actual information processing in the brain. What do you do with spikes? How do you translate it? For several years, we were struggling with it until we came up with a solution which was very pleasing to us. Finally, we have something that is really simple and powerful as an atomic module of learning in a single neuron with integrating its spikes and constants between two decision makings. This was the nature of the work we were doing at that time.

PC: As a counterpart, the last decade has seen an explosion in machine learning and work related. Has this also shifted a bit your interest or your work?

HS: [1:16:43] It has. I, myself, am in some sense going back to the early work that I did with my old friend and colleague, the bright scientist, the late Naftali Tishby[65] and Sebastian Seung[66]. The three of us met at the good old days of Bell Labs in the statistical mechanics of learning. Essentially, what we are trying to do is to reinvent the statistical mechanics of learning but for modern architectures and for more real-life problems.

I think this is the challenge that I see, the very important challenge today to bring physics back into the forefront of theory but in a landscape which has largely transformed. We cannot just talk about some random patterns and very simple architectures. We have to upgrade the statistical mechanics toolboxes to more complex architectures and richer data, richer statistics. Otherwise, it's less interesting. On the one hand, thinking about

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"Chaotic balanced state in a model of cortical circuits," *Neur. Comput.* **10**, 1321-1371 (1998). https://doi.org/10.1162/089976698300017214

[63] See, *e.g.*, R. Gütig and H. Sompolinsky, "The tempotron: a neuron that learns spike timing–based decisions," *Nat. Neurosc.* **9**, 420-428 (2006). https://doi.org/10.1038/nn1643

[64] Tempotron: https://en.wikipedia.org/wiki/Tempotron

[65] Naftali Tishby: https://en.wikipedia.org/wiki/Naftali_Tishby

[66] H. S. Seung, H. Sompolinsky and N. Tishby, "Statistical mechanics of learning from examples," *Phys. Rev. A* **45**, 6056 (1992). https://doi.org/10.1103/PhysRevA.45.6056

neural networks as a machine learning and AI, trying to advance the theory of those. Because you all know it's magic. Why they work? Why do you need that? All these questions which are still surrounding the field, we're trying to contribute to the theory of that. On the other hand, I truly believe that we have an opportunity, which we didn't have in the past, to understand better the brain, to have better models from brain circuits and brain information processing from the deep neural networks that are advanced primarily for machine learning and AI purposes. So, the field is coming back.

In the '90s and 2000s, it diverged largely and now it's coming back. For how long? I don't know. But for a while, we are benefiting from this meeting of interests and questions. I believe that we have a better understanding of vision as a whole. In the '90s, we were studying a local circuit in P1[67], in the primary visual cortex[68]. Now, we are thinking about the entire visual cortex. What does it do? How does it generate high-level perception? I really believe that we are now in a better situation to understand that. And we have already gained a better understanding from deep convolutional networks. I believe similar things will happen down the road. For instance, understanding language. If you'd asked me 10 years ago: "Haim, what about language in the brain?" I would say: "I don't want to hear it. It's so complicated. We don't know anything. There's no model. There is no neural model that actually is doing something like language. It's only about rules and symbols and grammar and syntax." I didn't know how to actually begin to think about a neural circuit that does language processing, generate language, understanding. Right now, I'm in the middle of it. These fantastic networks, are they like the brain or different from the brain? It's a big question. It's a question mark, but we have still something to put our teeth into. I believe now we are back into thinking about high cognitive functions, reasoning, language, thought, planning. All this is coming back, and it's coming back with a new set of tools, which is deep networks and AI. It's a fantastic time for the field. The field is rejuvenated.

PC: In this completely changed landscape you just described, is there still room for spin glass ideas? Or have they served their time in the '90s and then we have moved on?

HS: [1:21:41] First of all, I don't want to do any prediction, because in science—that's what's nice—things can come back and change.

[67] C1 and P1: https://en.wikipedia.org/wiki/C1_and_P1_(neuroscience)

[68] See, *e.g.*, R. Ben-Yishai, R. L. Bar-Or and H. Sompolinsky, "Theory of orientation tuning in visual cortex," *Proc. Nat. Acad. Sci. U.S.A.* **92**, 3844-3848 (1995). https://doi.org/10.1073/pnas.92.9.3844

PC: In your current work, then, to keep it immediate.

HS: [1:21:57] Look, spin glass is somewhat like chaos. At the beginning, it was all this nice, low-dimensional chaos phenomenon with fractal structure and beautiful algebra and renormalization group and all this. But if you ask what is chaos today? (There may be some people [who still do this]; I'm not saying nobody does it.) Chaos' contribution to science is much more than that. It contributed nonlinear dynamical systems, as a field, as an understanding. What is correlation and dimensionality and coarse-graining and so on? Very few systems really exhibit this chaos in this low-dimensional, non-random, non-stochastic sense, but chaos is the engine of non-linear dynamical systems. That's what it contributed deeply if you think about it.

I think spin glass is the same. Whether replica symmetry breaking will come back or even replica whether it is… First of all, replica is very important even today, all the time. Replica symmetry breaking is more exotic. But the whole field—replica, replica symmetry breaking, dynamical approaches**—**what spin glasses brought into physics and into science is the ability to think [about,] to analyze and to study complex systems, random systems, and disordered systems. That's what is the ultimate legacy. The realization that physics gives you ways and tools to think about those horribly dirty, heterogeneous complex systems. This is what spin glasses, I believe, contributed. This contribution is lasting, because there's no way to approach a complex circuit like a neural circuit in the brain without having the confidence that there are some intellectual, conceptual, and mathematical tools to deal with complexity.

PC: I'd like to loop back on a question we covered briefly earlier, which is the difference between European, Israeli, and American scientists on these questions. You have described the dichotomy. Do you have any ideas about what are the underlying cultural differences that led to this camping of the positions?

HS: [1:25:02] I don't know. I know some of my colleagues on both sides of the Atlantic—at that time at least—took it very seriously. As I said, I looked at it with more of a cool mind. I would say [that the] Italian and the French traditions in science—it's not new to you, I'm sure—tend to be more mathematical, more abstract, more formal, at least in some schools, and definitely in the field of spin glasses. I think Americans tended to be less formal and more down-to-earth—or more bottom-up—coming from experiments and building up models. I think there was some tension around that. The tension came up with the realization of one side of the Atlantic that their theory is not well received in that side and vice versa. I still don't think that these are important tensions to… They are different cultures, different styles.

That's what's nice about science. You shouldn't take [things] too seriously, shouldn't take it personally. Science is a big river or maybe rivers that flow with different paces, and different colors and different textures, and then they all join into the big sea of science. They contribute to the progress of human understanding of the world.

But that tension at that time was there. "You don't receive my papers in reviews and so on." At that time, some of this was reflecting on me. I have heard all kinds of stories. "Why is Haim Sompolinsky refusing all these papers?" I never was asked to review these papers! I don't know what they want from me. I was just doing my work. To this day, I am the least religious and dogmatic scientist, even in the field of the brain. In the brain also there are people who say: "This is the level. If you don't do this, you're not doing the brain. If you don't this…" Don't be dogmatic. We will make progress wherever we find room to make progress. The integration will be done later on. Yes, there are some tensions like that. I refuse to take it too seriously.

PC: At Bar-Ilan, at the Hebrew University or elsewhere, did ever you teach a class about spin glasses? Or teach about replica symmetry breaking?

HS: [1:28:37] Of course. At Bar-Ilan I taught. At the Hebrew University I taught for many years. Not every year, but every couple of years. I like to teach an advanced course in spin glasses and neural networks. I start from spin glasses, from the physical systems, where the ideas originated, from what experiments, from what questions, and then I go to neural networks. At Harvard, I plan to give a similar course—not this year but next year—also in the physics department. I love to teach spin glasses. I think it's very important for statistical mechanics researchers to know this field. Absolutely!

PC: Would you have started teaching this around 1984? Is that the first time you would have presented this material?

HS: [1:29:39] Probably already at Bar-Ilan, but definitely at the Hebrew University. In the mid-'80s, I was teaching about it. I remember not when, but Alessandro Treves[69] was a student [in it] and then was a TA in one of

[69] Alessandro Treves, *The onset of order in associative nets of neurons*, PhD Thesis, Hebrew University of Jerusalem (1989). https://huji-primo.hosted.exlibrisgroup.com/permalink/f/att40d/972HUJI_ALMA11276820160003701

these early times I taught it. Yes! It is topic that I have been teaching for many years, and I keep teaching it. Not every year, but periodically. I still think it is a foundational component of theoretical condensed matter, or statistical mechanics.

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

HS: [1:30:34] I would like to add about… We mentioned it, but maybe I haven't emphasized [enough] the dynamic aspect that came out of spin glasses. One of the contributions of spin glasses—I was fortunate to contribute to [it]—is the development of what is known now as dynamical mean-field theory. It largely originated from the early work on spin glasses, although of course it was built on path integral methods for dynamics which was developed for critical phenomena, like Martin-Siggia-Rose[70]. This is still one of the major tools that we, as a community, have to deal with complex systems and complex dynamics in neural circuits and neural networks in learning. Very often, we cannot translate it into Gibbs statistical mechanics problems. That happens all the time because of the nature of the problem. So, what do you do? I think that this, alongside the replica method, is a fantastic contribution from the early days of spin glasses. And it is a very powerful tool in many questions that I need, or that I have read in the literature other researchers are using. It's fascinating, but also challenging in the sense that I believe that we will face, and we will be attracted by dynamical questions not only of the kind of stationary equilibrium state, [but] also to understand transient dynamics, which has largely been ignored. We all like attractor dynamics and equilibrium dynamics, but I think to understand systems like neural circuits and other complex systems we will have to understand also the transient. There, there is room for developing further tools in the field. But I am hopeful that younger generations will pick up those challenges.

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?

HS: [1:33:33] I am afraid that I didn't keep them, unfortunately. One letter—I don't think I have it with me, but it's dear to me—is a letter that I received from Giorgio Parisi as a young postdoc at Harvard. One day comes a letter from the big, great scientist, Giorgio Parisi in his handwriting,

[70] P. C. Martin, E. D. Siggia and H. A. Rose, "Statistical Dynamics of Classical Systems," *Phys. Rev. A* **8**, 423 (1973). https://doi.org/10.1103/PhysRevA.8.423

congratulating me for the dynamics theory and asking me questions. Anyway, I don't think I have it with me, so I am afraid I'll have to disappoint you on that.

PC: If ever you do find them, I encourage you to consider saving them for posterity.

HS: [1:34:30] Thank you for this initiative. I think there will be an audience for understanding how fields evolve, how fields emerge, how different corners of science interact with each other and enrich each other.

PC: Thank you very much for your kind words and thank you very much for you time.

HS: [1:34:57] Thank you. Take care.