

# History of RSB Interview: J. Leo van Hemmen

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## Interviewers:

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## Location:

Over Zoom, from Prof. van Hemmen's home in Munich, Germany.

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**PC:** Good afternoon, Professor van Hemmen. Thank you very much for joining us. As we discussed ahead of time, the purpose of this interview is to go over the period during which replica symmetry breaking was formulated, which we bound roughly from 1975 to 1995. Beforehand, I'd like to ask a few questions on background. How did you first get interested in physics, and then what led you to pursue a PhD in mathematical physics?

**LvH:** [0:00:37] I got interested in physics in Dutch Grammar School<sup>1</sup>. Dutch Grammar School is, let's be honest, just for 2% of the kids. We had five foreign languages: English, French, German, and added to those Greek and Latin, which we should perfectly master on an academic level. During this time, when I passed to university, I had decided that...

Why study physics? I could answer that question already in those days through a single sentence: mathematically formulating physical reality. That's my goal. That's what I want. That's what I have wanted all my life and is still my goal: mathematically formulating physical reality. Of course, you can become philosophical and discuss what mathematically means and what reality means, but if you want a one-sentence description, it's this. That's why I voted for, or chose, physics. Of course, you could say: "You could have chosen something else as well." In 1965, I also considered mathematical economy, but mathematical economy wasn't as mathematical as physics yet. When I started, it had a 300 years tradition of mathematical formulation. It was far more challenging, though I took a serious look at mathematical economy. I hope this answers the question "why do physics"? Not to do experiments. I think doing experiments is a complete waste of

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<sup>1</sup> Categoriiaal Gymnasium of the *voorbereidend wetenschappelijk onderwijs* (preparatory scientific education): [https://en.wikipedia.org/wiki/Voorbereidend\\_wetenschappelijk\\_onderwijs](https://en.wikipedia.org/wiki/Voorbereidend_wetenschappelijk_onderwijs)

time as soon as you've seen that the interesting stuff is in its mathematical description. For me as theoretical physicist, doing experiments is wasting my time. Be that as it may. If you want to do or act differently, fine, go ahead. I don't care.

I do need the experimentalists. They're extremely important. If you become theorist, you should always have intensive contact with experimentalists who do the physics you are working on. Otherwise, you may develop a wonderful theory, but most probably good for nothing. At least, that's what I have found.

**PC:** How did you get to pursue a PhD in Groningen? What was your drive to pursue your studies there?

**LvH:** [0:03:22] That's very simple. I was born in the city of Groningen, so I'm a true *Groninger*. Groningen has an old university. It's the second oldest university in the Netherlands. Leiden was founded in 1575, and Groningen in 1614. For Americans, look for a university that was founded in 1614. The Sorbonne, of course, is older, but the University of Groningen is of a decent age. It's a good university and, in fact, mathematics and physics were at my time excellent. So why move to Amsterdam, if you have a very good university in your hometown?

**PC:** You then moved onto to a first postdoc near Paris, to work with David Ruelle<sup>2</sup>. What was the drive to go there and to work with him?

**LvH:** [0:04:30] The Institut des Hautes Études Scientifiques (IHES) in Bures-sur-Yvette. Lovely place. I was able to move to the IHES because my goal was to work with Joel Lebowitz<sup>3</sup>. Joel had a year off at the IHES. Indeed, I also interacted with Ruelle. He is absolutely great. I solved a few problems for him. But the goal of my stay was Joel Lebowitz. As you know, Joel was the center of the statistical-mechanical universe. So that fits.

**PC:** From there you moved to the US to take, I think, an assistant-professor position in Mathematics at Duke that lasted for only a year. Why?

**LvH:** [0:05:35] I was interested. First, it was an assistant professorship. That's nice for getting some experience. I have to admit, however, that my wife didn't like it at Duke. In the spring, when our first son was coming, she told me: "Leo, I see you like it here very much. I don't. If you want to stay, fine, but next year I'll be gone." What was I to do?

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<sup>2</sup> David Ruelle: [https://en.wikipedia.org/wiki/David\\_Ruelle](https://en.wikipedia.org/wiki/David_Ruelle)

<sup>3</sup> Joel Louis Lebowitz: [https://en.wikipedia.org/wiki/Joel\\_Lebowitz](https://en.wikipedia.org/wiki/Joel_Lebowitz)

But I was quite lucky. When I arrived at Duke, Richard Palmer also had just arrived<sup>4</sup>. We both started our first year as assistant professors, with all the ins and outs, at Duke. He in physics, a floor below me, and I in mathematics. We quickly got into contact. He explained to me what a spin glass was supposed to be. I immediately got interested, because I had a strong background in probability theory. There we go.

You may know that Richard and I have written meanwhile a rather well-known paper<sup>5</sup>. That was the upshot of our collaboration at Duke. That's why I immediately got involved quite heavily into spin glasses. Very simple.

**PC:** Before diving into spin glasses, what drove your problem selection up until that point? What led you to pursue certain questions?

**LvH:** [0:07:42] I would say accidents. If you arrive at a new place, meet an interesting person... Interesting is of course a subjective definition, and you may not find interest at all in what I'm saying, or find "Gosh, this guy is totally boring." But if you meet an interesting person, and get fascinated by the problem... Richard was really very excited about the spin-glass problem, and he could convince me to a large extent, immediately. So you are at a new place, you meet a new problem because you have the right background.

I've gotten my PhD in equilibrium statistical mechanics. After a while I picked the problem myself and proposed it to my first thesis advisor, Nico Hugenholtz, who immediately agreed: "Oh yeah, Leo, that's a very nice one", which is what I am still highly grateful for. My second thesis advisor was Erik Thomas<sup>6</sup>. They allowed me to complete my dissertation on dynamics and ergodicity of the *infinite* harmonic crystal<sup>7</sup>. That was a priori an infinite system. In those days, the a priori infinite system was very popular to study in equilibrium statistical mechanics. "You should take the system a priori infinite since taking all these thermodynamic limits is so boring. Just start with the infinite system. The idea looks nice, but constructing a dynamics of an infinite system is damn hard. Until now, it has hardly been done yet; except for lattice systems. You need some bounds, but the harmonic crystal was a nice example of an infinite dynamical system

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<sup>4</sup> Richard G. Palmer: [https://en.wikipedia.org/wiki/Richard\\_G.\\_Palmer](https://en.wikipedia.org/wiki/Richard_G._Palmer)

<sup>5</sup> J. L. van Hemmen and R. G. Palmer, "The replica method and solvable spin glass model," *J. Phys. A* **12**, 563-580 (1979). <https://doi.org/10.1088/0305-4470/12/4/016>

<sup>6</sup> Erik Gerard Frederik Thomas. See, e.g., T. Koornwinder *et al.* "In Memoriam Erik G.F. Thomas (1939–2011)—'A good definition is half the work'" *Nieuw Archief voor Wiskunde* **5/13**(4), 281-286 (2012). <http://www.nieuwarchief.nl/serie5/pdf/naw5-2012-13-4-281.pdf> (Consulted August 1, 2021.)

<sup>7</sup> J. L. van Hemmen, *Dynamics and ergodicity of the infinite harmonic crystal*, PhD Thesis, University of Groningen (1976). <https://rug.on.worldcat.org/v2/oclc/932441840> (Consulted August 1, 2021.)

that you could solve exactly.” That’s what I did: I’ve constructed a phase space, constructed a dynamics that must be based on the phase space, and only then can you indicate an equilibrium state. It’s a measure on the phase space, invariant under the dynamics. Then you can talk about ergodicity. Well, that’s what I did and could show that this system at the top of the ergodic hierarchy. You have ergodic, weakly mixing, strongly mixing, Kolmogorov, and Bernoulli. All this stuff is the ergodic hierarchy. I could show it’s a Bernoulli system, so it’s equivalent to playing cards, which is completely random if the cards have not been tricked, so it’s damned ergodic. That was the background, so that I was decently good in dynamics, and actually really interested in dynamics.

Then comes the spin glass system and equilibrium. It’s also, of course, a notion [that] for me —as one of the last representatives of the classical Dutch school of equilibrium statistical mechanics – looked very attractive. Meeting then Richard Palmer at Duke: “Let’s see what can be done!” The first question apparently was in those days a fascinating problem: characterize the transition to the spin glass state. What kind of transition is it? What state does the spin glass land into? This type of question fascinated me because of my background. I admit that equilibrium statistical mechanics was... I realized when I left Groningen that equilibrium statistical mechanics is over. We need to turn to something else, and then came Richard Palmer with spin glasses. That looked cute! It all fitted rather nicely. That’s why I started there, and have continued until I developed my own spin-glass model, but that’s presumably for later.

So the motivation? You have the right context, a colleague to talk to, Duke in those days was a very nice place. Let’s be honest. Your surroundings, where you work, are really important to make you creative for a decent period of time. Duke belonged to those places. At least, I experienced it that way.

What I hinted at is that my wife didn’t like it there. She loved it in Paris. She had finished her studies in French linguistics, and Paris in those days was the mecca of modern French linguistics, but at Duke it was just the opposite. It was one of the reasons... Students were reading French literature in English translation. That’s not quite what my wife was used to. That was an extra motivation to return to Europe. But be that as it may.

**PC:** You mentioned that first work with Richard Palmer and its genesis. What was the reaction to that work?

**LvH:** [0:13:41] First, I would say it was a pleasant surprise. There is an exactly soluble model. You do not need esoteric mathematics to solve it.

**PC:** Sorry. I meant your first paper with Richard about the SK model.

**LvH:** [0:14:09] That's the point. There was the replica method to solve the SK model, and people were wondering: "Why is the solution... It looks reasonable, but there must be something wrong. The entropy, as the temperature goes to zero, is negative. Oops! That shouldn't be. What is wrong there?" So I told Richard: "Look, we have to see what precisely is going wrong." Working on it, we discovered that the key notion that we needed there was the theory of convex functions. You have to know that when you do equilibrium statistical mechanics... What Sherrington and Kirkpatrick had obtained was a solution for the free-energy—that's the equilibrium statistical mechanics free energy—at any temperature on the basis of a really cute procedure, the replica method<sup>8</sup>. This is all equilibrium statistical mechanics. You get the free energy, then you differentiate it a few times, and you get the energy, the specific heat etc. The specific heat was linear at low temperatures, but the entropy was negative, which as we all know cannot be. How is that? That's the point.

The free energy is a convex function of the inverse temperature, beta (=  $1/k_B T$ ). We had to take the thermodynamic limit, and in the thermodynamic limit it was a hot discussion: Can you interchange that with the replica limit ( $n \rightarrow 0$ )? First you have the number of replicas, so the partition function raised to the power  $n$ , the number of replicas, and you can compute it. The average  $\langle Z^n \rangle$  of  $Z^n$  over the randomness, that's what you can compute with  $n$  integer, but not, of course, for  $n$  non-integer. For each positive integer, you have an explicit expression, and you take the evident extension to a neighborhood of  $n = 0$ , and then you differentiate  $\log \langle Z^n \rangle$  w.r.t.  $n$  at  $n = 0$ . That should give the free energy  $\langle \log Z \rangle$ . Differentiate it with respect to  $n$  at  $n = 0$  and you get the average  $\langle \log Z \rangle$  of  $\log Z$ . Cute idea.

Why did it work? Applying the theory of convex functions, we saw the following. We are going to take the thermodynamic limit ( $N \rightarrow \infty$ ). So first if you take the thermodynamic limit of  $(1/N) \log Z(N)$ , where  $N$  is the system size, that's what you cannot compute for the SK model. But  $Z^n$  you can for  $n$  positive integer and then average over the randomness. That's the point. You average it over the randomness and then you differentiate with respect to  $n$ , which is the replica variable. This works nicely, but we have to interchange [this limit] with the thermodynamic limit.

The second problem is the following. We know  $Z^n$  for  $n = 0$ . Clearly,  $Z^n$  for  $n = 0$  is 1, which everybody can compute. Then for  $n = 1, 2, 3, \dots$  That's nice. But we then extend it. We take the evident extension as a function of the real variable to  $n = 0$ . Is this extension unique? We could boil down the

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<sup>8</sup> D. Sherrington and S. Kirkpatrick, "Solvable Model of a Spin-Glass," *Phys. Rev. Lett.* **35**, 1792 (1975).  
<https://doi.org/10.1103/PhysRevLett.35.1792>

question as to why the result of the replica symmetric solution was wrong. Because interchanging the thermodynamic limit and differentiation in the neighborhood of  $n = 0$ , that's okay by the theory of convex functions. If the extension of  $\log \langle Z^n \rangle$  were unique, then  $1/N$  and taking the limit of  $N^{-1} \log \langle Z^n \rangle$ —doing it all correctly, that's the technical stuff—then the answer must be right. Since the entropy is negative, the extension must be—that was our conclusion—the wrong one. And lo and behold, a year later Parisi came up with replica symmetry breaking<sup>9</sup>, which is, if I may say so, inspired guesswork. That gave for the entropy something positive. It all looked ok. We now know, after a thick book with highly complicated hard mathematics, that you can mathematically prove it<sup>10</sup>. You do not need the *method* of inspired guesswork. As we all know, the most efficient way of solving a problem is by means of the method of inspired guesswork. The only trouble is it's not a method. That's our bad luck.

**PC:** Before Parisi, there were a couple of proposals for breaking the replica symmetry. What was your impression of those? Were you following that discussion at the time?

**LvH:** [0:19:54] Well, if you have proven that the problem is in the extension from positive integer  $n$  of  $\langle Z^n \rangle$  and then  $1/N \log \langle Z^n \rangle$ , where  $Z$  is the partition function of the system of size  $N$ , you take the limit of  $N \rightarrow \infty$ , you can prove—that's what we did—the thermodynamic limit is well defined. Then, if you have also shown that the problem is in the extension, the only solution is: if you want to know the solution you must find the right extension. Why then bother with any other method? These didn't work either. It's a waste of time. I'm decently efficient, and that's why I didn't want to waste my time on that. Sorry folks, that's it.

**PC:** When you moved to Heidelberg, you joined with the Sonderforschungsbereich 123, *Stochastische Mathematische Modelle* (SFB 123)<sup>11</sup>, the mathematical stochastic modelling group. What was this collaboration and in what way was it a natural fit for you?

**LvH:** The SFB—that's the official German name. I think they now call it a collaborative research center (CRC) but it's a bad translation of Sonderforschungsbereich—is a special research project. I must say, it's a

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<sup>9</sup> G. Parisi, "Infinite Number of Order Parameters for Spin-Glasses," *Phys. Rev. Lett.* **43**, 1754 (1979).  
<https://doi.org/10.1103/PhysRevLett.43.1754>

<sup>10</sup> M. Talagrand, *Spin Glasses: A Challenge for Mathematicians* (Berlin: Springer, 2003).

<sup>11</sup> Nowadays, Collaborative Research Center:  
[https://en.wikipedia.org/wiki/Collaborative\\_Research\\_Centres](https://en.wikipedia.org/wiki/Collaborative_Research_Centres) ; CRC 123: Stochastic mathematical models

really brilliant German idea that has hardly been followed abroad<sup>12</sup>. The idea is that you collect a group of people who work on a single unifying topic. The topic should be well chosen. That's the art of making a good SFB. On the one hand, the number of people who can work on this topic is large enough so that they can complement each other, and on the other end it is small enough so that these people can also talk to each other and exchange ideas. You have to choose this group. The topic, in particular, is essential. You have to choose it well. The SFB in Heidelberg, with the nicest number the German National Science Foundation could ever give, 123—that's what I'm still proud of—was highly successful. It existed for the maximum number of years. Five times three. After three years it was re-evaluated and the maximal number of times it can be renewed was four, so that the total period of existence was 15 years. It existed for the full 15 years, which tells you that the topic was well chosen. It was on the one hand special enough, and on the other hand general enough. Probability played an essential role in SFB 123. That's why I found it so attractive, because it was just right to me.

You need to know something that was in those days typical to the German academic system. If you ever wanted to get a professorship, then you had to pass—so to speak—an admission exam. That was the habilitation<sup>13</sup>—which you also have in Russia—that is a second PhD so to speak. In France, it is the *thèse de doctorat d'état*, which you get after your first PhD. Typically in my days a habilitation lasted six years. Can you still get the latter in France?

**PC:** It's now called *habilitation à diriger des recherches* in France.

**LvH:** Sorry, I was laughing. Habilitation! That fits nicely. In Germany of the old days, it was the habilitation and it was the admission exam for entering the professorship; the necessary but not sufficient condition to get a professorship. I started in Heidelberg with theory of maser physics<sup>14</sup>, but I quickly got the freedom to work again on spin glasses. In fact, after three more years, in 1984—the SFB was re-evaluated with my spin glass model<sup>15</sup> published in 1983—they gave me ample of time to work on my own topics.

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<sup>12</sup> See, e.g., "50 Jahre Sonderforschungsbereiche," *Forschung*, (3) 2-10 (2013).  
[https://www.dfg.de/dfg\\_profil/gesamtliste\\_publicationen/forschung\\_german\\_research/index.html](https://www.dfg.de/dfg_profil/gesamtliste_publicationen/forschung_german_research/index.html)  
(Consulted August 1, 2021)

<sup>13</sup> Habilitation: <https://en.wikipedia.org/wiki/Habilitation>

<sup>14</sup> See, e.g., J. L. van Hemmen and W. von Waldenfels, "On the dynamical structure of the Dicke maser model," *Physica A* **100**, 85-99 (1980). [https://doi.org/10.1016/0378-4371\(80\)90151-X](https://doi.org/10.1016/0378-4371(80)90151-X)

<sup>15</sup> J. L. van Hemmen, "Classical Spin-Glass Model," *Phys. Rev. Lett.* **49**, 409-413 (1982).  
<https://doi.org/10.1103/PhysRevLett.49.409>

Once in Heidelberg, or in Germany, you are *Privat-Dozent*<sup>16</sup> after your habilitation. You are not paid for that, but to keep the title you must give a lecture or seminar during each term. Unpaid for. You just give it for free. Then you can keep your title, and you need that if you want to become a professor. You did need that. Otherwise no way. In this way, you also got ample experience with teaching.

Of course, you can say: “At Duke, you have already had extensive and rather hard training in giving good lectures, because those who were not good were thrown out rather quickly.” To comfort you, mathematics was ok. For each term I had two courses that were identical, which was the usual regulation. In physics, in those days, each term you had to give two different new courses. Two different new courses... You came up for tenure after five or six years, two different new courses per semester. That was dead hard. Horrible. That was just horrible. But be that as it may.

I would say that the setup in Heidelberg was such that I could get my habitation with my spin glass model<sup>17</sup>. I also solved another hard-boiled problem but that's not for now. I hope this answers the question.

I had the great luck of getting involved in the SFB 123, whose chair—that's also important to say—was an excellent scientist. He was an applied mathematician, Willi Jäger<sup>18</sup>. Willi Jäger was a very good organizer with really bright ideas about what to do and how to do it. My direct boss belonged to the old German nobility, Wilhelm Freiherr von Waldenfels<sup>19</sup>, a very nice person, who gave me a lot of freedom. I still greatly respect that he gave me so much freedom, because spin glass was not—as you would say in Munich—his beer. Nevertheless, [what] I produced had good quality so that I could continue this way. That's also due to Willi Jäger and the SFB. It was a really good place. Fantastic! Franz Wegner was the physicist in those days who did wonderful work on renormalization group theory<sup>20</sup>. All in all, just a great place.

**PC:** Were you recruited to be part of that effort? You were not one of the founding members, right?

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<sup>16</sup> Privat-Dozent: <https://en.wikipedia.org/wiki/Privatdozent>

<sup>17</sup> Ref. 14 and the detailed publication J. L. van Hemmen, A. C. D. van Enter and J. Canisius, "On a classical spin glass model," *Z. Phys. B* **50**, 311-336 (1983). <https://doi.org/10.1007/BF01470043>

<sup>18</sup> Willi Jäger: [https://en.wikipedia.org/wiki/Willi\\_J%C3%A4ger](https://en.wikipedia.org/wiki/Willi_J%C3%A4ger)

<sup>19</sup> Wilhelm Freiherr von Waldenfels: [https://de.wikipedia.org/wiki/Wilhelm\\_Freiherr\\_von\\_Waldenfels](https://de.wikipedia.org/wiki/Wilhelm_Freiherr_von_Waldenfels)

<sup>20</sup> Franz Wegner: [https://en.wikipedia.org/wiki/Franz\\_Wegner](https://en.wikipedia.org/wiki/Franz_Wegner)

**LvH:** [0:28:38] No. I arrived there at the first date when the SFB started. It was September 1<sup>st</sup>, 1978. In a sense, I was lucky and I could stay there for 10 years altogether.

**PC:** A couple of years later you started collaborating with Ingo Morgenstern<sup>21</sup>, who was also at Heidelberg while you were there. I think this was the first time you were working on numerical simulations<sup>22</sup>. What led you to be interested in this problem?

**LvH:** [0:29:16] We were far more efficient than you suggested. Ingo did the numerical simulations while I did the formulation of the mathematical theory.

In those days, together with my first assistant in Heidelberg... That's the German setup. Once I had obtained my Habilitation, which was in 1983, I got my first full position as a postdoc. Together with my Dutch postdoc, Aernout van Enter, I did my theoretical physics of spin glasses<sup>23</sup>.

That was also in the days when I made the write up for my spin glass model. Imagine how boring it must have been for my graduate student<sup>24</sup> and Aernout, as my first assistant<sup>25</sup>. Each morning, Leo was telling them how he thought that one should develop the full mathematical theory for his spin glass model. It must have been a hard time for them. Of course, I had to work damn hard myself to fill out all the details, but that was fun. We did the mathematics and thought also about what equilibrium statistical mechanics could mean in the spin-glass context. This was, in those days, by no means evident. Of course, you can then ask: (a) what is equilibrium, and (b) what does it mean for a spin glass.

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<sup>21</sup> Ingo Morgenstern (1953-2020). See, e.g., Patrick Charbonneau, *History of RSB Interview: Kurt Binder*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 20 p. <https://doi.org/10.34847/nkl.5f2b685y>; "Im Gedenken an Prof. Dr. Ingo Morgenstern," Universität Regensburg (January 15, 2021), <https://www.uni-regensburg.de/universitaet/nachrufe/2020/prof-morgenstern/index.html> (Consulted August 1, 2021)

<sup>22</sup> J. L. van Hemmen and I. Morgenstern, "On sampling of Monte Carlo data and disordered systems," *J. Phys. C* **15**, 4353 (1982). <https://doi.org/10.1088/0022-3719/15/20/019>

<sup>23</sup> A. C. D. van Enter and J. L. van Hemmen, "The thermodynamic limit for long-range random systems," *J. Stat. Phys.* **32**, 141-152 (1983). <https://doi.org/10.1007/BF01009426>; "Statistical-mechanical formalism for spin-glasses," *Phys. Rev. A* **29**, 355 (1984). <https://doi.org/10.1103/PhysRevA.29.355>

<sup>24</sup> Joachim Canisius, *Low-temperature excitations in disordered systems*, PhD thesis, Heidelberg (1986). <https://katalog.ub.uni-heidelberg.de/titel/2537444> (Consulted August 2, 2021)

<sup>25</sup> See, for instance, Refs. 17 and 23.

Ingo collaborated in later days with Andrew Ogielski<sup>26</sup>. He was allowed to leave for the US. He then went to Bell Labs and worked with Andrew, whereas I, after I had gotten my habilitation and was *Privat-Dozent*, happily stayed in Heidelberg. (I visited Ingo at Bell labs. That was fun. We'll come back to him in a minute.) He did the numerics. Let's be honest, the large-scale numerical simulations that he did ~~with~~ together with Andrew Ogielski were in those days huge numerical simulations<sup>27</sup>. More or less, the upper limit of what you could do. That's what Ingo did with Andrew. They could show that the three-dimensional Ising ( $\pm J$ ) spin glass with nearest-neighbor interactions performed an equilibrium phase transition at a critical temperature  $T_c$  and had only two equilibrium states connected by spin-flip symmetry below  $T_c$ . As for us, the whole project was a collaboration. Ingo did the numerical simulation. Aernout and I myself did the mathematically founded theoretical physics. It was Ingo's conclusion that what his three-dimensional Ising spin glass performs is an equilibrium phase transition at a critical temperature  $T_c$ . What happens below  $T_c$ , that was something completely different. It was all Monte Carlo, but the spin glass had an equilibrium phase transition, different from what happens below  $T_c$ . My spin glass model predicted for a spin glass with an Ising anisotropy only two phases connected by up-and-down symmetry. That fits as that's what Andrew and Ingo confirmed numerically in three dimensions. What happens below  $T_c$  is still an ongoing discussion on how to characterize the spin glass phase below  $T_c$ . And what they could show was also supported by more analytical work of Bray and Moore, which was presented three years after the *Heidelberg Colloquium on Spin Glasses* at the *Heidelberg Colloquium on Glassy Dynamics* in 1986<sup>28</sup>.

(I had the fun of editing both proceedings together with Ingo. I did most of the editing and frightened the authors; Ingo didn't like that. As I had spent some time in the US, I knew how to do it. I would say the Lecture Notes in Physics that run under the title of *Heidelberg Colloquium on Spin Glasses*,

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<sup>26</sup> A. T. Ogielski and I. Morgenstern, "Critical behavior of three-dimensional Ising spin-glass model," *Phys. Rev. Lett.* **54**, 928 (1985). <https://doi.org/10.1103/PhysRevLett.54.928>; "Critical behavior of three-dimensional Ising model of spin glass," *J. Appl. Phys.* **57**, 3382-3385 (1985). <https://doi.org/10.1063/1.335103>

<sup>27</sup> See, e.g., P. Charbonneau, *History of RSB Interview: Andrew T. Ogielski*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École Normale Supérieure, Paris, 2021, 21 pp. <https://doi.org/XXXXXXX>

<sup>28</sup> Heidelberg Colloquium on Spin Glasses, University of Heidelberg, 30 May—3 June, 1983. *Heidelberg Colloquium on Spin Glasses*, J. L. van Hemmen and I. Morgenstern eds. (Berlin: Springer, 1983). <https://doi.org/10.1007/3-540-12872-7>; Heidelberg Colloquium on Glassy Dynamics, University of Heidelberg, 9—13 June, 1986. *Heidelberg Colloquium on Glassy Dynamics*, J. L. van Hemmen and I. Morgenstern eds. (Berlin: Springer, 1987). <https://doi.org/10.1007/bfb0057505>

was a book that was sold out after a few years, which as you may know does not happen to all the Lecture Notes in Physics.)

**PC:** I wanted to get back to this meeting in a few questions. First you mentioned your collaboration with your graduate student and your postdoc on a newly proposed model for spin glasses<sup>29</sup>, in which replica were not necessary. What was the drive to formulate and to study such a model at that point?

**LvH:** [0:34:58] First, to cook up an exactly soluble model that in one way or another you could solve mathematically straightforwardly. We used, in those days, a new technique, that of large deviations. Because of the SFB, I had the great luck—it must have been the winter of '81 or '82—to have a visitor, Richard Ellis<sup>30</sup>, who gave a series of lectures on large deviations. Also a colleague of mine, who became a *Privat-Dozent* in mathematics, was really very interested in the theory of large deviations. When I saw that, I thought: “Wow! That’s interesting. I would like to know and understand that.” The SFB provided me just right context to learn and to see what you can do with large deviations. Then, I realized that model...

First thing, you get an idea. The Mattis model<sup>31</sup>, with  $J_{ij} \propto \xi_i \xi_j$ , doesn't work because... Here, I must say there was in those days a strong interaction between the spin-glass aficionados.

G erard Toulouse had taught us that for spin glasses one of the ingredients is frustration<sup>32</sup>. Without frustration it's no good. The Sherrington-Kirkpatrick model had a lot of frustration, but so had mine. That was good. The big advantage is that I could solve mine straightforwardly without using the replica method. There's no need for discussion.

Afterwards came the confirmation by Ingo, but during the [1983] Heidelberg Colloquium Spin Glasses we still had hard discussions as to whether there should be any transition, and, if so, whether or not it should

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<sup>29</sup> Choy and Sherrington dubbed it the *van Hemmen model* in 1984, and the name has since persisted. See, e.g., T. C. Choy and D. Sherrington, “The van Hemmen model—a true spin glass?” *J. Phys. C* **17** 739-745 (1984). <https://doi.org/10.1088/0022-3719/17/4/015> and J. L. van Hemmen, “What is a true mean-field spin glass?” *J. Phys. C* **19**, L379-L382 (1986). <https://doi.org/10.1088/0022-3719/19/18/003>

<sup>30</sup> Richard S. Ellis: [https://en.wikipedia.org/wiki/Richard\\_S.\\_Ellis](https://en.wikipedia.org/wiki/Richard_S._Ellis) See, e.g., Richard S. Ellis, *Entropy, Large Deviations, and Statistical Mechanics* (Berlin: Springer-Verlag, 1985). <https://doi.org/10.1007/3-540-29060-5>

<sup>31</sup> D. C. Mattis, “Solvable spin systems with random interactions,” *Phys. Lett. A* **56**, 421-422 (1976). [https://doi.org/10.1016/0375-9601\(76\)90396-0](https://doi.org/10.1016/0375-9601(76)90396-0)

<sup>32</sup> G. Toulouse, “Theory of the frustration effect in spin glasses: I,” *Comm. Phys.* **2**, 115-119 (1977). Only two volumes of *Communications on Physics* ever appeared. The second article in the series appeared in a different journal. J. Vannimenus and G. Toulouse, “Theory of the frustration effect. II. Ising spins on a square lattice,” *J. Phys. C* **10** L537 (1977). <https://doi.org/10.1088/0022-3719/10/18/008>

be an equilibrium transition. Bray and Moore<sup>33</sup> said: “Yeah! It's quite possible that there's an equilibrium transition.” But what happens below the critical temperature they didn't know either. Anyway, that was, so to speak, in the air. What I wanted was having an exactly solvable model. Ingo came and said: “It fits, because I only get two pure states, connected by spin-flip symmetry.” That's what you can also clearly see in the PRL of Ogielski and Morgenstern<sup>34</sup>. That fits. It was a collective undertaking what I did.

My assistant, Aernout van Enter, and me were also thinking about interesting equilibrium statistical mechanics models with no connection to spin glasses, but containing randomness, long-range interactions<sup>35</sup>. How can you solve that? There's a whole bunch of interesting questions, problems, and models that you can study.

**PC:** Were you then aware of Bernard Derrida's work on the random energy model, which had similar objectives<sup>36</sup>?

**LvH:** [0:38:38] Yes, but Bernard Derrida's model is from my point of view quite different. I have been inspired strongly by John Mydosh<sup>37</sup>. Already in the early '80s I visited John at Leiden. You need to realize that I was born and grew up in Groningen, so each summer when my wife, me and our kids came to the Netherlands –the kids went to our parents—then I had the opportunity to stay there for a few weeks and could visit Leiden. I have had strong interactions with John Mydosh. The metallic spin glass of the copper-manganese type, or iron-gold was his focus. They are random-site and so is my own spin-glass model.

You also need to know my real background is actually not in statistical mechanics, but in solid-state theory, as it was called in those days. (Of course, you can say condensed matter theory.) In fact, I've spent my master's student time in the solid-state physics lab in Groningen<sup>38</sup>. (Nice

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<sup>33</sup> 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>

<sup>34</sup> See Ref. 26.

<sup>35</sup> See Ref. 23.

<sup>36</sup> See, e.g., 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>

<sup>37</sup> See, e.g., P. Charbonneau, *History of RSB Interview: John Mydosh*, 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.e1e3ob87>

<sup>38</sup> See, e.g., J. L. van Hemmen, W. J. Caspers, S. B. van der Molen, W. van der Lugt and H. P. van de Braak, “Analysis of the Knight shift for liquid alkali alloys,” *Z. Physik* **222**, 253-260 (1969). <https://doi.org/10.1007/BF01392124> ; J. L. van Hemmen, S. B. van der Molen, W. van der Lugt, “The Knight shift in liquid ternary and binary alkali alloys,” *Phil. Mag.* **29**, 493-511 (1974).

old building from 1901<sup>39</sup>; just the classical stuff, wonderful place.) That was my background. Then spin glasses agreed very well with my original background in solid-state physics. You see, it's not only statistical mechanics as I came from solid-state. Then, at Duke, Richard Palmer brought up again solid-state physics. "Hmm! That tastes good." Life is that simple.

You see a consistent picture arising. The interaction with Richard continued for a few years. Also, due to Richard, I came into contact with the Hopfield model many years later<sup>40</sup>. (Hopfield was '82.) That was also due to Richard. That put me onto another track after I had gotten my habilitation and finished my spin-glass model. I think, after a few years, you should start with something new.

**PC:** We'll get to that, but I first wanted to loop back to the two workshops you organized with Ingo Morgenstern<sup>41</sup>. How did these come about? What was the idea? They were very successful, as you mentioned, but was it obvious that these workshops needed to take place?

**LvH:** [0:41:49] They were the idea of Ingo Morgenstern and myself. Because we were in an SFB, we had money. Otherwise, forget it. The SFB was willing to invest money in well-organized meetings that were devoted to a fascinating topic. Thanks to its chair, Willi Jäger, who had an open eye for giving younger people the chance to present new fields of science they were active in.

You have to work hard to get a good meeting. If you have ever organized a successful meeting, you know why. The best-organized meeting is so well organized that you do not notice its organization, but it's there. You have the idea that things all run smoothly, but they don't. Due to the SFB, we had that opportunity. Of course, picking the right people was the job that Ingo and I did. Let's be honest, Ingo and me, we both have identified people and

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<https://doi.org/10.1080/14786437408213234> J. L. van Hemmen, *Dynamics and Ergodicity of the infinite harmonic crystal*, Ph.D. thesis, University of Groningen (1976); published as *Phys. Rep.* **65**, 43-149 (1980).  
[https://doi.org/10.1016/0370-1573\(80\)90095-2](https://doi.org/10.1016/0370-1573(80)90095-2)

<sup>39</sup> The Groningen Mineral Geology Laboratory at the Melkweg opened in 1901; after the Second World War the building also housed the Solid-State Physics Laboratory.

<https://www.rug.nl/museum/history/university-of-groningen/1876-present> (Consulted September 21, 2021)

<sup>40</sup> See, e.g., 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>

<sup>41</sup> See Ref. **Error! Bookmark not defined.** and Colloquium on Spin Glasses, Optimization and Neural Networks, University of Heidelberg, June 9-13, 1986. *Heidelberg Colloquium on Glassy Dynamics* J. L. van Hemmen and I. Morgenstern eds. (Berlin: Springer-Verlag, 1987), 121-153.

<https://doi.org/10.1007/BFb0057505>

invited them to Heidelberg to start together with the *Heidelberg Colloquium on Spin Glasses*.

**PC:** And its proceedings were very influential as you described. At about the same time, but with a different motivation, there is a community in mathematical physics who started to demonstrate the Parisi ansatz, such as in the works of Aizenman *et al.*<sup>42</sup>, and later Pastur and Shcherbina<sup>43</sup>, and then Guerra<sup>44</sup>. Were you following that work? Were you interested in it?

**LvH:** [0:43:50] Two remarks. You quickly skipped making proceedings. If you do it well, it is by no means evident that you will get good proceedings. I have actually done the editing work of the *Heidelberg Colloquium on Spin Glasses*. It was damned hard work. You have to go through the manuscripts, and in those days, you then xerox them. Many authors have gotten them back from me, nicely colored with red. They had to change that. Realizing that my English and my remarks were correct, they did so. You can take a critical look at the *Heidelberg Colloquium on Spin Glasses*, and you will see that it's well edited. Nice topics, well ordered, well edited. We invited speakers together with a topic, not to talk on anything but on an interesting topic, of course, fit to them. That's the purpose of the exercise.

Two, I followed all these developments, but at a distance. It was about 1985 that I decided I've contributed my part, my chunk, to the spin glass problem. In hindsight, it was not dead wrong, because for the next 20-30 years not that much happened, at least not from my point of view. Because a huge amount of time has been spent on the Sherrington-Kirkpatrick model. David Sherrington is a good friend of mine<sup>45</sup>. I like him very much. But with all due respect, I did not want to spend my time on SK-tology, as many of these considerations were too lofty for me. I decided to turn to something else, if it's interesting. I'll tell you in a minute that spin glasses did contain something quite interesting, but seen from a different point of view. As for the spin glass transition problem, I quit.

By accident, this was also due to John Mydosh, who turned to the low-temperature behavior of spin glasses. The low-temperature behavior of spin glasses means that tunneling should become important. If you have all these

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<sup>42</sup> M. Aizenman, J. L. Lebowitz and D. Ruelle, "Some rigorous results on the Sherrington-Kirkpatrick spin glass model," *Comm. Math. Phys.* **112**, 3-20 (1987). <https://doi.org/10.1007/BF01217677>

<sup>43</sup> L. A. Pastur and M. V. Shcherbina, "Absence of self-averaging of the order parameter in the Sherrington-Kirkpatrick model," *J. Stat. Phys.* **62**, 1-19 (1991). <https://doi.org/10.1007/BF01020856>

<sup>44</sup> F. Guerra, "Fluctuations and thermodynamic variables in mean field spin glass models," in: *Stochastic processes, physics and geometry II*, eds. S. Albeverio, U. Cattaneo and D. Merlini (Singapore: World Scientific, 1995). (see also arXiv:1212.2905).

<sup>45</sup> See, e.g., P. Charbonneau, *History of RSB Interview: David Sherrington*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 39 p. <https://doi.org/10.34847/nkl.072dc5a6>

spin glasses, they are with a magnetic moment. The spin quantum number  $S$  is four or five, or so. The spins are usually in an anisotropic environment. How can a spin then tunnel through an anisotropy? I thought I could find the problem in a thick book written by a brilliant Russian physicist<sup>46</sup>, and it would all be over. But it turned out there was no brilliant Russian physicist that had solved the problem. There was nothing. So together with András Sütő<sup>47</sup> from the Hungarian Academy of Sciences, we solved the problem of, in particular, WKB for quantum spins<sup>48</sup>. Only then could we answer the question of how does a spin glass behave when you have anisotropy, and how are spins going to tunnel? How do they tunnel? That's the first question, and you can then talk about the rest. If it's all frozen, then tunneling remains. That's how I came to tunneling of quantum spins. Through the spin-glass problem. Spin-glass behavior at low temperatures. That took me two years or so together with András. The Iron Curtain was still there, so we were spending our time at the Balaton during the summer<sup>49</sup>, trying out each night another bottle of Hungarian white wine. I discovered that Hungarian white wines were not bad at all. After two weeks, we had tried out many Hungarian wines, and I can only say they were good. András had good taste. That's a nice side effect. That's how I got into the tunneling of quantum spins, and left the endless discussion of what the spin-glass transition really means as blabla.

**PC:** Shortly afterwards, you moved to the study of neural networks. What led you to that research direction? You mentioned Richard Palmer's influence in making you aware of the Hopfield model, but was there something more immediate as well?

**LvH:** [0:49:49] Yes and no. There's a very nice combination of yes and no in German, *yein*<sup>50</sup>, which is something in between. Together with András I had published our WKB paper about the tunneling of quantum spins in full-blown generality, with a summary in Europhysics Letters volume one. It was a good volume. There were many more good papers. It was fun, but it

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<sup>46</sup> Say, L. Landau and E. Lifshitz, *Course of Theoretical Physics* [https://en.wikipedia.org/wiki/Course\\_of\\_Theoretical\\_Physics](https://en.wikipedia.org/wiki/Course_of_Theoretical_Physics), or A. S. Davydov, *Quantum Mechanics* (Oxford: Pergamon, 1965).

<sup>47</sup> Sütő András: [https://hu.wikipedia.org/wiki/S%C3%B3t%C5%91\\_Andr%C3%A1s\\_\(egy%C3%A9rtelm%C5%B1s%C3%ADt%C5%91\\_lap\)](https://hu.wikipedia.org/wiki/S%C3%B3t%C5%91_Andr%C3%A1s_(egy%C3%A9rtelm%C5%B1s%C3%ADt%C5%91_lap))

<sup>48</sup> J. L. van Hemmen and A. Sütő, "Tunnelling of Quantum Spins," *Europhys. Lett.* **1**, 481 (1986). <https://doi.org/10.1209/0295-5075/1/10/001>; "Tunneling of quantum spins," *Physica B+C* **141**, 39-75 (1986). [https://doi.org/10.1016/0378-4363\(86\)90347-5](https://doi.org/10.1016/0378-4363(86)90347-5)

<sup>49</sup> **LvH:** This was officially at the Hungarian Academy of Sciences Institute in Budapest. If in Budapest (during summer time), I stayed with András's family, and we did our research at an old family living near the Balaton.

<sup>50</sup> *Ja & nein = Yein ; -)*

also contains the summary of our telling how quantum spins tunnel in full generality. You can't imagine, but it's all there.

When you think about the Hopfield model, and then start thinking: "Hey! How does the brain do it? Is that a Hopfield model? How does it work? A Hopfield model provides a memory, but how does that work in reality?" That's how I then moved to "How does that work in reality?" to looking at real neuronal systems. A neural system is for me an artificial thing. By the end of the '80s, neural networks were already highly popular. Decently efficient, but the computers weren't good enough yet. But neuronal systems, how does nature do it? That's something different.

In 1990, I got my appointment as the chair of theoretical biophysics (of neuronal information processing) at the Technical University of Munich. Afterwards, I could fully concentrate on that. It was a flowing transition from spin glasses, tunneling of quantum spins, then asking: "How does that work in the brain?"

The Hopfield model is quite nice, but the brain no doubt must behave differently. How then does it work? Instead of jumping fully into memory, as I'm a dynamically interested person, I first wanted to know how neuronal dynamics works. Then, you have to answer a completely different type of question. Dynamics is what I'm really interested in, and it's also why I didn't want to spend the rest of my life in equilibrium statistical mechanics and studying endless cases of renormalization group theory. There's a citation of Frederick the Great: "Everybody should go to Heaven in his or her own way.<sup>51</sup>" Then let me do it in my way. I wish you the same. Be happy in your own way.

**PC:** As part of this transition, did you attend the workshop in Jerusalem, at the Institute for Advanced Studies<sup>52</sup>, or the one in Santa Barbara, at the Institute of Theoretical Physics<sup>53</sup>? Were you involved in those discussions?

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<sup>51</sup> "Die Religionen müssen alle toleriert werden [...], denn hier muss ein jeder nach Seiner Fassung Selig werden." (All religions must be tolerated [...], because here everyone must be blessed according to his own style.) See, e.g., Frederick II, "Immediat-Bericht des Geistlichen Departements. Berlin 1740 Mai 22," in: *Preussen und die katholische Kirche seit 1640. Vol. 2*, Max Lehmann ed. (Leipzig: Hirzel, 1881). <https://archive.org/details/preussenunddiek06grangoog/page/n25/mode/2up> (Consulted August 4, 2021.) Frederick II (the Great) of Prussia was a religious pragmatic, and hence tolerated all faiths in his realm. See, e.g., [https://en.wikipedia.org/wiki/Frederick\\_the\\_Great#Religious\\_policies](https://en.wikipedia.org/wiki/Frederick_the_Great#Religious_policies)

<sup>52</sup> In 1987. See, e.g., P. Charbonneau, *History of RSB Interview: Hanoach 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>

<sup>53</sup> John Hopfield and Peter Young, "Spin Glasses, Computation, and Neural Networks" September to December 1986, Institute for Theoretical Physics, University of California at Santa Barbara.

- LvH:** [0:53:22] At Santa Barbara, yes. To my great regret, in those days, I was not invited to Jerusalem. For a Dutchman this was a bit hard to accept, but that's it. Santa Barbara, yes. Richard Palmer, Dan Stein and I were all there in '86. That was fun.
- PC:** Is that what led to the genesis of the paper with Palmer and Stein in 1989<sup>54</sup>?
- LvH:** [0:53:57] Yes. That was a consequence of working together in Santa Barbara. It would have been nice to be in PRL. In those days PRL was (and still is) pretty good but peer reviewing is not always completely impartial. It's a well cited Physics Letter A. It dates back to those days, yes.
- PC:** You left this world of spin glasses, but did you pay attention to what was happening in that field, or did you cut links more or less?
- LvH:** [0:54:53] I kept abreast of what was going on there. Don't forget that I have attended (practically) all the American Physical Society March meetings, since I got appointed in Munich, until Covid-19 last year and this year. Last year was cancelled. By accident I had decided not to go. Thank heavens! Imagine you are there and then you are told: "Ah! Ah! Nice you are there, but you won't do it." That was a shame. Very bad style of the APS. But otherwise I've always been there. That more or less enabled me to talk to people, to see what's going on there, listen to interesting talks, and then take a look every now and then. So I followed it, but not intensely because I've been decently productive also in the theory of neuronal networks: Spike-timing-dependent plasticity (STDP)<sup>55</sup> is meanwhile a textbook notion and it stems from my chair.
- FZ:** You have a few papers on the problem of unlearning<sup>56</sup>, which is something that was following up on prior work by John Hopfield. Why did you become interested in that specific problem?
- LvH:** [0:56:52] That was my contact with Richard. There was this lovely small paper in Nature<sup>57</sup>—a single page or so, or even less than that—Hopfield, Feinstein, and Richard Palmer. That idea has appealed to me over the years, many years. I would still like to continue it a bit more on a theoretical level.

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<sup>54</sup> D. L. Stein, R. G. Palmer, J. L. van Hemmen, C. R. Doering, "Mean exit times over fluctuating barriers," *Phys. Lett. A* **136**, 353-357 (1989). [https://doi.org/10.1016/0375-9601\(89\)90414-3](https://doi.org/10.1016/0375-9601(89)90414-3)

<sup>55</sup> W. Gerstner, R. Kemptner, J. L. van Hemmen, and H. Wagner, "A neuronal learning rule for sub-millisecond temporal coding," *Nature* **383**, 76-78 (1996). <https://doi.org/10.1038/383076a0>

<sup>56</sup> See, e.g., J. L. van Hemmen, L. B. Ioffe, R. Kühn and M. Vaas, "Increasing the efficiency of a neural network through unlearning," *Physica A* **163**, 386-392 (1990). [https://doi.org/10.1016/0378-4371\(90\)90345-S](https://doi.org/10.1016/0378-4371(90)90345-S)

<sup>57</sup> J. J. Hopfield, D. I. Feinstein and Richard G. Palmer, "'Unlearning' has a stabilizing effect in collective memories," *Nature* **304**, 158-159 (1983). <https://doi.org/10.1038/304158a0>

Indeed in the early '90s, I've published a few papers, even about neuronal networks. I can send you my review of unlearning in *Network*, which is a decent summary<sup>58</sup>.

The problem is clear. In a neuronal network, you get lots of data, and your brain cannot store all these data because, *e.g.*, many of them are highly correlated. I needn't explain to you guys, or anybody else who works on networks, that as soon as you have correlated data you have to discern them if you store them though a local procedure that cannot see these global correlations. How can you make your code so that it can discern highly correlated data? Unlearning does it. The upshot of our work in Munich on unlearning is that we could extend it to spiking neurons—spatio-temporal patterns discretely functioning, so effectively spiking neurons. What is unlearning good for? It's highly efficient because you allow the network, more or less, to load practically to its maximum capacity. There is a maximum capacity for each coding system and each network. You can then practically load it up to its maximum. To formulate it in one sentence: The sense of unlearning is decorrelating correlated data.

The original idea... That was also the motivation of John Hopfield, who is an excellent biological physicist. John has always been motivated by biophysical problems. That's fantastic. This is, I think, a very good way to look at it, because REM sleep<sup>59</sup> was interpreted as an active way of unlearning. Whether it really is, we don't know yet but it nevertheless resembles rather well the algorithm that we now call unlearning in the theory of neural networks. If so, it makes a lot of sense, because many data are correlated. You know it yourself. Next morning, you certainly look through the problem, because apparently your brain has cleaned up the garbage, and you can download the solution. This feeling is what unlearning may well be good for in a neurophysiological sense. I would say that the meaning of REM sleep—or the implication of REM sleep—is just decorrelating correlated data. We have seen this theoretically for spiking networks and spatio-temporal problems. So it's highly probable, but the final proof is still missing.

**PC:** You have a pretty solid understanding of both the European and American physics communities, having spent a lot of time in both. Do you have any insight to offer as to why ideas of replica symmetry breaking, for instance, were received differently—if they were—between the two communities?

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<sup>58</sup> J. L. van Hemmen and R. Kühn, "Collective Phenomena in Neural Networks," In: E. Domany, J. L. van Hemmen and K. Schulten eds., *Models of Neural Networks. Physics of Neural Networks* (Berlin: Springer, 1991). [https://doi.org/10.1007/978-3-642-97171-6\\_1](https://doi.org/10.1007/978-3-642-97171-6_1); J. L. van Hemmen, "Hebbian learning, its correlation catastrophe, and unlearning," *Network: Comput. Neural Syst.* **8**, V1-V17 (1997). <https://doi.org/10.1088/0954-898X/8/3/001>

<sup>59</sup> F. Crick and G. Mitchison, "The function of dream sleep," *Nature* **304**, 111-114 (1983). <https://doi.org/10.1038/304111a0>

**LvH:** [1:01:53] I'll give the answer in the British way. I'm afraid there are differences. The American scene has always been far more reality oriented than the European one. I've spent my postdoc in Paris, so I know the Parisian scene. (That was a lovely stay, but that's something different.) French theoretical physics idea has every now and then the inclination to construct things that are slightly too lofty to be true. So a bit of realism is not only of practical value for science, it's also necessary.

I'll give you another example from the theory of neuronal networks. When I arrived in Munich, about 1990, there was a discovery of coherent oscillations in primate visual cortex<sup>60</sup>. Coherent oscillations—something like a bell rings with a frequency of 50 Hz—what is this good for? “It's nonsense,” that's what the Americans said. The Europeans said: “No! It must be there.” The upshot is that both are right. The coherent oscillations do play an important role, but it is not as dominant as what the other party thought. I would suggest: please, listen to each other. Then we come down, back to reality. That's far better.

**PC:** During your time at Duke, at Heidelberg or in Munich, did you ever get to teach a class on spin glasses and replica symmetry breaking? If yes, can you detail what those classes were?

**LvH:** [1:04:12] In Heidelberg, I was *Privat-Dozent*. So I did things that are useful to students, such as convex functions and applications (there are lots of the latter), fractals, metastability,... And indeed, during the fall term of 1985 I gave an introductory course on spin glasses, starting with the Hopfield model and including the Sherrington-Kirkpatrick and the van Hemmen model, dynamical stability, Moore-Penrose inverse, and optimization.

With Richard Palmer, back in Heidelberg I've given a seminar on complexity and its applications for both the physicists and the mathematicians. I've taught spin glasses quite often, certainly from the ergodic-decomposition point of view<sup>61</sup>. Certainly, also during my first ten years in Munich, because the Hopfield model was still something highly attractive to students, and I found teaching it fun. So why not do it?

The point is symmetry breaking. By itself, in equilibrium statistical mechanics it is a very important notion. We could delve into the question of: “Fine, if we talk about symmetry breaking, can we specify the right order

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<sup>60</sup> See, e.g., R. Eckhorn, J. W. Bauer, M. Brosch, W. Kruse, M. Munk and H. J. Reitboeck, “Coherent oscillations: A mechanism of feature linking in the visual cortex?” *Biol. Cyber.* **60**, 121-30 (1988) <https://doi.org/10.1007/BF00202899>

<sup>61</sup> A.C.D. van Enter and J.L. van Hemmen, “Statistical-mechanical formalism for spin glasses,” *Phys. Rev. A* **29**, 355-365 (1984). <https://doi.org/10.1103/PhysRevA.29.355>

parameters.” Choosing the right order parameters, we have to do that by means of the “method of inspired guesswork”. That’s an art. Symmetry breaking is extremely important in physics, but you have to look at the context. To characterize it, you need all the order parameter or parameters—singular, plural. How to find them? Hmm, that’s inspired guesswork, folks. Otherwise, it won’t work.

Replica symmetry breaking is exotic, if I may say so. Either use the replica method, a bright idea, absolutely wonderful, but then the next question will be: In what context? I doubt you have studied the book<sup>62</sup> from cover to cover, the mathematical proof as to why the Parisi solution is indeed the correct one. I would say symmetry breaking, yes, it’s extremely important. If it appears in some contexts—replica or whatever—then we have to take a careful look, because then it’s physically relevant. So my answer is yes, it’s relevant, as soon as you see its physical relevance. Have I thought about it? Yes, I did. Not quite from the—from my point of view—somewhat exotic Parisi-version point of view. It’s a lovely theory, no doubt, but some theories in physics are lovely, yet need an experimental verification. That may take some time.

**PC:** Is there anything else you would like to tell us about this era that we may have skipped over or have missed?

**LvH:** [1:07:56] It was an absolutely fascinating time. You can’t imagine when I arrived to Duke. Patrick, you know. At the beginning of the term at Duke—Duke in those days – 1977 – started directly after Labor day. Is that still true?

**PC:** We start mid-August almost, but there’s air conditioning.

**LvH:** [1:08:31] Ok. So we started just after Labor day. It’s still hot, the climate will turn over only by October 1<sup>st</sup>, but you’re standing outside Duke’s Physics Building and someone explains to you the spin glass problem, and then back in Europe you visit John Mydosh, who had done this wonderful experimental work. You get into contact with the experimental aspects of spin glasses. That was actually in Jülich. In Jülich, they studied the non-metallic spin glasses, which I found a bit dry to be honest, but that’s a matter of taste.

I like metals far better. To understand that you need to know that I’ve written my master thesis on the Knight shift in liquid alkali alloys<sup>63</sup>. Alloys, metals! The Knight shift in liquid alkali alloys had just been described in this wonderful lab in Groningen, as a function of the concentration. The Knight

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<sup>62</sup> See Ref. 10.

<sup>63</sup> See Ref. 38.

shift, which is a shift because of the conduction electrons, is absolutely linear in dependence upon the concentration. You could take the experimental points, then take a ruler and draw a straight line through them. I've made a theory for that. It has been published. You can look it up<sup>64</sup>. You will see that my theory goes exactly as a straight line through the data points. Even though the theory is nonlinear, it produces just straight lines through the data points. Anyway, that made me realize that solid-state physics and metals are interesting stuff.

Everything needs to be considered in its own context. The rest—what you are going to do—depends on the prejudice you have gained during your studies. I had the great luck of studying at a good university with an exceptionally good math department. The math department at Groningen was really exceptionally good—including probability theory—because that primes you for the rest of your life. As a postdoc you have no time to carefully study a probability-theory book. Either you know it or you don't. Your background is to a big extent determining what you're going to do.

That I then moved through spin glasses, tunneling of quantum spins—as soon as you start pondering about the low-temperature behavior of spin glasses, in anisotropy, the spin quantum number is much bigger than  $\frac{1}{2}$ —and theoretical biophysics of neuronal information processing is partially by accident, part of which I've sketched, and partially based on what you've learned during your education.

Also, keep an open eye on what you will see. I would say what happened in spin glass theory around 1980. Also, by accident—thanks to this Sonderforschungsbereich 123—we could organize the *Heidelberg Colloquium on Spin Glasses*. Personal meetings of this kind are worth gold. When you were there—it's important—people were able to talk to each other. There were no fights. In Heidelberg, there was a wonderful atmosphere. We had so much fun. There were strong, even hot discussions but people were always honest. The atmosphere was open. That's fantastic. If people are just arguing too vociferously, A may say it may all be right, but B thinks it's wrong and that's it. No! Listen to each another. That happened in those days. It was fantastic. That was fun.

For me, honestly, by 1985 the game was over. We have understood the kind of transition and the details. You should also be honest and think: Can I contribute anything there? Not for me. Let them run. I was damn right, if I may say so, in hindsight. It took 20-30 years more. That's a bit long for a human life. I would say quit earlier, that's far better. This makes you functioning properly, because it keeps you creative. This starting period in spin glasses was one characterized by open minds and critical discussion, but it was pure delight. With great respect, the way in which, in particular,

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<sup>64</sup> See Ref. 41.

David Sherrington functioned during both Heidelberg Colloquia. (I also know and appreciate Scott Kirkpatrick from my time at Bell Labs. He was A-OK but different.) But having David in Heidelberg was a joy. It was fantastic. It was a collaborative atmosphere, where everybody tried to discover what spin glasses really meant.

In hindsight, I must say, Phil Anderson had put us on the right track. He could have said: “Oh! Spin glasses are really boring.” As you know, he said the opposite. He was right. Phil is the person from whom Richard Palmer got his PhD. (Richard had to leave the game as did, by the way, Ingo Morgenstern, who died by the end of 2020. I'm greatly shocked as I learnt it this morning. That is to say, Ingo isn't among us anymore.) To summarize, Phil Anderson put us all on the right track, and that was really good. He stimulated the great development, characterized by open discussion, not dogmatic.

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

**LvH:** [1:16:03] That's a good point. Yes, I have an archive “Spin glasses 1978-1995. I want to deposit that, correct, but in a sense, if you had asked whether I have presented a well-accessible overview of the thoughts I have developed on spin glasses, some side remarks on what was happening there... My contribution<sup>65</sup> to the *Heidelberg Colloquium on Spin Glasses* is, I still think, one of my very best papers. One is not allowed to say so of one's own papers, but it's relative and long ago, and you may judge that as you like. It's a consistent overview, it's well written and it explains, defines everything you want to know. My archive I want to deposit it. Thank you for pointing this out to me. Now, I know that I'm not the only thinking this might be worthwhile.

**PC:** We certainly think so. As you said, this was a fascinating time at an intellectual, personal and other level. Thank you very much for your time.

**LvH:** [1:17:39] Thank you for letting me return to a fascinating period of my life.

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<sup>65</sup> J. L. van Hemmen, “Equilibrium theory of spin glasses: Mean-field theory and beyond,” in: J. L. van Hemmen and I. Morgenstern, Eds., *Heidelberg Colloquium on Spin Glasses* (Berlin: Springer, 1983), 203-233. [https://doi.org/10.1007/3-540-12872-7\\_50](https://doi.org/10.1007/3-540-12872-7_50)