History of RSB Interview: John Mydosh

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PC: Thank you very much, Professor Mydosh, for sitting down with us. As we've discussed ahead of time, the theme of this interview is the development of replica symmetry breaking. But in order to get to that, we have some background questions. Could you tell us a bit how you first got interested in physics? And what then led you to pursue a PhD in solid state or condensed matter physics?

JM: [0:00:35] I was born in New Jersey, in a small city called Bayonne. We were not a wealthy family, but when I went to college in Philadelphia I participated in three study-work semesters at the Saint Joseph's University, taking night-courses and then spending a semester in one of the many electronic laboratories in Philadelphia. This gave me tuition money, which helped out a lot. After finishing at three different laboratories of a now-defunct corporation[1], I had a knowledge of semiconductor physics. I also spent some months on military engineering, and I decided that after I got my bachelor's I would continue with semiconductors and go to a gallium arsenide laboratory at RCA[2]—they're now defunct, or rather taken over by General Electric. RCA gave me the possibility to do advanced development with new devices of this new material. My early "claim to fame"—or two claims to fame there—is that I built the first gallium arsenide transistor that had gain. This is about 1960. By making gallium arsenide diodes, I found a coherent source of light emission. I didn't know what it was then. My colleagues who did the advanced development also did not know what we were observing. The diode junction had a free material ring perimeter. It was at this plateau circumference that sharp light was streaming. Nowadays you go to the supermarket and get the same effect at the check-out.

[1] Philco: https://en.wikipedia.org/wiki/Philco
[2] RCA Corporation : https://en.wikipedia.org/wiki/RCA

I missed it because I had no physics background understanding laser coherent light. It took me years to learn what I missed, and what with my colleagues at advanced development we had failed to see.

After a few years I decided that I would quit this advanced development device engineering, and go back to the university (Stevens Institute of Technology) which was in a place called Hoboken, New Jersey. I did my PhD with a guy called Meissner[3]. Not the father Meissner[4], but the son of the famous German Meissner, who was trying to make a career in the United States. I researched superconducting thin films and understood about critical currents, critical fields, etc.[5] This was very good experimental training.

Then, after the PhD, I decided to join a physics faculty in New York City. My assistant professorship was at Fordham University. (It's a small Catholic Jesuit University in the Bronx, New York.) We were building a solid state physics group. There were maybe three or four of us[6] working very hard. It turned out that the physics department, moving into the '70s, was over-expanded. There would effectively be no chance of tenure. True, I did not get tenure. (I'm very happy about that.) However, before, I had gotten a Research Corporation grant and had a PhD student. With the grant of $5,000, I bought a mutual inductance bridge, which enabled me to do AC susceptibility measurements at low temperatures—helium temperatures—at different frequencies with enormous magnetic sensitivities. That is how we discovered—and we used for the first time (1972) in the Physical Review[7]—the term spin glasses.

PC: Before we get to that, I'd like to ask you something else. You mentioned the collaborative group at Fordham. There was Joseph Budnick, Stanislaus Skalski, Mahandra Kawatra … How collaborative was this environment?

JM: [0:05:05] Collaboration was very good initially. Three of us were on the tenure track. Joe Budnick[8], the senior Professor, was also the chairperson.

[3] Hans Walter Meissner (1922-2005). See, *e.g.*, https://academictree.org/physics/peopleinfo.php?pid=53295 (Last consulted February 14, 2021)

[4] Walther Meissner: https://en.wikipedia.org/wiki/Walther_Meissner

[5] J. A. Mydosh, *Dependence of the critical currents in superconducting films on applied magnetic field and temperature*, PhD Thesis, Stevens Institute of Technology (1965). https://stevens.on.worldcat.org/oclc/820885210

[6] See, *e.g.*, Sylvia Barisch, *Directory of physics faculties 1968-1969: United States, Canada, Mexico*. (New York: American Institute of Physics, 1968).

[7] Vincent Cannella and John A. Mydosh, "Magnetic ordering in gold-iron alloys," *Phys. Rev. B* **6**, 4220 (1972). https://doi.org/10.1103/PhysRevB.6.4220

[8] Joseph I. Budnick: https://academictree.org/physics/peopleinfo.php?pid=605792 (Last consulted February 14, 2021)

We were working very well and were publishing a number of strong papers. But the University was in a crisis. It was pulling their money from the Bronx campus to Lincoln Center, in Manhattan, where they would build a new campus for the law school, the social science school, and perhaps even the religious area. So they wanted to curtail physics uptown. This happened to make the situation a bit difficult for the non-tenured three of us. Eventually, I was very lucky. I was able to get what they called a faculty fellowship. They gave me half salary. The rest of the faculty took over my teaching load, with no extra expense to the University and I had this full-year faculty leave. My salary was probably $8000 or $9000/year, so with 50% I had enough to get me to go to London, to the Imperial College to do originally superconductivity research and then further into Germany.

At Imperial College, I met Bryan Coles[9]. Bryan Coles was commuting, or had his contacts—I don't think he was a Cambridge man (he was educated at Oxford)—with Sam Edwards and Phil Anderson, who was at Cambridge during this time. (This is about 1970, 1971, 1972.) Phil Anderson was guest professor and published papers both at Cambridge and at Bell Telephone Laboratories. Stimulated by these helpful discussions, at a distance, we could now better understand our previous move into magnetic alloys. We already had samples from Brookhaven laboratory, where the nuclear people published extensive Mössbauer experiments on many materials, like gold iron, for the whole concentrate range. They gave us all these expensive gold samples, and we started to measure them in the AC susceptibility. Here was the bad Fordham situation that I effectively escaped, and my PhD student, Vince Cannella, was able to get his PhD in 1971[10]. He immediately got a postdoc at Wayne State University, in Detroit, and was continuing the measurement during his postdoc for two or three years. In 1972, we published the big paper. That was the Physical Review B paper. We wanted to put the title "Spin glass magnetic ordering", but the editor at the time—What was his name? He just passed away!—forbade it, so we only got it into the abstract. That's the whole start of this spin glass adventure.

There was this conference in 1972 that was sponsored by Wayne State[11]. The lead speaker was Phil Anderson. He was using the word spin glass, but he didn't understand it at the time. This was stimulating. Our presentation

[9] D. Caplin, "Bryan Randell Coles. 9 June 1926 — 24 February 1997," *Biog. Mems Fell. R. Soc.* **45,** 51-66 (1999). https://doi.org/10.1098/rsbm.1999.0005

[10] Vincent David Cannella, *The Thermoelectric Power and Low-Field Magnetic Susceptibility of Gold-Iron Alloys*, PhD Thesis, Fordham University (1971). https://research.library.fordham.edu/dissertations/AAI7126959

[11] International Symposium on Amorphous Magnetism, August 17–18, 1972, Detroit, Michigan. See, *e.g.*, *Amorphous Magnetism*, Henry Hooper, ed. (New York: Plenum Press, 1973). https://doi.org/10.1007/978-1-4613-4568-8

of the data of a phase transition, a sharp phase transition that was field and concentration dependent, and had a frequency dependence—that's the unique part—in these different alloys. That's a long story short.

PC: If I understand correctly, you never embarked on a project to find spin glasses. Is that correct? If I look at Cannella's thesis, for instance, there's no mention of spin glasses at all.

JM: [0:09:52] Indeed, we didn't use the word until I had my sabbatical, or fellowship. It was September to December; I was back in the USA for Christmas after four months in England. At Imperial, there was—I would talk to him every day—David Sherrington. Sebastian Doniach[12] was also there; Martin Zuckermann[13] and Nick Rivier[14] were all present. And a number of experimentalists were scattered through the Department. I would go up the elevator with Bryan Coles, and Bryan would say repeatedly spin glass, spin glass, spin glass, and so I picked it up. (Bryan Coles died in his low 70s in age, unfortunately, many years ago.) He did publish papers on systems that didn't have local moments. He called those incorrectly spin glasses, bad luck, he was using the wrong systems, but everybody then was wrong. We did a good system, gold-iron; then we did copper (and silver)-manganese; we did a few other strong local-moment systems. This, I think, proved that and **J.L.You** had to go further and model what a spin glass was. In our 1972 PRB, we used the term antiferromagnet-like, and it was indeed antiferromagnetic behavior, however, without long-range magnetic order. Spin glasses have zero magnetization when they are zero-field cooled. But there was much more than that. The other people, *e.g.*, Lutes and Schmit[15] at Honeywell Laboratories—industrial research, were looking at similar alloys, but into too big a field and with too limited sensitivity. The field smeared out this delicate freezing process, *i.e.*, no cusp. You put a big field on and you screw up the spin glass physics and phase transition.

PC: Would it be fair to say that there was an experimental race to find a material that was a spin glass?

JM: [0:12:03] No. There was no experimental race with these alloys, which were concentrated. The experimental race was on the Kondo effect. The

[12] Sebastian Doniach : https://en.wikipedia.org/wiki/Sebastian_Doniach
[13] Martin J. Zuckermann (1937?-). See, *e.g.*, https://academictree.org/physics/peopleinfo.php?pid=565627 (Last consulted February 14, 2021)
[14] Nicolas Rivier (1941?-). See, *e.g.*, N. Rivier, *Contribution to the Theory of Localized Moments in Dilute Alloys,* PhD Thesis University of Cambridge (1968). https://idiscover.lib.cam.ac.uk/permalink/f/t9gok8/44CAM_ALMA21559205550003606
[15] O. S. Lutes and J. L. Schmit, "Low-Temperature Magnetic Transitions in Dilute Au-Based Alloys with Cr, Mn, and Fe," *Phys. Rev.* **134**, A676 (1964). https://doi.org/10.1103/PhysRev.134.A676

theorists needed a complete solution of the Kondo effect. All these famous people at Bell Labs, they would get [Jun] Kondo[16] to come over and explain what was the Kondo effect. The experts told us: "You're using too high concentrations. When you get the cusp in the susceptibility your concentration is too high, and you are destroying the Kondo effect. So why are you studying these magnetic alloys?" This was the topic of great interest (from 1965 to 1975), the Kondo effect. You could look up all their many review works. For example, [Melvin Drew] Daybell and [William A.] Steyert[17], all these guys were doing Kondo, a new many-body problem. However, for Kondo, you want a single impurity and conduction electrons. This model was a great breakthrough in many-body quantum physics from condensed matter, but it's not a spin glass. So the initial reaction was not to bother with interacting impurities. "You're too high in concentration. You should go to very low temperature with single impurities." We defied them. We were the outliers.

PC: How would you describe the reaction to your work, then?

JM: [0:13:35] The paper came out in 1972: Cannella and Mydosh. Measurements were mostly done in New York. Cannella continued in Wayne State. I had six months left from my full year sabbatical, and had gone over to Germany to the Institut für Festkörperforschung, Kernforschungsanlage in Jülich. (German is my second language, and unfortunately Dutch is my third language. We have to keep them straight.) When I went to Jülich, I worked in the superconducting group. I had as new mentor the well-known institute leader, a German guy called Werner Buckel[18], who said: "You want to do magnetism and superconductivity? You know, magnetism would destroy the superconductivity." However, he said: "Do it! Do it! Do spin glass!" He gave me enormous support. Throughout the 1970s—I guess we're moving into 1975—the spin glass problem bursts out. And who was in Jülich? Konrad Fischer[19] and Kurt Binder. These guys were constantly coming over to talk to me. I had the theoretical support; I had the experiments running; I had some visitors coming in. We were also doing alloys called giant moments—this does matter because it's mainly random ferromagnetism. Kernforschungsanlage in Jülich was a paradise. All the reactors

[16] Jun Kondō: https://en.wikipedia.org/wiki/Jun_Kond%C5%8D

[17] Andrew C. Anderson, William E. Keller, William N. Lawless, Ralph C. Longsworth, and Raymond E. Sarwinski, "William A. Steyert," *Physics Today* **42**(1), 100 (1989). https://doi.org/10.1063/1.2810895

[18] Werner Buckel: https://de.wikipedia.org/wiki/Werner_Buckel

[19] Alex Braginski, "Konrad H. Fischer Remembered," *Superconductivity New Forum*, June 1, 2016. https://snf.ieeecsc.org/obituary/konrad-h-fischer (Last accessed February 9, 2021.)

were there. They had powerful theoretical groups. Theorists like Gert Eilenberger and Herbert Wagner[20], who did the famous Heisenberg magnetism, that you can't have it in lower dimension[21]. All these guys were surrounding me as a young kid and I was doing experimental spin glasses.

PC: Did you have students there as well?

JM: [0:15:54] No, but I had visitors. I brought in some on sabbatical. The problem in Jülich is that you didn't have PhD students. You were maybe bringing students to do something called the *diploma arbeit,* a master's thesis. We haggled a bit with that. I would have a technician, but I wouldn't have a PhD [student]. That is the reason why we were planning to leave Jülich. I don't know if you've ever been to the village of Jülich. The research center is now a major biological/life science laboratory; it closed the neutron reactor facility. You couldn't get any students. It's in a small village, and it became very constraining after five years. So I and my German wife at the time decided we wanted to go to a university. She was a computer programming expert. She worked on the big computers in Jülich, so we thought we could both get university positions. The spin glasses were hot topic. I was probably giving an invited talk once a month. I would go to London to visit Bryan [Coles] and his group. Most important in 1975 Edwards and Anderson appeared[22], along with Sherrington-Kirkpatrick[23]. Here, David Sherrington went to IBM to work with Scott Kirkpatrick and their solution paper quickly came out. Later on John Hertz and Konrad Fisher wrote the first book[24]. And Peter Young and Kurt Binder had their 1000+ citation Review of Modern Physics[25]. Returning to the 1970s, at a later stage, came Giorgio Parisi in 1979 with the true replica breaking symmetry scheme.

PC: Before we jump to that, you've mentioned the year 1975 as being a landmark, with the Edwards-Anderson and Sherrington-Kirkpatrick models appearing back to back. First, how closely were you following those? What was your reaction to this work? Did it matter to you as an experimentalist?

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[20] Herbert Wagner : https://de.wikipedia.org/wiki/Herbert_Wagner_(Physiker)

[21] N. D. Mermin, H. Wagner, "Absence of Ferromagnetism or Antiferromagnetism in One- or Two-Dimensional Isotropic Heisenberg Models," *Phys. Rev. Lett.* **17**, 1133–1136 (1966). https://doi.org/10.1103/PhysRevLett.17.1133

[22] S. F. Edwards and P. W. Anderson, "Theory of spin glasses" *J. Phys. F* **5,** 965-74 (1975). https://doi.org/10.1088/0305-4608/5/5/017

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

[24] K. H. Fischer and J. A. Hertz, *Spin Glasses* (Cambridge: Cambridge University Press, 1991).

[25] Kurt Binder and A. Peter Young. "Spin glasses: Experimental facts, theoretical concepts, and open questions," *Rev. Mod. Phys.* **58**, 801 (1986). https://doi.org/10.1103/RevModPhys.58.801

JM: [0:18:40] It did matter. We wanted to use our experiments to confirm the theoretical models. We used much of the susceptibility results. We followed Edwards-Anderson, we looked at his susceptibility where it tracks Curie-Weiss, and where it drops with a cusp. I think we put that in one of our conference proceedings. We were following that. We didn't understand, at the time, the exact meaning and the replica symmetry calculation of the Edwards-Anderson order parameter. We published some papers, of course, citing that. This got us into Sherrington-Kirkpatrick, which was the same year. We put out a Physical Review Letter—in 1978—from Leiden on the Sherrington-Kirkpatrick[26] calculations. We nicely fit their phase diagram, because we were able to tune the system from random spin glass to ferromagnet. This was very rewarding for us. However, we were surprised when Sherrington and Kirkpatrick was deemed to be wrong.

PC: Did you follow the discussion about this model?

JM: [0:20:32] Yes. We were following the discussion about their model. I mean, I didn't do the mathematics to understand it. I didn't know the order parameter did not break replica symmetry. The order parameter was wrong. Although it was a beautiful statistical mechanical solution—I was dabbling in the statistical mechanics of the solution—as far as experimentalists get. I had statistical mechanics as a graduate student. I even took a course on statistical physics. I was using the different symmetries, the different methods of statistical mechanics. So we could fit. I had a good group running. (I had already made the move to Leiden. That's a different situation.) We were able to look at the solution, spent hours trying to learn it. The theorists in Leiden at the time were very famous guys: Mazur[27] and Kasteleyn[28]. You have to know them. (Both of them are now dead.) They were very famous in the field of statistical mechanics . They would follow with interest but not participate in the field of spin glasses. I would see them only at faculty meetings. They were on the top floor of the old Kamerlingh-Onnes building[29], and they did not like experimentalist walking through their hallow halls. While they did not actively participate, they gave me great support in the faculty and university at Leiden. Kasteleyn was on the border of it. He was perhaps interpreting a little bit of Edwards-Anderson; I don't think he got into Parisi.

This is 1976, 1977, 1978, in Leiden, having decided to move over from the big *Forschungszentrum* community in Jülich to Leiden. Leiden's Kamerlingh

[26] B. H. Verbeek, G. J. Nieuwenhuys, H. Stocker and J. A. Mydosh, "Evidence for a Ferromagnet—Spin-Glass Transition in PdFeMn," *Phys. Rev. Lett.* **40**, 586 (1978). https://doi.org/10.1103/PhysRevLett.40.586

[27] Peter Mazur : https://en.wikipedia.org/wiki/Peter_Mazur

[28] Pieter Kasteleyn : https://en.wikipedia.org/wiki/Pieter_Kasteleyn

[29] Kamerlingh-Onnes Gebouw: https://en.wikipedia.org/wiki/Heike_Kamerlingh_Onnes#Legacy

Onnes Laboratory was horribly traditional. Somebody had to open a window and let some research fresh air in. It was extremely painful time for me, similarly difficult for my wife and daughter. I almost came to Duke, for I was flirting with a number of American universities for a return to the States. Somewhere in the '80s I was interviewing at Duke. There was a very good group on superconductivity, on helium, [Richard] Palmer was there[30]. So there was great statistical physics, and also good low-temperature physics. I don't know really why, deep down, I did not push the negotiations to fruition at Duke. There were other than physics problems in my life.

We were then going pretty wild into spin glasses into the 1980s, in Leiden. I had suffered so much in Leiden, getting the group working and getting the language reasonably mastered. Very difficult for me was Dutch. It has some French words, but mostly English and German words. If you were familiar with all three main European languages, then you'd have no trouble with Dutch. Unfortunately, I only had two, [and no] French. So this is the problem. Once we became embedded with the NL culture, this was then why we decided to stay. Spin glasses were going good. I had enormous support from the Dutch National Science Foundation[31]. My wife and daughter were growing up speaking English, German, and Dutch. We were walking over this culture/tradition barrier. Yet, it took me five years to build the group. It was very painful, really painful.

PC: Even coming in as a full professor, you didn't come in at the top of the heap?

JM: [0:26:07] Wait a minute. No! This was the serious problem. The inbred Leiden system had something called professor, and then—what they now call associate professor—lector. I took over the chair of this famous Kondo guy by the name of van den Berg[32]. He could have found the Kondo effect—but he didn't know what it was—with another really famous guy called de Haas[33]. This is a long tradition of Dutch physics. Van den Berg had this big research group, but he was not a full professor. Yet he would be able to

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[30] Richard G. Palmer : https://en.wikipedia.org/wiki/Richard_G._Palmer
[31] Nederlandse Organisatie voor Zuiver-Wetenschappelijk Onderzoek (ZWO): https://en.wikipedia.org/wiki/Netherlands_Organisation_for_Scientific_Research
[32] Gerard Johan van den Berg (1911-). See, *e.g.*, G.J. Van Den Berg, *De Electrische Weerstand Van Zuivere Metalen Bij Lage En Zeer Lage Temperaturen*. (Amsterdam: Noord-Hollandsche Uitg. Mij., 1938); PhD Thesis, Leiden University. https://catalogue.leidenuniv.nl/permalink/f/n95gpj/UBL_ALMA21175280220002711; see also https://www.lorentz.leidenuniv.nl/history/proefschriften/ (Last consulted February 11, 2021)
[33] Wander de Haas: https://en.wikipedia.org/wiki/Wander_Johannes_de_Haas

have five or six PhD students, and a postdoc or two, along with metallurgical technicians. Unfortunately, he was incompetent, but he built the new laboratory wing, and he then retired. So they brought me in at his position, which was not a full professor. It was an associate professorship with tenure, but it was extremely well supported. After a five or more years with the spin glass work heavily cited I was promoted to full professor.

So, here I am 40 years later, working (or reminiscing) in my wonderful apartment as emeritus, looking out the window on to the Haarlem–Leiden barge canal. And that is how we got into the glasses between New York, Jülich and Leiden.

PC: Let's backtrack a bit about spin glass history. In 1979-1980, Parisi brought together the solution to the SK[34]. How did you react to the solution? How did you find out about Parisi's work in the first place?

JM: [0:28:10] We were always up-to-date getting new literature. We had all the available spin glass literature to study and review. And also through contacts, I well-knew the spin glass field. (By knowing the literature I became an divisional associate editor to Physical Review Letters but that was later.) I had Parisi's Physical Review Letters—I think it's 1979—and I did not understand it, of course. An infinite number of order parameters in RSB. So you struggle with it, you think about it, you talk to people, you really ponder it. You leave it for the theorists. I met with the theorists in Amsterdam. I was very sociable then, and had many conversations. The physical interpretation of Parisi was, for me, very difficult. I had to eventually learn this, struggling through it, sit down for 3-4 hours with these guys. Then he had other papers. His original work came out of—I think—Frascati, the nuclear lab in Italy[35]. Immediately after a few papers, he moved either to Saclay or to Paris. I think it was one of the Écoles in Paris. This is 1980. He was commuting. He wanted to get his chair at La Sapienza in Rome, but this took an enormous amount of time. In Italy, everything goes slow.

So you now have something called replica symmetry breaking. For me and my group—we had a couple of PhD students—we were struggling. I don't think I got too much help from people like Kasteleyn.

PC: Or from David Sherrington, or from Bryan Coles' group?

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[34] *E.g.*, G. Parisi. "Infinite Number of Order Parameters for Spin-Glasses," *Phys. Rev. Lett.* **43**, 1754 (1979). https://doi.org/10.1103/PhysRevLett.43.1754

[35] Laboratori Nazionali di Frascati : https://en.wikipedia.org/wiki/Laboratori_Nazionali_di_Frascati

JM: [0:30:23] I would go visit. David moved to Oxford and I lost contact. Bryan moved into high-level administration positions. I had Konrad Fischer. The book with John Hertz came out in the late eighties. Who did I contact? I really don't remember. I struggled for years with replica symmetry breaking. I knew then that Sherrington and Kirkpatrick did not break replica symmetry, and I was shocked. Looking back, now you wonder how stupid could you be. You see what the order parameter does from Sherrington-Kirkpatrick, and you see that the entropy goes negative. Everything breaks down. But we modeled and tested it experimentally. We reproduced their phase diagram from their famous Physical Review Letter into a system called palladium-iron-manganese. Manganese makes it a spin glass, iron makes it a ferromagnet. We tuned it by doing metallurgy, by doing different ternary compounds or alloys. So this was our major struggle into the 1980s.

PC: In 1983, you wrote a piece for a Europhysics News—and I quote—that "ample possibilities exist for the practical applications of spin-glasses, not only in computer switching, memories and layouts", but others[36]. You were pretty bullish on spin glasses at that point. What was motivating your bullishness?

JM: [0:32:30] You know the many "spin offs" starting into the mid-'80s. We had at the beginning these artificial neural networks by John Hopfield[37]. This opened a whole new area of different uses of spin glasses. So you can go over to the traveling salesman problem, Scott Kirkpatrick *et al*. And so forth, see my book for a listing of spin glass analogues.

PC: Sure, but I'm asking more specifically on the material side.

JM: You had polymers, actual polymers, which had spin glass-like behavior when you folded them. You had protein folding, which was the modeling of a protein structure. That involves something to do with spin glasses. You had biomolecules, which also had aspects of spin glasses. You had, of course, real glasses. Look at Parisi today! You're doing real glasses. I don't think that problem has been solved. It's the materials physics that driving the spin glass behavior.

PC: What I'm trying to get at is this. Was it clear, in your mind, in 1983, that spin glasses, as material alloys, would have very limited technological applications, and that therefore it's the ideas coming from their study that

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[36] J. A. Mydosh, "Spin-glasses," *Europhys. News* **14**(12), 2-4 (1983). https://doi.org/10.1051/epn/19831412002

[37] J. J. Hopfield, "Neural networks and physical systems," *Proc. Nat. Acad. Sci.* **79**, 2554 (1982). https://doi.org/10.1073/pnas.79.8.2554

would be more important? Or did it take more time for that realization to emerge?

JM: [0:34:04] I would firmly disagree. Even today disordered-materials in magnetism is a thriving field. When I went looking at the magnetic alloy situation, right now, we went from the transition metals—gold, iron manganese—I can find out 30, 40, 50 of them; we went into the rare earths—lanthanum, gadolinium; we went even into things called the heavy fermions, strongly-correlated metallic systems, which had a spin glass behavior when randomized; we went into superconductivity, which coexisted with the spin glass phase. We had a vast repertoire of magnetic materials that were, I would almost say, typical spin glasses. They didn't involve anything like the orientation of molecules. They were systems, in which computer device people are looking at something called exchange bias, *i.e.*, exchange coupling. A paper came out a few weeks ago in Nature Physics: exchange bias between a coexisting antiferromagnet and a spin glass[38]. These are the classical materials. Back then, my old group in Jülich immediately went into insulating chalcogenides. Then the materials people went into the spinels, pyrochlors, Heuslers, oxides, quasi-crystals, etc. I believe they are all good spin glasses. They're slowly becoming applicable, *e.g.*, exchange bias. The phenomenon of spin glasses has been, in the last 20 years or more, materials related. All these new materials are driving the basic spin glasses. That's what I tried to do, when I wrote this—I was asked by Laura Greene[39] to write a review article—Reports on Progress in Physics[40]. I concentrated on a vast list of materials. I still get many citations, fortunately, for materials, *i.e.*, disordered magnetic materials.

The underlying theory has mainly been put into books. A couple of guys at NYU wrote the book on complexity and spin glasses[41]. Every now and then a new theory comes out in the letters, but I would say if you look at Physical Review B, or Journal of Physics: Condensed Matter, every week, every month there's certainly another spin glass paper.

This is not completely what I mean. I must emphasize that there's the many spin-offs and the whole new area of complexity science. However, returning to the basic spin glass phenomenon, I tried to give four criteria to measure, and then to compare it with the theoretical models of how you draw

[38] Eran Maniv *et al.*, "Exchange bias due to coupling between coexisting antiferromagnetic and spin-glass orders," *Nat. Phys.* (2021). https://doi.org/10.1038/s41567-020-01123-w
[39] Laura Greene : https://en.wikipedia.org/wiki/Laura_Greene_(physicist)
[40] J. A. Mydosh, "Spin glasses: redux: an updated experimental/materials survey," *Rep. Prog. Phys.* **78**, 052501 (2015). https://doi.org/10.1088/0034-4885/78/5/052501
[41] Daniel L. Stein and Charles M. Newman, *Spin Glasses and Complexity* (Princeton: Princeton University Press: 2013).

the exchange coupling constant *J* as gaussians or delta function distributions.

There are continuing all sorts of "spin-off" issues. Evolutionary theory! We had a guy from the École Normale Supérieure giving a talk on evolutionary theory and relating it to spin glasses, a few years ago. Unfortunately, I forgot to mention another famous spin glass guy at the École: Mark Mézard was here a few months ago. We had a wonderful dinner together. He's doing very well using complexity in stock market valuation problems. Are you playing the stock market, getting wealthy? So you begin your career doing spin glasses and move on to other related fields.

PC: Moving back to spin glasses. In your landmark 1993 book, you write that experimental research on ideal spin glasses largely ended in 1985. Yet as you were just arguing, there's still lots of work being done on spin glasses as materials. What is the cusp in the research direction? What's the difference between the two types of work?

JM: [0:38:25] I was wrong. In the book, I was wrong. I have to take that back. Nobody is perfect. What was happening in the late 1980s into 1990s was that I became tired of doing spin glasses. There were constant pressure for more papers, meetings, conferences, conference-proceedings, etc. My metal physics group in Leiden and I went into strongly correlated materials, a new breakout field. As I look back on my career, I feel I made three contributions to physics over a 50-year period. Spin glasses, of course, is a major one. I can see these three contributions because the publications in each of them get over a thousand citations. (I use that criterion.) We did something called the colossal magnetoresistance[42]. The group went very full speed into that. It was a percolation problem of a semiconductor which had a metal-insulator transition and scanning tunneling microscopy clearly showed the spatial topology.

Now, I'm spending the last efforts of my life on something called hidden order[43]. Hidden order is a most unusual continuous phase transition, at present, without an explanation. For this "unknown type" of phase transition, in a strongly correlated uranium compound, there are many, many publications. The best condensed matter theorists have been working on it since 1985. It's still an unsolved problem. What's the order parameter and elementary excitations? We were lucky, I guess, in the spin glasses.

[42] See, *e.g.*, M. Fäth, S. Freisem, A. A. Menovsky, Y. Tomioka, J. Aarts, and J. A. Mydosh, "Spatially inhomogeneous metal-insulator transition in doped manganites," *Science* **285**(5433), 1540-1542 (1999).

[43] See, *e.g.*, John A. Mydosh and Peter M. Oppeneer, "Colloquium: Hidden order, superconductivity, and magnetism: The unsolved case of URu_2Si_2," *Rev. Mod. Phys.* **83**, 1301 (2011). https://doi.org/10.1103/RevModPhys.83.1301

Very lucky. Edwards and Anderson came along. Then you had Sherrington-Kirkpatrick at it. Then you had Parisi. Then you went to the droplet model. There were terrific theorists. America likes the droplet model. I don't know how many people like replica symmetry breaking in the United States. That's a funny part of it.

The hidden order problem is briefly this. With all the modern experimental techniques of neutrons, synchrotrons, X-rays, you name it, they or we can't find its cause: a beautiful phase transition. The phase transition is remarkable, stable, second-order. The specific heat has this enormous lambda point. Yet we still can't get an order parameter. So I've been presently very busy. I just finished a review article with two famous theorists—it came out February or March of last year[44]—on updating the hidden order. Why is it called hidden order? Because we don't know what it really is.

So this is why I turned away from the spin glasses for many years. In '83, '85 into the '90s, I built the group doing uranium. We did some fantastic work at the laboratory. (It's not the bomb-making uranium but its depleted isotope. You can get this in your lab, if you have a special license.) We did a lot of work with rare earths and actinides, mixing these with uranium. Here I put in a lot of effort, without completely ignoring the spin glass.

Probably about 1995-1996, we found a heavy fermion compound that had a pseudo-randomness that made it a spin glass. It was because ligand fields were random because of slight displacements of the ligand components. These sites were creating some sort of random interaction, a modified Ruderman and Kittel [RKKY] interaction, that created a beautiful spin glass effect. We published this in 1997[45]. This took me back into the spin glass research. Since then, my last effort with spin glass was this Report on Progress in Physics. That was 2014-2015. I still try to follow the field, however, at a distance.

I would really like to—I'm not going to redo the spin glasses—solve or understand the hidden order problem, which requires different band structure calculations and it's a whole new area. They don't know how to do the band structure for a hidden order state. All these famous guys in Rutgers and California, they still don't know how to do it. So that's the end of my career.

[44] J. A. Mydosh, Peter M. Oppeneer and P. S. Riseborough, "Hidden order and beyond: An experimental—theoretical overview of the multifaceted behavior of URu_2Si_2," *J. Physics: Condens. Matter* **32**, 143002 (2020). https://doi.org/10.1088/1361-648X/ab5eba

[45] S. Süllow, G. J. Nieuwenhuys, A. A. Menovsky, J. A. Mydosh, S. A. M. Mentink, T. E. Mason and W. J. L. Buyers. "Spin glass behavior in URh_2Ge_2," *Phys. Rev. Lett.* **78**, 354 (1997). https://doi.org/10.1103/PhysRevLett.78.354

PC: Since that book, you said you have kept abreast, to some degree, with the literature on spin glasses on experiments and theory. How have you done this? Was it mostly through contacts or…?

JM: [0:44:22] I read the literature. I was editor of Physical Review Letters during the 1990s, thus I would get an issue every week and I would follow that up. Presently, I have the library in Leiden, which is excellent and Research Gate as search engine. Now that I have the big Mac computer, I download Physical Review B, Physical Review Letters and Journal of Physics: Condensed Matter. I scan through it thereby still keep up-to-date on my interests. I hear occasionally Zoom or webinar lectures from the many visitors circulating in and about Leiden.

When I retired, which was around 2002-2003, I immediately went to the Max Planck Institute in Dresden. At the Max Planck Institutes, there were three big condensed matter centers. One of which is the institute for complex systems. Probably you know it. That's a big and powerful theory institute. There's the Molecular Cell Biology on the other side of Dresden. I stayed at the chemistry MPI, called the Chemical Physics of Solids. I would hear, per week, 3, 4, 5 different lectures. Maybe every day, I'd hear a lecture. If not I'd go to the physics department of the Technical University Dresden and back to Max Planck for complexity lectures. This guest professor period was an enormous stimulation: new physics, east German culture and different language. I continued commuting to the Max Planck for more than ten years, yet I maintained my office and apartment in Leiden. This kept me going and I broke into some new area. I did some new materials. I had a wonderful time in Dresden, although it's former East Germany and is now very conservative. I also spent five years full time during my retirement at the University of Cologne learning mainly oxides and multiferroics.

Upon surpassing 80 years I eventually stopped traveling. I'm now quarantined, locked down and have a 9PM curfew, so I can't do too much, except talk to you while I'm here and Zoomed in for the available talks.

PC: As you mentioned earlier, there was a differential in the reception to ideas about spin glasses in the US and in Europe, both on the experimental side and on the theoretical side. Having bridged both communities, do you have any insight to provide, or any perspective on what would explain that difference?

JM: [0:47:09] Now we're getting into culture. French culture is very different than German culture, which is—you probably know this—very different from United States culture.

Bell Labs was very famous. When Huse and Fisher came out with the droplet model they were the greatest[46]. It was immediately well received in the States.

I presume you early RSB guys were then in Paris when Parisi came with all his efforts into the group at École Normale Supérieure. They were very powerful and had Saclay for experiment and even more theory. I don’t know if they really talked to each other. Droplet model people did. And there was the British people with Moore and Bray. I think theoretically it's a cultural difference. Your education, and where you are. This gives you a different impulse with the theory. How do you think Parisi created his theory in 1978-79? How did he ponder about replica symmetry breaking and applied it to a spin glass problem? You have to answer that one with his interview.

PC: That's why we're building an archive.

JM: [0:49:08] I would be interested in [hearing from] the other people. I can’t answer this. My problem is as an experimentalist, you do a measurement then you have the data and you have the models to test, and so you are more in contact with hard material reality. There were some great experiments done in Paris on the spin glasses; in Saclay there were many systematic detailed experiments. That still continues. Every now and then there is a new experiment coming out of the French group.

PC: Was there also a difference between the US and Europe on the experimental side of spin glasses? Groups in Paris and you were working, while the response in the US was not as big, if I'm not mistaken. How do you interpret that distinction?

JM: [0:50:31] I don't know. Usually people spend sabbaticals or postdoc in different countries and they learn the culture. I don't really know who were are the great American experimentalists. Of course, there was lots of work at Bell Labs on spin glasses. During the early 2000s it petered out to various small university groups in the United States. There’s a group in Texas, there’s another group in Minnesota. They are fine. They're doing waiting time of 10^5 seconds. There’s not too many new materials. Everybody is

[46] Daniel S. Fisher and David A. Huse, “Ordered Phase of Short-Range Ising Spin-Glasses,” *Phys. Rev. Lett.* **56**, 1601 (1986). https://doi.org/10.1103/PhysRevLett.56.1601

changing now to high-temperature superconductors, oxides. Everybody was previously moving into the heavy fermions. Now, it's the Dirac and Weyl semimetals and graphene: topology.

Today, I don't think there's too much spin glass experimental activity, except in India. I have the program (a big PDF program) of the 2021 March meeting of the American Physical Society online. I have not yet seen sessions on spin glasses. Possibly there will be one session, and this would be the Texas guys, and the Minnesota people, and some other little group. Maybe, two or three talks. That's it for the United States. However, in India a vibrant spin glass community exists.

But, of course, then there are the prizes coming out for the statistical mechanics. If you go to the computer simulations, you have Peter Young, who will get another prize. Kurt Binder got a German prize. There's still a vibrant theoretical prize-winning community, for experimentalists not so.

PC: During your time at Leiden or elsewhere, did you ever get to teach material on spin gasses and replica symmetry breaking, in particular? You wrote the book, and it has a section on this. Did you ever use that material in a pedagogical context?

JM: [0:53:07] No. Not in a formal course, [but] certainly in special seminars. For me, the replica symmetry breaking—infinite number of order parameters—which I've used 10-20 times with students or in talks, is the multi-valleyed landscape. That's it. It's so simple-minded, but with that picture I can talk 15 minutes about spin glasses. That's replica symmetry breaking. That's the beauty. To get that picture—I think it's in my book—out of Parisi I feel that's fantastic. We had this guy—I keep repeating and wish I knew his name—two or more years ago, he was doing evolution theory. I have to go back on the computer speakers. I can find out three years ago who gave an Ehrenfest colloquium on a Wednesday evening from Paris, perhaps Ecole Normale Supérieure. In his talk he was using spin glasses for his theory of evolution, and it was about the multi-valleyed landscape. Isn't that beautiful? That's it. The multi-valleyed landscape. I've use that picture for the last 20-30 years.

PC: Is there anything that that I've missed, that you would like to share with us about that era?

JM: [0:56:22] That was great. However, I wanted to stop spin glasses when I finished my book. I worked on the random field Ising problem when I went to Santa Barbara in 1985. In 1990, I left family and the kid—my daughter and partner—and I returned to Santa Barbara at UCSB. Santa Barbara is great. Vince Jaccarino[47] was again my host, and he's one of my mentors. Lucky me, I had four mentors. Vince helped me at UCSB. I had this local apartment. I would get up at 7 o'clock in the morning and write, write, write. I started in September, October, November, December. I took a break at Christmas. January, February, March, April, May and June, and I had finished most of the book in this California atmosphere, and across the street at the Kavli Institute for Theoretical Physics. Along with the theoretical seminars and I had a bunch of experimental talks to hear. There is a great physics department with this beautiful weather at the beach. I don't know, now, looking back, how I could devote a year's time—9 or I guess 10 months—of my life writing a book, writing every day. That's how the book came out. My goodness, I can't do that now. That's my great enlightenment experience.

But I was often telephoning my group at Leiden. During the writing of the book, we made the big breakthrough in this hidden order problem. So the Leiden group was doing hidden order on uranium, strongly correlated. I was writing spin glasses with 6000 mile separation. While I was in Santa Barbara, the experiments were nicely running in Leiden. Thus, I'm pretty happy.

PC: The last question I have is about the materials. 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?

JM: [0:57:53] You mean how I made these alloys, materials?

PC: No. The physical correspondence, papers and notes. Not the materials themselves, but archival materials, I mean.

JM: I will go—if they let me in, not tomorrow but someday—into my Leiden office. (They gave me a very nice emeritus office.) It's a mess. I have books all over. When people walk into my office, they laugh at me. They point and they say: "Have you ever heard of being paper free?" I probably could find some of the letters. Now what would you suggest? A letter to David Sherrington? I went over to visit David Sherrington one and a half years ago at Oxford. We had a very, very nice chat. He's an old, dear friend. We were such happy kids in Imperial College. This would be around '72-'73, before he went to IBM. He stayed [at Imperial] a number of years. (Doniach was there and went to Stanford, and Zuckerman went to [McGill].) I'll take a look if I can find [something] in this massive paper [pile]. A letter from

[47] Vincent Jaccarino (1924 – 2019); See, e.g., https://chancellor.ucsb.edu/memos/2019-09-03-sad-news-professor-emeritus-vincent-jaccarino (last consulted January 13, 2021)

Phil Anderson? I don't think I ever corresponded with Phil. We had dinner a couple times in Aspen. Since, he just passed away. See my early publications 1970 to 2000 or the book.

PC: With Bryan Coles, for instance.

JM: [1:00:12] Unfortunately not, Bryan was really sharp in conversation. He was in this intensive, collaborative, sharing effort of the Brits between Oxford, Cambridge and Imperial. This triangle of cooperation with even some of the other schools. I would go to the various colleges of London University, *e.g.*, Birkbeck, Royal Holloway, etc. I even took a course on critical phenomena and went to hear magnetic mathematical physics at Imperial from David Edwards and Peter Wohlfarth[48]—he's also a spin glass guy. The spirit in Imperial was excellent. Maybe I can find some of the correspondences but I doubt it.

Bryan Coles got Neville Mott[49] at Taylor & Francis to ask me to write the book. Bryan did not want to write the book himself. (He could very well have done it.) Bryan was moving into administrative positions, higher up as Dean at Imperial, and presumably much higher. He got himself into a lot of political trouble in his deanship. (That's beside the point.) He said: "Do the book, and we give you $1,000. Live it well in Santa Barbara. Go and do it." However, Taylor & Francis wrote the book cover and it had three misspellings.

PC: Getting back to the archive. All correspondence of your scientific production would be of interest. I don't know if the University of Leiden has a manuscript department. They probably do.

JM: [1:02:27] The library at Leiden does have a great archive. For example, the correspondence between Ehrenfest and Einstein but they are not interested in me. More recently, they're interested in de Haas and [Lev] Shubnikov[50]. The archives are the exchange of letters between Moscow, Shubnikov and de Haas. Shubnikov, of course, got himself shot at a young age, when he did too much traveling. De Haas cooperated with the Germans during the war and they took away his professorship. These are very important archives. Nothing like this has happened to me.

PC: Yes, but I nevertheless encourage you to contact them.

[48] Peter Wohlfarth : https://en.wikipedia.org/wiki/Erich Peter Wohlfarth

[49] Nevill Francis Mott, "Harry Jones, 12 April 1905 - 15 December 1986," *Biog. Mems Fell. R. Soc.* **33,** 325-342 (1987). https://doi.org/10.1098/rsbm.1987.0012

[50] Leb Shubniikov: https://en.wikipedia.org/wiki/Lev_Shubnikov

JM: OK. Let us see. Thank you for spending this long time with me. I hope I've shed some light on the replica symmetry breaking, which is part of my career.

PC: Thank you so much.