History of RSB Interview: David Sherrington

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Location:
Over Zoom, from Prof. Sherrington's house in Oxford, UK.

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PC: Thank you very much, David, for agreeing to sit with down us. As we discussed, the purpose of this interview is mostly to talk about the spin glass and replica symmetry breaking period from 1975 to 1995. But first, we'd like to go into what led to that period. You sent us some notes about your youth and background that help us situate your upbringing in the UK, but those notes don't mention anything about physics. Where does your interest in physics come from, and what led you to pursue a PhD in physics?

DS: Well, I think maybe from childhood I was always inclined towards mathematics, so it was going to be some mathematical subject. In my secondary school, I actually had to specialize in what we called the sixth form[1], so some science subjects, which for me were mathematics, physics, and chemistry. I had outstanding teachers, and I liked those things. I guess I was probably inclining towards mathematics or maybe chemistry, but then I decided that really, no, it was going to be physics. If I were to answer that *now*, I mean thinking about what I would do *now*, it would be physics because that's what I feel interested in. There's a way of thinking that physicists have. But whether that's a consequence of the way I was brought up, and of the choices I made, or whether it's the reason that I made these choices, I'm not sure. That's why I think I mentioned to you that I think of myself as a theoretical physicist rather than a mathematical physicist. I meant to say [that I like] knowing how things work, and particularly many-body types of things, with interactions. I suppose it could have been chemistry. My maths teacher did send me to some place to find

[1] Sixth form : https://en.wikipedia.org/wiki/Sixth_form

out about mathematics at work, but it was some actuarial outfit. I decided that was going to be goddamn boring, and that probably drove me further towards physics.

I did pretty well in school. In my secondary school, there were three of us who [ranked] 1, 2, 3—we have five years, then you got into 6th form—and in the 3rd form of this stage, we were [ranked] 1, 2, 3, and they decided to move us all up to the 5th group missing out on the 4th. There we saw a challenge, and we came 1, 2, 3 in that year as well. I think that challenges have been things that have intrigued me. I think the way I view it, there's a huge number of challenges in what I call condensed matter physics, which I view in a rather broad sense. But I also used to like making things… I guess I can't really give a good answer, but I'm glad it's what I did.

PC: Then you decided to carry on with postgraduate studies, to pursue a PhD. How did that come about, and how did you choose a topic?

DS: [0:03:41] I think because I was always doing very well at school, and then at university[2], it was natural to keep going on. So I didn't seriously think about going out into industry, because I still seemed to be doing well. I think I thought about it even earlier, because when I was applying as an undergraduate… The system in Britain then was that you did two years in the 6th form, but if you wanted to go to Oxford or Cambridge you had to do a 3rd year. No boy in my school had ever been successful in that 3rd year effort, the few that did it. So I decided I wouldn't do that. I'd rather take that extra year towards a future PhD at university. So I was already thinking about it at that stage. I would ordinarily have moved to another university, because I believe that that's the best thing to do. It just so happens at that time I saw the best place to be in England would have been with Rudolf Peierls[3], who was at that time in Birmingham, had very strong group, but he was going to move to Oxford in what would be the middle of my PhD. It meant that I would be screwing up if I went to Birmingham or to Oxford, and so I decided that I would stay in Manchester.

I was keen to work with Sam Edwards[4], who had been my tutor and also one of my lecturers, but I was allocated to work with somebody else—Arvid

[2] DS: I came 1st in both my first and second years.
[3] Rudolf Peierls: https://en.wikipedia.org/wiki/Rudolf_Peierls
[4] S.F. Edwards: *Biogr. Mems Fell. R. Soc.* **63**, 243–271 (2017) https://doi.org/10.1098/rsbm.2016.0028; Eds. P. M. Goldbart, N. Goldenfeld and D. Sherrington, *Stealing the Gold: A Celebration of the Pioneering Physics of Sam Edwards*, (Oxford: Oxford University Press, 2004). DS: This title relates to Sam's philosophy of being the first to start new topics but then moving on; see, *e.g.,* p. 9. The book illustrates his success with this philosophy.

Herzenberg[5]—and so we worked on a different thing in that first year. That's when I did theoretical chemistry, if you like. I was involved in many-body theory of small molecules[6], using ideas and techniques from nuclear physics—not particle physics, but nuclear physics. There were no many-body books at that time in condensed matter physics so I used NORDITA nuclear physics lecture notes[7] and Thouless's book[8]. Anyway, that's what I did it for that first year, but it wasn't really what I wanted specifically. I wanted to work with Sam, so I changed in the second postgraduate year[9].

PC: You said you were assigned. Didn't you get to choose who was your PhD advisor?

DS: [0:06:09] I guess it depends on how much you pushed. But no, I think at that stage you were sort of assigned. I kind of assumed that I would be assigned to Sam. Maybe I had given some indication that I would want to work with Sam—I can't honestly remember now—but it didn't happen. Maybe they thought it was good for me to do something different. I don't know the reasons, really. But, yes, there was a certain element of assignment. But then going on to Sam later was by choice. I preferred he would take me on at that point.

PC: So you petitioned to change group and that worked?

DS: [0:06:51] There was no group. I petitioned. I spoke to Sam and he agreed. And maybe also to Arvid, who was my supervisor. We didn't have a committee, if that's what you mean by changing group. At least, not that I was aware of. I did serve for a little while on the faculty there, but I don't remember one then either.

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[5] "Arvid Herzenberg" In Susanne Blumesberger, Michael Doppelhofer, Gabriele Mauthe eds., *Handbuch österreichischer Autorinnen und Autoren jüdischer Herkunft* (Munich: K. G. Saur, 2002), p. 540.

[6] A. Herzenberg, D. Sherrington and M. Suveges, "Correlations of electrons in small molecules", *Proc. Phys. Soc.* **84,** 463 (1964). https://doi.org/10.1088/0370-1328/84/4/302 DS: I believe this was the first paper using many-body theory to show the importance of screening and correlation effects in molecules. Previous (unscreened) Hartree-Fock studies predicted π-excitations that were significantly too large.

[7] For instance, G. E. Brown, *Lectures on Many-Body Problems* (Copenhagen: Nordita, 1961).

[8] D. J. Thouless, *The Quantum Mechanics of Many-Body Systems* (New York: Academic Press, 1961),

[9] DS: Actually, 1st year of graduate study in Manchester then was not yet for PhD, but rather for a Diploma for Advanced Studies in Science.

PC: After your PhD,[10] you moved to a group that also has an intricate connection with chemistry, that of Walter Kohn[11], in La Jolla. I couldn't help but notice that you did this only a couple years after he formulated density functional theory[12].

DS: [0:07:33] He was more or less doing it at the same time. It was a year, maybe a year and a half after he invented DFT. There were two of us who worked with Walter as postdocs. The other was Norton Lang[13]. Norton continued with the density functional things, and he did that for a lot of his life after that. So it was going on there, and I was perfectly aware of it, but that's not what I worked on.

PC: How did you get to know Walter? How did you make the choice to go work with him?

DS: [0:08:02] The way this often happens. I consulted my advisors in Manchester about good places to go to, I knew some from the literature—although there was no internet nor any of these other things. Sam recommended, among other people, Elliott Lieb[14], at that time in Northeastern, Vic Emery[15], at Brookhaven National Lab on Long Island, and Walter Kohn[16], at UCSD. All three of them offered me places, but Vic, when I spoke to him about it, said: "Go to California!" And I haven't regretted that; it was a great place[17] Not only was it just after Walter did this kind of

[10] DS: I was not asked about the nature of my PhD studies. However, I consider them relevant to this spin glass history. I then considered the development of a many-body theory for strongly-interacting quantum many body systems without a simple small expansion parameter, using a functional integral Lagrangian formulation (*c.f.* particle field theory*)* with self-consistent expansion around what we termed 'maximal randomness'. S. F. Edwards and D. Sherrington, "A new method of expansion in the quantum many-body problem", *Proc. Phys. Soc.* **90**, 3 (1967). https://doi.org/10.1088/0370-1328/91/2/301 (Our $\partial/\partial\psi$ there plays a role analogous to the $\hat{\psi}$ of the Martin-Siggia-Rose formalism (P. C. Martin, E. D. Siggia and H. A. Rose, "Statistical Dynamics of Classical Systems," *Phys. Rev. A* **8**, 423 (1973). https://doi.org/10.1103/PhysRevA.8.423) as used in later papers on spin glass dynamics.) The Lagrangian formulation is also useful for mapping to auxiliary variables (D. Sherrington, "A new method of expansion in the quantum many-body problem: III The density field." *Proc. Phys. Soc.* **91**, 265 (1967). https://doi.org/10.1088/0370-1328/91/2/301; "Auxiliary fields and linear response in Lagrangian many body theory," *J. Phys. C* **4**, 401 (1971). https://doi.org/10.1088/0022-3719/4/4/002)

[11] Walter Kohn Nobel Prize Biographical Note: https://www.nobelprize.org/prizes/chemistry/1998/kohn/biographical/ (Last consulted January 11, 2021).

[12] A. Zangwill, "The education of Walter Kohn and the creation of density functional theory." *Arch. Hist. Exact Sci.* **68**, 775–848 (2014). https://doi.org/10.1007/s00407-014-0140-x

[13] Norton D. Lang : https://history.aip.org/phn/11604021.html

[14] Elliott Lieb : https://en.wikipedia.org/wiki/Elliott_H._Lieb

[15] Victor Emery : https://en.wikipedia.org/wiki/Victor_Emery

[16] Walter Kohn: https://en.wikipedia.org/wiki/Walter_Kohn

[17] See also, D. S. Sherrington, "A mean Martini" In: M. Scheffler and P. Weinberger (eds), *Walter Kohn*, (Berlin: Springer, 2003), p. 238-240. https://doi.org/10.1007/978-3-642-55609-8_84

work, and also started the excitonic insulator business[18], but it was also very close to the beginning of that university. And that university, of course, was created specially as a reaction to the Russians sending up Sputnik. There were some remarkable people. There were several outstanding faculty, including Nobel laureates on my corridor, where Physics went across to Chemistry[19]. I didn't know at that time that I was working with a future Nobel laureate, but I should say that Walter considered himself as a physicist, even though he did get the Nobel Prize in Chemistry.

PC: You were then recruited from that group to Imperial, in the department led by Bryan Coles,[20] if I understand correctly.

DS: [0:09:48] Bryan was running the solid state physics group there. There had been a small [solid state] theory group, with Seb Doniach[21] and Martin Zuckermann[22]. Martin was leaving and I was offered his job. But before I could go there, Seb decided to take a job at Stanford, and so I was left with nothing. I had to build that group back up again. But, you know, I had been in Manchester all the time. I had been there as an undergraduate, I had been there as a graduate, I had been there as a faculty member. It would have been very easy for me to go back there. It was a good university, it had a very good record, umpteen Nobel prizes of its own. But I thought I needed a challenge, and I thought Imperial was going to be good for such a challenge, and so I went there. I did discover it was a lot more expensive to live there than it was Manchester, but I went there for the challenge and I thought it can't really fail. I was only in my mid-twenties.

PC: How was it, then, to be the sole theorist. You were surrounded by a premier experimental condensed matter group, right?

DS: [0:11:08] When I went there, I had two people that were already engaged and that I was going to train: a Pakistani graduate student and a visitor from Vassar. Both were not particularly fancy places. However, there was some theoretical condensed matter physics going on in the

[18] DS: I worked on excitonic insulators, zero-gap semiconductors, and on effective bosons, Bose condensates and Meissner analogues in Fermi systems.

[19] We note Harold Urey (Chemistry, 1934; https://en.wikipedia.org/wiki/Harold_Urey), Linus Pauling (Chemistry, 1954; Peace, 1962; https://en.wikipedia.org/wiki/Linus_Pauling) and Maria Goeppert (Physics, 1963; https://en.wikipedia.org/wiki/Maria_Goeppert_Mayer).

[20] 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

[21] Sebastian Doniach : https://en.wikipedia.org/wiki/Sebastian_Doniach

[22] Martin Zuckermann left Imperial for McGill University, in Montréal, Québec, Canada: https://www.sfu.ca/physics/people/adjuncts-associates/martinz.html (Last consulted: January 11, 2021)

Mathematics department[23]. A group led by Peter Wohlfarth[24] and including David Edwards and Alex Hewson [was] interested in itinerant magnetism and Hubbard models and these sorts of things. And, in Physics, there was an excellent [so-called] 'Theoretical Physics' group. I was in the 'Solid State' group, but there was an outstanding theoretical particle physics group with Paul Matthews[25], Abdus Salam[26] and Tom Kibble[27], among other people. There were some very good theorists, but they just weren't in my immediate group. But it was a friendly place, and so I got to know all these other people too. There were also good, serious theorists in some other areas. In Manchester, I had been in a theoretical physics subgroup of the physics department; here, in Oxford, we've got a 'Theoretical Physics' Sub-Department; but at Imperial I was in a 'Solid State' group.

PC: To what extent were your scientific interests motivated by the group in which you were embedded at Imperial? Or was it pretty disconnected overall?

DS: [0:12:36] Well, you've seen that I was going from one subject to another, to another. That was part of my general belief that one should learn a number a different things that you then can draw on later, and put together things that may not be available to people in one area all the time. So, yes, I was influenced by the kind of things that were being done by the experimentalists, especially these different kinds of magnetism. At the time when I was in UCSD, there was interesting magnetism work, but theoretically it was mainly of a Kondo type of thing, single impurities. At that point in time, people started to become conscious about interacting impurities, at finite concentrations rather than at very low concentrations. One of the most important experimental groups for that was Coles' one. He and I thought in different ways, but he stimulated me to think about several things. I like to get stimulated, and then to go and think about them myself, rather than to follow existing tracks. That had a good influence.

And then, as I said, in the Mathematics department, there were these people that studied itinerant magnetism, and my interest in that was growing too. I suppose I was kind of in-between having earlier worked on

[23] DS: The presence of a larger solid state theory group in Mathematics rather than Physics was a consequence of earlier pressure from Harry Jones, FRS, to keep solid state theory in his Department. 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

[24] Peter Wolfharth : https://en.wikipedia.org/wiki/Erich_Peter_Wohlfarth

[25] Paul Taunton Matthews : https://en.wikipedia.org/wiki/Paul_Taunton_Matthews

[26] Abdus Salam : https://en.wikipedia.org/wiki/Abdus_Salam

[27] Tom Kibble : https://en.wikipedia.org/wiki/Tom_Kibble

systems with good moments—like the heavy rare-earth metals—and then having these other people working on itinerant systems, where the magnetism is acquired by the conduction electrons, and experimentalists like Coles' group, where you have metals that are sometimes of one form or sometimes another[28]. All these things kind of influenced the way that I was thinking. But usually, throughout my career, I've had things happen around me and it's spurred me into thinking about something, and then it goes!

PC: In that context: Coles is often credited for having discovered spin glasses, or, at least, for having named them. Were these materials discussed during your first years at Imperial? Were you aware of them?

DS: [0:14:46] Oh, yes! They were. I knew about them, and also about other types of transition-metal alloys: actinides, lanthanides, all sorts of these things. All these different (mainly local magnetic moment) metallic alloys were talked about. Prior to the experimental work of Cannella[29] and Mydosh[30], these alloys, which were called 'spin glasses', showed susceptibilities that had a rounded peak as a function of temperature. "That's not a phase transition, it's just a glassy slowing down", we thought. But it was clear that there were other things going on which were novel in some other way. For example, it was known that these alloys must have some sort of freezing of the localized magnetic moments over longish periods of time. You could tell that by looking at nuclear experiments, where you measure the nuclear hyperfine field. There's a local coupling, $A\mathbf{I}.\mathbf{S}$[31], between nuclear and electronic spins, which, incidentally, I knew about from my earlier work on rare-earth metals. If the electron spins freeze, then they provide a local moment, so if you use a local probe like a nuclear spin, such as you get in the Mossbauer effect, and observe line splitting, then it tells you that the localized electron moment is not changing over the time that it takes for a state transition to happen on the nuclear spin. That was seen. So there must be electron spin freezing or getting very slow somehow. It was also known that these alloys were not periodic magnets. They didn't show any overall magnetization; they didn't show up any new peaks in neutron scattering. I was exposed to all this

[28] There were also other kinds of experimental solid state physics.
[29] V. Cannella and J. A. Mydosh. "Magnetic Ordering in Gold-Iron Alloys," *Phys. Rev. B* **6**, 4220 (1972). https://doi.org/10.1103/PhysRevB.6.4220 (DS: See, in particular, Fig. 12.)
[30] John Anthony Mydosh : https://hoogleraren.leidenuniv.nl/id/1793
[31] This is the standard notation for the hyperfine exchange, with **I** and **J** the nuclear and electronic spins, respectively, and *A* the magnitude. See, *e.g.*, https://en.wikipedia.org/wiki/Hyperfine_structure.

stuff, and knew about it, but I wasn't necessarily doing a lot with it myself initially.[32]

Mydosh was known to us; I believe he had been a visitor to Coles' group. He did these experiments on gold-iron, and then he decided to do them in a very, very low field. And in a very low field, the susceptibility [as a function of temperature] that was rounded in normal fields became sharp. You can see in his 1972 paper with Cannella they got results with various very, very small field. It's rounded in moderate fields but as one goes down, down, down in a field, you start to see the peak becoming a cusp. A cusp is a signal to a theorist of a phase transition. It's not just some slowing down, messy stuff. It's a phase transition (or, perhaps it's a phase transition). And so that was a eureka moment for people, but one still didn't know how to solve it. My colleague at that time—when I went there I was able to employ a new lecturer and that was Nicolas Rivier—was interested in this and came out with a kind of mean-field theory for it[33], which wasn't right. But it was one of the things that stimulated Anderson to tell Edwards that: "These guys have got this theory. I think it's wrong, so let's think about it again."

I was exposed to these various things, but the first paper I ever wrote on spin glasses was on a kind of unusual thing[34]. Coles was doing experiments not only on metals where the magnetic element has a good moment even in isolation—which is what you have in standard spin glasses -- but also ones where the impurity was something which in pure systems would be an itinerant magnet but which isolated in the host system would not show a moment. It would only show fluctuations. There was a fashion at that time—particularly, for example, among my colleagues in the Mathematics department but also elsewhere in the world—that looking at finite concentration disordered systems one would use a kind of impurity-averaging. It's called the coherent potential approximation, or uniform enhancement, or whatever. Basically, you replace the quenched random alloy by an average system, which is then effectively pure, but had

[32] DS: I believe that the name "spin glass" was coined because in them (1) the spins seemed to 'freeze' but in a non-periodic/directionally 'amorphous' fashion, (2) the 'freezing' was rapid in *T* but not sharp, and (3) the low temperature specific heat was linear in *T*, as in a conventional glass. My own first paper on spin glasses was not published until 1974 (see footnote 34).

[33] K. Adkins and N. Rivier, "Susceptibility of Spin Glasses", *J. Phys. Colloques* **35,** C4-237-C4-240 (1974) https://doi.org/10.1051/jphyscol:1974443

[34] D. Sherrington and K. Mihill, "Effects of Clustering on the Magnetic Properties of Transition Metal Alloys," *J. Phys. Colloques* **35**, C4-199-C4-201 (1974). https://doi.org/10.1051/jphyscol:1974435
DS: This study drew on the methods I had developed earlier during my PhD, for mapping from electron to auxiliary spin density variables. Interestingly, it took on an extra relevance: more recently in connection with homovalent relaxor alloys: D. Sherrington "BZT: A Soft Pseudospin Glass," *Phys. Rev. Lett.* **111,** 227601 (2014). https://doi.org/10.1103/PhysRevLett.111.227601

different parameters. I thought that was boring; I was interested in problems where inhomogeneity was crucial.

At the same time, there was an interest going on in localization. Nevill Mott[35] was a regular visitor to our group at Imperial, and so, from time to time, was Phil Anderson[36]. So I was conscious of the idea that you could have localization. Particularly, Anderson localization was intriguing me. If you have statistical clustering of where the impurities are, then perhaps you can form localized moments. This made me then try to put the two things together.

Coles had already had relevant experiments on alloys like RhCo. Pure cobalt is ferromagnetic, but in isolation in rhodium, it does not have a moment. If you go to larger concentrations it shows spin-glass—like behavior. Where does it come from? The answer, in my view, is that the cobalt are distributed randomly, but, as we know, statistically there will be regions where there are more, and regions where there are less. The regions where there is a high enough density of cobalt effectively 'nucleate' so that it is like a piece of cobalt, rather than just an atom of cobalt, and can form a cluster moment.

I tried to think about this in the context of one of these alloys, and argued that you should have these clusters, and the clusters will then further interact and, just because these are metallic systems with effective oscillating interactions, they could be expected to have a spin glass possibility too. Although I didn't, at that time, know the theory of how we got the conventional spin glasses, I could figure this by analogy. The way I thought about it mathematically was in relation to some things I had thought about years back when I was doing my PhD on functional integral-based many-body theory. Often, one was interested in what was happening to the collective excitations, and I knew how you could change from formulation in terms of the electron variables to, say, the density variables or other things. In this case they became the local magnetic densities and so on, and so on, and so on. That was the first thing I did. But then, I still hadn't solved the spin glass problem. I only indicated why these particular alloys might become spin glasses, but needed a certain concentration [of solute] to become spin classes.

[35] Nevill Mott: https://en.wikipedia.org/wiki/Nevill_Francis_Mott

[36] Anderson held a 2/3 Visiting Professorship at Cambridge 1967-75. See, *e.g.*, https://www.nobelprize.org/prizes/physics/1977/anderson/biographical/ (Last consulted January 11, 2021)

Then, Edwards moved from Manchester to Cambridge but he was also head of our research council. It was called the Science Research Council[37]. It was Britain's analogue of the [US] National Science Foundation, covering all the sciences, and he was its head. So he had to go into London to run this outfit, but he wanted people to talk physics to. So there were two things that he did. One is that he'd go back into the Cavendish in Cambridge on the weekends and talk to Phil Anderson. And—as you've probably read in the tribute book I co-edited[38]—Phil suggested the spin glass to him as a problem to think about on the train to London, and so it went. But, I surmise, he also wanted someone else to talk to about it. He was in London and I was in London, and we knew one another, we got on rather well and we thought in similar fashions, and so he'd talk to me. That's how I learnt about it. But the paper, if you look at it—when I look at it—it was very difficult to read. It was all kinds of different things on every page. I had one thought, which was to try to see if there is any way…

PC: Before we dive in there, I just wanted to make sure we don't skip over anything. You said Sam would come and visit you at Imperial, or you'd meet in town, or you'd go to his office?

DS: [0:23:45] I don't remember precisely. He had a very, very bright graduate student in Cambridge whom he wanted to see a little more frequently than he would be able to do ordinarily, and so he had this guy come and base with me. I was his kind of nominal supervisor at Imperial, but Sam was his real supervisor, and so they would get together. I think that in that case it's mostly he—his name is Mark Warner[39], and he's doing really well—who would go to State House, or whenever, and talk to Sam there. I think I went a couple of times. I suppose what I really mean is that every now and then if Sam wanted to try out something on somebody else, then he would come and see me. He had this new idea, sufficiently different from anything other people were doing at that time. I can't remember exactly how it happened, but I do seem to recall that he told me about it in front of the blackboard in my office, or another room nearby. I think I went to his place too, but… I think it was an escape from this place. I suspect he was quite happy to escape from his administrative duties to somewhere that was more scientifically real.

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[37] The Science Research Council (SRC) was set up in 1965. Prof. Edwards was at its head starting October 1, 1973, until 1977. See, *e.g.*, https://en.wikipedia.org/wiki/Research_Councils_UK; "Prof. Sam Edwards" *Quest: The House Journal of the Science Research Council* **6** (July) (1973). (Source: http://www.chilton-computing.org.uk/acl/associates/politics/edwards.htm, last consulted : Nov 28, 2020)

[38] P.W. Anderson "Remarks on the Edwards-Anderson Paper" In: *Stealing the Gold: A celebration of the pioneering physics of Sam Edwards*, P. Goldbart, N. Goldenfeld, and D. Sherrington eds. (Oxford: Oxford University Press, 2004). https://doi.org/10.1093/acprof:oso/9780198528531.003.0014

[39] Mark Warner: https://www.phy.cam.ac.uk/directory/warnerm (Last consulted January 11, 2021)

PC: The first paper that I could find that you wrote that was related to the Edward-Anderson (EA) paper is a paper with maybe a postdoc of yours Byron Southern[40]. And it came out in the same issue as the EA paper.

DS: [0:25:33] When Sam told me about his stuff, it sent me thinking kind of immediately. Remember, Sam's paper was based on what is one of the simplest possible exchange distributions one could think to look at. I imagine he thought it's really the competition between positive and negative exchange interactions that's the important thing. So he chose to use a symmetric Gaussian-distributed *P(J)*. Now, I haven't talked to him about why he did that, but my guess from what I do know about him is that he did it because, first of all, he didn't want any other questions like: "Possibly it could be a ferromagnet, possibly it could be a straight antiferromagnet." It couldn't be if you took a symmetric distribution, it had to be something new. And, secondly, as we both knew, Gaussians are good things to work with. That's why I think he did that.

But remember, I was surrounded by an experimental group, who were looking at real alloys. One aspect real alloys have is different interactions, but that's not the important one. One of the features that they would find is—say, for something like copper-manganese or gold-iron—it's a spin glass at low concentrations, but becomes a ferromagnet at higher concentrations. What that means is that the actual phase clearly depends upon a balance between the variance of the effective interactions and their mean. If I was to get anywhere close to explaining this transition to my experimental colleagues, we had to have the possibility of a mean that is not zero as well. That was the first thing that Byron and I looked at. We did then also look at other kinds of hand-waving things, simpler mean-field theories, but without any definite proof.

What I wanted to do, really—and what I did do—was to find something for which I could apply a kind of test of other approximations Sam was making, by looking at a problem which should be exactly solvable. I had enough background to know what that might be. So I tried it, I went through all the motions, I showed that if I carried out these operations I would essentially obtain the same equation as Sam had got, albeit for an Ising system, which is what I looked at first, an Ising system just because my aim was to test the mathematical and conceptual steps of EA with minimal complication, yet maintain the important spin glass consequences. I suspect that if Sam

[40] D. Sherrington and B. W. Southern, "Spin glass versus ferromagnet" *J. Phys. F* **5**, L49 (1975). https://doi.org/10.1088/0305-4608/5/5/003; 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

had had a slightly longer train journey he'd have recognized that the simplest things would be Ising spins[41]. So I got all the equations. For a little while, I was saying: "Well, that's fine, it's no big deal." It was Scott, when I saw him, who said: "Let's actually calculate what these things give and draw some pictures, and so on." That was important in propagating the model and solution.

The other thing was the fact that I serendipitously chose Ising spins. I chose them because I thought they were simpler. But by choosing Ising spins, we were able to recognize that even though many of the results looked right, there was a serious problem, namely that the entropy was negative at T=0, which cannot occur for discrete spins. Had we done it for continuous spins—classical vector spins as Sam did—we would have gotten a negative entropy again, but we wouldn't have been worried because that's a standard pathology, and nobody would have cared. So I think that was somewhat serendipitous, but it was important, and I suspect that this is one of the things that drew the attention of some of the other people who went valuably further, like de Almeida and Thouless, Parisi, and so on.

PC: Before we move on, I'd like to talk a bit more about the genesis of that work. You started this work while you were Imperial, on your own, and then serendipitously as well you had organized a sabbatical at IBM to collaborate with Stephan von Molnár[42].

DS: [0:30:03] Yes. I can't remember now, quite what the time frames were, but it was about time to go to there. Stephan came to visit Bryan Coles and interested me in a problem which he had. Maybe it was during that year that we talked about going, that I would quite like to go and see Scott. Scott had visited. I knew him, and I knew that he was a good computer guy. It seemed to me absolutely clear that someone had to do some computer simulations on this problem, but I couldn't do it myself. Maybe I could, but I wouldn't have done it. I think it must have been during that year that I arranged with Stephan that I'd go to IBM[43]. I don't remember precisely how much in advance it was, but since I think he was just visiting me for that year before I went, it must have been during that period.

You mentioned the idea that I was looking forward to a sabbatical, but in fact Imperial had no formal sabbaticals. There was no sabbatical allowance, but they would let me go if I could get paid by somebody else.

[41] DS: Actually, he probably did, but chose three-dimensional vector spins to be closer to existing experiment.

[42] Stephan von Molnár: https://en.wikipedia.org/wiki/Stephan_von_Moln%C3%A1r

[43] Von Molnár held a Senior Research Fellowship of the Science Research Council Imperial during the 1973-74 academic year. (Source: von Molnár CV, July 5, 2013).

It may have been the fact that Stephan was able to offer me the financial support to go, that I was able to then persuade the Physics department that I could do it. But he only provided six months, so I also arranged with the ILL[44] for the remaining part.

PC: We'll get to that. First, you said that you knew Scott Kirkpatrick, because he had visited you. Could you tell us a bit more about these circumstances? How did you get to know him and his work?

DS: [0:31:54] I can't actually remember. He was doing things that were sort of interesting, so I read things in the literature. He came over at some point, and gave us a talk, and we went to lunch, that kind of things. I don't remember precisely how it came, but I felt I knew him well enough. Many of these things, this progress, has depended on chance encounters, which could or could not have happened, but when they did they formed a link and that link was conducting enough for something to come from it. For example, sometime further I went to Schlumberger-Doll[45] for a short period. And while I was there Nicolas Sourlas also visited. I had an apartment already, waiting for my wife and kids to come during the summer. He didn't have anywhere, so we joined one another, and we got to know one another very well, and we were in the same group, and that spread out further on, and so on. But you know, this is how things happen. It must happen to you, to everybody, I think, these kinds of interactions. I can't remember precisely.

PC: So you showed up at IBM, and you had already worked through a lot of the SK properties. Is that correct?

DS: [0:33:31] Yes. I had pretty much done all of the equations. I did not actually calculate the entropy, but I had got to the self-consistent equations that you would have to solve.

PC: What was left? What did you do with Scott? What was Scott's contribution to that work?

DS: [0:33:50] First of all, he persuaded me that it was interesting, and worth publishing. Secondly, when we did the plotting of various things, they showed interesting behavior, which seemed to agree with some parts of what was known experimentally. There were some which were a little bit different. And then, of course, we stumbled on this entropy at T=0. I guess

[44] Institut Laue-Langevin (ILL): https://en.wikipedia.org/wiki/Institut_Laue%E2%80%93Langevin

[45] In 1985, Prof. Sherrington went on leave to the Schlumberger-Doll Research Center, then in Ridgefield, CT, USA.

we had the equations for it, and just decided: "Well, let's see what is at the limit of T=0?" (T= ∞, of course, was very easy.) Scott was important, and of course, other things came from it, as bonus, as we know. Like, when we tried to do the simulations, and stuff like this, Scott found that it was very difficult to get the energy to continue to go down and to equilibrate. (Scott did all the computing.) We eventually got to realize that there must be lots of metastable states (for lack of better words), and you had to do something to get out of them. I think I remember one conversation when the idea came up: "Well, why don't we just give it a kick of energy somewhere, and get it over the hill?" I think that must have later stimulated Scott to invent simulated annealing.

I don't regret the fact that I didn't publish it alone beforehand, because… Well, maybe I do in one respect, but I think Scott certainly deserves to be there. Then, the second paper that we wrote, he wrote… Well, at one point, he was going to write it as another SK, and I said "No, let's put this back in alphabetical order, and you go first." It was good. Scott was already getting interested in various computer science things, and so on. Some of the ideas like graph partitioning were already bubbling up.

One regret which I had when I was in Steve's group, though, is that this was a magnetic semiconductor group but we did not demonstrate the first semiconducting spin glass. Previously, all the spin glasses that people had looked at were metals, but there was nothing I could see in what was needed for them to be metallic. Interactions just had to be frustrated. And I knew that some of the chalcogenides are magnetic semiconductors, some of which were ferromagnetic, some were antiferromagnetic (Typically, the next-nearest neighbors were antiferromagnetic, but the nearest neighbors could be either.) and some others were not. (SmS and EuS are magnetic, but SrS is non-magnetic.) So I felt there was a possibility to make some insulating spin glasses, and I was in a group that worked on these materials, where there was someone who could have made those materials, but I couldn't persuade anyone to do it. That first success came from Germany[46]. That was a little peeving.

PC: Can you tell us a bit about the immediate reception that your paper with Scott had. How did the news spread? As you mentioned [in an email] this is pre-arXiv, pre-email…

DS: [0:37:53] That's a good question. I am afraid I don't remember exactly what happened. I mean it did seem to grow, but I cannot remember the

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[46] H. Maletta and W. Felsch, "Insulating spin-glass system $Eu_xSr_{1-x}S$," *Phys. Rev. B* **20**, 1245 (1979). https://doi.org/10.1103/PhysRevB.20.1245

mechanisms. The one thing I do remember about an effect that the paper had, was actually at the APS. That's because IBM was developing a new scientific typesetting system, and Scott used this to typeset the paper. I think the thing that the people at Phys. Rev. Letters were most intrigued by was the typesetting. There was a battle going on that time between Bell and IBM, and he was using this other one.

Then, after it was published, I'm not quite sure. I went away somewhere else then. Certainly the paper had some effect, because I think… While I was at IBM, I went and visited Phil Anderson, and I talked to him about it, so he knew about it. I gave a talk at an APS conference, so people would have known then. And I've heard that David Thouless[47] was of one of our referees, and objected to our original title which was “Exactly solvable models of spin glasses” He said: “It can't be exactly solvable, because it's wrong!” But, of course, it’s the solution that wasn't exact. The model itself was solvable. He accepted ‘solvable’, he wouldn't accept ‘exactly solvable’, but anyway….

There was an Aspen meeting in the summer of 1976, where Phil Anderson, David Thouless and your colleague, Richard Palmer, were all together and came up with their TAP thing, which was an important paper[48]. I wasn't at that, probably because I was in France and I didn't want to add extra perturbations on top of things. I don’t really know the answer. Giorgio Parisi only came in somewhat later[49]. There were de Almeida and Thouless, who were looking at the fluctuations in replica space[50], so were Bray and Moore, who came up with an idea about replica symmetry breaking[51], Pytte and Rudnick[52], and a couple of other people were realizing those things. [Erling] Pytte was also at IBM, so I may well have told him earlier on. But by the time that paper came out, I was gone. I can’t really speak for other people.

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

[48] David J. Thouless, Philip W. Anderson and Robert G. Palmer, "Solution of 'solvable model of a spin glass'," *Philos. Mag.* **35**,593-601 (1977). https://doi.org/10.1080/14786437708235992

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

[50] J. R. L. de Almeida and D. J. Thouless. “Stability of the Sherrington-Kirkpatrick solution of a spin glass model,” *J. Phys. A* **11**, 983 (1978). https://doi.org/10.1088/0305-4470/11/5/028

[51] A. J. Bray and M. A. Moore. “Replica-Symmetry Breaking in Spin-Glass Theories,” *Phys. Rev. Lett.* **41**, 1068 (1978). https://doi.org/10.1103/PhysRevLett.41.1068

[52] E. Pytte and Joseph Rudnick, “Scaling, equation of state, and the instability of the spin-glass phase,” *Phys. Rev. B* **19**, 3603 (1979). https://doi.org/10.1103/PhysRevB.19.3603

PC: You mentioned a couple of times that you went to the Institut Laue-Langevin (ILL)[53], after your time at IBM. Why?

DS: [0:41:01] That's a good question. I fancied going somewhere else, and I quite enjoy France, and there was an opportunity there. I did know a reasonable amount of neutron scattering—you know it's a neutron facility—because of my earlier contacts, here in England, at AERE Harwell[54]. I think I mentioned in one of my notes that, during the summers of my PhD, I used to come down here at Oxford, and join the AERE theory group of John Hubbard[55]. There was also an experimental neutron scattering group at Harwell, whom I got to know, some of the experimentalists at IC were neutron scatterers, often using ILL. I knew several other full-time scientists at ILL, importantly including in the good condensed matter theory group.[56]

There were several other natural connections. Grenoble had (and has) a long strong reputation in low temperature magnetism, including spin glasses, in the CNRS, and in the CENG[57], both right beside the ILL.

I'd already been in Grenoble on short visits, but I've now forgotten the details. The opportunity came, and they would pay for me, I could take a year off. I felt that if I was going to go away I had to get paid. Then, I did go back a second time, but that's because toward the end of the first time—I must had been going on very well with the director—and he asked me if I would come back for a few years. Not permanently. Phillippe Nozières[58] was at that time the head of the theoretical group at the ILL. Philippe was away part of the time when I was there the first time—I actually lived in his house—on leave in America. But then he moved to Paris at the Collège de France, and they thought he would be away, so they're invited me back, essentially to head that group. In practice, Philippe kept his house in Grenoble and he used to come back regularly, and he continued to be there in the institute. But I had a couple of years there[59].

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[53] The Intsitut Laue-Langevin (ILL) is an international external user oriented facility: https://en.wikipedia.org/wiki/Institut_Laue–Langevin

[54] Atomic Energy Research Establishment (AERE): https://en.wikipedia.org/wiki/Atomic_Energy_Research_Establishment

[55] John Hubbard: https://en.wikipedia.org/wiki/John_Hubbard_(physicist)

[56] DS: The theory group at ILL was concerned with solid state physics, not tied to neutron scattering. Its head was Phillipe Nozières, Also there when I went in 1976 were Peter Young, Byron Southern, Stephen Lovesey, Hans Fogedby and Jim Loveluck.

[57] Centre d'études nucléaires de Grenoble : https://fr.wikipedia.org/wiki/CEA_Grenoble

[58] Philippe Nozières: https://en.wikipedia.org/wiki/Philippe_Nozi%C3%A8res

[59] DS: Duncan Haldane and Bernard Derrida were among the members of the ILL theory group during my second term.

PC: Just to make sure I understood correctly: the appointment at Grenoble was never meant to be permanent. You were always supposed to return to Imperial.

DS: [0:43:39] Correct.

PC: You mentioned in passing already the work by Thouless, Anderson, and Palmer. In *Stealing the Gold*, Anderson recalls Scott Kirkpatrick and you visiting his group, and to have him realize that simple thermal annealing was doomed to fail, in his words. How much did you get to know these efforts, Palmer, and that work at the time it was being written? How aware were you?

DS: [0:45:04] I can't quite remember. I mean I think I got to know of if it pretty quickly. I think that they sent the paper to me. This was after I had already left IBM. The visits to Phil, he's talking about, were while I was at IBM, and I went and stayed with Phil in his flat in Princeton. But I can't remember all the details of what we talked about. I can't remember precisely how quickly, but things flowed around reasonably quickly. The post worked, I mean. We may not have had the internet, but we were in contact. I worked on different things when I was at ILL.

PC: The first gathering, maybe, on the theme of spin glasses, you mentioned, is the Aussois conference on Glasses and Spin Glasses[60]. How important was this meeting?

DS: [0:45:23] Actually, I don't remember that this particular one was so important for me. But it was it was useful for making contacts and stuff like that. As I keep telling you, my memory is poor. There were various people I met at that time, and it was good occasion for me to make extra contacts, with Benoy Chakraverty, for example, from the CNRS. The two Heidelberg meetings[61] were much more ones where everybody was seen. In terms of what they did with respect to transmitting ideas from one place to another, I don't remember all of them, but they were very useful for bringing us together as a group.

[60] "Glasses and Spin Glasses - Low Energy Excitations in Glasses and Disordered Magnetic Substances" March 22- 25, 1977, in Aussois, France. See, *e.g.*, "Conferences" *Europhysics News* **7**(11), 3 (1976).
[61] Proceedings of a Colloquium held at the University of Heidelberg 30 May – 3 June, 1983: *Heidelberg Colloquium on Spin Glasses*, J. L. van Hemmen and I. Morgenstern eds. (Berlin: Springer-Verlag, 1983). https://doi.org/10.1007/3-540-12872-7; Proceedings of a Colloquium on Spin Glasses, Optimization and Neural Networks, Held at the University of Heidelberg, June 9-13, 1986: *Heidelberg Colloquium on Glassy Dynamics* J. L. van Hemmen and I. Morgenstern eds. (Berlin: Springer-Verlag, 1987). https://doi.org/10.1007/BFb0057505

PC: Speaking of that: the following year there was a Les Houches school on ill-condensed matter that you participated in[62].

DS: [0:48:10] I only participated very briefly. I gave a seminar. Scott gave a lecture. It was obviously important. I wouldn't have minded being there for the whole thing, but I don't think I was invited. I don't remember very well. Anyways, what I talked about there was something slightly different[63]. I do remember that, I think, because I was concerned about spin waves in spin glasses. I found them difficult to work out, and I was never completely convinced by Halperin and Saslow[64]. But I then tried to look at some other cases where you might have some analogies.

My difficulty with spin glasses is knowing what the ground state is, to then look at the perturbations away from it that are spin waves. On the other hand, however, there is a problem I sort of invented, where you take a Mattis spin glass[65]. If you do that for an Ising system it's fine, but if you now say that spins are actually vectors—Heisenberg ones—then you've got a question about the excitations. You've solved the Ising system for thermodynamics by applying a gauge transformation, which changes S_z to $-S_z$, on appropriate sites. On the other hand, for Heisenberg spins that screws up the commutation relations. You get an extra sign in there. It tells you there is something different about the excitations. That's what I wanted to talk about. So I called it something like "Complex excitations on a flat background," or something like that[66]. I don't remember exactly. That was the problem which I talked about. It was just part of my thinking process about excitations.

PC: How closely did you follow the steps that took place between 1975 and 1979? Were you part of the discussions, were these discussed at meetings or were you circulated preprints?

DS: [0:50:47] I was aware. I can't completely remember, to be honest. When things were coming about replica symmetry breaking, it wasn't really until

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[62] Les Houches, Session XXXI, July 3-August 18, 1978. Cf. *La Matière mal condensée/Ill-Condensed Matter*, Ed. R. Balian, R. Maynard, G. Toulouse (Amsterdam: North-Holland Publishing, 1979). https://doi.org/10.1088/0022-3719/10/1/002

[64] B. I. Halperin and W. M. Saslow, "Hydrodynamic theory of spin waves in spin glasses and other systems with noncollinear spin orientations," *Phys. Rev. B* **16,** 2154 (1977). https://doi.org/10.1103/PhysRevB.16.2154

[65] D. C. Mattis, "Solvable spin systems with random interactions," *Phys. Lett.* **A56**, 421 (1976). https://doi.org/10.1016/0375-9601(76)90396-0

[66] D. Sherrington. "Excitations in the Heisenberg-Mattis model: Non-trivial oscillations on a flat background"; in Proc. 23rd Les Houches school on Theoretical Physics, "La matière mal condensée/ III condensed matter", eds. Balian et al (North Holland 1979); "Long-wavelength dynamic response of the Heisenberg-Mattis model " *J. Phys. C* **12**, 5171 (1979). https://doi.org/10.1088/0022-3719/12/23/023

Parisi came on the scene when I started to take some serious note. I knew about TAP and de Almeida and Thouless, Bray and Moore, sorts of things. I don't think I had any brilliant idea about RSB. Once Parisi did come up with his scheme in 1979, then I could see that there were some interesting things to do.

Later, I wrote a number of papers where I've looked at other models, where you have more complicated aspects of the Parisi behavior, *e.g.*, other aspects with respect to whether you have replica symmetry, or an Almeida-Thouless line, or a Gabay-Toulouse line.[67] In vector-spin systems, you have a Gabay-Toulouse line, which is fairly straightforward to think about. It's just the spin glass freezing in the transverse direction. Then, GT also considered a de Almeida-Thouless/RSB type of transition by just looking at the longitudinal terms. I realized there are couplings between the transverse and the longitudinal RSB terms such that longitudinal, as well as transverse, RSB already onsets at the GT transition with only a crossover of strength near the quasi-AT line [68]. I worried about these sorts of things. It just depended on what I could do, you know, at the time.

I wasn't tied especially to the spin glass problem. I was just intrigued by the other things it led to, like—going on later—to things like hard optimization in other contexts, to graph partitioning, to neural networks, to all this sort of stuff.

PC: We will get that. I want to discuss the series of models you mentioned. What I was trying to understand while looking at the papers is what was your ultimate motivation. Was it my mainly statistical mechanics-driven, or was it mainly materials-driven? Looking at the Potts spin glasses, the quadrupolar spin glasses, the anisotropic spin glasses, what were you trying to probe? What was the overarching program?

DS: [0:53:24] [Consider first] Potts glasses[69]. Ordinary spin glasses were complicated, curious things. They also have a special feature of symmetry. In the Ising (or the Heisenberg) spin case, if you have an interaction

[67] M. Gabay and G. Toulouse, "Coexistence of Spin-Glass and Ferromagnetic Orderings," *Phys. Rev. Lett.* **47**, 201 (1981). https://doi.org/10.1103/PhysRevLett.47.201

[68] Dinah M. Cragg, David Sherrington and Marc Gabay, "Instabilities of an m-Vector Spin-Glass in a Field," *Phys. Rev. Lett*. **49**, 158 (1982). https://doi.org/10.1103/PhysRevLett.49.158

[69] D. Elderfield and D. Sherrington, "The curious case of the Potts spin glass," *J. Phys. C* **16**, L497 (1983). https://doi.org/10.1088/0022-3719/16/15/003; "Spin glass, ferromagnetic and mixed phases in the disordered Potts model," *J. Phys. C* **16**, L971 (1983). https://doi.org/10.1088/0022-3719/16/27/005 ; "Novel non-ergodicity in the Potts spin glass," *J. Phys. C* **16**, L1169 (1983). https://doi.org/10.1088/0022-3719/16/32/006; D, Sherrington, "Potts and Related Glasses," *Prog. Theor. Phy. Supp.* **87**, 180 (1986). https://doi.org/10.1143/PTPS.87.180

between a pair of spins which is ferromagnetic they both want to point in the same direction, antiferromagnetic that they want to point in opposite directions. They're both definite statements. There's a symmetry of definiteness. On the other hand, if you've got a Potts model[70], ferromagnetic means the Potts variables prefer to be both the same, whether that, that or that, but if it's antiferromagnetic then the preference is for the pair to be anything other than both the same. There's no balance. There's no longer any symmetry of definiteness. It's either "do this", or "don't do this", as opposed to "do this" or "do that". I thought that must surely do something, and that's what drove me to look at the Potts model[71]. When we looked, we discovered first of all that various different things happened as a function of the Potts dimension, especially concerning the Parisi $q(x)$. It was leading to something special when you got to Potts dimension four, which was probed further by Gross, Kanter and Sompolinsky[72]. But even before that we found unusual things, also with quadrupolar glasses[73], which again lack symmetry of definiteness. Partly with these studies I was asking: "Are there other interesting things that occur?" That study was just based on pure philosophizing, but it was fruitful.

For the anisotropic glasses[74], I knew that there were materials which were anisotropic and so on, and so it was largely driven by: "what other things are there that my experimental colleagues can look for?"

Similarly, when it comes to a vector spin glass, there's a difference between a sharp transition at the Gabay-Toulouse transition and a more rounded quasi-de Almeida-Thouless one.

I'm often driven by asking: "What if it's different from whatever we normally get?" Is it important? Sometimes it is, and sometimes it's not.

There are things which I didn't notice at the time, but which perhaps I should have been ready for. For example, having thought about looking, early on, at systems that could be either a spin glass or a ferromagnet, then

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[70] DS: $H = -\sum_{(ij)} J_{ij}\ (m\delta_{p(i)p(j)}$ -1); *p=1,..m*

[71] DS: There is a similar lack of symmetry of definiteness for quadrupoles, and hence for quadrupolar glasses; Paul M. Goldbart and David Sherrington, "Replica theory of the uniaxial quadrupolar glass**"** *J. Phys. C* **18**, 1923 (1985). https://doi.org/10.1088/0022-3719/18/9/026

[72] D. J. Gross, I. Kanter and H. Sompolinsky, "Mean-field theory of the Potts glass," *Phys. Rev. Lett.* 55, 304 (1985)

[73] P. M. Goldbart and D. Sherrington, "Replica theory of the uniaxial quadrupolar glass," *J. Phys. C* **18** 1923 (1985). https://doi.org/10.1088/0022-3719/18/9/026

[74] See, *e.g.*, Dinah M. Cragg and David Sherrington, "Spin-Glass with Local Uniaxial Anisotropy," *Phys. Rev. Lett.* **49**, 1190 (1982). https://doi.org/10.1103/PhysRevLett.49.1190

it's not so far to go to the problems of neural networks, where you have some spin glass states, but also have a lot of different quasi-ferromagnets, which are the different memories that the thing will settle in. So it's no great surprise to me that you could have that. The interesting thing from the point of view of memories is how many can you have before you can't store them. The interest from spin glass is slightly different.

Sometimes I'm motivated by experimental observations, sometimes it's just by thinking about them, sometimes the one can lead to the other. I am a bit of a butterfly.

PC: It's perfectly fine. It makes for a great career. I want to get to neural networks, but first I want to ask about the work that you did at Schlumberger, when you started a collaboration with Nicolas Sourlas[75]. That's the leave that you took in 1985, and that's when you started to work on graph bi-partitioning. Can you tell us how are you came to that research direction and the realization that it's related to spin glasses? [In your notes,] you mentioned the work of Fu and Anderson[76]. Was that the motivation? Or was there something else you had figured out?

DS: [0:57:27] It was a motivation partly in the sense that we'd already thought of the things that Fu and Anderson did, but we hadn't published anything, so we had to do something different. I think Scott and I already knew about the extensive Fu-Anderson case, because basically it's just the SK model with slightly different ways to get the distribution and in the SK model all that matters for the distribution are the first and second moments.

I think it's probably talking to Nicolas that came up with this, but we just thought: "Well, you know, what happens if it's dilute?" I suppose we also perhaps had in our mind the work of Viana and Bray who had recently looked at some diluted spin glasses too[77]. They were random networks—Erdős–Rényi networks with an average finite number of connections per spin—but we were thinking more in terms of ones where the vertices were all of the same finite valence. I guess we could have done that [Erdős–Rényi], but then we were probably conscious—I don't remember for certain now—if you check each possible connection only probabilistically,

[75] J. R. Banavar, D. Sherrington and N. Sourlas, "Graph bipartitioning and statistical mechanics," *J. Phys. A* **20**, L1 (1987). https://doi.org/10.1088/0305-4470/20/1/001
[76] G. Baskaran , Y. Fu and P.W. Anderson, "On the statistical mechanics of the traveling salesman problem," *J. Stat. Phys.* **45**, 1–25 (1986). https://doi.org/10.1007/BF01033073; Y. Fu and P. W. Anderson, "Application of statistical mechanics to NP-complete problems in combinatorial optimization," *J. Phys. A* **19**, 1605 (1986). https://doi.org/10.1088/0305-4470/19/9/033
[77] L. Viana and Allan J. Bray, "Phase diagrams for dilute spin glasses," *J. Phys. C* **18**, 3037 (1985). https://doi.org/10.1088/0022-3719/18/15/013

then there will be some instances of things which are disconnected completely. Small probability, but there will be some. On the other hand, if you take it that every single one is connected to a certain number of other ones, but that number is not extensive, finite, then you got another rather interesting problem. As you know, there's been a lot of work, later on, on these random graphs, as they call them. I guess we felt that the high connectivity situation was bound to be similar to the SK mode, but for lower connectivity it wasn't clear whether it would be or it wouldn't be. We couldn't solve it analytically, so we looked at various things simulationally, and found features[78] which you would find in spin glasses. Some of those things came from Nicolas's experience in particle physics beforehand, like using Dalitz plots[79], for example. [80]

PC: This brings up a question about which we've touched upon already a couple times, namely the relationship between your work and computational efforts. Obviously, there was the collaboration with Scott Kirkpatrick. In 1979, in a small piece for Physics Bulletin you wrote that computer simulations were instrumental in showing what is going on in disordered systems[81]. What other simulations than Kirkpatrick's were you following at that time? Were you ever personally involved in those simulations? If yes, to what extent?

DS: [1:00:31] The only other simulations that I think I was directly involved in were later, *e.g.,* with Peter Young and John Olive for vector spin glasses[82], and with Khanna[83] and McLenaghan[84] on amorphous, randomly positioned and tunably frustrated antiferromagnets.

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[78] DS: Such as self-averaging of the cost, ultrametricity for large enough connectivity, and non-trivial overlap distribution.

[79] R. H. Dalitz, "On the analysis of τ-meson data and the nature of the τ-meson," *Phil. Mag.* **44,** 1068 (1953). https://doi.org/10.1080/14786441008520365

[80] D.S. Later, I studied the intensively connected random-net spin glass and bi-partitioning problems analytically, with my postdocs Michael Wong and Werner Wiethege and student Peter Mottishaw. (See, *e.g.,* K. Y. M. Wong *et al*., "Graph partitioning and dilute spin glasses: the minimum cost solution," *J. Phys. A* **21**, L99 (1988). https://doi.org/10.1088/0305-4470/21/2/007) : and including the first attempt at replica symmetry breaking in the intensive case (K. Y. M. Wong and D. Sherrington, "Intensively connected spin glasses: towards a replica-symmetry-breaking solution of the ground state," *J. Phys. A* **21**, L459 (1988). https://doi.org/10.1088/0305-4470/21/8/006).

[81] David Sherrington, "Spin Glasses," *Phys. Bull.* **30**, 477 (1979). https://doi.org/10.1088/0031-9112/30/11/025

[82] J. A. Olive, A. P. Young and D. Sherrington, "Computer simulation of the three-dimensional short-range Heisenberg spin glass," *Phys. Rev. B* **34**, 6341 (1986). https://doi.org/10.1103/PhysRevB.34.6341

[83] S.N. Khanna and D. Sherrington, "Computer simulation of an amorphous antiferromagnet," *Solid State Comm.* **36**, 653-55 (1980). https://doi.org/10.1016/0038-1098(80)90107-6

[84] I.R. McLenaghan and D. Sherrington, "The homogeneously random Ising antiferromagnet: A computer study," *J. Phys. C* **17**, 1531-37, (1984). https://doi.org/10.1088/0022-3719/17/9/011; "A model for

I was earlier also intrigued by the simulations, by others, looking at field-cooled and zero-field cooled susceptibilities and IRM versus TRM, which showed one of the other features which is characteristic of spin glasses. For the susceptibility, as you know, it does not matter which way you measure it if the temperature is higher than the spin glass temperature T_c. But beneath T_c you get different things depending upon whether you cool first or put the field on first. There were some nice simulational experiments on these things, most of which were done by the Germans, I think Kinzel and Binder, or people like this[85]. They helped to build up the picture of what was going on.

Originally, many of the simulators were looking to see: "Does EA reasonably reproduce the experimental results?" That was the most important thing at the beginning[86]. The experiments were done on systems which had site order and interactions which were frustrated as a function of distance. The theory has almost exclusively been done on problems which have exchange disorder, following Edwards and Anderson[87]. It seemed to me important, in trying to access whether that's a sensible thing, to do the experiments—computer experiments in this case—on the random exchange disorder model systems. I think those were very important in trying to show it was the same physics.

As well as being able to perform valuable 'experiments' on the idealized models of theory, computer simulation has shown its value in being able to measure quantities and features that are not accessible with normal experiments, such as local correlations in time and clear evidence of what became known as 'rugged energy landscapes', all helping in the quest for understanding. More recently than your 1979 cut-off, many very useful new (and subtle) computer techniques have been developed[88] and applied more widely.

PC: You did, around 1988, start to work on neural networks as well. In the notes you sent us, you wrote that ultimately Hopfield was the motivation for this. But there's a long path between Hopfield and your work. What more immediately was driving this new direction for you?

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variable topological frustration," *J. Phys. C* **20**, 1701-1711 (1987). https://doi.org/10.1088/0022-3719/20/11/013

[85] See, *e.g.* Section 5B in K. Binder and A. P. Young, "Spin glasses: Experimental facts, theoretical concepts, and open questions," *Rev. Mod. Phys*. **58,** 801 (1986). https://doi.org/10.1103/RevModPhys.58.801

[86] DS: This is what I originally wanted to do with Scott.

[87] DS: Also mostly on Ising models.

[88] DS: Such as Binder plots and simulated tempering.

DS: [1:04:19] The part of Hopfield that I'm thinking on is his introducing of a model where you have a Hamiltonian, $J_{ij}s_is_j$, where s is plus or minus one, depending on the neuron firing or not firing, and the J_{ij} are given by the Hebbian formula[89], which is like SK, except looking at 'retrieval solutions'[90]. I didn't do a lot on that, but from what I mentioned to you earlier it seemed very reasonable that it's a kind of extension of SK, or Sherrington-Southern (SS), where you add something other than the (quenched random) deviations +/-J. You also add some means. The analogue of the SS/SK mean in the Hopfield case is slightly more complicated, the stored memories are analogues of the ferromagnet[91] [92]. So that's one thing. Then there was the important work by Derrida[93] and Gardner[94] in seeing there being an optimization problem[95], where you can try to calculate what is the maximum number of patterns you can stored by varying the $\{J\}$ [96]. And then, by extending the model, what is the maximal number of patterns you can store within a certain error, up to a certain error. So it also provided a lot of interesting problems of that kind.

I didn't do so much on Hopfield's model, other than the things I've just described, but at Imperial there was also a group in Electrical Engineering that was interested in artificial neural networks, which were Boolean networks, where the information was stored in Boolean functions, rather than in synapses[97]. So Wong and I started to play and get interested in those[98]. We did a number of things on neural networks which were complementary to those that were done by Hopfield. Conceptually, the

[89] DS: $J_{ij} = \sum_\mu \xi_i^\mu \xi_j^\mu;\ \xi = \pm 1$

[90] J. J. Hopfield, "Neural networks and physical systems with emergent collective computational abilities," *Proc. Nat. Acad. Sci. U.S.A*. **79**, 2554-2558 (1982). https://doi.org/10.1073/pnas.79.8.2554

[91] DS: Essentially, each stored pattern μ is a locally gauged ferromagnet and once you are inside its basin of attraction you can think of it as providing an analogue of the mean J_0 in SK while the other patterns provide an analogue of J in SK. The complication comes when the number of patterns is extensive.

[92] Daniel J. Amit, Hanoch Gutfreund and Haim Sompolinsky, "Storing Infinite Numbers of Patterns in a Spin-Glass Model of Neural Networks," *Phys.Rev.Lett.* **55,** 1530 (1985). https://doi.org/10.1103/PhysRevLett.55.1530

[93] Bernard Derrida: https://en.wikipedia.org/wiki/Bernard_Derrida

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

[95] E. Gardner and B. Derrida, "Optimal storage properties of neural network models," *J. Phys. A* **21**, 271 (1988). https://doi.org/10.1088/0305-4470/21/1/031

[96] DS: In this problem, the quenched random stored patterns are the analogues of the EA/SK quenched random interactions $\{J_{ij}\}$, while the synapses $\{J_{ij}\}$ are the variable analogues of the EA/SK spin states.

[97] DS: Only later did I learn of Kauffman's Boolean (n,k) model: Stuart Kauffman, "Metabolic stability and epigenesis in randomly constructed genetic nets," *J. Theor. Biology.* ***22, 437*** (1969). https://*doi.org/10.1016/0022-5193(69)90015-0*

[98] See, *e.g.*, K. Y. M. Wong and D. Sherrington, "Storage Properties of Randomly Connected Boolean Neural Networks for Associative Memory," *Europhys. Lett.* **7**, 197 (1988). https://doi.org/10.1209/0295-5075/7/3/002; "Theory of associative memory in randomly connected Boolean neural networks," *J. Phys.* A **22,** 2233 (1989). https://doi.org/10.1088/0305-4470/22/12/022

models are very similar. We also went through various stages of looking at analogies between neural networks and spin glasses.

But if you think about any kind of real—or a bit more real, shall I say—neural network, you'd have a system where there are neurons which react to the synapses as they are. They react very quickly, they have a fast dynamics. Interactions in spin glasses are fixed. They are random, but fixed. In a neural system, the synapses are not fixed, they can be changed, but they change on a much slower time scale. My picture of a quasi-model brain has neurons which have some sort of fast dynamics, synapses which have some slower dynamics. The neurons—when you're trying to recall something—respond quickly to whatever the synapses happen to be at that time. On the other hand, if you're going to learn, then you expose yourself to some 'images' for longer time—I keep looking at the picture of you—it fires various neurons, because it's effectively an applied field. That polarization of those neurons then feeds back on a slow dynamics of the synapses, and they gradually build up, so we learn that way. That's my picture.[99] I've never seen anybody actually carry that all the way through, but that's my picture. Fast neurons, slow synapses.

If you want to make it a little bit closer to replicas, then you can also say that there is noise in these two lots of dynamics[100]. The noise is an analogue of temperature, and so there are different effective temperatures for the neurons and for the synapses. The ratio of those two temperatures plays a role rather like n in the in the replica theory; and $n \to 0$ is what corresponds to the situation where the synaptic temperature is very high, so it doesn't care what the neurons are doing. The $\{J\}$ are just random, but the neurons still have to react quickly. So it seems to me that there are further conceptual ties between spin glasses and neural networks. There have been quite a few studies—although I haven't followed all of them—on the dynamics of neural networks, but almost completely, as far as I'm aware, in terms of the fast variables—the neurons, or the spins—while is I think there should be some study also of the dynamics of the synapses, which will be slower, and presenting various patterns to it.

PC: After the postdoc who worked with you, Wong, left, it seems that you also left the field of neural networks, is that fair? And if yes, why?

[99] R. W. Penney, A. C. C. Coolen and D. Sherrington, "Coupled dynamics of fast spins and slow interactions in neural networks and spin systems," *J. Phys*. A **26**, 3681 (1993). https://doi.org/10.1088/0305-4470/26/15/018

[100] A. C. C. Coolen, R. W. Penney and D. Sherrington, "Coupled dynamics of fast spins and slow interactions: An alternative perspective on replicas," *Phys. Rev. B* **48,** 16116 (1993). https://doi.org/10.1103/PhysRevB.48.16116

DS: [1:10:27] I left the field of neural networks, yes. I did quite a bit of things and interacted with a lot of people during that time. Then, I decided that I'd been doing it for long enough. I was getting loads and loads of graduate students applying to me to do it, but I was getting sick of it. I wanted to do something else. So I told them: "I'm sorry, I'm not doing any more neural networks." But Wong kept on doing some of these things, and then drove it on further, *e.g.,* to traffic flows and communication systems. The other thing was that in neural network, it was getting at the stage where many of the people who were working on it wanted to become more and more biologically relevant. They were all moving further over towards real neuro-stuff. People like Sompolinsky[101], and so on. I told you, I'm a physicist. I think like a physicist. I didn't want to be a biologist. I love to take biological problems, but I'm one of those 'arrogant' physicists, who think they know how it goes, but only so far.

PC: At about the same time, American physicists Kirkpatrick[102], Thirumalai[103] and Wolynes[104] [105], took upon your ideas from Potts spins glasses and developed RFOT as a theory of glass formation. How aware were you of that work? Did you follow it? Were you involved?

DS: [1:11:56] I was aware of it. I'm not sure if I didn't first hear Wolynes talk about it at Aspen. He was basing it on the Potts glass, and I had suggested that, so I knew something. I knew that in the Potts, one can also have some first-order, discontinuous transitions. I knew about them, but I didn't do a lot about it.

[Aside: Another topic that Wolynes is renowned for in is protein folding. I spent the a year as a Ulam Scholar at Los Alamos, in 1995-96, when the Director of CNLS[106] was Hans Frauenfelder[107], who is a very famous protein biophysicist. So I was aware of his picture of a hierarchical structure of

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[101] Haim Sompolinsky: https://en.wikipedia.org/wiki/Haim_Sompolinsky

[102] Ted Kirkpatrick: https://umdphysics.umd.edu/people/faculty/emeritus/item/269-tedkirkp.html (Last consulted, January 11, 2011)

[103] Dave Thirumalai: https://sites.cns.utexas.edu/thirumalai/biography (Last consulted January 11, 2011)

[104] Peter G. Wolynes: https://en.wikipedia.org/wiki/Peter_Guy_Wolynes

[105] See, *e.g.*, T. R. Kirkpatrick and P. G. Wolynes. "Stable and metastable states in mean-field Potts and structural glasses" Phys. Rev. B **36**, 8552 (1987). https://doi.org/10.1103/PhysRevB.36.8552; T. R. Kirkpatrick and D. Thirumalai. "Mean-field soft-spin Potts glass model: Statics and dynamics," *Phys. Rev. B* **37**, 5342 (1988). https://doi.org/10.1103/PhysRevB.37.5342; D. Thirumalai and T. R. Kirkpatrick, "Mean-field Potts glass model: Initial-condition effects on dynamics and properties of metastable states," *Phys. Rev. B* **38**, 4881 (1988). https://doi.org/10.1103/PhysRevB.38.4881;

[106] CNLS: Center for Nonlinear Studies at Los Alamos National Laboratory.

[107] Hans Frauenfelder : https://en.wikipedia.org/wiki/Hans_Frauenfelder

conformational substates[108], and of Peter Wolynes and his folding funnel concept too.]

I was aware of it, but I didn't pursue it far myself. It seemed to be a sensible approach, and I did have a kind of wish later to see if I could work out how to combine, or somehow bring together in their thinking, some of the different models of glasses and other things like this. For example there is all the work, which I was also involved in[109], on constrained dynamics in cases where Gibbsian statistical thermodynamics, which includes access to all state, would not have a phase transition, but rather constraints on the dynamics prevents the system from getting to them. All of these pictures, I suspect, are connected. I discussed possibilities for that with Juan Garrahan, but never got to the end [of it]. There have been a number of things which I've thought about for a while, but never really got them till the end.

PC: In the dynamical scene, I understand that you were an early proponent of the work of Jorge Kurchan[110] and Leticia Cugliandolo[111] [112]. How did you hear about that work, and why did you think it was important at the time, and since?

DS: [1:14:13] Well, they were doing nice things with my model, was one thing[113]. Also, their formulation appealed to me through a similarity to aspects of the functional integral and conjugate field formalisms that I had used in my PhD research.

I knew they were both very bright. I knew that Jorge Kurchan was one of brightest that they had had for a number of years in Argentina. I knew that directly from Argentina, and also from his postdoc adviser, Eytan Domany.

[108] H. Frauenfelder, F. Parak and R.D. Young, "Conformational substates in proteins:, *Ann. Rev. Biophys. Biophys. Chem.* **17**, 451 (1988); H Frauenfelder, S Sligar, P.G.;Wolynes **"**The Energy Landscapes and Motions of Proteins", *Science* 254, 1598 (1991)
[109] *E.g.* Tomaso Aste and David Sherrington, "Glass transition in self-organizing cellular patterns," *J. Phys. A* **32** 7049 (1999). https://doi.org/10.1088/0305-4470/32/41/301; Arnaud Buhot, Juan P. Garrahan and David Sherrington, "Simple strong glass forming models: mean-field solution with activation," *J. Phys A* **36,** 307 (2003). https://doi.org/10.1088/0305-4470/36/2/302
[110] Jorge Kurchan: https://fr.wikipedia.org/wiki/Jorge_Kurchan;
[111] Leticia Cugliandolo: https://en.wikipedia.org/wiki/Leticia_Cugliandolo
[112] Paul M. Goldbart, "David Sherrington as a mentor of young scientists," *J. Phys. A* **41**, 320302 (2008). https://doi.org/10.1088/1751-8121/41/32/320302
[113] L. F. Cugliandolo and J. Kurchan, "Analytical Solution of the Off-Equilibrium Dynamics of a Long-Range Spin-Glass Model," *Phys.Rev.Lett.* **71,** 173 (1993)**;** "On the out-of-equilibrium relaxation of the Sherrington-Kirkpatrick model," *J. Phys. A* **27**, 5749 (1994). https://doi.org/10.1088/0305-4470/27/17/011

I knew about Leticia's work with Daniel Amit[114] on the neural-network types of things.

Also, it looked to me that there were some very nice ideas there. I found them appealing, kind of pretty, elegant. I liked the ideas. I had them come to visit me, I was greatly impressed by them, and so I supported them.

PC: From 1989 on, you became the head of Theoretical Physics at Oxford[115], which you hinted lowered your availability for research, because of the extra service. But it also gave you a bit more power, as well. Have you ever been able to leverage that power, to tilt scale in favor of the study of disordered systems, generally?

DS: [1:15:43] I didn't actually try to do that. I did what I could to build up some of the best people in the world that we could engage from whatever field[116]. I didn't try to build my own empire. I did other things like getting some decent computers in, and encouraging people, and so on. You'll see that a lot of my papers are written alone. I have tended not to build large groups and large collaborations. I think that the people I've been instrumental in engaging in Oxford are very good, and have done very well. I consider interaction within Oxford Theoretical Physics to be one of strengths and I encouraged and practiced it, but I did not insist on my own research interests. I also interacted with some of the people outside of Physics, like some of the people in experimental psychology, and other aspects of the biological sciences, and so on. There is also now a complexity group in our business school[117]. I didn't necessarily feel that I had to persuade people in Oxford. I got some good students and postdocs, but I didn't try and build up a large group of faculty members in my image. I didn't think it was fair.

PC: Absolutely. We were just talking about collaborations. It's true that you did not develop a lot of authorship collaborations, but you did participate in a number of international collaborations that were structuring the field. And leveraging, in a sense, the resources of the European community to do so.

DS: [1:17:43] I guess I did use my 'power' that way.

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

[115] DS: Interestingly, this was the department that Rudolf Peierls moved to midway through my PhD. I succeeded him as Wykeham Professor of Physics, with Roger Elliott in between us.

[116] DS: Within the scopes of our broader research groups and with an expectation of interaction.

[117] CABDyN Complexity Centre at Oxford, where the acronym stands for *Complex Agent-Based Dynamic Networks*.

PC: Could you tell us a bit how the community[118]—the statistical mechanics or the theoretical physics community—was before and after those collaborations. What did it change from the UK standpoint, not necessarily on the full continental scale?

DS: [1:18:07] In the early part of the '70s, there was certainly an awareness of good science going on around various places, but it tended to be dominantly in certain groupings, and there were cross-groupings, with whom they were involved. Imperial had good connections with Orsay, and to some degree with École Normale[119]. Saclay and École Normale had good connections, some of those being on the more mathematical, field theoretical side, but not completely. Our experimentalists had good connections with Genoa, although that didn't continue in this grouping. And, of course, École Normale had very good connections with Rome, with Giorgio Parisi, and so on. They were often contacts that had come from being postdocs or other bits of chance.

I was a postdoc of Walter Kohn, and so was Gérard Toulouse. We weren't at the same time, but Gerard was immediately after me. Similarly, I just followed Maurice Rice[120], and so we had a contact that way; Hans Zittartz[121] had also been with Walter earlier. On the experimental side, there were people like Philippe Monod[122], and he and Zazie Béal-Monod—his wife who is a theorist, Philippe is an experimentalist—were both at UCSD the same time that I was. So there were various links. People knew people in certain places. There was a kind of dendritic knowledge of different groups, in different places, not necessarily anyone knowing all of the others. But it was becoming clear—to me anyway, and I think maybe to some of them—that there was something in this statistical physics of complex systems.

I wasn't particularly strongly taken to critical exponents and into the renormalization group. I could have been, because my PhD had been on functional integral stuff that was ideally suited for it, but I was doing other things. In any case, getting the exponent right and so on, didn't appeal to me. I was more interested in finding that there are transitions, that there is something rather than finding precisely what their values are.

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[118] DS: There had been effective pan-European networks since at least the beginning of the 20th century, and even longer. Some of the first Fellows of the Royal Society were from other countries.

[119] École Normale Supérieure, Paris (ENS).

[120] T. Maurice Rice : https://en.wikipedia.org/wiki/Thomas_Maurice_Rice

[121] Johannes Zittartz : https://de.wikipedia.org/wiki/Johannes_Zittartz

[122] Philippe Monod : https://fr.wikipedia.org/wiki/Philippe_Monod_(physicien)

So there was a growing group of different people. Then there were these couple of meetings. I guess from the Aussois one, although I think that was a little bit more on the tail end of the Kondo problem than the beginning of more concentrated systems, maybe partly a bit of each. Then, the two Heidelberg meetings brought together quite a lot of these different people. We had common inclinations for many things—common or overlapping or whatever you want to say. The desires were growing, but it was also becoming clear that different places had different problems. We were able to get postdocs, but we couldn't get travel money. The Italians could get travel money, but they couldn't get postdocs, and so on.

We were also getting the feeling that we were Europeans, and that we were good together. Never mind all those Americans over there, you know. There was a strength in Europe that we could build on. The European Economic community—as it was called at that time—decided to have a network program called "Stimulation Action"[123] [124]. We thought: "Well, you know, it would be good if we could get one of these things. It would help to bring us together, and fill in the gaps of where we were, make us able to interact more easily." It wasn't a lot of money, but we didn't have to write lots and lots of reports either. So, we applied, our application went down very well, and we got it. Our programme[125], then, supported various conferences, it also supported visits from one place to another, brought in some postdocs, and other things too. It was very successful and we went on to several further ones[126]. Occasionally, we'd have to tweak the application a little bit, so not everybody was in the same network. At one stage, we had a situation where our application failed because we were too big. So what we did was to cut it in half; nobody was allowed to be in both; each of them applied; each of them got funded; and we held joint meetings.

This also helped to bring a feeling of unity. As Francesco and you both know, you go to any of these European labs, and it doesn't matter where the people come from, it doesn't matter what their nationality is. They are all part of the same team. They're all European, or a lot of them are. There's something in common. I think that quite aside from the science that this has helped to bring together, it's also been extremely useful, in my view, for European science and, in principle, for Europe more generally,

[123] DSL I always recall it pronounced in French.
[124] "Council decision of 28 June 1983 adopting an experimental Community action to stimulate the efficacy of the European Economic Community's scientific and technical potential (1983-1985)." *Official Journal of the European Communities* **L181**, 20-23 (1983).
[125] DS: The programme was called "Statistical Mechanics and its Applications to Complex Problems in Physics, Engineering and Biology".
[126] DS: These were with the EC and the ESF.

removing barriers. (I voted "no" to Brexit, just in case you wondered.) I think that this was very important. For somebody that you probably know—you know Lenka Zdeborová[127]—one of the networks that we had—one called SPHINX[128], I think—involved the Czechs, and it paid for Lenka to go as a graduate student with Marc Mézard[129]. After that, the rest is history! You know they've provided lots of contacts, and in-common interests, and had some good people too. So I think they were a success.

In the States, I have had the impression, maybe not so much now, but a lot of the federal granting was fashion driven. "The subject this year is high-temperature superconductivity, or whatever it is, and anything else we're not so keen on." Whereas in Europe there was a great deal of freedom in selecting what the new things were. In the States, as I mentioned as well in my notes, I think there has be some differences with some of the private foundations, like the McDonnell-Pew Foundation[130]. They funded a "Centre for Brain and Behaviour"—or cognitive neuroscience—in Oxford to go across different specialties. They would fund things that were just starting, that regular agencies wouldn't fund, to get them going, and then they can take off. Your Simons one is another example[131]. These private Foundations are obviously doing a great job of complementing the government by supporting cross-disciplinary things that are not so clear or easy but have great potential.

At roughly the same time as our first network programme, in the USA along came the Santa Fe Institute[132] [133] championing interdisciplinary complexity science but this was funded in a slightly different way[134]. It's also got a different format. We're quite a theoretical sort of group. I think largely theoretical-conceptual with some deep theory, whereas the Santa Fe Institute is a bit broader and involves more social science and things like

[127] Lenka Zdeborová: https://en.wikipedia.org/wiki/Lenka_Zdeborov%C3%A1

[128] DS: SPHINX was the acronym for "Statistical Physics of Glassy and Non-equilibrium Complex Systems", an ESF programme.

[129] Marc Mézard: https://en.wikipedia.org/wiki/Marc_M%C3%A9zard

[130] In the early 1990s, the James S. McDonnell Foundation (https://en.wikipedia.org/wiki/James_S._McDonnell_Foundation) and the Pew Charitable Trusts (https://en.wikipedia.org/wiki/The_Pew_Charitable_Trusts) launched a joint effort on the theme of cognitive neuroscience. See, *e.g.*, Elizabeth Pennisi, "Two Foundations Collaborate On Cognitive Neuroscience" *The Scientist*, **3**(October) (1989).

[131] International Collaboration on Cracking the Glass Problem (2016-2023), funded by the Simons Foundation : https://en.wikipedia.org/wiki/Simons_Foundation.

[132] Santa Fe Institute: https://en.wikipedia.org/wiki/Santa_Fe_Institute

[133] DS: I have been fortunate to have had contacts with SFI for a long time, including as an External Professor since 2004. Also with the Center for Nonlinear Systems at LANL.

[134] SFI is a 501(c)(3) charitable organization, initially independently funded by visionary philanthropic individuals, foundations and companies, but now also with some federal support.

that. They both have something to offer. It seems to me that there are many things that the politicians could learn from us, both about getting together and also about the way to try and think of complex systems. Also recognition that doing the best thing in the moment isn't always the best thing in the long term.

PC: To get you back to those networks. I think the first such network was with Giorgio Parisi. Is that right?

DS: [1:26:32] There were three of us that set that going. There was Giorgio Parisi (La Sapienza, Rome), Nicolas Sourlas (ENS, Paris) and myself (Imperial/Oxford). I was the coordinator, and they helped me put it all together. We had done it such that there would be a British representative, a French one and an Italian, but it went further out than that. The three of us continued that way with later applications as well, until we got to the point when we got split into two halves, and I was in one and Giorgio was in another.

PC: How did you first meet Giorgio, then, so you could get this network going?

DS: [1:27:16] I think the first time that I met him physically was at meeting in Rome, on localization and disorder, or something I don't remember quite what is was called[135]. I remember that there was a photograph taken of four of us: Scott Kirkpatrick, myself, Parisi and Toulouse. There was a hypothesis—the PaT hypothesis[136] for the SK model—so this was the reason for it. I don't know whether I knew Giorgio before that. I knew about his work, but I have no evidence that Giorgio had any interest in statistical mechanics, until it came to the puzzle about the replica thing. He was being challenged by something new, "what can you do? $n \to 0$, can it mean anything? And so on…" I tried to ask him about his thought processes leading to his anzatz, but did not get an explanation; maybe you know better than I. It strikes me, for example, that once one had done one step replica symmetry breaking, and it was good but not quite enough, then one really had to go to a hierarchy, because 0 and 1, for me, are sort of special numbers (along with infinity) in a way that 2, 3, 4, 5 aren't. So the only other sensible possibility was to go to a whole new kind of structure, and it would have to be hierarchical. But whether Giorgio thought that, I have no idea.

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[135] Proceedings of the Conference Held in Rome, May 1981: *Disordered Systems and Localization* C. Castellani, C. Di Castro, L. Peliti, L. eds. (Berlin : Springer-Verlag, 1981). https://doi.org/10.1007/BFb0012537

[136] G. Parisi and G. Toulouse, "A Simple hypothesis for the spin glass phase of the infinite-ranged SK model," *J. Phys. Lett.* **41**, 361-364 (1980). https://doi.org/10.1051/jphyslet:019800041015036100

I think he was intrigued by the problem, and I think that that episode changed statistical physics an enormous amount, because he's had such a tremendous influence, and he's now truly a condensed matter physicist. If you look at his citation index, it's his particle physics things that come first, but that's partly because he published his ansatz in a sequence of small papers, so nobody ever knows which one to cite. But he also did me an unintentional disservice by misquoting my paper's reference in every single one of those first papers by giving the page numbers incorrectly. So I lost a lot of citations, because a lot of other people were then just copying the reference to SK from Giorgio. Anyway, I think he's an absolutely brilliant guy, and I think that our unphysical SK "solution" was the hook that got him into complex systems, to all our benefit.

FZ: I'd like to take this opportunity. I wanted to ask you a little bit more about the period between the publication of your work with Kirkpatrick on the SK model and its solution. It went quite fast, and there were many attempts from other people: there was this Blandin's work, and Bray and Moore, etc. Do you remember a little bit how it developed? I supposed people were kind of rushing to solve the low-temperature phase of the model…

DS: [1:30:08] I think not so many addressing the issue of solving the problem of the negative entropy at T=0 in the SK model[137]. We published our paper at the end of '75 and Giorgio's first paper was in '78 or '79, so there were four years. We already mentioned the important 1977 paper of Thouless, Anderson and Palmer[138], using a different procedure to obtain a more reasonable T->0 behavior. The next important development was a paper by de Almeida and Thouless[139], demonstrating that there was a problem. I mean further demonstrating, because it was obvious already from SK that there was a problem, but in this case it wasn't just the negative entropy. It was an instability of replica-symmetry breaking excitations. Then came Bray and Moore looking at these things, they called these excitations replicons[140], and they tried the first step in doing a replica symmetry breaking[141]. What they did was that they took the first *m* replicas as one group and then the remaining *n-m* as a second group, each group internally

[137] DS: There were several other papers following EA and SK.

[138] D. J. Thouless, P. W. Anderson and R. G. Palmer, "Solution of 'Solvable model of a spin glass'," *Phil. Mag. A*, **35**, 593-601 (1977). https://doi.org10.1080/14786437708235992

[139] J. R. L, de Almeida and D. J. Thouless, "Stability of the Sherrington-Kirkpatrick solution of a spin glass model," *J. Phys. A* **11***,* 983 (1978). https://doi.org/10.1088/0305-4470/11/5/028

[140] A. J. Bray and M. A. Moore, "Replica symmetry and massless modes in the Ising spin glass," *J. Phy. C* **12,** 79 (1979). https://doi.org/10.1088/0022-3719/12/1/020

[141] A. J. Bray and M. A. Moore, "Replica-Symmetry Breaking in Spin-Glass Theories," *Phys. Rev. Lett.* **41,** 1068 (1978). https://doi.org/10.1103/PhysRevLett.41.1068

replica-symmetric but different inter-group. They took the limit $n \to 0$, and $N \to \infty$, and argued for $m \to \infty$, but it didn't fully solve the problem. Blandin[142] had something which was a little bit more like Giorgio's idea, but didn't take it quite so far. Giorgio acknowledges those things in his papers. But I don't think that there were any other people who were involved in the replica symmetry breaking thing. There was a certain amount of concern about the inversion of the of the small *n* going to zero and large *N* going to infinity, but most people came around to think that wasn't the actual problem.

But then Giorgio came out with his anzãtze, which were magic. Weren't they? Perhaps a more understandable magic than some bits of Edwards-Anderson initially. When you think about it, it is an amazing kind of problem. I understand that what Giorgio found, the mathematicians hadn't got a clue about. What do you do when $n \to 0$? They didn't know. There's a difference between *n* going to zero and $n=0$. There have been earlier problems where you take an n->0 limit, and all works ok. Polymers is one example[143], and another is in Edwards' earlier work, back in the '50s[144]. He came out with a theory for how to deal with impurity scattering in resistivity. The impurities gave random potentials, electrons scatter off these potentials, and one can consider the overall effects on observable as a multi-scattering expansion. In the thermodynamic limit one can average the series over the impurity potential distribution which gives an effective interaction between the electrons, giving a diagrammatic representation that looks like a Feynman field theory expansion for a normal pure but interacting system, but with no closed electron loops. So then you can replace it by an effective normal interacting field theory with an extra field-dimension/replica-label (α=1,..n), but take the replica number n equal 0 at the end and it removes all the loops. I knew about that—and about EA's conceptual extension to allow an extra meaning to the inter-replica correlation freezing—but Giorgio's ansatz was just magic. But then you see the phylogenetic tree, and so on, and it looks all so reasonable. But exactly how Giorgio went through this, I don't know. He had to invent new things.

[142] A. Blandin, "Theories versus experiments in the spin glass systems," *J. Phys. Colloques* **6**, 1499 (1978). https://doi.org/10.1051/jphyscol:19786593

[143] P. G. de Gennes, "Exponents for the excluded volume problem as derived by the Wilson method". *Phys. Lett.* **38A,** 339 (1972). https://doi.org/10.1016/0375-9601(72)90149-1 DS: Sam Edwards earlier introduced the field theory/functional integral approach to polymers during the time when I was doing my PhD under his supervision (S.F. Edwards, "The statistical mechanics of polymers with excluded volume," *Proc. Phys. Soc.* **85**, 613 (1965), https://doi.org/10.1088/0370-1328/85/4/301).

[144] S.F. Edwards, "A new method for the evaluation of electric conductivity in metals," *Phil. Mag*. **3**, 1020 (1958). https://doi.org/10.1080/14786435808243244

It must have been wonderful, inventing a whole new mathematics. Wonderful! He later gave an enlightening physical explanation.[145]

PC: You are in some ways uniquely positioned because of your deep roots or connections to the to the US and to Europe. Maybe you can try to help us understand what was the American reception to ideas of RSB, in the first one or two decades.

DS: [1:35:07] I have the feeling that, with a few exceptions[146], they didn't give a damn. I don't remember that there was much of a general reaction. The main part of the spin glass history, I think, has been European. There was interest in the developments into computer science, but again largely European. Later we got the interest of probability theorists, because this was a new kind of probability. They didn't seem to have thought of the idea that you could have a connected system, where the connections were randomly chosen in some way, and extensively from a probability distribution that was intensive—that was the same everywhere, that is the distribution was the same. That seemed to open up loads of new ideas. But then they wanted a rigor that was completely different from what was available to us. Again, however, in my recollection, that has been mainly by Europeans.

That's another place, where I really feel that I am a theoretical physicist, not a mathematical physicist. Part of what theoretical physicists do is guess when you can't answer the questions, or go a little bit away from reality, and eventually come back with extra knowledge, tests and extensions. For a long time, we have been going off the energy shell in various of problems. You can't solve the problem that you want, so you change the problem a little bit—that gives you some ideas—and then try and go back, and this sort of stuff. I think it's good that we all come from different backgrounds. Mathematical probabalists like [Michel] Talagrand[147] and [Dmitry] Panchenko[148] have really brought some legitimacy. Other, more rigorous, statistical mechanicists, such as Pierluigi Contucci[149], Francesco Guerra[150], and many others have also made major advances.

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[145] G. Parisi, " Order Parameter for Spin-Glasses," *Phys. Rev. Lett.* **50,** 1046 (1983)**.** https://doi.org/10.1103/PhysRevLett.50.1946

[146] DS: *e.g.,* Daniel Fisher and David Huse, Charles Newman and Daniel Stein, who argued against RSB in finite-ranged spin glasses.

[147] Michel Talagrand : https://en.wikipedia.org/wiki/Michel_Talagrand ; *Spin Glasses: A Challenge to Mathematicians*, (New York: Springer-Verlag, 2003); "The Parisi formula," *Annal. Math.* **163**, 221 (2006). https://www.jstor.org/stable/20159953

[148] Dmitry Panchenko, *The Sherrington-Kirkpatrick Model* (New York: Springer, 2013).

[149] Pierluigi Contucci and Cristian Giardinà, *Perspectives on Spin Glasses* (Cambridge: Cambridge University Press, 2012).

[150] Francesco Guerra : https://en.wikipedia.org/wiki/Francesco_Guerra

PC: If I understand correctly, you had a pretty extensive relationship with Japan in the '80s already. Is that correct?

DS: [1:38:18] Yes. Well, it was really mainly with [Hidetoshi] Nishimori[151], who came on sabbatical to Oxford. I don't remember who invited him—whether he invited himself or whatever—but we got talking. He had already been doing this thing with the Nishimori line and so on. We did a few things together, and then I've been and visited him, but I wouldn't have said that any of the things that we did together were particularly fantastic. Some of things that he's done himself have. We talked for many years about the ideas of quantum annealing, before he got to do that, on a possible quantum analogue of the Nishimori line, but that came to nothing.

His initial work on the Nishimori line[152] was very important. There are so many things that grew out gradually, but they seem to me to be absolutely sensible. Like, if you try to get the best results of a signal down a noisy line, then you ought to try to do the retrieval with an algorithm which has a similar level of noise as the noise on the line, and so on like this. All these things sort of came out of certain models that came out these other things, but they're sensible. A number of these things were found simultaneously or quasi-simultaneously in different fields, of course. But different perspectives can be useful. The theoretical physicists' averaging over the disorder, and the computer scientists' looking at worst-case things are both interesting problems.

PC: We're getting towards the end. I only have a few more questions, one of them having to do with students. You mentioned a few them along the way, and I've read the story of Paul Goldbart[153], who says that he just showed up to your office, and that's how he became your student[154]. More generally, how did you recruit students and postdocs?

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[151] Hidetoshi Nishimori *Statistical Physics of Spin Glasses and Information Processing: An Introduction* (Oxford: Oxford University Press, 2001).

[152] H. Nishimori "Exact results and critical properties of the Ising model with competing interactions," *J. Phys. C* **13**, 4071 (1980). https://doi.org/10.1088/0022-3719/13/21/012; "Internal Energy, Specific Heat and Correlation Function of the Bond-Random Ising Model," *Prog. Theo. Phys.* **66**,11 (1981). https://doi.org/10.1143/PTP.66.1169

[153] Paul Goldbart, Dean, College of Natural Sciences, University of Texas at Austin, https://cns.utexas.edu/directory/item/10-deans-office/3605-pmg943?Itemid=349 (Last accessed January 11, 2021)

[154] Paul M. Goldbart, "David Sherrington as a mentor of young scientists," *J. Phys. A* **41**, 320302 (2008). https://doi.org/10.1088/1751-8121/41/32/320302

DS: [1:40:45] In most cases, they would have applied to do some research in our group. That's the way it goes at the moment. We look at all these different candidates, sieve them down to the ones that we think are the brightest, and maybe whose interests are closest to some of ours. For postdocs, there's more of a tendency now to look for people who already worked in the area that you want to work them in. Whereas in my day, it was more the case of looking at who's been showing that they can do something very good, and now I've got something that I want to do that it isn't necessarily the same as they did before. I've benefited from that.

I wanted people to be good, but I also wanted to get on with them. It's sort of partly dependent on kind of personalities and things too. I don't know of a perfect way.

PC: There's still no still no such thing as a perfect way! During your time at Imperial and later at Oxford, did you ever get to teach a class about spin glasses and replica symmetry breaking or was this always a research topic for you? If you did, can you give us some details about it?

DS: [1:42:54] I did give some lectures but I don't know if I recall giving lectures actually on a replica symmetry breaking, *per se*. I only remember it being a kind of colloquium, or something like this kind of context, where you didn't give any real details. I can't remember. I certainly didn't give a whole course on it. I did give a course in Trieste[155], but in Oxford I can't remember. It would have been too specialized for them.

PC: Even to your own students?

DS: [1:43:39] I would talk to them about it, but not as a special topic. There were a few of my students who did some replica symmetry breaking things, and I would give them private tutorials.

PC: Is there anything else you'd like to share with us about this epoch, or the work around it that we might have overlooked?

DS: [1:44:12] One thing that I feel has been very important from this stuff is a demonstration that theoretical physics is a valuable subject in its own right. There has been an understanding for some time that you can make discoveries in some material and then apply the material. Or maybe some

[155] The *Summer College in Condensed Matter on Statistical Physics of Frustrated Systems* was held at the International Centre for Theoretical Physics (ICTP), July 28- August 15, 1997, and organized S. Franz, M. Mézard and D. Sherrington. Prof. Sherrington gave a lecture, in three parts, entitled "Spin-Glass Mean Field Theory and Replicas". See, *e.g.*, (Last consulted Nov 29, 2020.)

techniques, superconductivity, or whatever. But I think that this study has shown how looking initially at some completely obscure materials—such as the metallic alloys of Bryan Coles and others—and trying to understand them, has led to a whole load of new conceptual issues that have had useful applications in many, many other places[156]. Many of those have nothing to do with physics, *per se*, but do have a lot to do with the theoretical physicist's conceptual thinking and methods to try to solve them. I think that that has been a really important example of the value of theoretical physics.

Even though I said that I don't have great many colleagues at Oxford working in exactly the same topic, I do have them around Europe, and some other places too, making these inter-topic moves, which I think are conceptually purely driven by theoretical physics, as a real subject in its own right. I also include computer simulations as another example of a valuable theoretical type of application. In this context, those computer experiments— the ones I'm interested in—are not trying to reproduce exactly what the real world is like. They look at computer experiments on models, which help to understand better what the underlying important aspects are. I think of those two things—the analytic and these numerical experiments that are done by computer simulations on simple models—have been extremely fruitful and will be extremely fruitful. So I felt very happy to do this as a head of Theoretical Physics, although I did enjoy also the time I spent with the experimentalists. We must talk to experimentalists, of course, but there is another tool that we've got, a mental tool and it's applicable.

PC: Do you do you still have notes, correspondence, papers from that epoch that you kept. if yes, do you have plans to deposit them in an academic archive at some point?

DS: [1:47:18] Very good question! I think my answer is a little bit like that of John Hopfield. I don't really think I've got anything that's very useful. I suppose earlier on there would have been letters and so on. I did use to file these things away, but I was never very good at that. And when I've been moved from office to office, after my retirement, I had to clear out huge amounts of stuff in relatively small times, and I think most of it simply just got binned. I might have a few bits, but not very systemically. I have a filing cabinet, in which I think I've kept some of the things about the networks and stuff, but I can't go in my office at the moment, because of

[156] DS: See also the series of *Physics Today* articles by Phil Anderson, starting with P. W. Anderson, "Spin Glass I: A Scaling Law Rescued," *Physics Today* **41**, 9 (1988). https://doi.org/10.1063/1.2811268

COVID-19. When I can, I'll look at those things. Otherwise, not a lot. I don't think I have a lot of things with a great deal of value. Some of the things I don't even know. So I might have a few bits and pieces, but not like the things that people 100 years ago used to keep, all those letters and so on. I never thought that what I was doing would be particularly important. I'm just an ordinary guy, from an ordinary place. There you go! But I think you're doing a great job here. You should remind the rest of your young colleagues that perhaps they should keep some of those notes that I haven't kept.

PC: Thank you very much for all your time and your efforts. It's been truly a pleasure to get to know you more, and to find out more about your work, and these very special times in which you got to do it.

DS: [1:49:32] Thank you too. Thank you both as well.