# History of RSB Interview: Raymund Jones

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**Interviewer:**
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Over Zoom, from Prof. Jones's home in Birmingham, United Kingdom.
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**PC:** Good afternoon, Prof. Jones. Thank you very much for joining us. As we have discussed ahead of time, the theme of this conversation is the history of replica symmetry breaking in spin glasses in physics, which we roughly bound from 1975 to 1995. Before we dive into that topic, I'd like to ask you a few background questions. First, can you tell us a bit more about your family and your studies before going to university?

**RJ:** [0:00:30] I'm from a small village in North Wales and attended a traditional Welsh Grammar School whose talented teachers ignited my interests in sciences, languages, music, and other academic subjects. My parents had had limited academic opportunities when they were young and were enormously supportive of my brother (who became an engineer) and myself and encouraged us to make the most of the educational opportunities offered by both school and University. I'm a single man.

**PC:** Was science part of your upbringing? Were your parents particularly interested in that?

**RJ:** [0:00:47] No, not particularly. Like many people, my father was a soldier in the second world war and went to college after the war on an ex-serviceman's grant[1] to train as a teacher. I'm the first member of my family to go to university.

**PC:** Then, how did you get interested in physics?

**RJ:** [0:01:10] I can only say it was at the grammar school which I attended (and at which my father had also studied when he was young). I was at the John

[1] Demobilisation of the British Armed Forces after the Second World War: https://en.wikipedia.org/wiki/Demobilisation_of_the_British_Armed_Forces_after_the_Second_World_War

Bright Grammar School[2], in Llandudno, a very traditional grammar school where mathematics and physics were both very well taught, I would say. I really think I was rather lucky. (The situation is different today in that the secondary school system in the UK is now significantly different from what existed then.) The grammar school focused on mathematics, physics, modern languages, literature and of course Latin!

**PC:** What led you to pursue a PhD in theoretical physics with Sam Edwards[3] from there?

**RJ:** [0:01:57] I went from my grammar school to university. I was an undergraduate at Manchester, where I discovered, as I went through the undergraduate course, the existence of theoretical physics as a sub-discipline in its own right. I realised that this connection between mathematics and physics was absolutely fascinating. I didn't enjoy lab work, although I had to do it for a couple of years. I remember leaving the lab after my last experiment, thinking that with any luck I might never have to do it anymore. It was a kind of purgatory for me (although I did recognise the importance of laboratory work). However, Sam Edwards gave a third-year course in quantum mechanics to students who were specializing in theoretical physics. It was extremely difficult—with Green's functions[4]and that kind of thing as you might imagine. Even if I did not always understand what was going on, he really caught my imagination. Despite the difficulty, I appreciated his unusual thought processes. They were different from those of many physicists I had met. I was very taken by it. At the end of the process, I decided I would like to do a PhD in theoretical physics which pulled together mathematics and physics. There were also particular undergraduate courses, which caught my interest and had nothing to do with where I ended up. One was special relativity and the mathematics thereof, which caught my interest in my first and second years, and particularly interested me in my third and final year, when we started to look at the covariant formulation of classical electromagnetism. This really got my interest. That was the  kind of thing that turned me on.

**PC:** So, this is how you met Sam Edwards.

**RJ:** [0:04:26] He lectured an advanced quantum mechanics course, which was taken by the theory students, a very small group of about 6 out of 140 who were selected to do this. Selection was done by interview. I remember Sam was on the small panel that interviewed me. I remember his asking: "If you

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[2] Ysgol John Bright: https://en.wikipedia.org/wiki/Ysgol_John_Bright
[3] Sam Edwards: https://en.wikipedia.org/wiki/Sam_Edwards_(physicist)
[4] Green's function: https://en.wikipedia.org/wiki/Green%27s_function

want to do a PhD here, what area do you want to work in?" I said: "I have a pretty open mind but the only one thing that I really don't want to work in is nuclear physics." So, Sam replied "Oh, yes! That's alright. That's alright." Did you yourself know Sam Edwards?

**PC:** No.

**RJ:** [0:05:15] He's from South Wales. So, I ended up, when I returned to the university that September, as one of Sam's research students.

**PC:** How did you get to select or pursue a thesis topic on spin waves in particular[5]? How did that come about?

**RJ:** [0:05:42] I remember Sam said he would take me on as a student. He initially was talking about some work involving the use of functional integrals in classical statistical mechanics, which involved the use of collective coordinates, and integrating over density functions and what have you. However, somebody in the coffee room said to me one day: "Ray, I think there is a paper in a recent *J. Math. Phys.*, which might be of interest to you." It contained everything that I had started and that I was intending to do, so we had a rapid change of track. Sam was, of course, very interested in disordered systems at that time. He started to talk to me about disordered magnetism and not long afterwards said: "I'm shortly going down to Harwell to the Atomic Energy Research Establishment[6], (which has a very high-quality theory group). Come down for the day with me." John Hubbard[7] was in charge of the solid state theory there. Eventually I spent two summers there during my PhD because there were very good people to talk to. I got some ideas there, and Sam (who was a consultant there) would also sort of throw ideas out at you. The whole topic of disordered systems, which in those days was focused more on systems with an underlying crystalline structure, but with the disorder—in my case randomly positioned missing spins which "should" have been there—and I was tasked with exploring what happens to the excitations of such a disordered system. It's now old hat, of course, but it very much wasn't in those days. There was a very important group in Oxford working on this as well.

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[5] Raymund C. Jones, *Spin Waves in Disordered Magnetic Systems*, PhD Thesis, University of Manchester (1970). https://www.librarysearch.manchester.ac.uk/permalink/44MAN_INST/1r887gn/alma992983073517401631

[6] Atomic Energy Research Establishment: https://en.wikipedia.org/wiki/Atomic_Energy_Research_Establishment

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

**PC:** You worked on a particular model that you called the *localized Heisenberg Hamiltonian*, whose structure seems to be very similar to that later studied by Edwards and Anderson[8]. Where did the idea for the particle model come from?

**RJ:** [0:08:02] We're moving on now from my time doing calculations in disordered spin systems for my PhD.

**PC:** No, I mean your PhD thesis.

**RJ:** [0:08:16] I knew about the basic ideas about things called spin glasses and I was aware of some early work by Walter Marshall[9] at Harwell, but I didn't know really what they were. I don't think I started looking more seriously at these things until I was a postdoc and moved to London.

**PC:** That Hamiltonian, you describe in your thesis, has RKKY[10] couplings between two spins, which you call the disordered Hamiltonian. Then, you also consider a localized version of it, which is just with nearest neighbors.

**RJ:** [0:08:57] The reason I worked on that is that it was easier to handle short-range forces. The fluctuating sinusoidal sign variations in a real RKKY system were replaced by randomly positioned nonmagnetic vacancies in a lattice That made it a more mathematically tractable problem. It was quite a good PhD problem as it was, I think. However, it was rather later that there was an attempt to improve the modelling.

**PC:** You were the first one to work on this particular model, I think. It was not a model that was in the air, inspired by someone else. Is that correct?

**RJ:** [0:09:26] It's a long time ago. I was of course aware of the significant work of Elliott[11] and Murray[12] at Oxford but trying, after a long period of time, to connect the different bits of what I was doing and in what time sequence

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[8] See, Ref. 5, p. 3. "The coupling between spins $s_i$ and $s_j$on sites $r_i$ and $r_j$ is described by a localised Heisenberg Hamiltonian $H = -\sum_{ij} J_{ij}\, s_i \cdot s_j$, where the sum is over all nearest neighbour pairs. $s_i$ takes a value appropriate to whether site $r_i$ has a spin or upon it; and $J_{ij}$ takes different values according as sites $r_i$ and $r_j$ contain two host spins, two impurity spins, or one of each."

[9] Walter Marshall: https://en.wikipedia.org/wiki/Walter_Marshall,_Baron_Marshall_of_Goring

[10] RKKY Interaction: https://en.wikipedia.org/wiki/RKKY_interaction

[11] Roger Elliott: https://en.wikipedia.org/wiki/Roger_Elliott_(physicist) See: R. J. Elliott and B. R. Heap, "Theory of random dilute magnets with application to $MnZnF_2$," *Proc. Roy. Soc. Lond. A* **265**, 264-283 (1962). https://doi.org/10.1098/rspa.1962.0008

[12] Gillian Gehring: https://en.wikipedia.org/wiki/Gillian_Gehring See: G. A. Murray, "The determination of the critical concentration for a dilute Heisenberg ferromagnet from the low-energy spin waves," *Proc. Phys. Soc.* **89**, 111 (1966). https://doi.org/10.1088/0370-1328/89/1/317

is not easy. I don't want to mislead anyone. It's really a question of being aware that in some of these systems you could have localized spins; in others you could have itinerant models, but I was interested in localized spins. You need to simplify the problem with RKKY, $1/r^3$ with a cosine oscillation. Clearly, you have randomness in a real system in the coupling between spins, but simplifying it in this way it in the way we did make it a lot more tractable (if further from reality)

**PC:** In your thesis, you described these models as forming local clusters of ferromagnetically ordered magnetic moments with antiferromagnetic couplings[13]. I guess you imagined domains that were interacting. Was that the general understanding of these systems at the time?

**RJ:** [0:10:34] We are moving off my thesis work now. I didn't really pursue the topic of spin glasses until later. My thesis work was concerned with working out the spin wave spectra of these localized models with Heisenberg Hamiltonians and with missing spins. There was a big industry in these things.

**PC:** I think you ended up publishing only part of your thesis. Is that correct? If yes, why did that happen?

**RJ:** [0:11:13] No. Pretty well everything on my PhD thesis was published in two papers with Sam[14]. One was on the ferromagnetic problem. The other was a less good attempt, I think, on the problem of antiferromagnetism in a disordered system, which we now know is a much more complicated issue. That was quite normal.

**PC:** I'm just saying this because you had a paper called part 1, and part 2 never appeared, or at least I couldn't find it.

**RJ:** [0:11:45] I see. You have really been hunting. Part 2 was the letter on the antiferromagnetic problem.

[13] See Ref. 5, p. 88. "Although not explicitly stated, the task we originally set ourselves in starting this work was to investigate the dynamics of random spin systems in the hope of understanding the low-lying excitations of a **magnetic glass**. Such a system has local clusters of ferromagnetically ordered magnetic moments which are coupled antiferromagnetically to other clus-ters, so that there is no nett [sic] magnetic moment in the system. »

[14] S. F. Edwards and R. C. Jones, "A Green function theory of spin waves in randomly disordered magnetic systems. I. The ferromagnet," *J. Phys. C* **4**, 2109 (1971). https://doi.org/10.1088/0022-3719/4/14/026; R.C. Jones and S. F. Edwards, "Spin waves in disordered antiferromagnets," *J. Phys. C* **4**, L194 (1971). https://doi.org/10.1088/0022-3719/4/10/004

**PC:** During your time in Sam's group, did you hear about the work he was doing on rubber using the replica trick[15], at the same time?

**RJ:** [0:11:58] Yes, of course. Sam was very busy. If I remember correctly, he had 12 research students and two postdocs. Most of them were starting to work in polymers and allied topics[16] but some were looking at the statistical mechanics of turbulence. I wasn't at that stage aware of the replica trick, but my ignorance was really happenstance.

**PC:** Can you tell us a bit about the group? Would Prof. Edwards be presenting his results? Would there be group meetings? Were there discussions?

**RJ:** [0:12:31] We had the weekly seminars for the theoretical physics group as a whole, which contained nuclear physicists, field terrorists, and general condensed matter theorists, (which latter probably focused on Sam's work). Sam was a rather unconventional solid-state physicist. But as far as formal meetings of subgroups of Sam students, we didn't do it. You just knocked on someone's door if you wanted to share ideas or perhaps clarify something you hadn't understood. Sam was very patient in many ways and also generous in crediting his students with work that was done together, but sometimes you had to go and talk to people who knew him, and his thought processes better than I did at the time in order to try and latch

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[15] See, *e.g.*, S. F. Edwards, "The statistical mechanics of rubbers," in Polymer Networks: Structure and Mechanical Properties–Proceedings of the ACS Symposium on Highly Cross-Linked Polymer Networks, held in Chicago, Illinois, September 14–15, 1970, edited by A. J. Chomp and S. Newman (Plenum Press, 1971) pp. 83–110. S. F. Edwards, "Statistical mechanics of polymerized materials," in Amorphous materials: papers presented to the Third International Conference on the Physics of Non-crystalline Solids held at Sheffield University, September 1970, edited by R. W. Douglas and B. Ellis (Wiley-Interscience, 1972) pp. 279–300.

[16] We note, Karl F. Freed, who was then a postdoc, as well as Robert Alexander-Katz, Alan G. Goodyear, and James William Vincent Grant, who were then PhD students. See, *e.g.*,

- U. Mohanty, M. Herman, J. Douglas and J. Dudowitcz, "Biography of Karl Freed," *J. Phys. Chem. B* **112**, 15945-15947 (2008). https://doi.org/10.1021/jp806182m
- Robert Alexander-Katz, *Topological Constraints on Long Molecules and Related Topics*, PhD Thesis, University of Manchester (1970). https://www.librarysearch.manchester.ac.uk/permalink/44MAN_INST/1r887gn/alma992979302363001631
- Alan Gordon Goodyear, *Dynamics of a Polymer Chain*, PhD Thesis, University of Manchester (1972). https://www.librarysearch.manchester.ac.uk/permalink/44MAN_INST/1r887gn/alma992984235924801631;
- James William Vincent Grant, *The effect of chain entanglements on diffusion and viscosity in bulk polymer systems*, PhD Thesis, University of Manchester (1972). https://www.librarysearch.manchester.ac.uk/permalink/44MAN_INST/bofker/alma9919278014401631

onto what these thought processes actually were. What was going on in his mind, did not always translate itself into what he wrote down on paper—as you may be aware when you read some of Sam's papers… He clearly had a very strong physical intuition into any problem he was working on, but that deep intuition doesn't always emerge in his published work, because of the particular and often unconventional mathematical language he used to express it in.

**PC:** You've mentioned your postdoc years already a couple of times. What drew you to do a postdoc at Imperial College with David Sherrington[17]?

**RJ:** [0:13:55] I already knew David. David was a lecturer in theoretical physics at Manchester, and coincidentally he had been at tutor in the hall of residence in which I had lived in Manchester. I completed my PhD in three years, and I had to find something to do. I clearly wanted to take things further. David, at that stage, was at Imperial. I contacted him for advice, and he pointed out that they were —in Bryan Coles[18]' condensed matter group in Imperial College—setting up a solid-state theory part of this condensed matter group. It contained David Sherrington and Nicolas Rivier[19]. (I think he's now back in Switzerland, but I'm not sure.) They were the lecturers, and I was David's first postdoc. Nic Rivier had David Pettifor[20] as a postdoc. David Sherrington suggested applying for an ICI fellowship[21]. I went down to London for an interview at the Senate House of the University of London, where I was interviewed by an alarmingly big panel, some of whom were wearing gowns. It was rather intimidating. I was lucky enough to get one of these fellowships. As soon as I got my PhD, I started in London. As I said I, already knew David and it was a very happy relationship, as it was with Sam. David himself had of course been an extremely successful student of Sam.

**PC:** Can you describe a bit how the groups were working with Nicolas Rivier, Coles and Sherrington at the time? Were there many interactions?

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

[18] See, *e.g.*, 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

[19] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Nicolas Rivier*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Nicolas Rivier, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 3 p. https://doi.org/10.34847/nkl.b0d6xpa9

[20] David Pettifor: https://en.wikipedia.org/wiki/David_Pettifor

[21] ICI Fellowships were endowed by the Imperial Chemical Industries.

**RJ:** [0:16:12] I shared an office with David, because there was an acute accommodation shortage there. Nic Rivier shared an office with David Pettifor who was the other postdoc. The research students were also on the same floors, as were the laboratories. People did talk to each other. That's where I first became aware of some of the important experimental work on spin glasses, [such as] being done by John Mydosh[22], who was, I think, spending time with the experimental group. So, we were all in one floor of the Imperial College Physics Department and it was very easy to talk to other people David's shared office was ideally situated! I could look out of the office window during the summer months when the *Proms*[23] concerts were taking place in the Royal Albert Hall[24]. I could just look at the back door of the Albert Hall and see when the queue was getting too long and could then nip out and get a ticket. Very well placed indeed!

**PC:** You said this is the first time you heard about spin glasses. So, you were there when John Mydosh came to visit?

**RJ:** [0:17:27] Yes. I think John was part of the group. I can't remember. It's 50 years ago. Bryan Coles was always talking about these odd copper and manganese alloys that had magnetic order, because when you measured their susceptibility, there was a cusp in the absence of a field. If you turn a field on, that cusp turns into a bump. Nic Rivier was looking at different aspects of that problem. I wanted to work on something different from that, because I had done a PhD in disordered magnetism. David proposed that I did some work on rare earth physics. I did calculations of the spin wave spectra in rare earths[25]. My postdoc position was at that stage for two years.

However, I remember Bryan Coles coming into the office and saying: "You ought to think about the future. I know you have another 18 months or so remaining in the group, but you want to think about what you'll do after this." He then added that: "I notice there is a lectureship in mathematical physics going in Birmingham. Why don't you apply because it'd just be good practice for when you seriously want a job." That was why I only spent a year at Imperial. Lectureships are like jewels. I applied for the

[22] P. Charbonneau, *History of RSB Interview: John Mydosh*, transcript of an oral history con- ducted 2021 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 19 p. https://doi.org/10.34847/nkl.e1e3ob87

[23] BBC Proms: https://en.wikipedia.org/wiki/BBC_Proms

[24] Royal Albert Hall: https://en.wikipedia.org/wiki/Royal_Albert_Hall

[25] See, *e.g.*, R. C. Jones, "Impurity spin wave models in a simple cubic Heisenberg ferromagnet," *J. Phys. C* **4**, 2903 (1971). https://doi.org/10.1088/0022-3719/4/17/020; R. C. Jones and D. Sherrington, "Coupled nuclear and electronic spin wave modes in the presence of quadrupolar interaction," *J. Phys. C* **6**, 1800 (1973). https://doi.org/10.1088/0022-3719/6/10/014

Birmingham job and was lucky enough to get it. I regretted however not having longer as a postdoc, because it was a very fertile group to be in. With Bryan Coles and Dave Sherrington and Nic Rivier, there were a lot of new ideas to be absorbed. I learnt a very great deal and I would have learnt even more had I had another year there. I owe that group a great deal.

**PC:** As you said, you started at Birmingham quite quickly. Then, you had a PhD student work with you, Gary John Yates[26], and you kept on working on spin waves in materials. What kept you interested in this problem?

**RJ:** [0:20:06] It was really the chance of a research student. I had a problem there that was worth exploring and which the right student would enjoy. It was also a problem to which I felt I could give very strong support and involves putting localised spins in positions determined by the classical statistical mechanics of liquids. The Mathematical Physics Department was led by Tony Skyrme[27] and had been focused in the past on particle physics and field theory and the largest subgroup worked in these areas. There was however a smaller and very active subgroup led by David Thouless[28] which worked on condensed matter physics. Mike Kosterlitz was already there as a postdoc in the particle physics area He told me later that he was a bit bored with particle physics, and he spotted David Thouless's talents as a source of new ideas and went to see if there were some interesting problems that could be worked on in the condensed matter area.

**PC:** When did you become aware of Sam Edwards' work and model for spin glasses[29]?

**RJ:** [0:21:09] Much later. I think I read Sam's papers much later than that, but I'm not absolutely sure.

**PC:** So, he didn't come to visit Birmingham, or you didn't keep in touch with him, such that you would have heard of this work?

**RJ:** [0:21:32] I did, because when I started talking to David Thouless and Mike Kosterlitz about some of these interesting problems in magnetism I got interested of course in the properties of random matrices as a possible way of modelling or simulating (rather crudely) a system with interactions with

[26] Gary John Yates, *Calculations on ferromagnetic and spiral rare earth spin systems and models of amorphous magnetism*, PhD Thesis, Birmingham University (1977). https://birmingham-primo.hosted.exlibrisgroup.com/permalink/f/vmc2c6/44BIR_ALMA_DS2181442850004871
[27] Tony Skyrme: https://en.wikipedia.org/wiki/Tony_Skyrme
[28] David Thouless: https://en.wikipedia.org/wiki/David_J._Thouless
[29] See, *e.g.*, S. F. Edwards and P. W. Anderson, "Theory of spin glasses," *J. Phys. F* **5**, 965 (1975). https://doi.org/10.1088/0305-4608/5/5/017

random signs (provided in a real system by the RKKY interaction with its cosinusoidal sign fluctuation. I think it might have been David Thouless who pointed me towards the, by that time older, literature on random matrices focusing on the work of Wigner[30]. I'll tell you a story if I may about this. I was at a talk in a plenary session of a solid-state physics conference, which was being held in Manchester[31]—and I had actually become bored stiff actually. It was the second plenary talk, and somebody had made specific heat measurements on some compound that I wasn't particularly interested in. So, I thought I'd creep out and wake up with a cup of coffee. As I did, I heard someone in the darkness of the lecture theatre saying: "Is that Ray?" It turned out to be Sam Edwards sitting in the front row who said: "Are you bored as well? Should we go out and have a cup of coffee?" So, out we went and had a chat about how life was going, and we talked about music, which was an interest of his and mine. He then asked what I was doing. I said that I had started to think about random matrices because of a possible link with modelling random magnetism and I'd looked at the work by Mehta[32]. It's really algebraically fearfully involved and hard to get the feel for. One of the published results seemed to be in error by a factor of root two. It seemed to be in error - but that might simply have been my own lack of comprehension. I asked myself how I might I check this?" The concern was over the Wigner semi-circle law for the (semicircular) eigenvalue density of a very large Hermitian matrix. Sam said: "There is another way of thinking about this…" and then he started talking about the problem. He said: "The problem one always has here is averaging a logarithm." I didn't know this at the time, but he'd used this trick about five years before in his polymer work. You probably can find the references to it. I didn't know this, but Sam just got a piece of paper and sketched something out. He said that the $ln(x)$ is the coefficient of $n$ as $n$ goes to zero in the expansion of $x^n$. I thought: "What the hell does he mean? I put two and two together later." He said: "I think you should be able to think of the problem this way and use polar coordinates and replication." I just went away, and I thought it was an interesting problem on its own. I then did the Wigner semi-circle calculation using the replication method. I realized that we ought to be able to extend this to the case where the matrix

[30] Wigner semicircle distribution: https://en.wikipedia.org/wiki/Wigner_semicircle_distribution

[31] It is unclear when this encounter took place. A yearly meeting on that theme was held in Manchester in those years. See, *e.g.*,

- 10th Annual Solid State Physics Conference Manchester, UK, 3-5 January 1973. "Conferences," *europhysics news* **3**(12), 1 (1972). https://doi.org/10.1051/epn/19720312001
- 11th Annual Solid State Physics Conference Manchester, UK, 2-4 January 1974. "Conferences," *europhysics news* **4**(12), 1 (1973). https://doi.org/10.1051/epn/19730412001
- 12th Annual Solid State Physics Conference Manchester, UK, 6-8 January 1975. "Conferences," *europhysics news* **5**(9), 8 (1974). https://doi.org/10.1051/epn/19740509008b

[32] M. L. Mehta, *Random Matrices* (New York: Academic Press, 1967).

elements $J_{ij}$ have a finite non-zero, positive mean so that the random matrix elements would have a normal distribution with a finite non-zero mean. I got the results out for what happens when you have a random matrix ensemble with a finite mean as well as the normal distribution. That was all in the paper that Sam and I wrote[33].

Sam, by this time had been chairman of the Science Research Council[34], which is the most powerful scientific government [agency]. It's the interface between government funding and funding coming into university. Sam's political interests in science politics were taking off at this time and he still retained this huge collection of research students and post docs in Manchester and so he was furiously busy. I remember I completed the calculations and then contacted Sam at the SRC. I said: "I've written up this work that we were talking about over coffee. Would you be interested in the joint publication? If so, then you'd better see the manuscript." I suggested we find time in his busy schedule and suggested he might like to come up to Birmingham to give a seminar on whatever he wanted. Sam welcomed the idea and came up to Birmingham. We had a useful discussion about the draft manuscript, and he made some nice helpful comments on, and small changes to, the manuscript, so that what got published had been through a kind of checking process by Sam. One of the things that happened shortly afterwards, was my getting into trouble with the Vice Chancellor's office for inviting the Chairman of SRC to give a talk at the University without informing them. Sam for his part was just happy to come and talk about physics for a day. You can perhaps picture the situation. It was after this that David Thouless, Mike Kosterlitz and I started talking.

**PC:** Before we jump into that, I'm trying to understand something. These discussions you were having with Sam, was this prior to his work with Phil Anderson, or was it contemporary to it?

**RJ:** [0:27:55] I would guess it's very roughly contemporaneous. Sam mentioned to me the idea of what a spin glass was because I picked it up at Imperial—where the experimental problems were of great interest. I hardly knew Anderson and I certainly wasn't aware that he was working on such things.

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[33] S. F. Edwards and R. C. Jones, "The eigenvalue spectrum of a large symmetric random matrix," *J. Phys. A* **9**, 1595 (1976). https://doi.org/10.1088/0305-4470/9/10/011

[34] Science Research Council: https://en.wikipedia.org/wiki/Science_and_Engineering_Research_Council

**PC:** So, your paper on random matrices was published later than the Edwards-Anderson series, because that's largely because he was busy with other things?

**RJ:** [0:28:49] I guess so. There was also some other work that Sam had done on random matrices with the late Mark Warner[35]—in Cambridge. I think Sam had asked me: to have a look at it before Mark published it. It arrived from Mark. I made some comments and returned the manuscript to Mark. I think it was published about three years later[36] than our work.

**PC:** As you said, after that work, you went and talked to your colleagues Kosterlitz and Thouless at Birmingham.

**RJ:** [0:29:37] One of the things that worries me in talking to you is that I'm not absolutely sure about the right sequence of what happened 50 years ago as there was a great deal of activity over a short period. But I do know that the paper that Sam and I wrote on large *N*x*N* random matrices with a finite mean was criticized by two other authors, who said it was just wrong[37]. I really was pretty confident it wasn't. I had been talking to Mike and David about this. There was indeed a serious error in this paper which criticized our work, and which yielded a completely different answer. Our finding was that if the individual matrix elements had a finite mean, then you got an isolated eigenvalue of the random matrix that split off from the Wigner semicircular band of eigenvalues, when the scaled mean is bigger than the variance. This isolated eigenvalue lies outside the Wigner band and sits there. If the scaled mean is smaller than the standard deviation then it is absorbed in the band, and is a minor resonance, which is neither here nor there. They came up with a completely different answer. I started talking to Mike and David about this and it was really quite productive, because we realized when we were talking together, that you could make the problem look mathematically similar to the problem of an isolated heavy impurity in a host chain of coupled atoms in a solid - which I did know something about. Under the correct conditions it is possible to get a localized vibrational mode of oscillation outside the main band of phonon type vibrations which is mathematically pretty well the same as the isolated eigenvalue which Sam and I had predicted for a large random

[35] Warner started a thesis with Edwards, at Cambridge, but moved to Imperial under the supervision of Sherrington, in order to be closer to Edwards, in London. Mark Warner, *Molecular motion of polymeric systems*, Ph.D. thesis, Imperial College London (1977). https://library-search.imperial.ac.uk/permalink/44IMP_INST/fv0fdm/cdi_proquest_journals_1854126263

[36] S. F. Edwards and M. Warner, "The effect of disorder on the spectrum of a Hermitian matrix," *J. Phys. A* **13**, 381 (1980). https://doi.org/10.1088/0305-4470/13/2/007

[37] V. K. B. Kota and V. Potbhare, "A note on the ensemble-averaged eigenvalue spectrum of large symmetric matrices," *J. Phys. A* **10**, L183 (1977). https://doi.org/10.1088/0305-4470/10/11/002

matrix ensemble whose elements have a finite mean finite mean using the *n→0* replica trick. Whether it was David's, Mike's or my suggestion I'm not really sure, but that's what we used. We were able to reproduce exactly what Sam and I obtained in the random matrix problem in an unambiguous way (and we also found the mistake in the other paper[38] which criticized our work). It was an interesting vindication of the usefulness of using the replication method in the context of the random matrix problem.

David Thouless, did you know him? He was ferociously talented and very quick. You repeatedly had to say: "Again. Sorry, can you repeat that?" in most discussions. His understanding of everything was so deep. I was very lucky to be working with him. Of course, the work that Mike Kosterlitz and David Thouless were doing on the topological phase transition was starting at that time[39]. Mike, by that time, had of course a permanent lectureship in Mathematical Physics. Somewhere between the random matrix paper with Sam Edwards, and the publication of the work with Kosterlitz and Thouless, appeared the seminally important Sherrington-Kirkpatrick paper[40] which proved something of an inspiration to many others and used the *n*→0 replica trick to look at the thermodynamics of a particular model of a spin glass. It has proved to be, the starting point for a host of authors who sought to understand some of the puzzles involved in constructing a soluble model of a spin glass. I reread it recently. It's not an easy paper, but it's very nice indeed. David Sherrington and Scott Kirkpatrick were worried about the interchange of the limits *n* goes to zero, *N* goes to infinity. At the end of their *PRL*, one of the problems associated with their particular model was that it gave the result that the entropy becomes negative at appropriately low temperatures. In the kind of model which they studied, with discrete Ising spins, that should not be the case and the result was not expected. They were well aware of this, and they flagged it up very clearly in their paper. So, they were speculating on what might be the cause of this negative entropy problem. Thouless, Kosterlitz and myself were prompted to ask if you can't get an exact physically reasonable solution of their model, let's see if there's another model we could look at and which was soluble was soluble in the absence of disorder. That gave us the idea of using the Berlin-Kac spherical model[41], which is beautiful in its own way and exactly soluble in the absence of disorder. By introducing a normal distribution of interactions between spins and averaging the

[38] R. C. Jones, J. M. Kosterlitz and D. J. Thouless, "The eigenvalue spectrum of a large symmetric random matrix with a finite mean," *J. Phys. A* **11**, L45 (1978). https://doi.org/10.1088/0305-4470/11/3/002
[39] Berezinskii-Kosterlitz-Thouless transition: https://en.wikipedia.org/wiki/Berezinskii%E2%80%93Kosterlitz%E2%80%93Thouless_transition
[40] D. Sherrington and S. Kirkpatrick, "Solvable Model of a Spin-Glass," *Phys. Rev. Lett.* **35**, 1792 (1975). https://doi.org/10.1103/PhysRevLett.35.1792
[41] Spherical model: https://en.wikipedia.org/wiki/Spherical_model

logarithm involved in calculating the Free Energy's logarithm of the partition function over the distributions of these interactions and using the $n \to 0$ replica trick, the solution came out really quite quickly. Once again, the results obtained using the replica trick checked exactly with arguments based on the known properties of the spectrum of a random matrix. As a model of a real physical system, it isn't, and in a sense it's a theorist's dream. However, It did turn out to be an exactly solvable of a form of spin glass. That's the genesis of our *PRL*[42]. David Thouless in particular continued to worry about the problems which Sherrington and Kirkpatrick had encountered and clearly wanted to pursue this further.

**PC:** Before we go in this direction, I have a couple of questions. If I understand correctly, the work on the spherical model came after your work on the random matrices, and you having had discussions with David and Mike about the random matrix confusion.

**RJ:** [0:35:42] Yes, roughly speaking. Honestly, there wasn't much time between all these things. There was a lot of work done in a short period. We were talking together about random matrices and the spin glasses and the use of random matrices in modelling such spin glasses, and I think Sherrington and Kirkpatrick's seminal paper came out at about the same time. So, I'm not quite sure now what kicked what off.

**PC:** You also mentioned Kac's model. Mark Kac had obtained also a similar result to the one you obtained with Sam Edwards a few years before but did not publish it. There was a preprint that that circulated but it didn't come out to print until decades later[43]. I was curious if you were aware of that, or if that had ever popped on your radar.

**RJ:** [0:36:32] No. I met Kac once. He came to Manchester when I was a research student and gave a seminar and Sam introduced me. That was the only time. There were a couple of offprints. I probably still have them somewhere, but I'm retired now, so I don't have quick access to some of these older things. There was one by Kac, but I cannot remember for the life of me what it was about. I think it was something to do with impurities in an otherwise ordered system.

**PC:** So, this would have been in the late '60s, '68-'69?

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[42] J. M. Kosterlitz, D. J. Thouless and R. C. Jones, "Spherical model of a spin-glass," *Phys. Rev. Lett.* **36**, 1217 (1976). https://doi.org/10.1103/PhysRevLett.36.1217

[43] M. Kac, "On certain Toeplitz-like matrices and their relation to the problem of lattice vibrations," *Trondheim preprint series Arkiv for Det Fysiske Seminar i Trondheim* 11-1968 (1968); M. Kac, "On certain Toeplitz-like matrices and their relation to the problem of lattice vibrations," *J. Stat. Phys.* **151**, 785–795 (2013). https://doi.org/10.1007/s10955-012-0675-7

**RJ:** [0:37:18] Yes, this was at the time when I was doing my PhD. Around 1969-1970.

**PC:** So, Mark Kac and Sam Edwards knew of each other and had even met?

**RJ:** [0:37:31] Yes. Well, you know that Sam had been at Harvard. He was a student of Schwinger[44]. That's how he got interested in functional integration and field theory.

**PC:** So, he met Mark Kac at that time?

**RJ:** [0:38:58] He must have.

**PC:** Before I interrupted you, you were mentioning Thouless and Kosterlitz's interest in the spin glass conundrum and the associated surprises. What was your impression of their interest? Were you following closely their interest and their discussions?

**RJ:** [0:38:25] On the work on spin glasses, yes. They had a very talented Brazilian research student, Jairo de Almeida[45]. (I don't quite know where he is now.) He has produced several papers, some of which have my name on, but they all have Kosterlitz and Thouless as co-authors, that's for sure. It was typical of his insight that David was worried that when he looked at the way Sherrington-Kirkpatrick had addressed the problem and also the way that everything else was handled, you ended up using the Hubbard-Stratonovich transformation[46]—the so called auxiliary field identity—and you look for extrema in the exponentials in which the auxiliary field over which you are integrating , is independent of the replication indices. Each spin carries two indices in the calculation: one refers to the site and the other labels the particular replica you are sitting in. That leads, in the case of $\mathbf{s}_j.\mathbf{s}_j$, type interactions, to things like an auxiliary field which has a general form like $y_{\alpha\beta}$, which couples different replicas. They were interested in understanding why the Sherrington-Kirkpatrick model yielded negative entropy at low temperatures. Sherrington and Kirkpatrick were quite open about this problem. Was it that the $N$ goes to infinity and $n$ goes to $0$ could not be interchanged? But David Thouless then began to wonder if maybe one should not be looking at having just one auxiliary field for $y$, instead of

[44] Julian Schwinger: https://en.wikipedia.org/wiki/Julian_Schwinger
[45] P. Charbonneau, *History of RSB Interview: Jairo de Almeida*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 23 p. https://doi.org/10.34847/nkl.7de8emt7
[46] Hubbard-Stratonovich transformation: https://en.wikipedia.org/wiki/Hubbard%E2%80%93Stratonovich_transformation

a $y_{\alpha\beta}$, in other words you should perhaps consider breaking replica symmetry. David wondered if that was the source of the difficulty and was right in this. He wrote a paper with Jairo de Almeida, in which they looked at the stability problem[47]. They noted that in performing the saddle point integrations one should not assume that there is one replica symmetric *y* but should rather expand $y_{\alpha\beta}$ about the replica symmetric form *y,* as far as quadratic terms in the exponential and investigate the stability of the resulting quadratic form. For stability, all the eigenvalues of the matrix in the quadratic form this ought to be either all positive or negative. The essence of the paper that David Thouless and Jairo de Almeida wrote is very beautiful but involves horrendous algebraic calculation. They found that indeed there were problems which involved your switching over from being on either a maximum to minimum (or vice versa) in taking a limit as *n* goes to zero. They showed that in the high-temperature region, that is above what would have been the spin glass transition temperature of Sherrington and Kirkpatrick, the stability calculations that they were doing with de Almeida produced identical results to those of Sherrington and Kirkpatrick but that once you got into the spin glass phase, it looked as if there was a sign change and you were in fact looking at a maximum rather than a minimum of the free energy, and so you got a different expression for the free energy at low temperatures. They suggested that maybe replica symmetry breaking should be looked at more carefully.

**PC:** You mentioned the work you did with Jairo and Mike and David about the finite *m* component version of the model[48]. You had looked at the spherical one before.

**RJ:** [0:43:00] That was interesting because our spherical model was subject to the standard spherical constraint, $\sum_{i=1}^{N} {s_i}^2 = N$. I don't know quite where it started, but David and Mike went over to United States for the summer and asked if I could look after Jairo. It was a great pleasure to do so because he was one of the really strongest self-propelled students I have encountered. It was a pleasure talking to him. Jairo did much of this work almost unsupervised. He would knock on my door occasionally and check if his results were reasonable. It's a pleasure to work with people like that. I think I might have suggested his exploring the other version of the spherical model in which one has *m* component spins in which $m \to \infty$ I'm not sure, I don't want to claim that anything which wasn't mine or wasn't my idea. Indeed, it turned out that you can get exactly the same results as

[47] 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

[48] J. R. L. de Almeida, R. C. Jones, J. M. Kosterlitz and D. J. Thouless, "The infinite-ranged spin glass with *m*-component spins," *J. Phys. C* **11**, L871 (1978). https://doi.org/10.1088/0022-3719/11/21/005

in the standard Berlin-Kac spherical model - but only when *m* is actually infinite. It cannot be less than infinity. It's got to be exactly there. That ensures you'll find the quadratic form ceases to be positive, or positive semidefinite of something. I can't remember which. You could see where the problems were coming from and you could see that you were going to end up, as in the other work, on the wrong branch of the free energy. That was again from Jairo's significant contribution.

**PC:** I think that after that collaboration with Jairo, you largely left the field of random matrices and disordered systems. What drew you away? Or what took you away?

**RJ:** [0:45:12] David published a paper with Phil Anderson in which they use a form of the diagrammatic expansion in the mean-field theory[49], which again explored this business about the eigenvalues. It backed up the work that Jairo had been involved with, and the spherical model work that we had done. But then I was aware that there were other people starting to think about breakdown of replica symmetry, one of one of whom was of course Mike Moore[50] and his group in Manchester. I decided not to take any further active interest in this and got involved in different problems.

**PC:** Did you nevertheless keep abreast of the work that was happening in these ideas of replica symmetry breaking?

**RJ:** [0:46:45] Not really, no. I had a succession of other students and some very different problems that I worked on. Towards the end of my career, I had some very talented research students. Gurjeet Dhesi[51] continued to explore some further problems on random matrices. By chance, as I will explain, I started looking at some models of wave guide propagation, which you could handle with path integration[52]. You connect together two fibre optic guides, (the sort of device that they stick down your throat if you're unlucky) and join them together with a so-called coupler in between them. You want to work out how much energy goes from one to the other. You can set this thing up as a problem in path integration rather like

[49] D. J. Thouless, P. W. Anderson and R. G. Palmer, "Solution of 'solvable model of a spin glass'," *Philo. Mag.* **35** ,593-601 (1977). https://doi.org/10.1080/14786437708235992

[50] P. Charbonneau, *History of RSB Interview: Michael Moore*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 26 p. https://doi.org/10.34847/nkl.997eiv27

[51] Gurjeet Singh Dhesi, *Part 1: Spectral properties of random matrix ensembles. Part 2: Sabolev inequality in [phi] [superior] 4 (x) quantum field theory*, PhD thesis, Birmingham University (1988). https://birmingham-primo.hosted.exlibrisgroup.com/permalink/f/vmc2c6/44BIR_ALMA_DS2178999220004871

[52] See, e.g., C. C. Constantinou and R. C. Jones, "Path-integral analysis of tapered, graded-index waveguides," *JOSA A* **8**, 1240-1244 (1991). https://doi.org/10.1364/JOSAA.8.001240

quantum mechanics in two dimensions. The engineers were using brute force on these problems. We found a whole series of problems where you can get analytical solutions to some realistic distribution of refractive indices in the coupler. The key lies in identifying a model distribution of the refractive index in the guide. In the simplest model of an optical fibre one might choose the refractive index to be uniform everywhere but near the edges of the fibre—in order to encourage total internal reflection. There were a significant number of distributions of refractive index distributions that proved physically useful in the description of the coupler and for which the Feynman Path integrals could be performed analytically. This produced a significant major series of paper with Costas Constantinou[53], who now has a chair in engineering in Birmingham. It was one of these occasions where my knowledge of solid-state physics and path integrals proved of use in an area that was clearly engineering. I was asked if I could talk to a colleague who I had known and lectured to as a student when I taught a mathematics course in an engineering department for a number of years at the start in my career. This colleague, Mick Mehler[54], I remembered as being very talented person, and he became the director of the BT Research[55] laboratories at Martlesham. I remember meeting him and he said: "Do you remember me, Ray? You lectured to me." He was ferociously gifted. I do remember that. He had left Birmingham, and had used the techniques of differential geometry, to solve antenna problems for which he had learnt General Relativity in order to do this. We're still in contact. Mick Mehler told me that he himself had a talented research student who was talking about path integrals and asked me if I knew what they were?" My reply was to say that they are part of the background of any theoretical physicist although I could not claim to be an expert in their use. The student was a Cypriot and Commonwealth Scholar called Costas Constatinou, and he proved to be very talented and hardworking, and I ended up supervising his PhD. He is now a good friend of mine. The work we did together produced six substantial papers.

This work was later taken over by my last research student in Birmingham, a man called Paul Hollister[56] who is no longer working in an academic area. He did some beautiful variational calculations, which backed up some of

[53] Constantinos Christofi Constantinou, *Path-integral analysis of passive, graded-index waveguides applicable to integrated optics*, PhD Thesis, University of Birmingham (1991). https://birmingham-primo.hosted.exlibrisgroup.com/permalink/f/vmc2c6/44BIR_DR2_DS581

[54] Mick Mehler, "Mick Mehler," *IEEE* (2007). https://ieeexplore.ieee.org/iel5/4446726/4451309/04451310.pdf (Consulted July 14, 2023.)

[55] BT Research: https://en.wikipedia.org/wiki/BT_Research

[56] Paul David Hollister, *A study of the use of Feynman path integrals in paraxial optical propagation*, PhD Thesis, University of Birmingham (2002). https://birmingham-primo.hosted.exlibrisgroup.com/permalink/f/vmc2c6/44BIR_ALMA_DS21125364260004871

the analytical work that we had done. It's the sort of problem that you get in quantum mechanics, where you've got an imaginary amplitude $e^{iS}$ instead of statistical mechanics with $e^{-\beta H}$. This also produced a series of papers I didn't return to the other things, but would prick up my ears if somebody was talking about these.

**PC:** Do you have any insight into how your Birmingham colleagues reacted to replica symmetry breaking ideas, or were you already disconnected from them and these ideas?

**RJ:** [0:51:12] No. Eventually, of course, Mike and David moved to the USA. It was a great loss for us; definitely your gain to have them both. David, of course, died only relatively recently. I saw Mike and David in [2017][57]. The university gave them an honorary degree here, just after they had won the Nobel Prize.

**PC:** During you time at Birmingham. Did you ever teach about spin glasses?

**RJ:** [0:51:50] No. I gave a general postgraduate course in statistical mechanics and doing simple renormalization calculations. Lots of my experimental colleagues had heard the language of RG being used, but they didn't really know what it was about. Mike Kosterlitz, I remember, had given a short course on RG methods before he left Birmingham. I had a term's sabbatical, so I decided it was time that I learnt this stuff up properly, (or at least to an approximation of what was learning properly).

**PC:** Is there anything else you would like to share with us about that era that we made overlooked or missed?

**RJ:** [0:52:45] For myself, I think, I consider myself extremely lucky. I worked with Sam Edwards, who was an absolute inspiration, even if not the easiest person to follow. He was so busy. When you read his papers, as I mentioned earlier, you can see the ideas are there, but they don't always shine through in the mathematics. I was rereading some of these papers to remind myself before we talked. I think Sam's insight must have meant that he regarded much of the work going on around him as the tidying up of details at the edges. That was not his style of physics. Then, working with Mike and David was an inspiration. Mike was actually a very good friend. We occasionally played squash together (he was much better than I was) or went out for curry. David, I would see at concerts. I think that it is

[57] "Nobel Prize-winning scientists awarded honorary degrees," *University of Birmingham* (11 July 2017). https://www.birmingham.ac.uk/news-archive/2017/nobel-prize-winning-scientists-awarded-honorary-degrees (Consulted June 26, 2023.)

with time that I have realised just how talented all these people all were, [albeit] in their very different ways, and perhaps almost orthogonal in their ways of thinking and expressing themselves. I just think I was very lucky to share some part of that. I make no claims for my abilities, of course. I just think I was lucky. At that particular time, the spin glass and the replication work felt really rather beautiful. I was quite pleased. The paper I wrote with Sam on random matrices, was quite elegant in some ways and I was quite pleased with that. Anything else you'd like to ask?

**PC:** Just one last question. In closing, do you still have notes, papers, or correspondence from that epoch? If yes, do you intend to deposit them in an academy archive at some point?

**RJ:** [0:55:08] I don't know. At some stage, I need to go through things. I retired when I was 67. I continued working and just lecturing for five years after that, and then COVID came…. I have only recently gone back in and tried to sort out things in my office, but eventually I'll get kicked out of that as the predators come hunting for space. It's interesting, though. I'll see if I can locate the Kac paper. I do however remember that Sam Edwards' secretary in Manchester hadn't been told how his name was pronounced and asked if I knew when Dr. *Kack* was due to arrive coming- to some considerable amusement.

**PC:** Thank you very much for your time.

**RJ:** [0:56:04] Is that helpful? I'm sorry I'm vague on an awful lot. It's been a long time ago and I'm really quite disconnected now. It has proved hard work rereading some of the papers with which I was involved in the past. I'm just very lucky to have worked with Sam Edwards and David Sherrington at the start of my career, and then with David Thouless and Mike Kosterlitz further down the line. I am grateful to them for all the insights I gained from them.

**PC:** Thank you.