History of RSB Interview: Rowan T. Deam

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Interviewers:

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Location:

Over Zoom, from Dr. Deam's home in Melbourne, Australia.

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PC: Good morning, Dr. Deam. Thanks again for joining us. As we discussed ahead of time, the goal of these interviews is to go over to genesis of the ideas behind replica symmetry breaking, which in your case concerns mostly the work did during your thesis days. But before we dive into that, I have a few background questions I'd like to ask. First of all, can you tell us a bit more about your family and your studies before starting university?

RD: [0:00:31] I was born over here, in Australia, [and] moved to the UK at about the age of seven and a half… my father worked in the oil industry. He was a chemical engineer, and he got asked to come over to Great Britain. So we packed our bags and we went over there. It was supposed to last for three years, and it never ended. So basically, my education was British. My interest in science, I suppose, was influenced by my father, who's a chemical engineer, as I said. He worked for British Petroleum and ended up doing energy economic modelling using linear programming at the time. A thousand rows was considered to be a huge model in those days. It was the days of Hollerith cards[1] and that sort of things. Do you know what a Hollerith card is?

PC: The 72-row punch cards, right?

RD: [0:01:45] Yeah. All that sort of stuff. Anyway, then I went to school, obviously. I was lucky at school that we had some really good science teachers. The chemistry teacher, Jack Dowrick, he was probably the best of them. And Jim Bennett, the physics teacher, he was great. Then Mr. Pentecost was the maths teacher. I was inclined to do mathematics. My father was—I would say he was upset about that—but he said: "You got to do chemistry, mate. Really, you know, that runs in the family." I preferred physics, although actually I have to admit at the time I used to do better at

[1] Hollerith cards: https://en.wikipedia.org/wiki/Punched_card#Hollerith's_early_cards

chemistry. Anyway, I ended up doing physics, which is where I wanted to be. But I always had a mathematical bent, or interest, I should say.

PC: What then led you to pursue a PhD in theoretical physics at Manchester, in particular.

RD: [0:02:56] That's an interesting story. I went to Southampton University and the physics department was very good there, although I didn't know how good it was. The reason I selected Southampton university is that they had a dedicated dojo and I'm really keen on judo. That's why I went there. So I did the degree there. During the degree, I wanted to do a little bit more mathematics, so I got permission to take one or two of the maths courses to go towards the degree. (I think there was one on integral equations. I can't remember the other.) That sort of says that I was more interested in the theory side, in the mathematical side of things. Now, come the great laying on degrees and whatnot. I didn't manage to get the first[2], which meant I couldn't… I wanted to do research, and my chosen field would have been particle physics, but they had a cutoff in particle physics. If you didn't get a first, you didn't get to study particle physics. Ok. Fair enough. I had a look around and there was nothing that really interested me. I thought: "Well, I'm going to have to go and work in industry." One of the tutors there said: "There's this guy up in Manchester who is fairly theoretical and he's counting the conformations of the polymer chains.[3]" I thought: "My god, that sounds boring." I thought about it a bit [more]. I don't know whether I looked up papers or whatnot, but I thought: "Right, you know, if it's mathematical, if it's physics, I'll do it."

I went up to Manchester and it was great! The first year was at Manchester because Sam then moved to Cambridge, and on that move to Cambridge… (Manchester, by the way, the judo was even better, so that was a double plus.)

The problem [I was given] was the free energy of rubber[4]. That was the problem. I got to understand what the problem was fairly quickly but building up the sort of understanding and mathematical ability took quite some time to get going. At the time, he was working with Geoffrey Allen[5]

[2] First Class Honours: https://students.duke.edu/info-for/students/incoming-students/common-experience/

[3] See, *e.g.*, Y. Oono and T. Ohta, "The Coarse Grained Approach in Polymer Physics" In: P. Goldbart and N. Goldenfeld, eds, *Stealing the Gold: A celebration of the pioneering physics of Sam Edwards* (Oxford: Oxford University Press, 2007).

[4] Rowan Thomas Deam, *Entanglements and excluded volume in rubber*, PhD Thesis, Cambridge University (1974). https://www.repository.cam.ac.uk/handle/1810/250693}

[5] Geoffrey Allen: https://en.wikipedia.org/wiki/Geoffrey_Allen_(chemist)

and Geoffrey Gee[6] over in the chemistry department, which was just the other side of the road. They were looking at swelling of rubber. They looked at the vapor pressure of, I think, benzene, but I'm not quite sure about the rubber, to work out what the free energy of the rubber was when it was swollen, and to get the contribution of the free energy from the cross-links[7].

It was good, actually, because Sam sent me down to UMIST, the University of Manchester institute of Science and Technology[8], which at the time was a separate institute. There was a guy down there called Treloar[9]**,** who was quite famous in the rubber field. He did some lectures for the technology students there. Sam sent us down there and said: "You got to get a feel for this." That was pretty good as well.

When he moved to [Cambridge], he said: "You can join Allen and Gee, and do the chemistry side of it, or you can come to Cambridge and do the physics side of it." It was no contest, really. The chemistry they were doing there was actually quite difficult, because you had to control the number of [cross-links]. What they are trying to do is to eliminate the wasted links where you get a loop in a chain, which doesn't contribute to the free energy. They were doing some really cunning chemistry, which really I didn't get involved with, so that the chains couldn't cross-link with themselves. I don't know how it worked. Anyway, it struck me as being out of my league. My chemistry days, I thought, were well over.

So I went down to Cambridge. That was pretty good. The judo was not quite as good at Cambridge, but it was pretty good. There was two years there, and that was the time that Sam became chairman of the Science Research Council (SRC)[10]. I don't know quite when that happened, but as PhD students we didn't see him as much as maybe some of us would have liked. But that suited me. There were plenty of people to talk to that were quite smart, quite brilliant. You would bump into people like Phil

[6] G. Allen, "Geoffrey Gee, C. B. E. 6 June 1910-13 December 1996," *Biogr. Mem. Fellows R. Soc.* **45**. 184-194 (1999). https://www.jstor.org/stable/770271

[7] See, *e.g.*, G. Allen, M. J. Kirkham, J. Padget and C. Price, "Thermodynamics of rubber elasticity at constant volume," *Trans. Farad. Soc.* **67**, 1278-1292 (1971). https://doi.org/10.1039/TF9716701278

[8] University of Manchester Institute of Science and Technology: https://en.wikipedia.org/wiki/University_of_Manchester_Institute_of_Science_and_Technology

[9] L. R. G. Treloar: https://en.wikipedia.org/wiki/L._R._G._Treloar

[10] S. F. Edwards was chairman from October 1, 1973 to 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). http://www.chilton-computing.org.uk/acl/associates/politics/edwards.htm (Consulted September 11, 2022.)

Anderson[11], who was over for his summer sabbatical. (You know, he came over in the summer from Bell Labs.) It was very stimulating. In fact, Manchester was the same. People came and gave lectures and whatnot. Very stimulating time. As a young person who thought, "Oh! Physics, great," it was a wonderful time.

PC: In your thesis, you describe how to average over the topologies of rubber, in order to determine the free energy of the model you were trying to study. Do you remember where the idea came from, and how you learnt about it?

DR: [0:09:14] I learnt about it from Sam. He published… Let me try to get the argument. There was an argument over how many degrees of freedom a cross-link took away. It was $n\ kT$, or $n/2\ kT$. Whatever it was, Sam did the first calculation with the replica method and got $n/2$[12]. He was happy with that, but I can understand why you might want a PhD student to work it through and try and pick it apart and what not. The rest of it, that came basically in the last year of my thesis was putting in the excluded volume and, in particular, the entanglements. I don't think that was expected. I didn't expect it anyway. But basically, the method that I learnt from Sam was well established. Well, in my mind, it was well established. You had to take an average of something which was being taken out equilibrium, without the cross-links going back to equilibrium. I looked at it as: "You gotta take an average of an average." But it's not, if you see what I mean, the straightforward average of an average. It's an average of something that is changed. It's a neat method of doing that.

In the last couple of years… That was the other thing. When you did a PhD there, the supervisors liked to send you off to summer schools. (I don't know if you do the same thing in the North America.) There was one in Leeds, which was very polymer oriented[13]. There was one in Loughborough, at which various people from industry came and gave lectures on the different sorts of polymers and what their problems were and whatnot[14]. The final one, which was my last year at Cambridge, was

[11] See, *e.g.*, Andrew Zangwill, *A Mind Over Matter: Philip Anderson and the Physics of the Very Many* (Oxford: Oxford University Press, 2021).
[12] S. F. Edwards, "The statistical mechanics of rubbers," In: A. J. Chomp and S. Newman, eds. *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* (Plenum Press, 1971), 83–110.
[13] Science Research Council Summer school on Polymer science and technology, hosted by the University of Leeds, July 1971.
[14] Science Research Council Summer school on Polymer science and technology, hosted by Loughborough University, July 1972.

the Les Houches summer school in 1973, in France[15]. That was just brilliant, because it was all theoretical. It was called *Molecular Fluids*. De Gennes was there as well[16]. You had lectures from really quite important people in the field, although at the time I didn't realize it. They were just these stars, and you didn't realize that it was happening.

PC: Did Sam bring the replica trick early on in your study? Was this the first calculation he got you to work on, or did this come up later?

RD: [0:12:10] That was it, basically. I can't remember what the first supervision was, but it would certainly have been in the first year. The added advantage is that I had to read Feynman and Hibbs' book as well[17]. That was a connection with quantum mechanics, which is like particle physics and whatnot, so I thought: "This [thesis topic] was not a bad choice after all! It's quite mathematical." The days were spent filling wastepaper basket with calculations that had gone wrong. My measure of success is how full the wastepaper basket was. I liked that, actually. The best part of it were the people around. There were some very stimulating conversations. You'd talk about their problems, and they'd talk about your problems. That was the best part of it. There were some really bright people around. You do your first degree, you come out thinking: "That's it. I know everything now. I'm going to show the world." When you finish your PhD it's: "Bloody hell! I know nothing."

PC: Sorry for the technical question again, but in your thesis, you write an explicit expression for the partition function, for integer number of replicas n, and you state that the result can be analytically continued to the reals. Was this analytical continuation ever a source of concern or was this a natural way to approach it?

RD: [0:14:10] I could say I never really thought about it. The only thing I thought about the replica system was that maybe you could get some second moments or third moments if you went to n^2 and n^3 and all the rest. But no, it just seemed a mathematically sound way of getting the result that you wanted. The only thing you learn about dimensionality was this business [that in] three dimensions you can get a steady state and two dimensions you don't. It's the same in electromagnetic theory, when you're calculating the field around a wire rather than around a sphere. A

[15] *Molecular Fluids*, Roger Balian and Guilaume Weill, August 1973, Les Houches, France. Proceedings: R. Balian and G. Weill, eds. *Molecular Fluids/Fluides Moléculaires* (London: Gordon and Breach Science Publishers, 1976).

[16] P.-G. de Gennes, "Nematodynamics," In: R. Balian and G. Weill, eds. *Molecular Fluids/Fluides Moléculaires* (London: Gordon and Breach Science Publishers, 1976), 393-401.

[17] R. P. Feynman and A. R. Hibbs, *Quantum Mechanics and Path Integrals* (New York: McGraw-Hill, 1965).

wire, there's a logarithmic term. You got to put an outside bit. It's the same thing with a polymer wandering around. It comes up in three dimensions and I don't think it does in two dimensions. Anyway, that's the only dimensionality thing that I came across that was interesting. I didn't really think about what it meant. Your question was non-integer dimensionality, is that right?

PC: Non-integer number of replicas, actually. You were solving for integer *n*, then taking *n*=0. So, you're implicitly doing an analytical continuation of *n* to the reals. Was this ever a concern?

RD: [0:16:08] When I say I'm interested in mathematics, I'm more an applied mathematician than a pure mathematician. The pure mathematician in me, if there was one, would say: "What does this all mean mathematically?" But to me, you look at the formula and say: "Yes, you get the average that you want if you take the limit $n \to 0$." I'm ashamed to say that was good enough for me.

PC: What was the reception to your work? How was the thesis received? Or when you were talking about it in these schools.

RD: [0:16:46] I gave a preliminary talk at Les Houches about it[18]. At the time, Sam had already moved on into the dynamics of polymer melts and things like that, which he was far more interested in[19]. I think I was a bit piqued actually, because I thought: "If that's more interesting I should be doing that. I haven't solved this one yet." I think the interest came after the publication[20]. Because the thesis was basically… Sam had a look at it and said: "I'd like to rewrite bits of it and stick it into a *Proceedings of the Royal Society*." After that, I got lots of reprint requests. But at the time I was doing industrial research for the electricity board[21]. And I had just gotten married[22] and that sort of things, so priorities had changed. I think I went to a couple of meetings discussing these sorts of things, but it was only out of interest. In some ways I regretted leaving university, but in other ways, you see… When it came time to choose what to do, I looked around me and I saw all these really bright people trying to get positions in academia.

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[18] R. T. Deam, "The Free Energy of a Phantom Chain Network," In: R. Balian and G. Weill, eds. *Molecular Fluids/Fluides Moléculaires* (London: Gordon and Breach Science Publishers, 1976).
[19] See, *e.g.*, S. F. Edwards, "The Configurations and Dynamics of the Polymer Chain," In: R. Balian and G. Weill, eds. *Molecular Fluids/Fluides Moléculaires* (London: Gordon and Breach Science Publishers, 1976).
[20] R. T. Deam and S. F. Edwards, "The theory of rubber elasticity," *Philo. Trans. Roy. Soc. London. Ser. A* **280**, 317-353 (1976). https://doi.org/10.1098/rsta.1976.0001
[21] Marchwood Engineering Laboratories of the Central Electricity Generating Board: https://en.wikipedia.org/wiki/Marchwood_Power_Station
[22] R. T. Deam married Mary C. Montague in August 1974.

I thought: "Well, I'm going to lose out on the competition. I may as well go straight into industry." That's what I ended up doing. The other thing is that I didn't really fancy teaching. Somewhere in the back of my brain, I realized that if you became an academic you obviously had to do some of that. That wasn't really my forte. I liked to do research, and so when I was offered this position at the electricity board doing industrial research. [I thought]: "That's pretty good." Having said that, I still got summer students because there was a scheme where the kids who were about to go to university came and worked with you for a month or two. Actually, once I got a really bright student who really pushed my project along. When you come across a mind like that you think: "Wow! Gee!"

PC: In your thesis, you cite an unpublished work by Prof. Edwards on spin glasses that relates to the work you were doing on this replica business[23]. How aware were you of Prof. Edwards' own efforts in this direction? And what can you tell us about it? Did you hear about?

RD: [0:19:38] I certainly heard about it. I think I knew that he and Phil Anderson were having conversations, but this was at the time when he was chairman of the SRC, so he was spending most of his time down in London. We had the occasional supervision. It's just one of those things. I didn't mind it at all. I think some of the students would have preferred more hands-on [involvement], but it suited me. So, I knew about the spin glasses, but I didn't know much about it.

PC: Did he share preprints with you, or did you see drafts of the calculation? Or was it just what he would tell you about it?

RD: [0:20:21] No. I didn't look into it in any great detail at all. I certainly didn't see preprints or anything.

PC: As you mentioned, afterwards you left the field of statistical mechanics. You kept abreast a bit of the advances in the field of the rubbers, but did you pay any attention to spin glasses?

RD: [0:20:51] No. Not really. My mind was filled with predicting dynamic instabilities in nuclear boilers at the time. Engineers, really want to know…. If you make a prediction, it really has to be spot on. Otherwise, it could cost quite a bit of money. I've always kept an oversight of general physics whatnot. I'm a member of the Institute of Physics[24]. I still get the magazine.

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[23] Deam (1974), p. 71, mentions a preprint that was most likely to become 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

[24] Institute of Physics: https://en.wikipedia.org/wiki/Institute_of_Physics

You can get it online now. I still like to look at those sorts of things and see what's happening. But no, I didn't keep up with the field at all.

PC: Is there anything else about this era that we may have missed or that you think would be interesting to share with us?

RD: [0:21:57] To me, as a young PhD student, everything was interesting. It was like a young kid in a lolly shop. We must have had lots of conversations about different things. Maybe there were conversations about spin glasses. I can't remember the details, but I can remember there would always be something interesting to chat about. In Manchester, the sixth floor was occupied by the theoretical physicists. They had their own seminar room, or tearoom. You would go up there and chat to everybody. The interesting thing about Sam's supervision and his lectures is that he'd start off at a really low level. You'd think: "Yes. I'm following this. That's fine." And suddenly he'd take off. You think: "I've missed this." You'd come out after the supervision with a couple of pages full of formulae and whatnot. Us, students, would sit around on the table and we'd bring bits and say: "Well, I recognize what this bit was, but where does this one come from here?" That was actually quite good. That was his technique. You have to be really careful at first, at the beginning of those lectures. You have to listen really carefully, because all of a sudden, you're in stratosphere before you realize it.

I can't think of anything… I suppose as soon as we end this call, I'll think; "Oh, I should have mentioned so and so" But I can't think of anything in particular.

PC: How often did you meet with him while he was in London? Was it once a week, once a month?

RD: [0:23:45] It was certainly less than once a week. He had a very busy schedule when he was chairman of the SRC. The SRC awarded all the grants for the English universities. There were occasional meetings in London. I can remember a few get-togethers, not just one-on-one supervisions. I think the supervisions must have dropped to something like once a month or something like that. I can't remember the details. As long as you got the general direction… There were lots of people around there to talk to about things. Admittedly, a lot of the people were doing solid-state physics, rather than… When he came along to the Cavendish that was the first of the—if you like—soft matter physics that started up. It was called Theory of condensed matter, the group. When he came along, they got this extra bit to it, which was the polymers and liquid crystals and all those sorts of things, even going into biophysics now, I think. Thank heavens, I'm not in that because that seems to be such a big field.

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

RD: [0:25:34] I don't have anything, I'm afraid. As I said, I filled lots of wastepaper baskets. I guess that access to those is now well and truly gone. There was no email in those days, so there's no trail to follow. The only thing I've got from that era are the copy of the Les Houches lectures, in 1973. Not so long ago, before Sam died, there was a book published called *Stealing the Gold*[25]. Have you heard of that? That makes an interesting read as well. I look at it and I think: "Wow! Gosh! Was he busy or what?"

PC: Thank you very much for that conversation.

RD: [0:26:37] Oh! My pleasure.

[25] P. Goldbart and N. Goldenfeld, eds, *Stealing the Gold: A celebration of the pioneering physics of Sam Edwards* (Oxford: Oxford University Press, 2007).