

# History of RSB Interview: Byron W. Southern

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## Interviewers:

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## Location:

Over Zoom, from Prof. Byron's home in Winnipeg, Manitoba, Canada.

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**PC:** Good morning, Prof. Southern. Thank you very much for joining us. As we discussed ahead of this interview, we will be covering the period during which your work overlapped with the spin glass and replica symmetry breaking ideas, which we bound roughly from 1975 to 1995. Before we get to that, we have a few background questions to ask so that we can better situate the work you did during that period. Would you please tell us a bit about your family and what your studies were like before you started university?

**BS:** [0:00:43] Before I went to university, I was obviously in the Ontario high school system, which went up to grade 13<sup>1</sup>, and from there I went to York University<sup>2</sup>, which opened a brand new campus in Toronto, for four years in the interdisciplinary science program, which allowed you to sample biology, chemistry, physics. In the the last two years, you specialized to whatever field you wanted. I went into physics. From there, I went to McMaster University. I worked there on a Masters<sup>3</sup> and PhD<sup>4</sup> with David Goodings, who was working in the area of magnetism, spin excitations in magnetic heavy rare earth systems.

**PC:** What drew you to physics first, and then to theoretical physics for a PhD?

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<sup>1</sup> Thirteenth grade: [https://en.wikipedia.org/wiki/Thirteenth\\_grade](https://en.wikipedia.org/wiki/Thirteenth_grade)

<sup>2</sup> York University was founded in 1959. See, e.g., [https://en.wikipedia.org/wiki/York\\_University](https://en.wikipedia.org/wiki/York_University)

<sup>3</sup> Byron Wayne Southern, *Theory of Spin Waves in the heavy Rare Earth Metals*, MSc Thesis, McMaster University (1970). <http://hdl.handle.net/11375/20200>

<sup>4</sup> Byron Wayne Southern, *Magnetoelastic Effects in Rare Earth Metals And Compounds*, PhD Thesis, McMaster University (1973). <http://hdl.handle.net/11375/8502>

- BS:** [0:01:42] I liked problem solving. I was pretty adept at mathematics, and I didn't really like the biology. It was too much memorization. I gravitated to the physics problem-solving type of area.
- PC:** Then you went on for a NRC-funded postdoc<sup>5</sup> at Imperial College.
- BS:** [0:02:08] Yes. I had a NRC postdoc with David Sherrington<sup>6</sup>.
- PC:** What drew you to Imperial College and/or to David Sherrington? What was the connection there?
- BS:** [0:02:18] He had been working on rare earth systems as well when he was in La Jolla—I think—spiral systems and magnetic systems<sup>7</sup>, and I was interested in that. Imperial College had a very good reputation. The scholarship allowed me to go anywhere I wanted, so that's what I chose, Imperial College.
- PC:** Did you know him ahead of time, or only through his publications?
- BS:** [0:02:46] No. Only through his publications.
- PC:** You contacted him and he accepted you?
- BS:** [0:02:50] Yes, because it didn't cost him anything.
- PC:** We understand that the Sherrington group was part of the larger Coles group<sup>8</sup>. Can you describe how the Coles and the Sherrington groups functioned when you arrived there?
- BS:** [0:03:09] David was a reader at the time. He was a theorist. I think that Norman March<sup>9</sup> was the head of the department at that time. Bryan Coles, of course, was the head experimentalist in that group, so there was a lot

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<sup>5</sup> National Research Council Canada: [https://en.wikipedia.org/wiki/National\\_Research\\_Council\\_Canada](https://en.wikipedia.org/wiki/National_Research_Council_Canada)

<sup>6</sup> See, e.g., P. Charbonneau, *History of RSB Interview: David Sherrington*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 39 p. <https://doi.org/10.34847/nkl.072dc5a6>

<sup>7</sup> See, e.g., D Sherrington, "Coupled nuclear and electronic spin wave modes in non-commensurate spiral and conical spin structures," *J. Phys. C* **3**, 2359 (1970). <https://doi.org/10.1088/0022-3719/3/11/017>; D. Sherrington, "The tilted helix: an intermediate magnetic phase," *J. Phys. C* **6**, 1037 (1973). <https://doi.org/10.1088/0022-3719/6/6/017>

<sup>8</sup> D. Caplin, "Bryan Randell Coles. 9 June 1926 — 24 February 1997," *Biog. Mem. Fell. R. Soc.* **45**, 51-66 (1999). <https://doi.org/10.1098/rsbm.1999.0005>

<sup>9</sup> Norman Henry March (1927-2020). See, e.g., "In memoriam: Professor Norman March", University College Oxford, 19 November 2020. <https://www.univ.ox.ac.uk/news/in-memoriam-professor-norman-march/> (Consulted November 16, 2021)

of interactions in terms of trying to understand spin glasses, or random magnets at the time. (I think that Bryan Coles was attributed with coining the term at a conference. I can't remember where that was now.) David was interested in it, but there was also Nick Rivier<sup>10</sup>, who was in the department, and he had an interaction with Sam Edwards<sup>11</sup>. Sam Edwards was coming to the department once a week or so and talk with Nick. He talked to Nick about this replica method that he and Anderson were looking at. It was a classical spin model and trying to average the logarithm function led to this  $n \rightarrow 0$  limit of replica. I talked to Nick quite a bit about this and David did as well. We got a preprint as well of a paper by Edwards and Anderson<sup>12</sup>. That sparked our interest even more, so we started looking at that problem ourselves. David and I looked at adding competition between ferromagnetism and spin glass<sup>13</sup>.

**PC:** When did you actually first hear about spin glasses, and how were you introduced to them as concept?

**BS:** [0:04:50] As I said, it was through mainly conversations with Nick Rivier, who had frequent conversations with Sam Edwards, and also through Bryan Coles. Bryan Coles' group was obviously very interested in those things and there were a number of talks and seminars on it.

**PC:** Had you met John Mydosh<sup>14</sup> when he visited, for instance?

**BS:** [0:05:14] Yes. I had met him.

**PC:** So this topic was mentioned in group meetings or in seminars?

**BS:** [0:05:21] Seminars. There weren't so many group meetings. It was just casual conversations, with me anyway. That was the main way of doing it.

**PC:** You mentioned what led you to work with David Sherrington on the competition between ferromagnetic and spin glass order. What was the

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<sup>10</sup> P. Charbonneau, *History of RSB Interview: Nicolas Rivier*, transcript of an oral history conducted 2021 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 3 p.

<sup>11</sup> Sam Edwards: [https://en.wikipedia.org/wiki/Sam\\_Edwards\\_\(physicist\)](https://en.wikipedia.org/wiki/Sam_Edwards_(physicist))

<sup>12</sup> 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>

<sup>13</sup> 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>

<sup>14</sup> P. Charbonneau, *History of RSB Interview: John Mydosh*, transcript of an oral history conducted 2021 by Patrick Charbonneau, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 19 p.

<https://doi.org/10.34847/nkl.e1e3ob87>

reaction of Nick Rivier and the rest of the Coles group to this work when you started talking about it?

**BS:** [0:05:55] I can't remember too much about the reaction. I think they were very interested in it. The paper by Edwards and Anderson maybe had appeared by that time. Ours was a follow up to that. Then, David Sherrington and Scott Kirkpatrick ... When I left in Imperial College in '75, I went to the Institut Laue-Langevin<sup>15</sup> and David went on a sabbatical to IBM in Yorkton Heights<sup>16</sup>, where he collaborated with Scott Kirkpatrick and they did an Ising version of the calculation. That obviously got a lot of interest, because it appeared in Phys. Rev. Letters<sup>17</sup>.

**PC:** Were you aware of David's work on that model before he left for IBM?

**BS:** [0:06:49] Yes. Both of us had worked on it together a bit, but we didn't really finish it, and we didn't really do the numerical work. That was all done at IBM.

**PC:** Once you moved to ILL, you started working with Peter Young<sup>18</sup>, in particular.

**BS:** [0:07:07] Peter and I both went to ILL at the same time. That was in 1975. Peter had been a student of Roger Elliott at Oxford. He had been working on various magnetic problems. He had been working with Robin Stinchcombe as well on renormalization methods<sup>19</sup>. Peter and I sort of collaborated on various problems having to do with spin glasses. In the end, we applied the real-space renormalization group technique to three-dimensional spin glasses<sup>20</sup>.

**PC:** You were both sort of postdocs. Can you tell us a bit how ILL worked?

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<sup>15</sup> Institut Laue-Langevin (ILL): [https://en.wikipedia.org/wiki/Institut\\_Laue%E2%80%93Langevin](https://en.wikipedia.org/wiki/Institut_Laue%E2%80%93Langevin)

<sup>16</sup> Headquarters for IBM Research: [https://en.wikipedia.org/wiki/Thomas\\_J.\\_Watson\\_Research\\_Center](https://en.wikipedia.org/wiki/Thomas_J._Watson_Research_Center)

<sup>17</sup> D. Sherrington and S. Kirkpatrick, "Solvable model of a spin-glass," *Phys. Rev. Lett.* **35**, 1792 (1975). <https://doi.org/10.1103/PhysRevLett.35.1792>

<sup>18</sup> P. Charbonneau, *History of RSB Interview: A. Peter Young*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 20 p. <https://doi.org/10.34847/nkl.2fef8760>

<sup>19</sup> A. P. Young and R. B. Stinchcombe, "A renormalization group theory for percolation problems," *J. Phys. C* **8**, L535 (1975). <https://doi.org/10.1088/0022-3719/8/23/001>; "Real-space renormalization group calculations for spin glasses and dilute magnets," *J. Phys. C* **9**, 4419 (1976). <https://doi.org/10.1088/0022-3719/9/24/012>

<sup>20</sup> B. W. Southern and A. P. Young, "Real space rescaling study of spin glass behaviour in three dimensions," *J. Phys. C* **10**, 2179 (1977). <https://doi.org/10.1088/0022-3719/10/12/023>

- BS:** [0:07:56] At ILL, you were basically what they called a staff scientist. We were really in the Philippe Nozières' group<sup>21</sup>. He had a large group of theorists in what we called the Theory College<sup>22</sup>. Duncan Haldane<sup>23</sup> came a bit later. Bernard Derrida<sup>24</sup> was in that group, Hans Fogedby, Ted [Theodore] Burkhardt was there. There was a number of us working on various different problem. Each person was basically independent in some sense. It wasn't really a postdoc, because we didn't see Philippe that often. We certainly interacted with another group across the way, in CENG<sup>25</sup>, with Jacques Villain<sup>26</sup>, who wasn't too far away. There were joint seminars between the two groups.
- PC:** At the of time you were there, how many of you were there?
- BS:** [0:09:01] There was about 10 or 12 in the theory group.
- PC:** Can you tell us a bit what was the genesis of this work? Did the two of you just started talking and then realized you had a common interest in spin glasses?
- BS:** [0:09:17] Peter starting applying the real-space method to random bonds in spin glasses and doing numerical calculations. We were discussing it a lot. It then developed that we thought we could do something analytical with it in terms of Gaussian distributions, and what not. So that continued until we put that paper together. Then, Peter left there and went to IBM as a visitor for a year, and after that he went to the math department in Imperial college. (Then on to Santa Cruz, eventually.) We've kept in contact. We did publish something later on<sup>27</sup>. In the '90s, I went there for a visit. I spent a year with him.
- PC:** In what ways were your approaches or your knowledge of spin glasses complementary in working on this problem?

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<sup>21</sup> Philippe Nozières: [https://en.wikipedia.org/wiki/Philippe\\_Nozi%C3%A8res](https://en.wikipedia.org/wiki/Philippe_Nozi%C3%A8res)

<sup>22</sup> See, e.g., *ILL Annual Report 1975* (Grenoble: Institut Max von Laue—Paul Langevin, 1975).  
<https://www.iaea.org/inis/collection/NCLCollectionStore/Public/07/279/7279538.pdf> (Consulted November 17, 2021.)

<sup>23</sup> Duncan Haldane: [https://en.wikipedia.org/wiki/Duncan\\_Haldane](https://en.wikipedia.org/wiki/Duncan_Haldane)

<sup>24</sup> See, e.g., P. Charbonneau, *History of RSB Interview: Bernard Derrida*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 23 p. <https://doi.org/10.34847/nkl.3e183b0o>

<sup>25</sup> Centre d'études nucléaires Grenoble: [https://it.wikipedia.org/wiki/CEA\\_Grenoble](https://it.wikipedia.org/wiki/CEA_Grenoble)

<sup>26</sup> Jacques Villain: [https://en.wikipedia.org/wiki/Jacques\\_Villain](https://en.wikipedia.org/wiki/Jacques_Villain)

<sup>27</sup> B. W. Southern and A. P. Young, "Spin stiffness in frustrated antiferromagnets," *Phys. Rev. B* **48**, 13170 (1993). <https://doi.org/10.1103/PhysRevB.48.13170>

- BS:** [0:10:19] I'm not sure. We were sort of on the same footing almost. I remember going to give a talk at Oxford about a different problem. (It was before going to ILL.) Then I met with Roger Elliott and Peter Young privately, and discussed the approach of Edwards and Anderson and discussed the problem with negative entropy and things like that with them, before going to ILL. We sort had met then. It was just sort of a 30-minute talk or so together. After that, when I got to ILL, I found out that Peter was going as well, so that conversation sort of continued.
- PC:** Do you know what Nick Rivier's reaction was to the Edwards-Anderson and to the Sherrington-Kirkpatrick ideas? He had been working on spin glasses from a different approach, so I was curious how this played out.
- BS:** [0:11:33] He never really expressed any opinion, so I don't know for sure. Not that I recall, at least.
- PC:** You also worked on percolation with Peter Young.
- BS:** [0:11:51] Yes. The effect of percolation on spin glass order with Peter and Pierre Pfeuty<sup>28</sup>. This was towards 1978-79, just before I left. That was a generalization of what we had done on the spin glass to include non-occupied sites on the problem.
- PC:** As you just mentioned you left ILL in '79 to take up a faculty position at the University of Manitoba. From your publication record at least, it seems that you left the field of spin glasses at that point.
- BS:** [0:12:26] I did, yes. I collaborated with another person at King's College, David Lavis<sup>29</sup>. We were looking at using renormalization group methods on other problems, so I didn't continue with the spin glass. Also, replica symmetry breaking came out in '79 as well. That was sort of evolving in another direction and I wasn't involved with that, so I didn't continue.
- PC:** Did you nevertheless keep abreast of these advancements?
- BS:** [0:12:56] Yes. I was interested in what was going on. I wrote a review article in '87 or so, trying to outline that<sup>30</sup>. That was a dedication to Allan

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<sup>28</sup> B. W. Southern, A. P. Young and P. Pfeuty, "Effects of percolation on spin glass order," *J. Phys. C* **12**, 683 (1979). <https://doi.org/10.1088/0022-3719/10/4/005>

<sup>29</sup> See, e.g., B. W. Southern and D. A. Lavis, "Renormalisation group study of a two-dimensional lattice model with directional bonding," *J. Phys. A* **13**, 251 (1980).

<sup>30</sup> B. W. Southern, "Spin Glasses: Recent Advances in Mean-Field Theory," *Can. J. Phys.* **65**, 1245 (1987). <https://doi.org/10.1139/p87-199>

Morrish<sup>31</sup>, who was head of our department and was obviously known in magnetism as well.

**PC:** In that review, in particular, you thank Bernard Derrida, Cirano De Dominicis and Peter Mottishaw for helpful discussions.

**BS:** [0:13:29] Yes, I had a sabbatical in Saclay in '86-'87 roughly. We discussed a lot of problems [during my] time there.

**PC:** Is that when you familiarized yourself with the advances in RSB?

**BS:** [0:13:47] Yes.

**PC:** I better understand now. Despite having re-immersed yourself in spin glass problems, you didn't really pursue much in this direction afterwards. Can you give us an idea or a feeling why?

**BS:** [0:14:03] I was getting a little more interested in what we call fully frustrated systems. I did publish a paper that was "Nonuniversality in two-dimensional frustrated lattices<sup>32</sup>" with Chui and Forgacs. That was in 1980, soon after getting to Manitoba. From there on, I was getting a little more interested in frustrated triangular systems, I was also a little more interested in excitations in magnetic systems, and applying rescaling methods to that problem, because there was another colleague at Manitoba, Peter Loly, who had been working on various excitation problems. It was more a problem of who you collaborated with easily<sup>33</sup>.

**PC:** At the University of Manitoba or elsewhere, did you ever teach a class that included notions of spin glass or replica symmetry breaking? If yes can you detail?

**BS:** [0:15:13] I never taught replica symmetry breaking. It required an advanced course in statistical mechanics really. We had a general course. I only gave the advanced course once, and I didn't really get into that problem. I only mentioned it. I also mentioned things like the KT

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<sup>31</sup> Allan H. Morrish: <https://academictree.org/physics/peopleinfo.php?pid=221379> (Consulted November 17, 2021.)

<sup>32</sup> B. W. Southern, S. T. Chui, G. Forgacs, "Non-universality for two-dimensional frustrated lattices?" *J. Phys. C* **13**, L827 (1980). <https://doi.org/10.1088/0022-3719/13/30/001>

<sup>33</sup> See, e.g., B. W. Southern, A. A. Kumar, P. D. Loly and A.-M. Tremblay, "Real-space rescaling method for the spectral properties of tight-binding systems," *Phys. Rev. B* **27**, 1405(R) (1983). <https://doi.org/10.1103/PhysRevB.27.1405>

transition<sup>34</sup> and thinks like that, but I didn't get into things like the details of replica symmetry breaking.

**PC:** We're about to reach the end of our conversation. Is there anything else that you would like to share with us that we might have overlooked or forgotten to explore?

**BS:** [0:15:57] I don't think so. I think that pretty well covers everything that I can remember anyway.

**PC:** Has your work and spin glasses been influential in any of the research efforts you have undertaken afterwards or is it a disconnected part in terms of your research program?

**BS:** [0:16:20] The methods, of course. You apply all these methods. The renormalization group method can be applied to all sorts of problems, whether it's magnetism or other problems. Mean-field theories are always useful. I'm continuing to collaborate with a group at Dalhousie now and it's not in the area of spin glasses or anything, but we're using a lot of these methods still<sup>35</sup>.

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

**BS:** [0:17:00] I may have some things in my office, but we haven't been on campus because of the pandemic. I think I have an original preprint of the Edwards-Anderson paper somewhere. As far as correspondence go, I don't think I have any real correspondence letters or anything, so that's about all I have.

**PC:** Your collaborators were always nearby.

**BS:** [0:17:27] That's right.

**PC:** Thank you very much for this conversation.

**BS:** [0:17:30] You're quite welcome. Good luck!

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<sup>34</sup> Kosterlitz-Thouless Transition: [https://en.wikipedia.org/wiki/Kosterlitz%E2%80%93Thouless\\_transition](https://en.wikipedia.org/wiki/Kosterlitz%E2%80%93Thouless_transition)

<sup>35</sup> See, e.g., A. Zelenskiy, T. L. Monchesky, M. L. Plumer, B. W. Southern, "Anisotropic magnetic interactions in hexagonal A B-stacked kagome lattice structures: Application to  $Mn_3X$  ( $X = \text{Ge, Sn, Ga}$ ) compounds," *Phys. Rev. B* **103**, 144401 (2021). <https://doi.org/10.1103/PhysRevB.103.144401>



**PC:** Thank you.