

# History of RSB Interview: Jairo de Almeida

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## Interviewers:

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## Location:

Over Zoom, from Professor de Almeida's home in Recife, Brazil.

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**PC:** Good morning, Professor de Almeida. Thank you very much for joining us. As we've discussed ahead of this interview, the main theme of our conversation is replica symmetry breaking and, in particular, the period from 1975 to 1995. But to get to that there are a few background questions I want to ask you. If you don't mind, could you tell us a bit about your family and your studies before you started university?

**JdA:** [0:00:35] I come from a lower middle class [family]. I didn't attend primary school, except for one year, when I was 10 years old<sup>1</sup>. Because of that, when I was in age to attend secondary school, I failed getting admission to a good secondary school. So my mother, who was a quite bright woman—she comes from the countryside; her parents were peasants—so she found me a third-class secondary school. I went to that school. It was a third class education. I didn't have a good education, primary schooling. At some point, I was doing very badly, due to a very bad mathematics teacher. So my mother found me... She was very bright, my father who died very early was a bright guy and was also poor. They got to the university through hardships. As I was on the verge of failing in a third class secondary school, and she knew that I was a smart guy, she found me a good tutor. That changed everything. From being on the verge of failing in a third-class secondary school, this guy took me by the hand and started teaching me fractions and all the math which is taught in secondary school. This made all the difference for me.

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<sup>1</sup> **JdA:** It was during 1960 at Escola Municipal Edmundo Bittencourt, in Rio de Janeiro. Their teachers left lasting sweet memories.

Knowing mathematics makes all the difference. If you know that you can pick up a mathematics book and read for yourself and learn it, then [you think:] “Wow! I am an intelligent guy.” The time went on and it came the time to get admission at the university. All the tests that I undertook I was successful [in]. I went to the university.

At that time—it was '69 when I went to the university—here in Brazil, the politics, everything was out of place<sup>2</sup>. No teachers in the university. Everything was put upside down. Sometimes the teachers would come; most of the time they didn't come. I saw that in the first two years of the university.

In the third year of the university, some guys—idealists, I would say—[wanted] to set up a physics department here in Recife. By miracle, I met one of them who had been my teacher in my first-year course of physics, in the electrical engineering [track]. (I was to be an electrical engineer.) He offered me a scholarship for doing physics. He said: “Oh! You did well in physics. Wouldn't you like to get a scholarship for doing physics?” At the time, I [thought]: “Well, I like to study. Then I'll get this scholarship. It's good money and I need money.” I accepted the scholarship for doing physics. Then I spent my time doing physics, studying physics and doing the electrical engineering course. At the end, the pay for graduates, as university physics teacher would be similar [to], or perhaps a little better than, for being an electrical engineer. So I said: “Ok. I'll be a physicist.” That is in reality what attracted me to physics. Because as a child, I could be anything: lawyer, ...

**PC:** What drew you towards theoretical physics, then?

**JdA:** [0:07:22] Here, there wasn't the option to be an experimentalist. There was no laboratory. [For you] to have an idea: [for] the lab classes, you just saw the teacher do the experiment my first year. The teacher would do the experiment and you would see it. Sometimes they would ask us to do some measurement also, but there was no such thing as a physics lab here. So there wasn't this option to be an experimental physicist.

**PC:** In your thesis, you mentioned that the CNPq<sup>3</sup> and the Universidade Federal de Pernambuco financially supported your graduate studies, and that K. A.

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<sup>2</sup> Brazilian Military Junta of 1969: [https://en.wikipedia.org/wiki/Brazilian\\_military\\_junta\\_of\\_1969](https://en.wikipedia.org/wiki/Brazilian_military_junta_of_1969)

<sup>3</sup> Conselho Nacional de Pesquisas [National Council for Scientific and Technological Development]: [https://en.wikipedia.org/wiki/National\\_Council\\_for\\_Scientific\\_and\\_Technological\\_Development](https://en.wikipedia.org/wiki/National_Council_for_Scientific_and_Technological_Development)

Chao helped you come to Birmingham<sup>4</sup>. Can you tell us a bit how that happened?

JdA: [0:08:30] Being attracted to the physics department that was being funded here, I saw many of my colleagues going to the United States, to Europe, to Sweden. At that time they invited some physicists to spend some time here, one year, six months. Professor Chao<sup>5</sup> was here in 1974, I think, or 1975. As being the best student in the class, I think I gave him a good impression during his stay here. When I finished my masters thesis<sup>6</sup> here, in Recife, I was admitted as a teacher at the university.

Then, they wanted me to be in the first class of people who would take their PhD here. But then I got fed up. I didn't see that it would take me very far. I decided to go elsewhere. I knew Professor Chao. He was a very fine teacher from the University of Linköping, Sweden, and I think he liked me. I fought my colleagues here. I said: "I want to go away." They said: "No. You assumed the compromise to stay here and you are not going away. We are not going to support you." I said: "I will go." Then, I asked the CNPq and the university. Then I wrote to Professor Chao: "I want to go do a PhD elsewhere. Please find me a place." Very soon he sent me a letter saying: "Hi Jairo, I met professor David Thouless in an airport and I said that have this Brazilian student who wants to take a PhD. Can you take him?" Professor Chao said to me: "I assured him you are a good student. Then write him to get admission to Birmingham University." That was a place I didn't know where it was. I didn't know about David Thouless. I was a complete innocent in being a physicist. I hadn't heard of David Thouless at that time, although he was famous [already] at that time. So Professor Chao made me this great favor. It was a miracle. It was a casual encounter in an airport. Then, I decided to go. I told my colleagues: "I'm going to take my PhD in Birmingham with Professor Thouless."

Then the CNPq refused to give me the fellowship. The university said: "Ok. You are going. You have the right by law to go, but you are going with half

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<sup>4</sup> Jairo Rolim Lopes de Almeida, *On the mean field theory of spin glasses*, PhD Thesis, University of Birmingham (1980). [https://birmingham-primo.hosted.exlibrisgroup.com/permalink/f/1q64cdc/44BIR\\_ALMA\\_DS2181906250004871](https://birmingham-primo.hosted.exlibrisgroup.com/permalink/f/1q64cdc/44BIR_ALMA_DS2181906250004871)

<sup>5</sup> See, e.g., João Florêncio, Jr and K. A. Chao, "Antiferromagnetic Ground State in the s-Band Hubbard Model," *Phys. Rev. Lett.* **35**, 741 (1975). <https://doi.org/10.1103/PhysRevLett.35.741>

<sup>6</sup> J. R. L. de Almeida, *Espalhamento Inelástico de Luz por Excitações Magnetoelásticas* [Inelastic Light Scattering by Magnetoelastic Excitation], MSc Thesis, Universidade Federal de Pernambuco (1975). <https://www.ufpe.br/ppg fisica/teses-e-dissertacoes-defendidas/1975> (Consulted September 1, 2021) See also J. R. L. de Almeida and S. M. Rezende, "Scattering of Light by Magnetoelastic Excitations in Ferromagnets," *Phys. Stat. Sol. B* **73**, 661 (1976). <https://doi.org/10.1002/pssb.2220730235>; J. R. L. de Almeida and Cid B. de Araujo, "Inelastic Scattering of Phonons by Magnons in Ferromagnets," *AIP Conf. Proc.* **34**, 245 (1976). <https://doi.org/10.1063/1.2946088>

salary.” I was very adamant. I said: “I will go.” Thanks to a very generous uncle of mine, he gave me the tickets to Birmingham. I knew that I had money just for half a year or so, because [that is what] half of salary would allow me to stay in Birmingham. But I took the chance thanks to my wife. My wife was fundamental in that decision. “Ok. Let’s go! Let’s go! Things will change.” Then we went to Birmingham. I arrived there, knowing that maybe I would have to come back at the end of the year due to lack of support. But I wrote again to CNPq and they changed their mind. They gave me the fellowship. That allowed me to continue. Would that answer your question?

**PC:** Yes. It’s a fascinating story. Once you got to Birmingham, how did you hear about spin glasses and the replica trick?

**JdA:** [0:14:58] The thing was, I was a new student when I arrived in Birmingham. At that time the mathematical physics department in Birmingham, which had been a very famous department under Rudolf Peierls<sup>7</sup>, was in the process of being closed. Although Professor Skyrme<sup>8</sup> was the head of the department, I never heard of Professor Skyrme and all the famous people that were there. There was a Portuguese girl<sup>9</sup>, a student of Mike Kosterlitz, who said: “Ok. Stay here in your office, and just keep quiet. After six months or so, they will give you a problem. Stay here, and ask them something to do.”

So I went to see Professor Thouless and Mike Kosterlitz. Professor Thouless, it was fun because he had a big office full of preprints and papers. I said: “Ok. Give me something to read.” He gave me a pile of papers. I went to Mike, and Mike gave me some papers too. And I asked the Portuguese girl: “What are you studying?” [She told me Ma’s] *Modern Theory of Critical Phenomena* book<sup>10</sup>. I didn’t know anything about statistical mechanics, so I bought Ma’s book and Stanley’s *Introduction to Phase Transition and Critical Phenomena*<sup>11</sup>, and started reading those books. Once in a while I went to Mike’s office to ask him about renormalization group and other [things].

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<sup>7</sup> Rudolf Peierls: [https://en.wikipedia.org/wiki/Rudolf\\_Peierls](https://en.wikipedia.org/wiki/Rudolf_Peierls)

<sup>8</sup> Tony Skyrme: [https://en.wikipedia.org/wiki/Tony\\_Skyrme](https://en.wikipedia.org/wiki/Tony_Skyrme)

<sup>9</sup> Maria Augusta Oliveira Pereira dos Santos, *Crossover phenomena near phase transitions*, PhD, University of Birmingham (1979). [https://birmingham-primo.hosted.exlibrisgroup.com/permalink/f/vmc2c6/44BIR\\_ALMA\\_DS2181631810004871](https://birmingham-primo.hosted.exlibrisgroup.com/permalink/f/vmc2c6/44BIR_ALMA_DS2181631810004871)

<sup>10</sup> Shang-keng Ma, *Modern theory of critical phenomena* (Reading : W.A. Benjamin, 1976).

<sup>11</sup> H. Eugene Stanley, *Introduction to Phase Transition and Critical Phenomena* (Oxford: Oxford University Press, 1971).

After, I think, three or four months, Mike called me and said: “Ok. Pick this paper” (It was Sherrington-Kirkpatrick’s *Phys. Rev. Lett.* paper<sup>12</sup>.) “Read it and see if you can do the calculation. And then we have a problem for you.” Then I went through all the calculation in the paper of Sherrington-Kirkpatrick. That was the first time I really heard about the replica trick and spin glasses. After three or four months being in the middle of Ma’s book and Stanley’s book, Mike called me and said: “Read this paper and do this calculation that it contains.” And so I did. Afterwards, he said: “David wants to talk to you.” That’s how I got in contact with spin glasses and the Sherrington-Kirkpatrick model. That was about May-June ’77.

**PC:** Do you know what was David Thouless’ interest in this work?

**JdA:** [0:18:59] No. Everything was new for me. I didn’t know anything about spin glasses. I even learned about the infinite-range models, because it is in Sherrington-Kirkpatrick’s model. In Stanley’s book, it’s Kac models. In Stanley’s book, he does the calculation for the pure system, the infinite-range ferromagnetic model, with all the details, so I was fascinated with all that calculation. But I didn’t learn that he had done the spherical model for the spin glass<sup>13</sup>. Did I answer your question?

**PC:** A bit. Do you know why Mike Kosterlitz, who gave you this paper first for you to read, was interested in the problem then?

**JdA:** [0:20:20] I guess they had been interested in this spin glass problem—at least Professor Thouless—for a very long time. I think Mike went into the problem through Professor Thouless, because Professor Thouless interested him in the problem. By ’75, I think, in the Michael Moore interview<sup>14</sup>, he mentioned that Mike showed him the Hamiltonian, and so they did the spherical model by ’75, I think. So Mike was already working with Professor Thouless on spin glass problems. In my first months there, they’ve wondered what kind of problem they would give me. After three or four months, they said: “Well, let’s make him work on spin glasses.” And so it was.

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<sup>12</sup> D. Sherrington S. Kirkpatrick, “Solvable model of a spin-glass,” *Phys. Rev. Lett.* **35**, 1792 (1975).

<https://doi.org/10.1103/PhysRevLett.35.1792>

<sup>13</sup> 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>

<sup>14</sup> 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>

**PC:** Can you tell us a bit about the genesis of your first paper with Thouless, about the instability of the SK solution<sup>15</sup>? How did that come about?

**JdA:** [0:21:38] Yes, for sure. But first, I think I have one thing that may interest you very much. You are doing a very important work [with] this historical effort. It is your show, so I am here to say anything that you ask, and I don't have time limitation, as I told you. Since you contacted, I said: "Ok. Let me go through what I have left." You always ask: "Have you kept anything..." Unfortunately, like many people, I didn't keep much. There was a leak in my garage which ruined many things. But a dozen letters, I saved. And I found one very important for you. This letter is from Professor Thouless to Phil Anderson on the 25 of November 1975. That is roughly one month after Sherrington and Kirkpatrick sent their paper [to] Physical Review Letters. As you know, Professor Thouless was one of the referees. In this letter, which I will read: "Dear Phil, have you people made any more progress with this spin glass since I saw you?" In this letter, he gives the final details of the TAP equations<sup>16</sup>. Then, he discussed with Phil Anderson the details, points out some mistake or error in the methods, about random matrix. It says: "Well, I was getting the wrong result because in Mehta's book<sup>17</sup> there is this factor and it is wrong. I deduced that proof and it's a misprint or an error." Then he makes all the details and sets up the TAP equations. To answer your question, at least partially, at the end of this letter... Maybe if you have access to Phil Anderson's letters or archives, maybe he's so famous that probably somewhere there must be an archive about Phil Anderson.

**PC:** Actually, I'm told that very little is in his archive<sup>18</sup>, but that's a different story.

**JdA:** [0:24:26] In this letter—that David Thouless sent to him on the 25<sup>th</sup> of November 1975—he comments at the end about this Sherrington-Kirkpatrick paper, saying... I'll read for you if you have the patience. A paragraph before, he says: "We should write the [TAP] paper, but I wouldn't want to write it myself." He even mentioned that he made the initial numerical so-

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<sup>15</sup> 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>

<sup>16</sup> D. J. Thouless, P. W. Anderson and R. G. Palmer, "Solution of 'solvable model of a spin glass'," *Phil. Mag.* **35**, 593-601 (1977). <https://doi.org/10.1080/14786437708235992>

<sup>17</sup> M. L. Mehta, *Random matrices and the statistical theory of energy levels* (New York : Academic Press, 1967). <https://doi.org/10.1016/C2013-0-12505-6>

<sup>18</sup> *Philip W. Anderson Papers*, Manuscripts Division, Department of Special Collections, Princeton University Library. <http://arks.princeton.edu/ark:/88435/fn106z01j>

lution for the TAP equations and how he did it. And he tries [for] Phil Anderson [to] encourage Scott Kirkpatrick<sup>19</sup> to do the numerical calculation for solving the TAP equations. Then he said: “We should write a draft, but I wouldn’t want to do it myself.”

In the last paragraph, he says: “I suspect the error in the SK treatment comes in the assumption that the main contribution to the steepest descent integral comes from a point at which all the  $n$  values of  $x^\alpha$  are equal and all the  $\frac{1}{2}n(n-1)$  values of  $y^{\alpha\beta}$  are equal”. That, I think, is the genesis of the RSB business. In this letter, he says it explicitly where the problem is. But he continues [on] to say: “Hard to argue about this when  $n$  is zero.” In the zero-dimensional space, it’s hard to prove it. [He] suspects where the problem is, told Anderson, but I don’t know if they continued. I have this letter.

Maybe you are wondering: “Why do you have a letter from David Thouless to Phil Anderson, and have you reread it?” That is very easy. I read it again because of you. I said: “I think that Patrick and Francesco would find this interesting.” I had this letter because Professor Thouless interested me in trying to solve the TAP equations using his method for the TAP. We even wrote a draft, a written version, but it wasn’t published. It is called “Further thoughts on the TAP solution”, but it had a problem, which was pointed out by Michael Moore and Alan Bray. We couldn’t circumvent the problem, so it wasn’t published. Professor Thouless gave me this copy of his letter, saying: “Ok. Read this to see some details of the TAP equations.

**PC:** That’s a fascinating letter. If there’s any way you could make us a copy, we would be extremely grateful. How did you get to work on the instability of the SK solution, after having read the paper and maybe the letter as well? Can you tell us about the genesis of that work?

**JdA:** [0:29:08] I will stick to Mike. As Mike [Kosterlitz] told many times—if you ever saw some of his Nobel presentation<sup>20</sup>—he always says: “I didn’t know anything. David knew everything.” Have you ever watched any of Mike Kosterlitz’s Nobel prize or similar talks? [He’d say:] “David knew everything, and I knew... I was after some problem to work on and David took

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<sup>19</sup> See, e.g., P. Charbonneau, *History of RSB Interview: Scott Kirkpatrick*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 24 p. <https://doi.org/10.34847/nkl.cba615t7>

<sup>20</sup> J. Michael Kosterlitz *Biographical*, The Nobel Prize (2016). <https://www.nobelprize.org/prizes/physics/2016/kosterlitz/biographical/>; John Michael Kosterlitz, *Topological Defects and Phase Transitions*, Nobel Lecture, December 8, 2016. <https://www.nobelprize.org/prizes/physics/2016/kosterlitz/lecture/> (Consulted August 31, 2021)

me by the hand and taught me all those strange ideas.” Then I said: “Ok. That’s fine. I’ll do that.” That is it.

Professor Thouless knew everything, and so he was thinking about this problem, I think, since 1975, after seeing the SK paper. He had almost everything in his pocket, when he asked me to do something. He was a very generous man, and very patient. He said then: “Ok. See this work by Sherrington-Kirkpatrick,” [for] which I had already redone all the calculation, and said: “The problem is with the replica symmetry thing that they are doing. If you do the stability analysis, then it is going to be negative.” But he had done it only for the case of zero ferromagnetic mean. Just for a simple case. “The problem is here, then let’s do the numerics.” He then said to me... I even have his handwriting [on]—I kept everything that was dear to me—the two sheets of paper that he wrote down: “You are going to do this calculation here, and you are going to find this, and you are going to do the numerics to trace out this line...” So he explained to me. I didn’t even know how to do the numerics, so I asked him: “Those complicated integrals, Gaussian integrals, two coupled equations, how the hell am I to do that.” He said: “Take this. You are going to do this way. Let’s transform these equations into partial differential equations, and then transform it for the coupled equation and the instability line in partial differential equation.” So he said: “Ok. It’s easy to solve the differential equation, because there are many methods. Then you go and write a FORTRAN program and do that in such increments<sup>21</sup>...” He was very patient and then I set out to do all that he asked me to do.

There was a computer. At that time we used punched cards. I spent a lot of time going to the computer and punching the cards. All that stuff that we used to do in that time. I still have the results of the computer numerics. The first date that I have in this big page... I don’t know if you’ve ever used [those] big pages that the IBM computers used. I went out to see the dates, and I think it is July ’77, the one that I have kept. So in July I was doing these numerics for the instability line and I had done all that proof carefully for the instability.

I insisted with him that we should do the complete [analysis] for the ferromagnetic piece. It was funny, because he didn’t work out the instability with a ferromagnet component. Then, I insisted sometimes with him and with Mike, because I was a very obedient student. So I said: “Why don’t we do the complete calculation?” Mike, I remember very well, saying: “Oh! It’s very difficult to do that, to increase the size of the matrix.” I said once to Professor Thouless: “Let’s do that.” He just stayed quiet, said nothing.

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<sup>21</sup> **JdA:** The approach was an improved Euler method.



After three times—I think—I insisted, he said: “Ok. Go ahead and do it!” Then I calculated that, trying to spend my time...

It is funny, because I didn’t know they were so famous. It was just a big brother for me, my older brothers, both. It was fun because other people looked at them as celebrities. But I wasn’t looking at them as celebrities. I was looking at them as my older brothers. Something like that.

After getting their permission to do the calculation, I went out and did it, and continued doing the numerics for the zero-ferromagnetic case. After finishing that, I went to Professor Thouless’ room. It was in the end of the afternoon. I went to his office and I said: “Professor Thouless, with the ferromagnetic component, there will be an instability line too. And part of the ferromagnet phase diagram is, below the instability line.” He looked at me not believing it. He said: “Are you sure that the ferromagnetic phase will have an instability too?” “There’s an unstable phase here.” And he said: “Well, if this is so, the entropy of the ferromagnet phase at zero temperature should [not] be zero too. Go and calculate it! Get the entropy in the ferromagnet phase.” I said: “Ok!” Then as I left his office, and he said: “Tomorrow morning, I need to have this calculation gone through.” I went home and I said: “Today, I will only sleep after getting the entropy at zero temperature, very close to  $T=0$ .” Then I spent some time doing it. Today, it’s very easy. Everything is in the market. Everybody [can] do [it]. But at that time, I remember some colleague [who was] having a hard time doing this calculation asking me how to do it. (The first time, it’s always very hard in anything.) Then I succeeded. Next morning, I went very happy. All the happiness of the world. I know he will come after me asking: “Have you done the calculation?”

Then, he arrived in my office. He and Mike were like old brothers, I would say. He knocked, and I said: “Yes professor.” “Did you do the calculation?” “Yes professor, I did.” “Show me!” I remember this because it was funny. Was it funny? It was typical. You cannot forget these things. I showed him and he said: “Well, I think you have a factor wrong in your exponent, because it goes like  $-e^{-J_0/J^2}$  .... I don’t remember exactly the exponent, but he said to me:

–You have a factor wrong there.

–I don’t think I have. I did it carefully.

–I did the calculation and you have a wrong prefactor here.

–Let me go to my office to check my calculation.

Then he left, and after half an hour or so he came back and said: “Ok. Jairo, you are right!” It was fun. He was a very simple man. That thing I remember very well.

I'm not quite sure, but I think it was after that instability in the ferromagnetic phase that he gave the first seminar on the instability lines in Oxford. I went along with him. I think Sherrington, or someone who was in Oxford, invited him to give a seminar. He gave a seminar. I guess it was August or something like that of '77. He said: "I'm going to give a seminar in Oxford. Why don't you come with me?" We went there. David, he went there on his own, but I went with Ray Jones<sup>22</sup>, who worked on that spherical model. Then he gave a seminar in Oxford. I was there to attend his seminar. I think it was the first time—at least as I see it. I don't remember he saying that he gave a seminar on that work before that.

**PC:** What was the reception to that work, in the seminar and in the preprint that you circulated?

**JdA:** [0:43:46] I think all the reception or effect of it was directed toward him. I didn't really see... I saw just the comments. Because if he went somewhere and talked... Once he went to the United States and said: "I talked to Kadanoff<sup>23</sup> and showed him our work. And Kadanoff asked: 'Is it just for Ising model or is it general? If you use Heisenberg? What this and that?'" Kadanoff was very happy about it, interested in seeing the work. He commented about that, but as his student I was a level below that thing. It was mainly his, because he was talking about it. He and Mike. Mike also knew since '75, I think, or '76.

I don't think that Professor Thouless did comment on it, but I don't he did blow it up for the wind that the problem is that. Few people would know that he had that in mind. Mike was surely one of them. Mike Kosterlitz, I'm sure, I heard him comment: "This symmetry has to be broken, but how to break this damn symmetry?" I remember him saying something, at least to us.

**PC:** Were you following the discussion about replica symmetry breaking at the time? And if yes, how? Say the work of Mike Moore, the work of Blandin<sup>24</sup> and others. Were you aware of it?

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<sup>22</sup> Raymund C. Jones obtained his PhD in theoretical physics from the University of Manchester (1970), under the supervision of Sam Edwards. He was then postdoctoral ICI Research Fellow in the Condensed Matter Theory Group of the Physics Department at Imperial College before taking up took up a lectureship in Mathematical Physics at the University of Birmingham in 1971. See, e.g., Raymund C. Jones, *Spin Waves in Disordered Magnetic Systems*, PhD Thesis, The University of Manchester (1970). [https://www.librarysearch.manchester.ac.uk/permalink/44MAN\\_INST/1r887gn/alma992983073517401631](https://www.librarysearch.manchester.ac.uk/permalink/44MAN_INST/1r887gn/alma992983073517401631); Honorary Professor Ray Jones Staff Profile, University of Birmingham. <https://www.birmingham.ac.uk/staff/profiles/physics/jones-ray.aspx> (Consulted September 17, 2021)

<sup>23</sup> Leo Kadanoff: [https://en.wikipedia.org/wiki/Leo\\_Kadanoff](https://en.wikipedia.org/wiki/Leo_Kadanoff)

<sup>24</sup> See, e.g., A. Blandin, "Theories versus experiments in the spin glass systems," *J. Phys. Colloques* **39**, C6-1499 (1978). <https://doi.org/10.1051/jphyscol:19786593>

**JdA:** [0:46:22] Sorry, but I didn't follow your question.

**PC:** Following your paper and about the same time, there were other groups who were trying to break that symmetry, as you said. Mike Moore did, for instance, and André Blandin, in France, was also trying to. Were you following those efforts?

**JdA:** [0:46:44] *Si!* Our paper was basically done. Our work was finished by September '77 or something like that. I think we wrote... I still remember very well one given day Professor Thouless came to me and said: "Ok. Jairo, write the paper. Write this stuff." I think that was about September-October '77. I'm amazed, because in less than one year I was writing a paper, while the other guys there, the students, could take two years or more. Then, I think: "Oh, great! from being taken as an imbecile as a child, I was working with one of the finest physicists of the world. Oh, that's great! Oh, how I should be... thank heavens!" That was about September, October that he came to me and said: "Write the paper, Jairo." And so I drafted that, of course after drafting it he polished it.

I think that by November we had that preprint that we sent out. Before we sent out that preprint, nobody ever talked about breaking replica symmetry. You see, I've been seeing dates. The *Journal of Physics A* received our paper on the first of December 1977. So we must have sent it by the end of November or mid-November, something like that, because that takes time. After we sent it out, people started working on the problem. I think Michael Moore and Alan Bray were the first ones to propose some scheme to break replica symmetry. You see the dates of their paper. All these papers that you mentioned are '78 or something like that. Nobody ever said anything about breaking replica symmetry before our paper.

Of course I was following very closely all that effort that people were doing. At first, I saw the paper by Bray and Moore<sup>25</sup>. That didn't make many sense to me, breaking the replica symmetry in two sets: one [with]  $m$  and [one with]  $n - m$ . I didn't see any sense in that, but the way they did their field theory they could somehow take the limit  $n \rightarrow 0$ . Because if you take that in the SK model and try to make  $n \rightarrow 0$ , that doesn't work there. I said: "This cannot be." But Blandin's scheme I remember very well, because he got a decrease in the (absolute value of the) entropy. That was very good.

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<sup>25</sup> A. J. Bray and M. A. Moore, "Replica-symmetry breaking in spin-glass theories," *Phys. Rev. Lett.* **41**, 1068 (1978). <https://doi.org/10.1103/PhysRevLett.41.1068>

I remember very well, because I met him in Les Houches summer school<sup>26</sup>. He asked to meet me. I didn't know him. I was an innocent guy from a place lost at the end of the world. So I didn't know anybody, but I was there and Blandin asked: "I want to talk to you." I was very happy and very sad simultaneously. Because when I was at the Les Houches summer school my wife got sick. Badly sick. And I had to leave the school, in the middle of this. It was exactly on the day Blandin was there, asking to talk to me. He almost got me by the arm to show me his work. It was exactly at the time that I had to leave the school, because of the trains. I remember [being] sad for not having the opportunity to discuss with him more closely. Because it would have been wonderful. But I was informed about his work. There were other people. I think he co-authored this work with Thomas Garel and another guy, I think<sup>27</sup>.

After that, I think it was Parisi who came out with his work<sup>28</sup>, at the end of '78 or something like that. Parisi always sent his preprints to Professor Thouless who—I think it was most of the time, or Mike Kosterlitz—would give me.

**PC:** Is that how you found out about the Parisi solution? Through a preprint that he sent to Mike Kosterlitz or to Professor Thouless?

**JdA:** [0:53:34] Yeah.

**PC:** What was your reaction to the Parisi's solution?

**JdA:** [0:53:50] I think it was... Having seen Michael Moore's and Blandin's, I just thought it was a clever generalization of Blandin's scheme, and that it might be the proper one. But it just appeared... We were scientists looking for a solution and that may be [it]. Looking back at the letters now... (I got half a dozen letters.) I got a couple of letters from Professor Thouless to Mike [Kosterlitz], saying: "I'm enclosing a preprint from Parisi. I'm very excited about what he finds." He got very happy with Parisi's solution. He started immediately reproducing... He says explicitly: "I don't think his numerics is very good. There must be some error in his calculation, but it's

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<sup>26</sup> Les Houches, Session XXXI, July 3-August 18, 1978. Cf. *La Matière mal condensée/III-Condensed Matter*, R. Balian, R. Maynard and G. Toulouse eds. (Amsterdam: North-Holland Publishing, 1979).

<sup>27</sup> A. Blandin, M. Gabay and T. Garel, "On the mean-field theory of spin glasses," *J. Phys. C* **13**, 403 (1980). <https://doi.org/10.1088/0022-3719/13/3/015>

<sup>28</sup> G. Parisi, "Infinite number of order parameters for spin-glasses," *Phys. Rev. Lett.* **43**, 1754 (1979). <https://doi.org/10.1103/PhysRevLett.43.1754>

very promising. I think it's the right solution." So we all started doing Parisi's scheme that resulted in a paper where we carried out the stability analysis<sup>29</sup>.

**PC:** Because, at that point, Professor Thouless was not at Birmingham anymore. So you were contacting him mostly through letters, is that right?

**JdA:** [0:55:44] Yes. Mike was still there. I was an orphan... as a child... and at the end I was orphaned of supervisors in Birmingham, because both Mike Kosterlitz and Professor Thouless left Birmingham, and I was left there alone in my last year<sup>30</sup>. Professor Thouless left and then after a while Mike left too. In my last year, only Ray [Jones] was there, who had worked on the spherical model, but wasn't into spin glasses anymore.

**PC:** So you wrote that last paper, where you studied the stability of the Parisi solution together nevertheless.

**JdA:** [0:56:47] Professor Thouless took some time—and Mike too—to sell his house. Once in a while he would be there and take care of some business that he left of something. I don't know. But only for short periods. I think that paper we finished while Mike was still there. I don't remember exactly.

**PC:** After completing your PhD, you yourself left Birmingham and went back to Recife to be on the faculty. Shortly thereafter you had a visitor from Porto, Eduardo Lage, who worked with you on a spin glass model<sup>31</sup>. Can you tell us a bit more about how that came about?

**JdA:** [0:57:40] Yes. I can tell you. Back in '78, I saw that advertisement about the Les Houches summer school, and I asked Professor Thouless: "Professor, can I go to this Les Houches summer school that you were announced as one of the lectures." He said: "Yes. You can. I'll write you a recommendation letter." So I applied to the Les Houches summer school. He gave me the recommendation letter. It's very nice to be with famous people, because when you have an application and the guy is famous, is a celebrity,

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<sup>29</sup> D. J. Thouless, J. R. L. de Almeida and J. M. Kosterlitz, "Stability and susceptibility in Parisi's solution of a spin glass model," *J. Phys. C* **13**, 3271 (1980). <https://doi.org/10.1088/0022-3719/13/17/017>

<sup>30</sup> **JdA:** When Mike left Birmingham Ray Jones was designed my official supervisor.

<sup>31</sup> E. J. S. Lage and J. R. L. de Almeida, "Stability conditions of generalised Ising spin glass models," *J. Phys. C* **15**, L1187 (1982). <https://doi.org/10.1088/0022-3719/15/33/003>; J. R. L de Almeida and E. J. S. Lage, "Internal field distribution in the infinite-range Ising spin glass," *J. Phys. C* **16**, 939 (1983). <https://doi.org/10.1088/0022-3719/16/5/020>

you know beforehand that you are going to be accepted. *It's so nice to be friend of the king...*<sup>32</sup> So I was accepted to the Les Houches summer school.

I don't know if you were going to ask me about that, but you asked some people about... At least to Bernard [Derrida]<sup>33</sup>. So I advance this question to you. For me, it was nice. A place to meet people. To learn a thing it was a complete waste of time, but to know people, to find people, to see people, to talk to people, it was wonderful. I met Gérard Toulouse<sup>34</sup>, Blandin. Cirano De Dominicis<sup>35</sup> wasn't there when I was still there, but I saw Phil Anderson<sup>36</sup>. Nozières<sup>37</sup> came around once and was very brightful in [his] talking. That makes himself noticeable. That's how I see him. I met Bernard Derrida, Thomas Garel. One Brazilian was there, Amós Troper<sup>38</sup>, who had been my teacher, back in the secondary school, and in the first year of university. I think now he is in Israel.

Then I met Eduardo Lage. A nice guy, from Porto. He had taken his PhD with [Robin] Stinchcombe in Oxford<sup>39</sup>. He [had] worked on the replica trick<sup>40</sup>. He used the replica trick for percolation problem etc. He knew much more than me about the impact of our work. So we became very good friends.

As an aside, he's a very rich man, he and his wife... Well, I didn't know that, but I have heard it. Just as an aside, Patrick, he gave me a wonderful week in Porto, because he said: "When you come back to Brazil, come to Porto. Come to visit me." I did accept. [I have] colleague who was also from Porto, but the invitation [was] of Eduardo. Because he and his wife were very rich, and they gave me an apartment with everything [furnished] in Porto, where I could stay as long as I wished to know Portugal. So I know Portugal because of him. It was very nice out of him. There were cars! Apartments! See how nice it can be, when you find the right people.

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<sup>32</sup> This passage is a reference to a poem by the well-known Brazilian poet, Manuel Bandeira, "Vou-me embora pra Pasárgada" In *Libertinagem* (Rio de Janeiro: Paulo, Pongetti, 1930) [https://en.wikipedia.org/wiki/Manuel\\_Bandeira](https://en.wikipedia.org/wiki/Manuel_Bandeira)

<sup>33</sup> 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.3e183b00>

<sup>34</sup> Gérard Toulouse: [https://en.wikipedia.org/wiki/G%C3%A9rard\\_Toulouse](https://en.wikipedia.org/wiki/G%C3%A9rard_Toulouse)

<sup>35</sup> Cirano De Dominicis: [https://de.wikipedia.org/wiki/Cyrano\\_de\\_Dominicis](https://de.wikipedia.org/wiki/Cyrano_de_Dominicis)

<sup>36</sup> Philip W. Anderson: [https://en.wikipedia.org/wiki/Philip\\_W.\\_Anderson](https://en.wikipedia.org/wiki/Philip_W._Anderson)

<sup>37</sup> Philippe Nozières: [https://en.wikipedia.org/wiki/Philippe\\_Nozi%C3%A8res](https://en.wikipedia.org/wiki/Philippe_Nozi%C3%A8res)

<sup>38</sup> Amós Troper: [https://pt.wikipedia.org/wiki/Am%C3%B3s\\_Troper](https://pt.wikipedia.org/wiki/Am%C3%B3s_Troper)

<sup>39</sup> E. J. S. Lage, *On the theory of disorder in the transverse ising model*, PhD Thesis, University of Oxford (1975). <https://solo.bodleian.ox.ac.uk/permalink/f/89vilt/oxfaleph019876747>

<sup>40</sup> See, e.g., E. J. S. Lage, "A new approach to the quenched bond-diluted Ising model," *J. Phys. C* **10**, 701 (1977). <https://doi.org/10.1088/0022-3719/10/5/014>

After coming back to Brazil, I invited him, saying: “Eduardo, why don’t you come to Brazil? I can ask CNPq to give you money.” It was easy, because of some good friend who could influence CNPq to give the support. I was at the top of the spin glass business, and he wanted to learn about Parisi and about all this stuff that I had done with David Thouless and Mike Kosterlitz. So he came to Brazil to learn it. He wouldn’t have come if I had done something else, but he was very interested in working on this spin glass. So I met him in Les Houches, we became good friends, so he came here, he stayed here six months. It was very nice.

**PC:** In the following years, did you follow what was happening in the development of replica symmetry breaking with the work of Mézard, Virasoro and others<sup>41</sup>?

**JdA:** [1:04:59] Sure. I was reading that all the time and seeing the progress. I really did follow it. I followed all the progress that people was doing. As I see it, it’s a big business that involved many people. It’s a social thing. You cannot do it alone. No one does it alone. So it’s a group and a social activity. I saw it flourishing, and I thank you all for all the progress that the field has done through the years with the collaboration of many people.

I’ve seen this meeting, 40 years of RSB<sup>42</sup>. You know how the community has grown—thanks to [Giorgio] Parisi and Sherrington<sup>43</sup>, and many others who have been leaders—the importance of the field.

You know that Phil Anderson said that this is one of the most important developments in modern theoretical physics, this evolution of the spin glass problem. The physical object, per se, is not directly [relevant], like in high- $T_c$  superconductivity or superfluid, but the spin offs that came from the study of these dilute magnetic systems is wonderful. All the fields. I get amazed.

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<sup>41</sup> See, e.g., M. Mézard, G. Parisi, N. Sourlas, G. Toulouse and M. Virasoro, “Nature of the spin-glass phase,” *Phys. Rev. Lett.* **52**, 1156 (1984). <https://doi.org/10.1103/PhysRevLett.52.1156>

<sup>42</sup> *40 years of Replica Symmetry Breaking : A conference about systems with many states* 10-13 September 2019, Sapienza University of Rome, Italy. <https://sites.google.com/view/rsb40> (Consulted August 31, 2021)

<sup>43</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>

[Since] you invited me, I saw this quantum gravity, black holes... I saw this paper about black holes and parallel universe and all this stuff<sup>44</sup>. Good gosh! Breaking of replica symmetry, other universe, string [theory]... How far this has gone.

**PC:** You could follow, but, as you've said, it was a very social thing, so from Recife it was difficult to be involved in that community, is that right?

**JdA:** [1:07:38] Yes. That's it.

**PC:** You went on sabbatical a few years later to UCLA, with Robijn Bruinsma<sup>45</sup>, to work on the random field Ising model (RFIM)<sup>46</sup>. Why? And how did that come about?

**JdA:** [1:08:11] Look, I failed three times going to Canada. But I think that was not it. When I decided to go to [Birmingham], after I had to come back to Brazil. Usually, when one finishes the PhD, one goes to a postdoc somewhere. But I had a contract, and that contract obliged me to come back to Brazil. If I didn't come... I wanted to be an honest man. Some people do that and say: "Ok. Go to hell. And give bananas to the country." They go outside and take a postdoc. But I had to come back and spend at least the time that I was away here, before going away again. So I came back. I could probably have gotten a good place to go to get a postdoc, but I didn't do that.

I came back here and after some time—it was six years—I decided: "Ok. Now I can go for a postdoc." I could have gone to a good place, like Rome, Paris or something like that. That would [have been] much more enlightening for me than going to... Nowadays, I think I should have gone to Paris or Rome. Why did I go to the United States? I think that maybe it was *USA, made in USA*, the United States, the big power, the great place. That influenced me. The fun...Disneyland...

The family... This I learned. I needed some money, because I had a big family. I had a problem, because one of my kids is handicapped and the support for the postdoc alone that I would get wouldn't be sufficient. Maybe I had miscalculated that I wouldn't get some support from elsewhere if I had chosen Rome or Paris. I wouldn't get support to go there. There was a Brazilian at the University of Los Angeles getting his PhD there, Fernando

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<sup>44</sup> See, e.g., E. de Giuli and A. Zee, "Glassy gravity," *Europhys. Lett.* **133**, 20008 (2021). <https://doi.org/10.1209/0295-5075/133/20008>

<sup>45</sup> Robijn Bruinsma: [https://en.wikipedia.org/wiki/Robijn\\_Bruinsma](https://en.wikipedia.org/wiki/Robijn_Bruinsma)

<sup>46</sup> See, e.g., J. R. L. de Almeida and R. Bruinsma, "Replica symmetry breaking in random-field systems," *Phys. Rev. B* **35**, 7267(R) (1987). <https://doi.org/10.1103/PhysRevB.35.7267>



Machado<sup>47</sup>. When I asked him: “Oh. Would you know if Ray would like me there and he would give me some support.” He talked to Ray, who was very enthusiastic about my going there.

**PC:** Ray Orbach<sup>48</sup>?

**JdA:** [1:11:44] Ray Orbach, yes. Very nice. I liked him very much. He gave me some support to go to Los Angeles.

It was very nice, because I had health care from the university. I got money from UCLA, and I was enrolled in their health care program. I had to take with myself a maid to take care [of the children], because my wife said: “I wouldn’t go if you don’t get someone to help me out with these kids.” Then, usually, in the United States, because of the support, basically anything that business [of looking after my family] will be fine. The United States is [good] for that. And it was very fine for my family. It was very nice. You know, Patrick, the difference that a good education makes. I spent one year at UCLA.

You didn’t ask, but in ’85 I went to San Francisco. There was an international conference on magnetism in San Francisco<sup>49</sup>. I went to that conference thanks to my masters thesis advisor<sup>50</sup>, because I applied for a support from the CNPq, and they refused the support for me to go to the conference. But this guy, who is very influential here, said:

- Ok. Let’s go. Let’s prepare the baggage. We’ll be outside.
- They refused my support.
- What? That can’t be true.

He picked up the phone and talked to someone in Brasilia. The next day I had the tickets and the support. This country here is incredible. In some sense, we are in a pre-French revolution situation. In other things, due to the Internet, no. But in many things, Brazil is still a pre-French revolution country.

Coming back to the earlier [story]. I went to San Francisco and then I knew some of the United States. Then I went to UCLA with my family. There, my daughter went to a very good public school, Castle Heights Elementary

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<sup>47</sup> See, e.g., Fernando Luis de Araujo Machado, *Low Temperature Heat Capacity of the Quasicrystalline Icosahedral Al<sub>0</sub>Mn<sub>20</sub> and Decagonal Al---Mn<sub>22</sub> Alloys*, PhD Thesis, University of California, Los Angeles (1987). [https://search.library.ucla.edu/permalink/01UCS\\_LAL/17p22dp/alma9922576203606533](https://search.library.ucla.edu/permalink/01UCS_LAL/17p22dp/alma9922576203606533)

<sup>48</sup> Raymond Orbach: [https://en.wikipedia.org/wiki/Raymond\\_L.\\_Orbach](https://en.wikipedia.org/wiki/Raymond_L._Orbach)

<sup>49</sup> 10<sup>th</sup> International Conference on Magnetism (ICM ’85), 26-30 August, 1985, Hilton Hotel, San Francisco, California, USA. See, e.g., James J. Rhyne, “Preface,” *J. Mag. Mag. Mater.*, **54–57**, vii–viii (1986). [https://doi.org/10.1016/0304-8853\(86\)90461-0](https://doi.org/10.1016/0304-8853(86)90461-0)

<sup>50</sup> Sérgio Machado Rezende: [https://pt.wikipedia.org/wiki/S%C3%A9rgio\\_Machado\\_Rezende](https://pt.wikipedia.org/wiki/S%C3%A9rgio_Machado_Rezende)

School. Soon, she was talking English because she had class in English. After one year in the United States, my daughter who was eight years old, was speaking English very nice. Her teachers said to me: “She’s very bright.”

I went to Saclay, because in ’85 Cirano de Dominicis, met me in San Francisco and put a note on the table there: “Jairo Almeida, I want to know you.” (Very nice guy, Cirano. We became friends. He came many times to Brazil.) He had put a note there. I attended his talk in San Francisco. “Jairo Almeida, I want to meet you. Meet me at that time and that place.” So I said to him:

- Ok. Wouldn’t you like me to take six months of my postdoc at your place.
- Oh! Fine! Come here.
- And get me supported too in France?
- That is fine. Come here!
- Surely, I will come.

It was very nice. I loved Saclay. Les Ulis<sup>51</sup>, [I lived there]. It was very fine from him. Then, after my one year at UCLA, I went to France.

You see clearly the effects of a good education. My eight-year-old daughter speaking English, we went to France. I tried to learn French, but I didn’t dedicate very much. But after talking to people all the time, seeing TV, after... you could speak a little bit. But my daughter went to a good school in Les Ulis, École Primaire Le Bosquet, well, in Saclay. In three months, the little girl was talking French. Before we left, we had to take her along with us, whenever we had to go someplace where we needed someone to talk French, we took her. This school was incredible. My girl was nine and she spoke three languages. That was incredible. That is one of the effects. I could barely speak Portuguese at that age. That was it. I think it was wonderful.

I don’t know if you were going to ask about Saclay, but in Saclay it was wonderful. You see, the difference of... There isn’t such thing as doing physics in the desert, in a lonely place. Because at Saclay, Cirano and I started working on spin glasses on a random tree, and we extended a work by Katsura<sup>52</sup>, where he used a kind of Sompolinsky and Viana-Bray model, where they took a solution for the internal field like delta function, and Katsura did put a continuous part<sup>53</sup>. We extended that... I don’t remember all the details.

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<sup>51</sup> Les Ulis: [https://en.wikipedia.org/wiki/Les\\_Ulis](https://en.wikipedia.org/wiki/Les_Ulis)

<sup>52</sup> Shigetoshi Katsura (1922-), Japanese physicist. See, e.g., <https://prabook.com/web/shigetoshi.katsura/458495> (Consulted September 17, 2021)

<sup>53</sup> J. R. L. de Almeida, C. De Dominicis and P. Mottishaw, "Dilute random spin systems with finite connectivity at low temperature: solution with continuous components and longitudinal stability," *J. Phys. A* **21** L693 (1988). <https://doi.org/10.1088/0305-4470/21/13/004>

The thing is, at some point we had to do triple Bessel integrals. It's a complicated thing. Just doing a double Bessel integral, you have to go to a handbook of integrals. Imagine the product of three Bessel functions. After that, Cirano looked at me and said: "Well. At Saclay you have 5000 PhD people working. I know someone who has done all kind of Bessel functions. I think he will help us out." He phoned the guy and the guy said: "Well, I have a monograph here with all kind of Bessel integral." That is it. This saved us I don't know how many months of calculations. Just a phone call, and being in the right place, you got the right answer at your door. If you go to the wrong place, you go nowhere. You are stuck all by yourself. It was very nice.

**PC:** Was that Peter Mottishaw, who knew these integrals, or was it someone else?

**JdA:** [1:21:25] No. We cited a work that he had done with Peter Mottishaw, but I don't know him personally, Peter Mottishaw. He had already left Saclay.

**PC:** Do you remember the name who knew these Bessel integrals well?

**JdA:** [1:21:46] No. He had all the kind of triple Bessel integral done in a monograph<sup>54</sup>.

**PC:** I see. After your postdoc/sabbatical in the US and in France, you came back to Brazil, and you mostly started working on short-range spin glasses, if I understand well. A few years later, you wrote in a review : "The present status of the studies of spin glasses reflects how hard the problem is, and the far reaching consequences of its eventual comprehension."<sup>55</sup> What were you trying to comprehend? What was the research program for you at that point.

**JdA:** [1:22:39] You are mentioning a review from where?

**PC:** The *Brazilian Journal of Physics*, from 1994.

**JdA:** [1:22:45] I think I was just following Anderson, because there is that series of papers by Anderson in *Physics Today*<sup>56</sup>. I kind of copy things. I was just

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<sup>54</sup> It was Henri Navelet. See, e.g., A. Gervois and H. Navelet, "Infinite integrals involving three spherical Bessel functions," *SIAM J. Math. Anal.* **20**, 1006-1018 (1989). <https://doi.org/10.1137/0520067>

<sup>55</sup> J. R. L. de Almeida and S. Coutinho, "Spin-glass: An unfinished story," *Brazilian J. Phys.* **24**, 4 (1994). See also arXiv:cond-mat/9411053.

<sup>56</sup> See, e.g., P. W. Anderson, "Spin Glass: A Scaling Law Rescued," *Physics Today* **41**, 9 (1988). <https://doi.org/10.1063/1.2811268>

kind of copying him about this. All the implications and the use in other fields of knowledge were coming about all the time, popping out.

**PC:** How influential was the work that you have done on spin glasses on the rest of your research program afterwards?

**JdA:** [1:23:38] Basically, by '78 I had almost finished my thesis in Birmingham. I never had a kind of mental tutor in life, so before Professor Thouless left Birmingham I asked him: "Hey Professor, what do you suggest for me to do?" And he just said: "Try something else!" But I didn't follow his advice, and I said to myself that I think...

You two through this project gave me a chance to revise a bit, to see back in my life what I had done. How it developed. I think I put too much effort to get to where I came, because nothing came easily. It was very hard. As I said to you, I did not get a regular primary school, except for one year, and I failed. And my mother who was a bright woman tried to make admission for a famous college, a school. I failed badly. I'll never forget that. Then, I think she admitted: "Ok. It's better he finishes secondary school in a bad school than nowhere." Then, I went to a third-class secondary school, and I almost failed there that she'd have to take serious actions, to find someone. Then, I went to Birmingham to work with someone, but I didn't know anything about that thing. I took that book of Ma's and said: "Ok. I have to learn this thing and finish this PhD no matter what."

After I finished my PhD, I took a breath for a while, but then in 1981 my life changed completely. After that, even here in Birmingham—starting in Les Houches—my wife started having serious health problems. She spent six months in the maternity [ward]. In Les Houches, she started bleeding and in (Birmingham) the doctor put her almost all the pregnancy on the maternity [ward]. I went there twice a day. Back in Recife, in 1981, my second son was born handicapped due to medical incompetence. That thing made all the difference in my life. From that on, I practically could not go anywhere. I did travel here in Brazil sometimes, but it came to a point where I couldn't go anywhere else, except when the travel involved one-day travel. I did many of these trips. I would go early in the morning and come back late in the night, on the same day, because she would not manage the house alone. I think that made me not to try...

I had just to survive. To get some money from CNPq, I had to put something. The easiest path is : "Ok. This guy didn't do this, didn't do that." There is always something that is left over for doing. If you try to do something very ambitious, you may end nowhere, as Mike Kosterlitz says it very well in his talk, when he went back (to England) from his time in Trieste.

He said: “Two or three times, I was on the verge of writing a paper and some group had done, so I stopped trying to do it myself alone here in Birmingham, or I risked my time.” I guess I stayed with spin glasses and this business. I put out one single author [paper] once in a while and that is it. I [also] had some experimentalists here, as you may see in my publication list<sup>57</sup>. There are some magnetism groups who do experimental [work] in [Recife]. We talked a lot.

**PC:** During your time at UCLA, at Recife or elsewhere, did you teach a class about spin glasses or replica symmetry breaking?

**JdA:** [1:30:16] No.

**PC:** Is there anything else about that era that you’d like to share with us that we may have skipped over?

**JdA:** [1:30:31] Let me see. It was a real miracle me going to Birmingham... I’ve said that to you.

I just would like to say that I had the happiness of working under two Nobel prize laureates who were my advisors. To be linked to just two is fine. You get happy about that. I’m more happy about that because I’m sure that I’ll be linked to three Nobel laureates. I’m sure—I was hoping it was last year, but maybe this year—Parisi is going to get a Nobel prize. Maybe this year. I’ll be very happy when that happens, because it’s always a commemoration of human intelligence to give the Nobel prize and because I somewhat know him.

It was kind of him to give me a copy of his book about the glass in infinite dimensions<sup>58</sup>. He gave me a copy, although Francesco was a bit suspicious about it. Parisi said: “No. It’s alright. He’s not going to make a copy.” I even refused to one of my best friends, Fernando Oliveira<sup>59</sup>, who asked me to give him a copy, and I said: “No. I promised Parisi not to give a copy of this book.” One thing I would like to say is that I’m very happy I will be linked to three Nobel prize laureates. And this time—as an aside for you—when David Thouless and Mike Kosterlitz were nominated Nobel laureates... You know that big feast with food and shows and the flowers from Sanremo in

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<sup>57</sup> See, e.g., A. D. Araujo, F. L. A. Machado, A. R. Rodrigues, A. Azevedo, F. M. de Aguiar, J. R. L. de Almeida, S. M. Rezende and W. F. Egelhoff Jr., “Spin-glass and random-field effects in exchange-biased NiFe film on a NiO single-crystal substrate,” *J. Appl. Phys.* **91**, 7754-7756 (2002). <https://doi.org/10.1063/1.1447496>

<sup>58</sup> G. Parisi, P. Urbani and F. Zamponi, *Theory of Simple Glasses: Exact Solutions in Infinite Dimensions* (Cambridge: Cambridge University Press, 2020).

<sup>59</sup> Fernando Oliveira: <http://loop.frontiersin.org/people/350909/overview> (Consulted October 5, 2021)

Italy<sup>60</sup>. The Nobel committee gives away some invitation, but you have to ask for that. I applied for that invitation. I said: "You know, I was a student for two Nobel laureates. I guess I should be there." But they didn't give me the invitation. I'm sure I'll reapply and ask for Parisi support and he'll give me that support. It's a big feast and it's free.

**PC:** You've mentioned that you've kept some papers, some mementos, to which you're clearly attached from that era. I strongly encourage you to consider having them deposited in an academic archive at some point. At least to make plan for it.

**JdA:** [1:35:07] I have letters from... I kept a dozen letters from Bray and Moore. It's nice that you gave me the opportunity to read again that letters. I could have gone to... Michael Moore invited us, Professor Thouless and me, to go to Manchester. They had money for us to go there and to work on solving the TAP equations and maybe that could result in a paper. Alan Bray pointed out some points and gave me some details of his calculations.

I have a very beautiful letter from Professor Thouless on his 65<sup>th</sup> anniversary. I was invited, but I missed it. I could not attend. He invited me three times, but due to my family problems I didn't go any of the times.

I almost went to Canada, to Toronto, there's an international center for the sciences there. Maybe you know that. I applied for one year there. I almost went there. They gave me the support, but I had some problem and I couldn't.

It was nice to talk to you, to think back in my life, to see what I have done.

I wish you a good success to do this. (And see how fundamental education is. In my second time in the United States<sup>61</sup>, my daughter went to a secondary school, Westbury Christian School. I even was called by the mathematical and physics teachers. They said: "Your daughter is very bright. You should take care of that and that.") You see, it's amazing. Talking to you sincerely, as a last thing. I watched all the interviews that you have done. I don't envy anything, because everyone has his own life and cannot complain about it. Life is what it is, and let it be that way, and try your best. But I think that most people who went to the best schools in the world and

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<sup>60</sup> See, e.g., L. Gozzo, "Alfred Nobel's final years in Sanremo," NobelPrize.org. <https://www.nobelprize.org/alfred-nobel/alfred-nobels-final-years-in-sanremo/> (Consulted September 17, 2021)

<sup>61</sup> **JdA:** In 1994, I took a sabbatical leave at the University of Houston/Texas Center for Superconductivity, under the auspices of Prof. George Reiter. See, e.g., "In Memoriam: George Reiter," University of Houston. <https://uh.edu/nsm/news-events/stories/2019/0408-george-reiter-km.php> (Consulted September 17, 2021)

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having the best teachers in the world, and tutors, and all the conditions to bloom, [to do the] things that you wish to do, that makes a great difference. Education is fundamental. Otherwise, I'm a very lucky guy the way things turned out to be.

**PC:** Thank you very much. We were lucky to talk to you.

**JdA:** [1:39:00] I wish you all the best in this project.