History of RSB Interview: Michael Moore

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Location:
Over Zoom, from Prof. Moore's home in Manchester, England, United Kingdom.

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PC: Thank you, Prof. Moore, for sitting with us today. As we've mentioned ahead of time, the purpose of this interview is mostly to discuss the period during which replica symmetry breaking was formulated, which we roughly bound from 1975 to 1995. But to be able to talk about this, we first want to ask a few questions on background, if you allow. In particular, how did you get to be interested in physics, and then to pursue a PhD in theoretical physics?

MM: That's such a long time ago in my case. I was an undergraduate student in Oxford, and I really liked being there. To do a PhD was an easy way of staying there. They offered in those days very generous scholarships, so there was no issue with money. I was useless at laboratory work, very clumsy, so I ended up doing theoretical physics. I started out being a many-body physicist, as it was called in the '60s. I was interested in topics such as superfluid helium-4 and things of that nature[1]. Only gradually did I move into stat mech and problems like critical phenomena.

I became a postdoc in Urbana in 1967, at the time when John Bardeen was there[2]. He had two Nobel prizes, and somehow when you get two Nobel prizes it seems to unlock the funders' purse strings. I was just one of 35 postdocs on his contracts. When I arrived there, I went to see the great man to ask him what he wanted me to do. He was incredibly shy for someone who, at the time, was also president of the APS. He was so shy, he found it hard to tell me what I should be doing. So he said: "Come back tomorrow. I think I may have thought of something by then." I went to see

[1] See, *e.g.*, M. A. Moore and R. B. Stinchcombe, "The superfluid density and the Patashinskiĭ-Pokrovskiĭ theory of liquid He near Tλ," *Phys. Lett. A* **24**, 619-620 (1967). https://doi.org/10.1016/0375-9601(67)90650-0

[2] John Bardeen: https://en.wikipedia.org/wiki/John_Bardeen

him the next day, and he said why don't you work on showing that you can't have superconductivity at a temperature of more than 35 Kelvin. What he had in mind was fiddling with a formula called the McMillan formula[3], so that I could get an upper bound on the transition temperature for superconductors. Well, that project really didn't get very far, and I gradually drifted into critical phenomena, working mostly with Michael Wortis and his graduate student David Jasnow.

Then having finished in Urbana, I returned to Oxford for two years where I worked mostly on ferroelectricity before moving to the University of Sussex as a lecturer. Tony Leggett[4] was there. Shortly after I arrived, superfluid helium-3 was discovered, and I sort of drifted into studying that. No one knew what the pairing state was in the superfluid. Everyone hoped it was a *p*-wave paired superfluid, and there was some evidence that it could be that. There was a critical test of whether it was really *p*-wave pairing, and that involved measuring the nuclear magnetic resonance frequency shifts at the so-called polycritical point, at which point all the nasty many-body corrections would drop out from their ratio in the two superfluid phases and you get an unambiguous test of the nature of the pairing states. An experiment was done, and unfortunately what is now the accepted answer did not seem to be consistent with the experimental data. People like David Mermin[5], myself, and others looked at alternatives to *p*-wave pairing. I looked at the *f*-wave paired superfluid state[6]. It was horrible. The Landau-Ginzburg theory of that has a 42 component order parameter and there are 14 Landau-Ginzburg quartic terms, all with unknown coefficients. One was trying to minimize the free energy and then work out the NMR frequency shifts to see which fitted the data better than *p*-wave pairing. Having worked on this problem for two years, and not found anything which really worked, it was discovered that the so-called crucial experiment had been done incorrectly. The corrected results worked very well with what people thought the superfluid helium-3 pairing state was all along. As David Mermin wrote to me—people wrote in those days, there were no email —"Well, we won't get our rewards on Earth, but perhaps in Heaven we'll get them."

So that's what I was doing. In 1976, Mike Kosterlitz[7] came to Sussex. I remember chatting to him after his seminar. He wrote two Hamiltonians on

[3] W. L. McMillan, "Transition temperature of strong-coupled superconductors," *Phys. Rev.* **167**, 331 (1968). https://doi.org/10.1103/PhysRev.167.331
[4] Anthony Leggett: https://en.wikipedia.org/wiki/Anthony_James_Leggett
[5] David Mermin: https://en.wikipedia.org/wiki/N._David_Mermin
[6] G. Barton and M. A. Moore, "The likelihood of f-wave pairing in superfluid ^{3}He," *J. Phys. C* **8**, 970 (1975). https://doi.org/10.1088/0022-3719/8/7/014
[7] Michael Kosterlitz : https://en.wikipedia.org/wiki/J._Michael_Kosterlitz

the board: one was the Edwards-Anderson Hamiltonian[8], the other was the Mattis Hamiltonian[9]. He was asking me why would the Edwards-Anderson model be any better than the Mattis model? Or whether it would even be different? I had never seen either of these Hamiltonians before. We just had a desultory conversation, but that was the first time I ever heard about spin glasses.

FZ: Why were the Hamiltonians on the board?

MM: [0:07:03] He was trying to interest me in this problem, because back in Birmingham he and Thouless had been worrying about spin glasses.

FZ: So he came to you, I understand. He wrote the Hamiltonians on the board.

MM: [0:07:13] Yes. You know how you chat after seminars. He was telling me about this problem by way of conversation. Now, if we had worked at it harder we might have discovered things like frustration and so on. As I was hearing about spin glasses for the first time, I wasn't really highly motivated to spend a lot of time on it.

About that time, Tony Leggett was offered the job of Professor of Theoretical Physics in Manchester, which had become vacant because Sam Edwards had moved to Cambridge. I remember when he was offered the job, some of us took Tony to the pub one evening and gave him thousands of good reasons why he shouldn't leave and go to Manchester. He eventually declined the offer of a job there. The job was readvertised, and I applied for it. I was duly offered it. I noticed my colleagues in Sussex didn't really try that hard to get me to stay there! I took the job to Manchester, but before I went there, while I was still at Sussex, I had a message from Sam Edwards saying that he would like to meet me. He was curious to know who had been appointed in his place in Manchester. I had never met Sam until that moment, and so I was quite keen to meet him. He invited me to his London club, the Athenaeum—one of these London dining clubs[10]—for lunch.

I went up on the train from Sussex to London, but it just so happened the French president Giscard [d'Estaing] chose that day to make a state visit to London[11]. That involved him flying into Gatwick Airport, and out of security

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[8] S. F. Edwards and P. W. Anderson, "Theory of spin glasses," *J. Phys. F* **5,** 965-74 (1975). https://doi.org/10.1088/0305-4608/5/5/017

[9] Daniel C. Mattis, "Solvable spin systems with random interactions," *Phys. Lett. A* **56**, 421-422 (1976).

[10] Athenaeum Club, London: https://en.wikipedia.org/wiki/Athenaeum_Club,_London

[11] Valéry Giscard d'Estaing visited the UK in late June 1976, then the first state visit by a French president in 16 years.

concerns all the trains which went through Gatwick Airport were stopped. So I was just stuck for an hour and a half on a non-moving train, and I missed my lunch appointment with Sam as a consequence. (In those days without mobile phones I couldn't communicate to Sam that I was not going to be there.) Sam rearranged the lunch meeting, and before then it struck me: "What could we talk about?" So I looked at his paper with Anderson on spin glasses. That was really the first time that I had looked at a paper on spin glasses. It turned out I needn't have bothered because we just gossiped at lunch. Spin glasses were never mentioned on that occasion. Over the years, I kept meeting him—back in the '80s and the '90s—and he would shake his head about spin glasses. He'd say: "It's all got too complicated for people like myself." It's a card I sometimes play myself these days, as I've got older and older.

After that, when I moved in Manchester, Alan Bray—I was the professor and he was appointed as a lecturer—we started to work together. We didn't work on spin glasses immediately. We worked on critical phenomena at surfaces[12]. I was also working on polymers and things like that[13].

PC: Before we go there, if you allow us, can you describe how you chose problems before you got to spin glasses?

MM: [0:11:48] Well, when I was at Sussex, there were myriad problems in connection with superfluid helium-3, because it had only been discovered about 1972, and there was many things you could do. That was an easy way to write papers. Any subject which is new provide lots and lots of opportunities, whereas these days, in spin glasses, there are no easy papers left to write. All those have been written, alas!

PC: In 1976, you moved to Manchester…

MM: [0:12:35] They wanted me, I think, to work on polymers, because Sam Edwards had worked on polymers and I was in a sense his replacement. As a consequence of that, I used the *n*-goes-to-zero trick, which came to play a big role in spin glasses. That was the first time I had come across that sort of idea, though it was used for quite different purposes than for the disorder average in spin glasses. I was at Manchester for about two years before we moved on to working on spin glasses. It was because in that period I

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[12] *E.g.*, A. J. Bray and M. A. Moore, "Surface critical exponents in terms of bulk exponents," *Phys. Rev. Lett.* **38** 1046 (1977). https://doi.org/10.1103/PhysRevLett.38.1046

[13] *E.g.*, D. Jasnow and M. A. Moore, "Dynamical scaling exponent z for polymer chains in a good solvent," *J. Phys. Lett.* **38**, 467-471 (1977). https://doi.org/10.1051/jphyslet:019770038023046700

kept running across David Thouless, and he was very interested in a problem which had come up in connection with the Sherrington-Kirkpatrick model, namely that the entropy went negative at low temperatures. Thouless' original hypothesis was that Sherrington had simply cocked up the calculation completely, but then he did the calculation himself and he discovered it was perfectly ok, except for the assumption, possibly, of replica symmetry. I think it was in his paper with de Almeida that he suggested that what one should do was break the replica symmetry[14]. It seemed very difficult to understand. How can you have replicas, which are not the same? This was a very hard concept to grasp. But Thouless didn't seem then to be interested in actually doing a replica symmetry breaking calculation himself.

I remember him explaining why: "There must be an infinite number of ways to break replica symmetry. What are the chances that one hits on the right scheme?" With an *n* x *n* matrix, you can parameterize it in endless ways, then you start fiddling with it. What criteria would you use to choose a solution? In those days, we were not even sure whether one should minimize or maximize the free energy. Later on, there emerged another criterion, namely that the saddle point solution should be a local minimum. But there could have been thousands and thousands of ways in which that could have been achieved. I think that Thouless felt it was just too much of a longshot to attempt to explicitly break replica symmetry.

PC: In what context were you meeting Thouless? In meetings, or …?

MM: [0:15:43] The theoretical physics community in the UK is small. Then you have PhD students, who need external examiners, and so on. I think Thouless had come up to Manchester and given us a seminar, and I'd been down to Birmingham and given a seminar. In those days, Skyrme[15], of skyrmion fame, was the organizer of the Birmingham seminars. When I got to Birmingham, I was a little early and he was busy marking exam papers. He had a huge stack of them on his desk. He said: "These are very interesting exam papers. As you've got some time, would you like to mark some?" I passed on this opportunity. So I was down in Birmingham on several occasions and I certainly was de Almeida's PhD external examiner[16]. At this

[14] "The nature of the instability may be significant, in that it breaks the symmetry between the replicas, but it is not obvious how to handle such a broken symmetry in a zero- dimensional space." Jairo R. L. de Almeida and David 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

[15] Tony Skyrme : https://en.wikipedia.org/wiki/Tony_Skyrme

[16] Jairo Rolim Lopes de Almeida, "On the mean field theory of spin glasses," PhD Thesis, Birmingham University (1980). https://birmingham-primo.hosted.exlibrisgroup.com/permalink/f/1q64cdc/44BIR_ALMA_DS2181906250004871

stage, they had done this complicated calculation, where they discovered the de Almeida-Thouless instability line, upon which many of us have worked since. He was interested in trying to go below the line, possibly by breaking replica symmetry.

Alan Bray and I just talked about this problem, but did nothing about spin glasses. There were issues with working on spin glasses, because they had no interesting experimental properties whatsoever. They had a kink in their susceptibility as a function of temperature, but that was it. Whereas when you have a superconductor or a superfluid you get all these interesting experimental features. But in spin glasses, there is this kink in the susceptibility as a function of temperature, but that was it, nothing else. No one has ever discovered any useful property of a spin glass. In fact, this has been one of the problems of spin glass research over the years. It's only been the spin-offs from theoretical work which have been important. There have been no useful spin-offs from experimental work on spin glasses. I recollect that one NSF program director had realized how useless spin glasses were, with no technological significance, and he determined that on his watch there will be no work funded on spin glasses. That is why all the work which was done in the United States on spin glasses was done in industrial labs like Bell Labs (Daniel Fisher and David Huse[17]), and at IBM (Scott Kirkpatrick). Universities couldn't really get in much on the action, because you couldn't get an NSF grant to work on spin glasses. It became a long-standing problem.

FZ: This was in the '70s?

MM: [0:19:54] This was probably in the '80s, when spin glass research was becoming a big activity, at least in Europe. People in the States had trouble joining in, because they couldn't get funding for it in that period. (There might have been somebody somewhere who had funding from NSF, but the people I knew about were in labs which didn't require NSF funding.)

PC: Before jumping to the '80s, we'd like to go back earlier some; 1977 was quite a seminal year for you: you started collaborating with Alan Bray, you did Monte Carlo simulations for the first time, and you first worked on spin glasses[18].

[17] In the mid-80s, Fisher was at Princeton, while Huse was at Bell Laboratories. See, *e.g.*, Daniel S. Fisher: https://de.wikipedia.org/wiki/Daniel_S._Fisher; David A. Huse: http://www.nasonline.org/member-directory/members/20041821.html (Last consulted Jan 8, 2021).

[18] A. J. Bray and M. A. Moore, "Monte Carlo evidence for the absence of a phase transition in the two-dimensional Ising spin glass," J. *Phys. F* **7**, L333 (1977). https://doi.org/10.1088/0305-4608/7/12/004 ; A. J. Bray, M. A. Moore and P. Reed, "Vanishing of the Edwards-Anderson order parameter in two-and three-dimensional Ising spin glasses," *J. Phys.* C **11**, 1187 (1978). https://doi.org/10.1088/0022-3719/11/6/024

MM: [0:20:40] Our first work was on Monte Carlo simulations. In those days, one just looked up how to do it and did it. Now it's become more professionalized, with lots of clever tricks, but we just bashed on and did it. The big issue which came along was that Binder and Schröder… Binder, even then, was the great man of numerical simulation. He had done simulations of spin glasses, and was pretty sure that there wasn't a phase transition in two dimensions[19]. He wasn't really confident that there was a transition in three dimensions, it was unclear. In those days, one didn't really know how to judge whether one had equilibrated the system. It was only in the '80s, that Peter Young came along with the idea of convergence from above and convergence from below by using two different methods. After that, you could tell whether you had adequately equilibrated your system. In the early days, what you used to do is simply run for twice as long, and if nothing much changed, you said: "Ah, well! It's probably equilibrated." We hadn't realized in those days that you had to run for not only twice as long, but decades longer to be really sure that the system had equilibrated. Of course, we were doing all this work in the years of punch cards. Missing a right parenthesis would set you back a whole day, and so on.

PC: Was that your first experience using computers at that point?

MM: [0:22:44] No. When I was at the University of Illinois, I used to do what were called high-temperature series expansions, which were the only way in the '60s to get a critical exponent. You got this series which you could extrapolate, using things like Padé approximants[20], to get estimates of critical exponents. You had to grind out the series for, say, the susceptibility of the Ising ferromagnet or the Heisenberg ferromagnet. Anyways, it was very, very boring. I did both Heisenberg and the Ising ferromagnet susceptibilities to about the 12th order in the Ising case and also the spin correlation functions. You have to sum something like half a million diagrams to do it. I was using a mixture of hand calculators and an IBM 365[21], which was a fancy computer in those days. My program ran to three boxes of cards, and I think each box held about 1000 cards. It was horrible. So I had done a lot of computing before the Monte Carlo simulation. But it was another style of computing. It wasn't simulation. It was just using the computer to evaluate lots and lots of diagrams.

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[19] K. Binder and K. Schröder, "Phase transitions of a nearest-neighbor Ising-model spin glass," *Phys. Rev. B* **14**. 2142 (1976). https://doi.org/10.1103/PhysRevB.14.2142

[20] Padé approximant: https://en.wikipedia.org/wiki/Pad%C3%A9_approximant

[21] IBM System/360: https://en.wikipedia.org/wiki/IBM_System/360

In fact, while I was in Urbana, this young guy—actually, he was older than me, but he seemed young—wrote to me asking could he come to Urbana and talk to me about series as he was also generating series. This young guy duly turned up, and it was Ken Wilson[22]. I was convinced he was a complete loser, because he'd only gotten eight terms in the series and I was up to 12, and every order required twice as much work as all the previous ones put together. My scorn must have percolated through to Ken Wilson, and perhaps he decided that there must be a better way to understand critical phenomena than grinding out these series. Maybe from that came the renormalization group!

But we had guessed from series expansion work that there were universality classes, and that Heisenberg was not the same as Ising. We had even deduced that there was probably a phase transition in the two-dimensional XY model, but not in the two-dimensional Heisenberg model, which is still the belief. So they were not without their uses, but the effort involved in doing that work was awful. It is up there with doing a replica symmetry breaking calculation in terms of nastiness!

PC: So you started doing Monte Carlo simulations. In a book review that you wrote about Monte Carlo simulations 10 years later[23], you hinted that such books are quite helpful because they avoid you making mistakes or, at least, learning about the difficulties. What challenges did you meet when running those first simulations?

MM: [0:25:43] I think the challenge which we had was that Binder seemed to always do it better. We realized that we couldn't really compete with Binder, so we decided that we would do other things than that kind of numerical work. In those days the guru of statistical physics was Michael Fisher[24], and he used to say you should only do numerical work when you knew the answer. In the late ‘70s, we didn't really know whether there was a spin glass phase transition in three dimensions. Binder’s simulation work was ambiguous on that point. There was experimental work, which seemed to indicate that there was a phase transition, but it was not as sharp as one was used to in those situations. So it was very unclear whether one should even be expecting there to be a spin glass phase transition.

[22] Kenneth G. Wilson: https://en.wikipedia.org/wiki/Kenneth_G._Wilson
[23] M. A. Moore, “Monte Carlo Methods Vol 1: Basics,” *Phys. Bull.* 38 149 (1987). https://doi.org/10.1088/0031-9112/38/4/034
[24] Michael Fisher: https://en.wikipedia.org/wiki/Michael_Fisher

In fact, when Alan Bray and I did our first paper on replicas in spin glasses, what we did was we started out with the Q^3 field theory[27]. At mean-field level it is replica symmetric. Replica symmetry breaking is required when you go beyond the mean-field level in order to remove the instabilities which then come up. We had this replica symmetry breaking scheme, the two group scheme, which led to the prediction that the lower critical dimension for spin glasses will be four, because there's an integral which diverged in four dimensions. Of course, we now know that if you actually do the same kind of calculation at Gaussian order, using the propagators which Kondor and de Dominicis got about the RSB solution, you would also come up with a $1/k^4$ divergence, and you'd predict naively that four is again the lower critical dimension[25]. Of course, there could be further corrections which change this.

So when Binder wasn't sure there was a phase transition and four was a potential lower critical dimension, we were left in the situation where we weren't really sure whether for the spin glass problem, the phase transition really was there or not. It was very confusing. And what should be ones' attitude to replica symmetry breaking calculations? When we were doing our work in '78, Alan and I had no idea of what we were doing—I don't know whether anybody had much idea—because we had no feeling for what different numbers of replica indices associated with correlations actually meant back in spin space. We now know that if you repeat the replica index it's like having two thermal averages, averaged over the disorder. All that stuff, we just didn't know about. It was just a game to us, involving various propagators. The fact that these things could have a physical significance just passed us by.

We were influenced, I think, by Thouless' idea that perhaps it's all going to be too hard to get the right symmetry breaking scheme. We were just giving it a go to see what would turn up. We never thought it would be of any lasting significance; it seemed likely to be just a long shot. We bashed away at this two-group method of breaking replica symmetry, which divided the replicas into two groups: m in one category, $n - m$ in the other. It seemed to be the simplest scheme one could think of, but much to our astonishment we could get a stable solution as judged by no negative eigenvalues. It wasn't until a few months later that we repeated the calculation using that replica symmetry breaking for the SK model. We still didn't notice that the drawback of our scheme was that Z^n—that's the partition function—was not 1 when you took n to zero. It was Giorgio who noticed this drawback; his scheme didn't suffer from this defect. Later on, it turned out that

[25] C. De Dominicis and Imre Kondor, "Eigenvalues of the stability matrix for Parisi solution of the long-range spin-glass," *Phys. Rev. B* **27**, 606 (1983). https://doi.org/10.1103/PhysRevB.27.606

that the value of Z^n was equal to the complexity associated with TAP solutions.

PC: Before we move on to your complexity calculation. In that 1978 paper you mentioned, there's a lot of references that are actually not real references—they are to be published—if you look at it. How was the information circulating between the community at that point? How were you aware of all those unpublished works?

MM: [0:33:15] There were preprints which were circulated by mail from one group to another. I remember those from Saclay always came with a bright orange cover. Different institutions had different colors for the frontispiece of their preprints. You could find out what was about because everyone sent their papers to Michael Fisher, who once a month sent out a listing of all the papers he had received. If you saw some promising titles, you could write to the authors and ask them to send you a copy of their work. That's how things were done. The invention of the arXiv was a fantastic step forward[26]. One of the chores in Urbana was to sort out all the preprints received, put them into filing cabinets, and then produce a list of what was there so that one could find them. These filing cabinets quickly filled entire offices. The arXiv was just fantastic. It's probably the most important breakthrough in scientific communication during my lifetime.

PC: So you were reading preprints, and you were amused by the idea of breaking replica symmetry, is that how you'd describe it?

MM: [0:34:55] Well, it seemed like a contradiction in terms: replicas, but they're not the same! It was amusing, as you say, that this could be a way to go. We never took it seriously. For example, I remember we discovered these massless modes in our calculation; massless modes, like magnons, phonons, spinons. So what to call these modes? We toyed with various names, like *moorons*, but we thought that would have the wrong overtones. So we called them replicons[27]. Perhaps we should have chosen better, but that name seemed to stick in the community.

PC: Were you aware that other groups were playing the same game, at the same time? That there was some race to the replica…?

[26] The arXiv appeared in 1991: https://en.wikipedia.org/wiki/ArXiv

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

MM: [0:35:55] There was some work by Blandin[28]. I think we'd heard about his work. He had quite a different scheme, which I think was closer to the one-step replica symmetry breaking scheme. I found his paper—I did eventually see it—a bit hard to understand. People were also writing papers where they did replica symmetry calculations, and then finding various properties of that calculation. Pytte and Rudnick had a very nice paper, where they developed the field theory of the Edwards-Anderson Hamiltonian[29]. They put in all those fourth-order terms which come up. That was a useful piece of work, which influenced us a lot. The Edwards-Anderson paper itself was not much help, because the definition of an order parameter there was dynamical and we were all doing equilibrium calculations. It was a very ingenious idea, and later on, of course, one understood how to get at it, but at the time the definition of the Edwards-Anderson order parameter as the projection of $s_i(t)$ on $s_i(0)$ after a long time, just seemed an unnecessarily complicated way to proceed. We wanted a static definition, and, of course, it became routine how to have that. But at the time, the Edwards-Anderson paper was not something which I spent time studying.

PC: One last question about your first RSB paper. In the text you say that: "this is a first step toward the sensible mean-field theory of spin glasses." But in our conversation you suggested that this was not necessarily a first step for you. Did you have a vision of how to go beyond?

MM: [0:38:50] The first paper which appeared was a Phys. Rev. Letter[30], which was on the SK model, but the actual first paper which we wrote appeared later than the Phys. Rev. Letter, but it was actually written beforehand. That was done to one-loop order for the cubic field theory. It was quite simple, because without those horrible quartic terms the mean-field theory is replica symmetric. The propagators at mean-field level had no instabilities in them. They only developed when you went to one-loop order. Within the context of the cubic field theory, the calculations are quite simple. De Almeida and Thouless kept all the terms (see de Almeida's thesis), where he has something like 15 distinct eigenvalues for the full theory, whereas if you keep to just a cubic field theory there are only three eigenvalues, two of which are degenerate and the third is zero. That enabled us to go to one-loop order without too much difficulty. In fact, it was easier to do that than to do the mean-field calculation involving the quartic

[28] André Blandin, "Theories versus experiments in the spin glass systems," *J. Phys. Coll.* **39**, C6-1499 (1978). https://doi.org/10.1051/jphyscol:19786593

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

[30] 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

terms, and involved less algebra. The cubic field theory is a good place to start.

The curse of replica symmetry breaking became apparent to us in the early '80s, when we had Imre Kondor as a postdoc—though because he was so old at the time we had to pay him at a much higher rate! He had already been working on the calculation of the Gaussian fluctuations about the Parisi mean-field solution, and his work was quite amazing to me. He had these huge sheets of computer printer paper, which came out in big wide rolls. What he would do is write on these enormous pages, and cover them with long, long formulae. He told me that one equation alone had taken something like 40 pages of this computer paper to write down in its entirety. It really impressed upon us that the replica symmetry breaking was just getting too complicated. You couldn't even do Gaussian order without this pain. This work wasn't published in its entirety until about 1998, and it was started around 1980[31]. The curse of replica symmetry breaking is its algebraic complexity. The calculations are just horrible, even if straightforward in principle. Since then, or course, people have found ways of simplifying these calculations, but they still remain horrible to this day. Rarely does anyone go beyond the mean-field approximation. There has been little progress in pursuing the loop expansion into physical dimensions.

PC: To get back to one of the first calculations, that of Giorgio Parisi, which followed closely in time, at least, your work. How did you become aware of it? And how did you react to it when you saw it?

MM: [0:43:38] By then, we had already written our paper on replica symmetry breaking. And Giorgio, I think, sent his papers to J. Phys. A and J. Phys. C. Because we had written one paper on replica symmetry breaking, we were obviously the experts on it! So all these papers were sent to me to referee. In fact, they were coming at me thick and fast, because Giorgio was sending them to different journals. I remember having on my desk, two of them at the same time. In the second one, he had already gone beyond the one-step replica symmetry breaking of the first paper, and he was considering taking the limit to infinitely many steps of replica symmetry breaking. I had this dilemma. Should I accept the first one, knowing that there was a much better one in the pipeline? In the end, I thought: "Oh, God! Let's just accept the lot." But I wasn't that interested really, because I was still under the influence of Thouless' idea that no one would ever guess the right scheme. So I just put down what he was doing as another long shot which could be interesting but was unlikely to be the correct answer, because there

[31] C. de Dominicis, I. Kondor and T. Temesvari, "Beyond the Sherrington-Kirkpatrick Model," In: *Spin Glasses and Random Fields,* A. P. Young ed., (Singapore: World Scientific, 1998). arXiv.cond-mat/9705215.

seemed to be so many ways one could proceed. Why would that be the right one, when it was really just a guess as to what one did? I still think it is a miracle that the second scheme on the market was the correct answer. For example, we ourselves tried other replica symmetry breaking schemes which never got published. We published the two-group model of replica symmetry breaking, but clearly there's obviously a generalization to a three-group model, where you divide your replicas into three groups. All that happens is that the algebra goes up in complexity, and we never published it. Though when I moved out of my office to another office, I discovered the old calculation, which filled a whole box file. The journals were spared because we couldn't bring ourselves to write up this algebra. I don't remember that there was anything very interesting in it either. So there were many, many more schemes than have perhaps seen the light of day, but it looks as though Giorgio's scheme is the right one, so there's no need to worry about other possible schemes.

PC: It was not obvious at the time. So how long did it take you to realize, or to accept it as the valid solution to the SK model?

MM: [0:47:13] I think that by about 1981, when we were working on the complexity of the TAP solution, we realized that the low-lying TAP states, which you had to treat in the complexity calculations by breaking replica symmetry, and if you broke replica symmetry using Giorgio's method, you got something which made a whole load of sense. I remember we wrote some comments to the fact that these calculations, which seemed to be making sense, had convinced us of the correctness of Giorgio's way of doing things. It seems extraordinary that it could give these sensible answers, were it not correct. If you used other approximations, like what de Dominicis was calling the "innocent replica approximation"[32], that gave stupid answers for the low-lying states, whereas Giorgio's method gave very sensible answers. So, by that time, I think we were convinced that it was the right way to go. Giorgio's scheme was developed quite quickly and was accepted very quickly as well. I don't think anyone was saying it wasn't the right solution, at least for the SK model, at any time. No one could think of anything negative in connection with it, because it seemed to be stable. There was a nice paper written by Thouless, Kosterlitz and de Almeida, where they had a limited calculation of the stability within certain sectors—the hard sector — and showed it was stable[33]. A very nice but much neglected calculation. I say it's nice because it's easy to understand it, whereas the

[32] C. de Dominicis *et al*., "White and weighted averages over solutions of Thouless Anderson Palmer equations for the Sherrington Kirkpatrick spin glass*," J. Phys*. **41**, 923-930 (1980). https://doi.org/10.1051/jphys:01980004109092300

[33] 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

later work of de Dominicis and Kondor was so complicated that it was just about impossible to follow it. Works like that convinced us that probably everything about the Parisi solution was sensible. We never doubted that it was the correct replica symmetry breaking scheme.

PC: You've mentioned a couple of times that you were working on a complexity calculation for the SK model, which, if I understand well, was the first complexity calculation done. You don't even use the word complexity at that time.

MM: [0:50:46] The complexity is just the log of the exponentially large number of TAP solutions. (I don't remember when the word complexity first began to be used.) There was an earlier paper by Edwards and Tanaka[34]—I don't know which Tanaka that was—where they had done something similar, but they had just done it at zero temperature[35]. They had looked at the number of states where the spins were parallel to their local fields. We generalized that to the TAP equations themselves, which provide more scope because Peter Young had showed that the low-lying free energy solutions of the TAP equations correspond to the pure states of Parisi[36]. So there was this connection between Parisi's work and that of TAP, which is nice, because it makes it worthwhile to study the TAP equations. In our paper on solving the TAP questions numerically in about 1978[37], where we had looked at the Hessian matrix associated with the TAP equations, we discovered that the solutions which turned up all seemed to have massless modes, which pleased us a lot, because we were looking into replicon modes in the two group model, and we thought that we had seen the replicon modes in the TAP equations, which indeed is what we were seeing. Later on, by 1981, we realized that there were lots of other TAP solutions, which didn't have a band which included massless modes[38]. In 2019, Timo Aspelmeier and I wrote a paper where we pointed out that if you solve the TAP equations by iteration—and that's the only way you can really solve

[34] Fumihiko Tanaka co-wrote these papers with Sam Edwards, while visiting the Cavendish, on leave from the University of Tokyo.

[35] F. Tanaka and S. F. Edwards, "The ground state of a spin glass," *J. Phys. F* **10**, 2471 (1980). https://doi.org/10.1088/0305-4608/10/11/019; "Analytic theory of the ground state properties of a spin glass. I. Ising spin glass," *J. Phys. F* **10**, 2769 (1980). https://doi.org/10.1088/0305-4608/10/12/017; "Analytic theory of the ground state properties of a spin glass. II. XY spin glass," *J. Phys. F* **10**, 2779 (1980). https://doi.org/10.1088/0305-4608/10/12/018; A. J. Bray and M. A. Moore, "Metastable states in spin glasses," *J. Phys. C* ***13***, L469 (1981). https://doi.org/10.1088/0022-3719/13/19/002

[36] Allan Peter Young, "The TAP equations revisited: a qualitative picture of the SK spin glass model," *J. Phys. C* 14, L1085 (1981). https://doi.org/10.1088/0022-3719/14/34/004

[37] A. J. Bray and M. A. Moore, "Evidence for massless modes in the 'solvable model' of a spin glass," *J. Phys. C* 12 L441 (1979). https://doi.org/10.1088/0022-3719/12/11/008

[38] A. J. Bray and M. A. Moore, "Metastable states in the solvable spin glass model," *J. Phys. A* **14**, L377 (1981). https://doi.org/10.1088/0305-4470/14/9/012

them—you always end up at the borderline between replica symmetric and broken replica symmetric states[39]. That is really the origin of these massless solutions, which you find in the explicit solutions of the TAP equations. That's what I'm looking at in this lockdown. I'm doing a long replica symmetry breaking calculation related to the complexity of the TAP equations. I had forgotten how horrible those things were. It's coming all back, but now I've only got three brain cells left, whereas in my younger days I could proceed more reliably.

PC: We will get back later to talk about your more recent work. Could you describe to us how exchanges took place. You've mentioned the preprints, but were there meetings in the late '70s and '80s, where those ideas were discussed—that you attended—that were particularly important?

MM: [0:54:29] I can remember going to a Gordon conference on disordered systems[40]. I think it must have been about 1979, where there were two talks on spin glasses, one by myself and the other by Shlomo Alexander[41]. His talk had baffled everyone. In my talk I argued that the divergent integrals below four dimensions at one-loop order suggested that there might not be a spin glass phase in three dimensions. After my talk the chair took a vote as to whether there was a spin glass phase transition in three dimensions and the vote was nearly unanimous that there was not!

Then, around 1980-81, there was a meeting at Aspen organized by Bert Halperin[42] on spin glasses. Nearly everyone seemed to be there. Sompolinsky[43] was there, Anderson was there… Giorgio [Parisi] wasn't there… I remember on the first day Halperin went around and asked everyone what they've been working on. He talked to everybody, and then he announced that he had talked to everyone, as a consequence of which he didn't think there was any point in having any talks! So we were completely free for the full three weeks just to work, without having to listen to everyone giving seminars. I wonder if anyone else has ever followed that method of organizing a workshop. It was very enjoyable as a consequence.

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[39] T. Aspelmeier and M. A. Moore, "Realizable solutions of the Thouless-Anderson-Palmer equations" *Phys. Rev. E* **100**, 032127 (2019). https://doi.org/10.1103/PhysRevE.100.032127

[40] Dynamics of Quantum Solids and Fluids Gordon Research Conference: Properties of Disordered Systems, July 10 - 14, 1978, Plymouth State College, NH, USA, chairs: Raymond Orbach and M. J. Stephen chairs, https://www.grc.org/dynamics-of-quantum-solids-and-fluids-conference/1978/ (last consulted January 19, 2021)

[41] Zeev Luz, Robijn Bruinsma, Yitzhak Rabin, and Pierre-Gilles De Gennes, "Shlomo Alexander," *Physics Today* **51**(12) 73 (1998). https://doi.org/10.1063/1.2805729

[42] Bert Halperin: https://en.wikipedia.org/wiki/Bertrand_Halperin

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

PC: In a note you sent us, you mentioned that Alan Bray and you would regularly travel to Orsay to talk to various experimentalists in the spin glass field.

MM: [0:57:28] Yeah! There was this organization called CECAM, which still exists. Its head was an American called [Carl] Moser, who was very eccentric[44]. In fact, he had an office in Orsay, and he used to come in with his two little dogs, which used to sit under his desk. Whenever you went into his office, they would growl at you, and he'd have to spend the next 10 minutes calming these creatures down. He was the funder of our visits. While we were in Orsay, we'd go into Paris and talk to people like Gérard Toulouse[45] and so on. They were great visits.

But then, in the early '80s, we were very interested in the experimental side of spin glasses, because we realized—well, everyone realized—that the actual Ising Hamiltonian was not really a good description of the spin glasses which were being studied experimentally. If you take a canonical one, say manganese in copper, first of all that's probably better approximated by a Heisenberg spin model. Second, there are other terms in the Hamiltonian: a dipolar coupling term, and something called the Dzyaloshinskii–Moriya interaction[46]. All that greatly complicated the effective Hamiltonian. One could write a paper just about every month, where you put in one or two of these extra terms and worked out consequences for, say, the critical exponents with these extra terms. It was a bit like the early days of critical phenomena with the epsilon expansion. There were lots of models which you could easily study, and quickly get out some results on the fixed points and the universality classes for complicated spin glass Hamiltonians. That activity has now run its course.

People these days, even though they're applying their results to canonical spin glasses like manganese in copper, pretend it's an Ising spin glass though in reality that must be a very poor approximation. They invoke excuses that small extra terms like the dipolar coupling and Dzyaloshinskii–Moriya interaction convert the effective Heisenberg Hamiltonian into an Ising Hamiltonian at the critical point, but the crossover length scales for that will be very large. It was a big activity in the '80s trying to look into what went on in real spin glasses. I can remember looking at questions like magnetoresistance of spin glasses and how the magnetoresistance changes as you pass through the spin glass phase transition. These days, I

[44] G. Battimelli, G. Ciccotti and P. Greco, "CECAM and the Development of Molecular Simulation," In: *Computer Meets Theoretical Physics* (Switzerland: Springer, 2020), 87-110. https://doi.org/10.1007/978-3-030-39399-1_5

[45] Gérard Toulouse : https://en.wikipedia.org/wiki/G%C3%A9rard_Toulouse

[46] Antisymmetric exchange : https://en.wikipedia.org/wiki/Antisymmetric_exchange

think most people in the spin glass community would not even know what magnetoresistance is. All that is a forgotten side of spin glasses, probably because there are now so few experimentalists left in the field. In the '80s, there were lots, and many of them, *e.g.*, [Henri] Alloul, [Albert] Fert[47], and [Zazie] Béal-Monod were in Paris and its environs.

Spin glasses just seemed like another topic in experimental magnetism in those days. Conferences which one went to were often the regular magnetism conferences rather than stat mech oriented meetings, because there was a large community interested in disordered magnetic materials, and an active experimental scene. Alas, I think people ran out ideas for things to do experiments on. There's perhaps only Ray Orbach's group[48] left doing experimental work these days, plus possibly the group in Uppsala[49] and that's it. There's nobody else. That's the way it goes.

PC: Beyond interaction complexity, one of the very obvious experimental reality is the finite-dimensional nature of the spin glasses. Around the mid-'80s you published a series of seminal works on the finite-dimensional behavior of spin glasses[50]. What can you tell us about this research program? How did these studies come about? And what resources you had at your disposal to carry them?

MM: [1:03:17] I think what influenced us was that to extend the Parisi replica symmetry breaking work down to three dimensions, it would involve going past the horrendous work, which Imre Kondor and Cirano de Dominicis had been doing. We just thought: "Oh no! That's going to be totally impossible to carry out." So we were looking for other ways of proceeding. One of the reasons we were interested in looking at the TAP equations is that they seemed so much easier than replica symmetry breaking. The only trouble with the TAP equations is that you have to solve them, but conceptually it was all nice and simple. You could write them down—Thouless and company had already done that—solving them was the problem. In fact, one of my pet peeves is that very little effort is being devoted into how to actually solve the original TAP equations. People have found methods which work well above the de Almeida-Thouless line, but who cares about what goes on there? It's what happens below the de Almeida-Thouless line which interests people. Getting numerical solutions is very difficult. As you make the system larger and larger, it gets harder and harder to get any solution. Anyway, it's conceptually simple. I had the idea that using the

[47] Albert Fert: https://en.wikipedia.org/wiki/Albert_Fert
[48] Raymon L. Orbach: https://en.wikipedia.org/wiki/Raymond_L._Orbach
[49] Including Per Norblad, Olof Karis and others.
[50] *E.g.*, A. J. Bray, M. A. Moore, "Critical behavior of the three-dimensional Ising spin glass," *Phys. Rev. B* **31,** 631 (1985). https://doi.org/10.1103/PhysRevB.31.631.

magnetization, m_i, and avoiding the averaging over the bonds might be the way to go. The replica trick itself was the cause of all the grief which one has with broken replica symmetry etc., and perhaps it was going to be a lot simpler if one avoided using the replica trick entirely.

So I became interested in a paper of McMillan in Urbana[51], where he'd done a real space renormalization group calculation, which was a generalization of the Migdal-Kadanoff style of approximation[52]. We thought: "Wow! This is really simple. This is what we've been looking for." It clearly worked in two and three dimensions, and you could get results with very modest efforts. It was easier to do than simulations, and it was certainly a lot easier than doing replica symmetry breaking calculations. Later on, about 1986[53], Fisher and Huse wrote their papers on the droplet model, where you got away from particular renormalization calculations, like, *e.g.*, Migdal-Kadanoff, and their work provided a general framework for understanding what might come out of such calculations; it was not tied directly to any particular approximation. That became the droplet picture. It was, of course, incompatible with the Parisi solution. That fact has kept us busy ever since. Questions arise like: While the Parisi solution is correct for the SK model, as you move away from infinite dimensions, is there a special dimension where you change over from replica symmetry breaking to the replica symmetry of the droplet picture? However, the Janus collaboration has attempted to show numerically that the replica symmetry breaking picture applies in all dimensions[54]. In this century, that's been quite an activity. People have just become exhausted with it now. I can barely bring myself to read any of the papers on this theme, but the dispute is still ongoing.

PC: To get back to the genesis of all this. How did you first become acquainted with the droplet scenario? Did you just read the paper or was there…

MM: [1:08:47] I think it was the work of McMillan, who kicked the ball off, and then we got into trying to improve upon his work and so on. Then, the paper of Fisher and Huse came along. We extended some of their calcula-

[51] P. W. Anderson, "William L. McMillan 1936–1984: A Biographical Memoir," *Biographical Memoirs* **81,** 2-17 (Washington, DC: National Academy Press, 2002).
[52] W. L. McMillan, "Domain-wall renormalization-group study of the random Heisenberg model," *Phys. Rev. B* **31**, 342 (1985). https://doi.org/10.1103/PhysRevB.31.342
[53] Daniel S. Fisher and David A. Huse, "Ordered Phase of Short-Range Ising Spin-Glasses," *Phys. Rev. Lett.* **56**, 1601 (1986). https://doi.org/10.1103/PhysRevLett.56.1601
[54] Janus Supercomputer : http://www.janus-computer.com/ (Last consulted December 22, 2020)

tions and wrote a review article, which appeared in the Heidelberg proceedings of a conference, which was held about '86[55]. Unfortunately, it's one of my best cited paper but it doesn't appear in all the citation indexes, because it appeared in *Lecture Notes in Physics*, which isn't a proper journal, and so it is not always picked up. It's by far and away my best cited paper. There's a warning there. Never publish in something which hasn't got the right ISBN number. In our library at the University of Manchester, *Lecture Notes in Physics* are just shelved amongst the journals. You wouldn't know it wasn't a proper journal from its appearance.

PC: At that time you also started working with Peter Young. How did that come about?

MM: [1:10:22] Peter Young was then in England; he hadn't yet gone to the States. So the few of us who were working on these problems in England usually met in Paris at these meetings in Orsay. It was very enjoyable: Mike Kosterlitz used to come as well. There was a whole load of us Brits coming over to Paris at the expense of CECAM to talk to French experimentalists. The local French theorists seemed to be too grand to talk to their local experimentalists. I think the French experimentalists quite liked us talking to them. That's how I met Peter, though he and I had been students at the same time in Oxford where we both worked on a model called the transverse Ising model, which is the quantum version of the Ising model in which you put on a field in the *x*-direction. He'd worked on that problem, and I had worked on it as a model for ferroelectricity. I wrote several papers on it in the late '60s. So we had worked on the same sort of problems. He was working on the TAP equation as well in the early 80's. He worked with Cirano and their results seemed to contradict ours[56]. We wrote a paper where we understood how actually the two sets of results were both the same, they only looked different. That was our first paper together[57]. Then Peter felt, like the rest of us, that all this stuff with replica symmetry breaking was getting too complicated, and that simulations might be the way to go. He went into that and made a great success of it.

[55] Proceedings of a Colloquium on Spin Glasses, Optimization and Neural Networks, Held at the University of Heidelberg, June 9-13, 1986: A. J. Bray and M. A. Moore*, "*Scaling theory of the ordered phase of spin glasses" in *Heidelberg Colloquium on Glassy Dynamics* J. L. van Hemmen and I. Morgenstern eds. (Berlin: Springer-Verlag, 1987), 121-153. https://doi.org/10.1007/BFb0057515

[56] C. de Dominicis and A. Peter Young, "Weighted averages and order parameters for the infinite range Ising spin glass," *J. Phys. A* **16**, 2063 (1983). https://doi.org/10.1088/0305-4470/16/9/028

[57] A. J. Bray, M. A. Moore and A. P. Young, "Weighted averages of TAP solutions and Parisi's $q(x)$," J. Phys. C **17**, L155 (1984). https://doi.org/10.1088/0022-3719/17/5/006 ; A. P. Young, A. J. Bray and M. A. Moore, "Lack of self-averaging in spin glasses," *J. Phys. C* **17**, L149 (1984). https://doi.org/10.1088/0022-3719/17/5/005

PC: Could you tell us a bit more about your collaboration with Alan Bray. It was very symbiotic in some ways. In what way were the two of you complementary in working on those problems?

MM: [1:13:00] Alan was a very, very, good calculator. He was astonishingly reliable. He was a great person to collaborate with because he usually got things right, whereas it was more problematic if I did a piece of algebra, whether it would be right or not. We were in this big departments of about 80 academic staff, and we were the only two people with the same interests, so it was natural for us to collaborate. When I got to Manchester as a professor, you were allowed to hire a junior person, and he'd come as a lecturer, as the junior positions are called. In a sense, it was natural that he should work with me, but he soon became a professor himself. So that's why we were collaborating. There was nobody else for us to talk to, so we had to work together! By the early '80s, Thouless had gone off to the States, Peter Young had gone off to the States, and there were very few people left in the UK working on spin glasses. Sam Edwards had got other interests. He was, at that stage, saying: "Spin glasses have got all too complicated" and he was no longer working on them. Alan was a great person to talk to and fantastic as a collaborator.

PC: Towards the late '80s, early '90s, you had a brief foray in neural networks. You had a grad student who went on to be a professor in the field as well, as a famous neuroscientist.

MM: [1:15:35] Yes, Neil Burgess[58]. He went to University College and became a professor there. He was one of these people who got into magnetic resonance imaging of what goes on in the brain. He's been doing that for a long time. But when he was a student of mine, we were much more modest in our aims. It was very hard to train neural nets to do anything. It was awful, in fact. I was quite amazed that there has been a resurgence of activity in the field of neural networks for applications, with all this deep learning stuff and so on. Because back then we could never train these networks to do anything useful. Training took enormous amounts of computer time. We tried to find simple examples of where networks might work. Imagine a child trying to learn a language. They hear all these sounds, and they have to create the mapping to presumably a smaller subset which are the words. So we thought we'd have a toy version. We just took out the spacing between words and asked the neural network to break up the resulting string of letters of the alphabet back into words. Well, it would be easy to do using a dictionary, but that presumably isn't how the brain does it. We just couldn't persuade the neural network to learn how to do it.

[58] Neil Burgess: https://en.wikipedia.org/wiki/Neil_Burgess_(neuroscientist)

Then we started collaborating with people in the psychology department, who described themselves as boxologists[59]. They believed in trying to understand the processes the brain has to go in order for certain task to be carried out. For example, say you were playing tennis and you're receiving the serve. You know the ball is coming, you know it's going to be fast. So you have to see the ball, and the brain has to interpret what it sees to get you to move to the right position to make an appropriate response. There's all these tasks, which have to be sorted out, before anything can happen. These boxologists drew lots of boxes, which correspond to the needed ways for processing the information. Neil and Graham Hitch wrote a paper, which attracted a huge amount of interest, and was influenced by that procedure. They were trying to understand what's called the short-term verbal memory. Apparently, if you ask people to memorize a list of things, they keep muttering it to themselves. They can remember lots of things if the tongue can easily go from one word to another. Certain words are easier to say next to each other than others. If the words are short, you can remember more of these words. This explains why the Chinese are very good at mental arithmetic—much better than say English people—because names of the numbers in Chinese are very small words, whereas the words are longer in English. In Welsh, they have long names for the numbers. Welsh people are supposed to be hopeless as a consequence at mental arithmetic. These differences arise as you can only have in your memory so many syllables, and you need to have them in your memory to do mental arithmetic. They built a model of how this works, and they got a prize for the best paper in the field of psychology in the years 1991 to 1993, or something like that[60]. It was a very nice paper, but I thought it was incredibly dodgy stuff. I had kept away from it. I thought it was too speculative. You just had to believe there were boxes and they would correspond to different brain processes. What was in the box, and where the boxes were in the brain was not specified.

PC: So you worked for a couple of years on neural networks—I now understand better the context—but you quickly left the field unlike many of the people who had worked on replica symmetry breaking, who moved on permanently in some ways to neuroscience. Was this related to your skepticism with respect to some of the neuroscientists' practitioning?

[59] Boxology : "The construction and ostentatious display of meaningless flow charts by psychologists as a substitute for thought". N. S. Sutherland. *The International Dictionary of Psychology*. (New York: Continuum, 1989), p. 58.

[60] N. Burgess, G. J. Hitch, "Toward a network model of the articulatory loop," *J. Mem. Languages* **31**, 429-460 (1992). https://doi.org/10.1016/0749-596X(92)90022-P

MM: [1:21:35] Partly we failed to make a success of teaching neural networks to do tasks. When you'd go to meetings on neural networks people would say to each other: "What we want is a killer application, which will really bring people into the field." And none seemed to be forthcoming at the time. It wasn't until later on, when they developed programs that could play Go and so on, that the whole field took off. You could teach a system to learn how to do tasks well. In the '90s, such examples were not available. We had ambition, but we didn't have the right computers actually to carry out that ambition.

PC: You also left the field of spin glasses for nearly a decade at that point.

MM: [1:22:48] Yes. I didn't do anything on spin glasses for nearly a decade. I went into high-temperature superconductivity. I remember coming to a meeting again in Paris. (Everything seems to have happened in Paris!) It was at a meeting on stat mech about 97 in Paris when I bumped into Imre Kondor, and he said to me: "It was a shame that the droplet model turned out to be wrong." I said: "What!" It was news to me that this was the case. I thought: "I better look up what had been going on in the years I'd been away from the subject." Then I got back into it, trying to understand how it is that the simulations could seemingly support the existence of replica symmetry breaking, but still fundamentally the system has replica symmetry. It was only Imre's remark that the droplet model had died that got me back in the field.

PC: You mentioned early on that you've kept on toiling in the field ever since, including in retirement. What do you find so compelling about this problem that keeps you active?

MM: [1:24:34] Well, I don't think the issues have been resolved satisfactorily. That's an advantage for an old codger like myself. You don't have to learn anything really new. The issue is: Which is the correct way of looking at spin glasses. Is it the droplet picture—or its cousins like the Newman and Stein variants and so on—or is it the broken replica symmetry picture with its hierarchy of states and all that kind of thing? It's a well-posed question and one would like to know the answer. There have been many attempts to sort it out. There hasn't been a satisfactory resolution of the matter, to everyone's satisfaction anyway. It's a topic which keeps on giving, though to do actual calculations gets harder and harder, because all the things which are easy to do with this matter have already been done. But hopefully, there'll be a resolution. I'm not actually doing any of this at the moment. I've gone back to straightforward SK model mean-field calculations. It's a holiday from that topic. It's always been a matter of some disappointment to me that the rigorous results community have never managed to satisfactorily resolve the issue as to the nature of the order parameter in spin glasses. You'd think they would have been able to sort that problem out, but unfortunately they have yet to do it. I think Newman and Stein had the ambition, but they left the question open: Their results admit both the possibility of a droplet-like picture and of replica-symmetry breaking-like features.

PC: In all your years as a lecturer or as teacher in Manchester, did you ever get to teach a class on replica symmetry breaking and spin glasses?

MM: [1:27:29] The closest I came to that was when we used to have a graduate course on statistical physics. I remember talking about replica symmetry and things like that in the course. But never at the undergraduate level. I don't think they would take it. People who pay good money in fees, they are not going to be fobbed off with stuff like that and keep on paying!

PC: How did your students learn? Or did any of your students learn?

MM: [1:28:10] I don't think we've ever been cruel enough to give any students a replica symmetry breaking calculation. We've had students do calculations, where they've had to use replicas. For example, I think the most famous one is [Stephen A.] Roberts[61], who worked with Alan Bray—the Bray-Roberts calculation for the critical exponents across the de Almeida-Thouless line[62]. In those days, in the early '80s, first-year graduate students were meant to do a calculation for a report which they had to write at the end of the first year. This was Roberts' report, the Bray-Roberts paper. Alan never did the calculation himself. Roberts didn't find a fixed point; Alan felt there should be a fixed point. He thought maybe Roberts had messed it up, but it was quite fiddly to check. You had to be very careful with all those replica summations to get the right answer, and he never got around to checking it. I used to come across Alan musing from time to time: "If you change this 18 to a 16, then there would be a fixed point in these equations. The calculations has now been checked by lots of people, including I guess by implication yourself, Patrick[63]. Roberts got it right. We just didn't like to give students replica calculations because there were very hard work to do.

[61] S. A. Roberts, "Theoretical Investigations into the Effect of Applied Fields and Anisotropies on Spin Glass Behaviour," PhD Thesis, University of Manchester (1982). https://www.librarysearch.manchester.ac.uk/permalink/44MAN_INST/1r887gn/alma992976680351301631

[62] A. J. Bray and S. A. Roberts, "Renormalisation-group approach to the spin glass transition in finite magnetic fields," *J. Phys. C* **13**, 5405 (1980). https://doi.org/10.1088/0022-3719/13/29/019

[63] Patrick Charbonneau and Sho Yaida, "Nontrivial Critical Fixed Point for Replica-Symmetry-Breaking Transitions," *Phys. Rev. Lett.* **118**, 215701 (2017). https://doi.org/10.1103/PhysRevLett.118.215701

Then, of course, later on came the *p*-spin type of models, which we missed out on, basically. We got into it later, when everybody else had moved on. I came to it late with some papers with Joonhyun Yeo[64]. That's another ongoing activity. It's puzzled me, actually the attitude of the community towards *p*-spin models. They are the background for the RFOT theory, which is of course still a very active theory. Yet if you look at *p*-spin models in three dimensions using simulations, the results of those simulations are actually nothing like the mean-field theory. Parisi and others have written papers pointing this out in around 1998[65]; Silvio Franz wrote some of them too[66]. It is just so different in three dimensions: The fluctuations about the mean-field theory must be enormous. They transform what happens from the well-understood mean-field results to something which is not well understood at all. What happens in three dimensions in these models, which at mean-field-level are the basis for the RFOT theory? I'm puzzled that there's so little activity on that topic. Joonhyun and I are trying to work on this and it turns out to be hard work also. There are again no easy calculations left.

PC: We're coming to a close, so we wanted to make sure that we didn't miss anything important. Is there something else we should know that you would like to share with us?

MM: [1:32:29] I think I mentioned this in the email which I sent you. When everyone lost interest in whether the Parisi scheme was correct or not and everyone agreed it was, we discovered that the actual Parisi replica symmetry breaking scheme was not completely correct! We noticed this when we tried to go beyond the thermodynamic limit for the SK model, and compute the finite-size corrections in the SK model. There, you have to keep little *n* in the problem, rather than take it simply to zero. When you keep little *n* in the problem, you realize that the Parisi solution just makes no sense. Timo Aspelmeier and I wrote a paper, where we had to generalize the Parisi scheme. Basically, the Parisi scheme is just one of the blocks. There are an infinite number of such blocks, inside of which the symmetry is broken according to the Parisi scheme. That got rid of all the problems

[64] *E.g.*, M. A. Moore and B. Drossel, "*p*-Spin model in finite dimensions and its relation to structural glasses," *Phys. Rev. Lett.* **89**, 217202 (2002). https://doi.org/10.1103/PhysRevLett.76.1142; M. A. Moore, "Interface free energies in *p*-spin glass models," *Phys. Rev. Lett.* **96**, 137202 (2006). https://doi.org/10.1103/PhysRevLett.96.137202; J. Yeo and M. A. Moore, "Renormalization group analysis of the *m*-*p*-spin glass model with *p*= 3 and *m*= 3," *Phys. Rev. B* **85** (10), 100405 (2012). https://doi.org/10.1103/PhysRevB.85.100405

[65] *E.g.*, Matteo Campellone, Barbara Coluzzi and Giorgio Parisi, "Numerical study of a short-range *p*-spin glass model in three dimensions," *Phys. Rev. B* **58,** 12081 (1998). https://doi.org/10.1103/PhysRevB.58.12081

[66] *E.g.*, S. Franz and G. Parisi, "Critical properties of a three-dimensional *p*-spin model," *Eur. Phys. B* **8**, 417 (1999). https://doi.org/10.1007/s100510050707

with taking the *n* goes to zero limit. You could then compute the finite-size corrections[67]. Later on Cirano de Dominicis and Philippe Di Francesco showed that you could keep the original Parisi scheme, provided you put restrictions on the form of the blocks (a restriction which was not in the original Parisi scheme[68]). So there have been some features, which were overlooked in the original Parisi scheme and which now have been re-solved. But there's been little follow-up work as finite-size corrections are nearly as hard as to do as three dimensions. It's curious that Thouless' initial pessimism about ever finding the correct replica symmetry breaking scheme was in fact borne out. No one doubts, in the thermodynamic limit, that the Parisi scheme is correct, and as it was formulated it stood for years. But it is not completely correct.

FZ: I think also Bernard Derrida has been working on this same problem of computing finite-size corrections, the fluctuating blocks and things like that.

MM: In the REM, in the GREM, yes.

FZ: Did you discuss with him?

MM: [1:35:40] Yes, but he doesn't use replicas to do the calculations. He has another technique, which is probably a good thing. That's probably why he's making progress. He told me that he was hoping to re-do the calculations one day entirely in the language of replicas.

PC: In closing, have you kept any notes, correspondence, and alike from that epoch over the years? If yes, do you have any plan to deposit them in an academic archive at some point?

MM: [1:36:29] There are one or two bits of correspondence from famous people over the years, which I think have kept copies of, and I'm happy to deposit them. I remember getting a letter from Anderson where he accused Alan Bray and I of deliberately misunderstanding his paper with John Hertz[69], and then receiving a letter from John Hertz, that says that only when he

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[67] T. Aspelmeier and M. A. Moore, "Free energy fluctuations in Ising spin glasses," *Phys. Rev. Lett.* **90**, 177201 (2003). https://doi.org/10.1103/PhysRevLett.90.177201

[68] C. De Dominicis and P. Di Francesco, "An exact solution for Parisi equations with R steps of RSB, Free energy and fluctuations," *J. Phys. A* **36,** 10955 (2003). https://doi.org/10.1088/0305-4470/36/43/019

[69] J. A. Hertz, L. Fleishman and P. W. Anderson, "Marginal fluctuations in a Bose glass," *Phys. Rev. Lett.* **43**. 942 (1979). https://doi.org/10.1103/PhysRevLett.43.942; A. J. Bray and M. A. Moore, "On the eigenvalue spectrum of the susceptibility matrix for random spin systems," *J. Phys. C* **15**, L765 (1983). https://doi.org/10.1088/0022-3719/15/23/008

saw our paper did he understand his own paper with Anderson. I had another letter from Mandelbrot[70], who acknowledged that I had cited his work quite correctly, but he was complaining that I hadn't used sufficient enthusiasm in citing his work. I think he wanted phrases like: "In the groundbreaking study of Mandelbrot…".

PC: So you have a few items, but you have not kept extensive notes…

MM: [1:38:20] No. Because once email came along, letters began to fade out. I can remember being in Santa Barbara in about 1980 something or other, when there was a guy, whom we had had lunch with, and he said: "After lunch, I'm going to go back to my office, and I'm going to send an email." We all said: "What's an email?" "Well come and watch". So we all went along and watched him send an email. We all fell over laughing. "Why don't you just pick up the phone and talk to the guy?" Little did we realize that we'd spend half our lives doing email. It changed everything.

All my emails before the year 2002 were lost when the computer where they were stored was removed. Somehow, I never bothered to transfer them to the new setup. They don't last forever, unless you take special measures to keep them. You think at the time it doesn't matter. Only later on do you regret that you didn't bother. One's got so much of this stuff, and most of it is complete dross. It's hard to pick out those which could be of interest in the future.

Because the truth of the matter is in the future no one will care about any of it! I guess when you're making an archive you don't like to think about that side of things, because you hope that people will consult them. But the evidence is that in the long run everything is forgotten.

PC: I nonetheless encourage you to consider it. At least talking to the University of Manchester. I don't know if they have a program for archiving faculty papers.

MM: [1:40:25] For my sins, I'm a Fellow of the Royal Society and they encourage you to deposit papers and letters in their archive, and that's what I'll probably do. Actually, in these COVID-ridden times it probably would be good idea for me to do that now. You never know when infection might strike.

PC: Mike, thank you so much for your time. It's been an extremely interesting and amusing conversation as well.

[70] Benoit Mandelbrot : https://en.wikipedia.org/wiki/Benoit_Mandelbrot

MM: I hope it will a bit of some use to you.