History of RSB Interview:
J. Michael Kosterlitz

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PC: Prof. Kosterlitz, thank you for joining us. As we discussed ahead of time, the purpose of this exchange is to go over the period during which spin glass models and replica symmetry breaking, in particular, were formulated, from roughly 1975 to 1995. Before we get to that, we have a couple of background questions, if you allow us. In your Nobel biography[1], you mentioned that you started to work with David Thouless[2] when you joined Birmingham as a postdoc, and that you had given statistical physics little attention before then. What was your impression, as a theoretical physicist, of statistical physics at that time?

MK: [0:00:49] We're talking about the early 1970s. At that time, I didn't understand much of the rigorous statistical mechanics—solutions of the eight-vertex model[3] or any statistical mechanics in fact—but I was interested in phase transitions. So, when the renormalization group came along, I said: "This looks interesting as a way of dealing with this situation in a relatively simple way." I learned renormalization group from the papers by Phil Anderson and Gideon Yuval on the $1/r^2$ Ising model, written in '70-'71[4].

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[1] *J. Michael Kosterlitz Biographical*, The Nobel Prize (2016). https://www.nobelprize.org/prizes/physics/2016/kosterlitz/biographical/ (Consulted July 17, 2021)
[2] David J. Thouless: https://en.wikipedia.org/wiki/David_J._Thouless
[3] Eight-vertex model : https://en.wikipedia.org/wiki/Eight-vertex_model
[4] P. W. Anderson and G. Yuval, "Exact Results in the Kondo Problem: Equivalence to a Classical One-Dimensional Coulomb Gas," *Phys. Rev. Lett.* **45**, 370 (1969). https://doi.org/10.1103/PhysRevLett.23.89; P. W. Anderson and G. Yuval, "Exact Results for the Kondo Problem: One-Body Theory and Extension to Finite Temperature," *Phys. Rev. B* **1**, 1522 (1970). https://doi.org/10.1103/PhysRevB.1.1522; P. W. Anderson, G. Yuval, and D. Hamann, "Exact Results in the Kondo Problem. II. Scaling Theory, Qualitatively Correct Solution, and Some New Results on One-Dimensional Classical Statistical Models," *Phys. Rev. B* **1**,

PC: Once you started working with David Thouless what guided your problem selection? And how did the two of you worked together?

MK: [0:02:06] That's quite simple. I was originally a high-energy theorist, and I was doing long tedious calculations. About twice, I was just about to start writing up my calculations for publication when the preprints arrived at my desk, doing exactly what I had done. The first time that happened, I threw my hands up and said: "Ok. These things happen." Then I started a new problem and exactly the same thing happened again. The work was being done by a group at Berkeley[5], and I decided there's no way I could compete with a group of several people, while I was stuck in some office in Birmingham by myself.

I started walking around the department, asking everybody I found: "Do you have a problem I could look at?" The answer was consistently no, until I got to David Thouless' office. He started talking and I really did not understand much. He was writing things on the board and talking while I got more and more lost as he proceeded. At some point I said: "David, sorry, I have to stop you there. I am completely lost. Could you please explain where did the first equation you wrote down come from?" He turned around and said: "Didn't I tell you that?" I could honestly say: "No, you didn't." At which point he said: "Oh!" Then he proceeded to give a very clear and coherent explanation. I decided that, should I have much contact with this man in the future—I'm sure that I would be in the same position, not understanding what he's talking about—I could just assume that he would have done the same thing, that he'd missed out something important. That gave me the courage to ask the stupid questions. Somehow, he seemed to appreciate this, and we got on very well afterwards. So we started working together.

PC: Was it mostly that he would come up with problems, and you would work on them together? What was the modus operandi?

MK: [0:05:15] The way it worked. He was interested in the problem of phase transitions in certain two dimensional systems, because there was a conflict between some rigorous and accepted statements. The Mermin-Wagner theorem[6] says that there's no long-range order in two-dimensional

4464–4473 (1970). https://doi.org/10.1103/PhysRevB.1.4464. See also: P. Coleman, "Phil Anderson's Magnetic Ideas in Science" in *PWA90: A Lifetime of Emergence*, P. Chandra, P. Coleman, G. Kotliar, P. Ong, D. Stein and C. Yu eds. (Singapore: World Scientific, 2016), 187-213.

[5] Stanley Mandelstam : https://en.wikipedia.org/wiki/Stanley_Mandelstam. See, *e.g.*, S. Mandelstam, "Dual-resonance models," *Phys. Rep.* **13**, 259-353 (1974). https://doi.org/10.1016/0370-1573(74)90034-9

[6] Mermin-Wagner Theorem : https://en.wikipedia.org/wiki/Mermin%E2%80%93Wagner_theorem

systems with continuous symmetry, and therefore according to the lore that existed in the early 1970s, there could be no phase transition. A low-temperature phase has to have long-range order, and therefore this Mermin-Wagner seems to exclude a phase transition in systems like two-dimensional XY and Heisenberg models. It seemed very reasonable, but then David pointed out some experimental data on superfluid helium films. This is a two-dimensional system with continuous symmetry, short-range interactions, etc., so that system should not have a phase transition, according to the mathematical theorems. But it clearly did have. The data said: "Look! There's a phase transition." David said: "This needs an explanation. What's going on here?" That was the start of our collaboration. Eventually, he decided that the essential thing which will destroy superfluidity is vortices. We started up with a two-dimensional superfluid system. We asked the question: "What sort of excitation will destroy this superfluidity?" The only thing that can destroy superfluidity is vortices. Local excitations do nothing. These vortices interact logarithmically in two dimensions, so this was obviously the system to look at, this set of point particles interacting logarithmically in two dimensions. That's the start of everything[7].

PC: Then how did you first hear about spin glasses and in what context?

MK: [0:08:35] Thouless was also very interested in random systems. He was working a lot on localization and on spin glasses. I got intrigued by spin glasses, so I started trying to understand the literature and understand what's going on, not really successfully.

PC: Do you have any insight into where David Thouless' interest in those models and problems came from?

MK: [0:09:13] David was a funny person. I would classify him as a genius, same as Feynman[8], Schwinger[9], and so on and so forth. David was always intrigued by unusual problems, contradictions between theory and experiment. Stuff like that always fascinated him when something needed an answer. Thouless was the sort of person who had a very flexible mind, and could see through, and really understood the physics, understood all the standard wisdom, and could immediately pick on anything that was wrong. He was an amazing person to talk to, because he would always focus on the essential points and discard all the trivial unimportant points. He would

[7] J. M. Kosterlitz and D. J. Thouless, "Long range order and metastability in two dimensional solids and superfluids. (Application of dislocation theory)," *J. Phys. C* **5**, L124 (1972). https://doi.org/10.1088/0022-3719/5/11/002; "Ordering, metastability and phase transitions in two-dimensional systems," *J. Phys. C* **6**, 1181 (1973). https://doi.org/10.1088/0022-3719/6/7/010

[8] Richard Feynman: https://en.wikipedia.org/wiki/Richard_Feynman

[9] Julian Schwinger: https://en.wikipedia.org/wiki/Julian_Schwinger

identify those instantly. It was just an incredible experience talking to him, working with him, to see through all the garbage and fluff, and getting straight to the essential points. It was quite an experience and this was the first time I really understood what physics is all about.

PC: Can you walk us through the genesis of your first works on spin glasses, which were about the spherical model[10]? How do you get from the SK and the Edwards-Anderson proposals to that?

MK: [0:11:31] Thouless was also interested in these random systems. This was a problem which was important and extremely difficult. There was really nothing known about it. The Edwards-Anderson stuff, it looks alright as a mean field theory for spin glasses, but what to say? I was a person who liked to see some exactly solvable model. If you have some class of systems, I always wanted to see some silly trivial but exactly solvable model which contains some of the essential aspects of the physics. Things like spherical models for phase transitions. Nice simple soluble model, which at least displayed some of the phenomena of a continuous phase transition. So I thought maybe one could play the same sort of trick for the mean field spin glass system to see what would happen. It would be nice if you could solve this problem, at least that would give you an exact solution to this very difficult problem. It seemed to work quite well and wasn't too difficult. I was quite surprised that it worked out.

Then of course we couldn't understand the Parisi symmetry breaking business. I could follow the steps that Parisi did, but to me it was obviously mathematically completely illegal, and didn't seem to make much sense.

PC: We will get to the Parisi solution. I wanted to talk about the pre-Parisi period of a bit more. If I understand correctly, this idea of the spherical model was largely driven by your curiosity and interests in this approach. David Thouless' interest was in disordered systems, but this particular approach was your spin on it.

MK: [0:14:57] It just seemed this spherical limit giving you a solution to the standard ordering phase transition problem might work for the spin glass too. We tried and it seemed to make some sort of sense.

PC: What was the reaction to that work from the community?

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[10] 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; "Spherical model of a spin glass," *Physica B+C* **86**, 859-860 (1977). https://doi.org/10.1016/0378-4363(77)90716-1

MK: [0:15:20] Not much. People were more interested in the Ising spin glass, because it's more interesting and more realistic. The spherical limit is so far from reality that it was of no particular interest. Then also, of course, people were very intrigued by the infinite range Edwards-Anderson model that was a more realistic model but nobody could solve it. Basically, we were looking for some doable model. Because, to me, it was more that this system was so complicated that any realistic model was completely insoluble. So maybe it was worth looking for a toy model.

FZ: I have a question related to this idea of looking for a solvable case. In statistical mechanics people have used at least two different approaches. One is the idea of sending the dimension of space to infinity, then start by solving the mean-field description then doing some renormalization group or some loop expansion looking for the upper critical dimensions and so on. The other is to send the number of spin components to infinity and to do some kind of 1/*N* expansion. You worked quite a lot on the 1/*N* expansion in disordered systems and in spin glasses, but I think recently people have been looking mostly at the 1/*d* expansion. Do you have any insight on why the 1/*N* expansion has been kind of abandoned in the '80s?

MK: [0:17:45] I don't really have any insight. The only guess I would have is that the 1/*N* expansion for spin glasses was incredibly difficult. Even for relatively simple situations, e.g., uniform systems, the 1/*N* expansion is, to say the least, somewhat tedious. For the spin glass problem, it was so tedious as to be almost impossible. Anyway, this sort of expansion is an expansion about some solvable model. The spherical system with *N* goes to infinity limit is a soluble problem, it's the spherical model. But in the spin glass system the solution didn't really exist. Even if it did, to formulate a 1/*N* expansion wouldn't get you very far, because even for a real system it's not a very good approximation. Anyway, it was simply algebraically too hard for me to do this 1/*N* expansion.

PC: In your Nobel biography, you mentioned that your multiple sclerosis diagnosis in 1978 deeply affected your research choices and productivity for a few years afterwards. Did it have a particular impact on your study of spin glasses?

MK: [0:20:00] It basically impacted everything. At the time I was living a double life. Half my life was spent in the mountains, and half in physics. At the time the mountaineering part was more important than the physics part. At least, I enjoyed it more. With the multiple sclerosis, I had to give up climbing because my balance had gone. Basically, I had to give up half my life and that was not an easy thing to do. I went into a bit of a depression for a while until eventually I came out of it. At least, I had the physics half of my life left. Certainly this diagnosis affected things quite badly. Basically, I lost interest in most things, but since I had to keep my job, if you wish, I continued working in physics.

PC: At about that same period, there were a few proposals for replica symmetry breaking that were popping up from different circles. Did you have any impression of those ideas in the pre-Parisi context?

MK: [0:21:58] I had no thoughts about replica symmetry breaking. The replica trick was one thing, but symmetry breaking was, to me, a step too far. I could follow the mathematics, but none of the mathematics made sense. It didn't connect with reality.

PC: Is that also your impression of the Parisi ansatz?

MK: [0:22:54] I could swallow the *n* goes to zero limit as a way of averaging the free energy, but that was as far as I could really swallow it. Beyond that, the mathematics seemed to have absolutely no justification whatsoever. As you start breaking replica symmetry, and getting further and further from any reality, I just couldn't really swallow it.

PC: In 1980, shortly after you left Birmingham, you published with David Thouless, a paper on stability analysis of the Parisi solution[11]. How did that come about if you were so skeptical of the Parisi ansatz?

MK: [0:24:05] It was still worthwhile looking at whether that solution could make any sense and be stable to perturbations. I wasn't saying that it was wrong, all I was saying was that I couldn't swallow it.

PC: Did you know of Giorgio Parisi before that came about?

MK: [0:24:40] No, but I met him later.

PC: So how did you find out about his solution? Through the literature or the grapevine?

MK: [0:24:52] I can't really remember. I think it was through David Thouless. Parisi sent him a preprint or something and David showed it to me. I think that's how I found out about it. Then I tried to understand it and failed.

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[11] 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

PC: Once you moved to Brown—just a couple years later—your first PhD student there, Anuradha Jagannathan[12], worked on the random anisotropy model, which has a glass phase. What led you to study this particular spin glass model?

MK: [0:25:48] It just seemed like one of these problems in randomness that may be possible to solve, that's all. At the time, I decided that trying to formulate some realistic model for randomness was just ridiculous because it would be far too complicated to do anything with. So any way of introducing randomness to any model was worthwhile following up to see what would happen. That's why we looked at this particular model.

PC: Shortly after that, you started doing numerical work[13]. You transitioned from exactly solvable models to numerically solvable models. What guided that decision? And had you paid much attention to numerical work before then?

MK: [0:27:07] No, I paid no attention to it at all, basically because I never learnt to code. Also, when I was a graduate student at Oxford, I was sitting in an office with three or four other graduate students. We were all doing high-energy physics. The other graduate students were all heavily involved in computation. I noticed that they were sort of running around, as time got nearer to thesis time and getting paler and paler. I did a little bit of numerical work, and decided that coding was just too horrendous to even contemplate. I decided that no, it was not for me.

Later on I realized that for all physics problems, it's good to be able to formulate them, but once you formulate them they're all too difficult to do anything with analytically. Or they are already solved. So out of necessity, I decided that since I can't do it analytically and I want to understand something about the system, I've got to do it numerically. Then I discovered that graduate students in the [United] States, most of them, were very good at coding, so it was a natural step to take.

PC: Did you ever code yourself in that context, or was it always your students' work?

MK: [0:29:09] I tried to do some coding myself calculating some integral numerically. The machine spat out a number which looked perfectly reasonable,

[12] A. Jagannathan, *A 1/N expansion for the random anisotropy model*, PhD Thesis, Brown University (1986). https://search.library.brown.edu/catalog/b1238221 (Consulted July 19, 2021)

[13] See, *e.g.*, J. M. Kim and J. M. Kosterlitz, "Growth in a restricted solid-on-solid model," *Phys. Rev. Lett.* **62**, 2289 (1989). https://doi.org/10.1103/PhysRevLett.62.2289

and then I discovered that this number was actually wrong. It was the difference between 4 and 6. The correct answer was 6 but the computer was spitting out 4. I could not understand. It took forever to find the place where the error was. The trouble was that I wrote the code, put it in the machine and it ran immediately. I thought: "Wonderful, I'm an expert." And then it spat out this number which looked right but turned out to be wrong. Then it took me ages to track down the mistake. I guess this is ridiculous. I can write a piece of code which looks perfectly good, but it's not right, and then the stupid machine spits out a number. I previously had the impression that if you made a mistake in the code the program just wouldn't run. Then I came to the realization that this wasn't quite true. It could run and spit out a number which looked right.

Then, there was another episode of coding when I was trying to write some code for statistical mechanical simulation. The code ran, but the answer didn't look right. So I simplified the code to a point where the problem was actually trivial, and I could follow every step mentally. Then I ran it and it didn't work again. It spat out completely wrong numbers. I said: "What the hell is going on?" Then we discovered that for some reason at one point we declared an array size to be 64. I said: "Let's just change this number from 64 to 65 and see what happens." Then it ran and it seemed to be perfectly correct. Next, we discovered that if we put any array size which was 2^n something went wrong. Then, we eventually discovered that there was a bug in the compiler. It was an IBM desktop. We wrote an angry letter to IBM complaining about this bug in their compiler. We got back a letter to the effect that was a non-fatal bug and was really of no interest. The amount of time we'd spent tracking this bug was just unbelievable, and to get this letter saying that it's a nonfatal bug, just drove me crazy. From then on, I decided coding is just not worth the effort. So now I've gotten to the point where it's just the graduate students doing the coding.

PC: After Anuradha Jagannathan's thesis, you didn't work on spin glasses for the better part of a decade. Did you stay in touch with the spin glass community during that time? If yes, how?

MK: [0:34:00] I just followed the literature, and just saw that people weren't really getting anywhere. It's like a lot of difficult problems. What seems to happen is that there's a lot of very smart people, the only trouble was that all these people were basically doing the same thing. They're all getting to some point, and then they're not making any more progress. They're just going around in circles, producing papers and stuff, which actually isn't going anywhere. It just seems to me that the only way of making progress in fields like this is for somebody, some ignorant fool, coming from left field, and just saying: "Let's forget about the standard wisdom and see if we can do something else." Sometimes this succeeds, mostly it doesn't.

PC: In the mid-'90s you did return to the study of spin glasses using numerics[14]. What made you think that this was potentially one of those interesting approaches?

MK: [0:35:31] It seemed to me that it's possible to construct a reasonable description of a spin glass with short range interactions and simulate several realizations of disorder. With a graduate student, Nobuhiko Akino[15], I tried the Ising spin glass with very limited success. We blamed the lack of computer power available to us but I do not think that was the real issue. After I'd done the two-dimensional planar rotor I was talking about—those topological defect orders and stuff—I thought to myself: "Look, in these systems what seems to be happening is that the system can be decomposed into a Hamiltonian describing smooth fluctuations plus another piece describing the interaction between vortices." At least for the two-dimensional planar rotor model, the only important bit is the vortex-vortex interaction, the smooth spin wave part is unimportant and you can just ignore that. Maybe the same thing happens in XY spin glasses. So it was possible to write down the problem where the frustration of the vortices decouples from the smooth variation of the phase. So if you just concentrated on the frustration part, maybe [one] could do something.

Then, there was a question, of course, that if you're going to do something like finite-size scaling, which seemed like a powerful way of doing problems where you want to figure out whether a distinct ordered state exists, then finite-size scaling is a very good way of doing it. Like [for a] *d*-dimensional uniform planar rotor model where the defect (domain-wall) energy scales like the system size to the power of *d*-2. If this *d*-2 [exponent] changes to some negative exponent, this means that the energy of a defect vanishes for *L* large and there will be a lot of these defects, which means that the system is disordered, like many domain walls in a magnet. If this exponent is positive, then these defects cost too much energy and, at low temperatures there won't be any. Presumably it's an ordered phase. If the stiffness exponent is negative, the energy of a defect when the system gets

[14] J. M. Kosterlitz and N. Akino, "Numerical study of order in a gauge glass model," *Phys. Rev. Lett.* **81**, 4672 (1998). https://doi.org/10.1103/PhysRevLett.81.4672; "Numerical study of spin and chiral order in a two-dimensional XY spin glass," *Phys. Rev. Lett.* **82**, 4094 (1999). https://doi.org/10.1103/PhysRevLett.82.4094;

[15] Nobuhiko Akino, *Numerical Study of XY Spin Glass and Gauge Glass Models*, PhD Thesis, Brown University (1999). https://bruknow.library.brown.edu/permalink/01BU_INST/9mvq88/alma991029962389706966

very big, goes to zero. It will be there and this will correspond to no phase transition. This is actually quite a powerful way of doing things.

The question was: Could one actually do this numerically for a random system? It's not easy. People had attempted this before but they'd been a bit too naïve, because they'd just used the method which works for a uniform system by comparing the energy with periodic boundary conditions versus antiperiodic boundary conditions in one direction. In a ferromagnet this induces a domain wall. If this defect energy increases with system size, clearly there's no phase transition. But of course for a random system, this starts getting a bit difficult because periodic and antiperiodic boundary conditions are just random conditions. If you have just some random interactions, and you're looking at a defect, the question is what the hell is the ground state? To find a ground state, you need to apply the correct boundary condition first. For instance, suppose you take an antiferromagnet. If you have an even number of lattice sites in one direction, then the appropriate boundary condition to respect the ferromagnetic ground state is periodic, but it makes the number of lattice sites odd in that direction then the proper boundary condition is antiperiodic. Therefore, for a random system it's clear that periodic and antiperiodic boundary conditions are just two sets of boundary conditions which don't respect anything in particular. If you want to do something analogous to what you do in a uniform system, you'd have to first find the boundary conditions which are consistent with the ordered state. But since you got randomness, you don't know what the ordered state is, and therefore finding the appropriate boundary condition is a bit difficult.

We actually managed, for the XY spin glass—or at least the gauge glass, if you wish—to exploit this by arguing that the boundary conditions are themselves—with some sort of path to be decided by the structure of the state—part of the simulation. Those boundary conditions have to be determined by the simulation, as those boundary conditions minimize the energy of a system. Then, to induce the defects, you simply change the boundary condition in one direction by putting an extra twist of π across the system. Then, the energy with those conditions will be the energy of the system with a defect, and therefore that energy is almost by definition larger than the original one. If your simulations make any sense at this point, it's a good check to make sure that you actually see that changing the conditions will actually increase the energy. The most difficult part of the calculation was to work out what these boundary conditions consistent with the ordered, lowest energy state [and] actually how to impose them. That took us a long time to figure out. We'd stand there at the blackboard, arguing and going around in circles. But after a month of this we finally managed to figure out what to do. We realized that it was actually possible to impose this boundary condition and then make a uniform twist in the phase about this boundary condition to calculate the energies with the ground state BC and also with the twisted BC which imposed a defect..

I remember when we published our first paper on this, there was a general reaction of blank, of not understanding, which really surprised us. It had taken us about two months of arguing and thinking about nothing else, so we could understand what we were doing. Eventually, we did understand it, and so our method seemed to work but most of the spin glass community were hostile to our approach..

You see, it was analogous to this. If you think about a ferromagnet, you know that the boundary conditions consistent with the ground state are periodic. And if you want to induce a defect you simply flip the boundary condition in that direction from periodic to antiperiodic. Simple! For a random system it's very different, but one still want to do the analogue of this periodic/antiperiodic boundary conditions. The analogue of the periodic BC are those boundary conditions which minimize the energy. So these boundary conditions have to be determined by the simulation. Once these are known, the defect is introduced by a uniform twist of π.

PC: In your Nobel biography, you mentioned that at about the same time as this work came out, or maybe even following this work, you lost your NSF funding that was associated with spin glasses. And that you didn't really understand why that happened. Have you since learnt more?

MK: [0:47:48] No, I haven't bothered, because I found that I could work perfectly happily without a grant. I thought: "It's not worth my time applying for grants." Even if I did put the effort to write a grant proposal, it absorbs too much time. I did put in the effort once and I was turned down again. So I thought that it's just not worth it.

PC: Some have mentioned that a change of program manager at the Condensed Matter and Materials Theory group at NSF might have been effecting a change in taste for physics problems[16]. Is that your impression as well?

MK: [0:48:38] I have no idea, because I just take the point of view that I'm to the point where I decided if I want to do physics, I want to solve the physics

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

problems which turn me on. I don't care what everybody else is doing, because I don't like to follow the crowd and do whatever else is doing. I want to solve problems which turn me on. Since I took that attitude, I decided that there's no point in applying for more grants, because after the experience of turning down this grant on random system, probably the referees will not understand what I'm talking about anyway, so what's the point.

PC: In that context of a free exploration of physics, how influential has spin glass research been on your overall efforts in physics, if any?

MK: [0:49:53] It hasn't had any particular influence, except in the sense that physics problems are hard and we need the people with a new idea, who are apt to think outside the standard box.

[I] believe those things are not going to get you a grant, because the funding agencies want results and unless you already have results you will get turned down. Say you write something like: "I want to study this because there's a lot of unsolved problems which are interesting." That doesn't get you funded. What gets you funded is saying: "I'm going to do this and this, because I will get this result from this, that result from that, and so on and so forth." Basically, you have to do the work before you apply for the grant. For me, that's not research, because if you already know what the answer is, why bother? You're not going to solve the problem because you've already solved it, but you have to solve it to write the proposal to get the grant. In my advanced years, the only conclusion you could come to is: "What's the point of wasting two or three months writing a grant proposal?"

PC: From you having worked both in Europe and in the US, do you have any insight into the difference in perception into replica symmetry breaking between the two physics communities?

MK: [0:52:14] I will just say that there's one paper that had a very heavy influence on me. That was the paper by Daniel Fisher and David Huse[17], who basically were not playing any replica symmetry games, but I thought their approach made more sense. I was thinking: Since we don't know what this symmetry breaking stuff does and whether it makes any sense, if you want to look at a short-range spin glass or a random system with short-range interactions, you are better off not trying to use replicas. Just do the simplest, most obvious way of doing the averaging. Take a set of interactions, calculate the free energy or whatever, and then repeat this over and over

[17] D. S. Fisher and D. A. Huse, "Ordered phase of short-range Ising spin-glasses," *Phys. Rev. Lett.* **56**, 1601 (1986). https://doi.org/10.1103/PhysRevLett.56.1601

again for different realizations of randomness, and then do the averaging. This is the obvious thing to do numerically. That method seems to make a lot more sense, so that's what I tried to do and it seemed to work. It made the computations a little bit tedious because repeating the computation many times for different sets of randomness is a bit time-consuming but then you know what you are doing.

PC: During your time at Birmingham, at Brown or elsewhere, did you ever teach a class that talked about spin glasses, and maybe even replica symmetry breaking?

MK: [0:54:43] I think in an advanced statistical mechanics course I may have touched on it, but in advanced statistical mechanics courses there's so much material that there's barely time to do anything in detail.

PC: We're approaching the end of our conversation. Is there anything else you would like to share with us about this era, that we might have skipped over or neglected?

MK: [0:55:19] Not really. Just that that era was the period when I was young enough to actually look at new things and with a bit of luck actually make some progress, sometimes. I think it was a fun period, but then I think that is just because I was young enough to enjoy myself. It's always been my mantra, especially for young people. You should always do what you enjoy and have a lot of fun doing it. Otherwise what's the point of studying what you study? If you don't have any fun doing it, then don't do it.

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

MK: [0:56:38] To tell the honest truth, no, because my method of keeping notes was just to write on bits of paper, and then those bits of paper would gradually disappear. They would sit in a pile and gradually disappear. So I don't actually have any notes of anything. One of the ways I have of keeping things it to type something on a computer which lasts as long as the computer memory lasts.

PC: Prof. Kosterlitz, thank you very much for your time.

MK: You're welcome.

FZ: Thank you very much.