History of RSB Interview: Imre Kondor

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Interviewer:

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Location:

Over Zoom, from Prof. Kondor's home in Budapest, Hungary.

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PC: Professor Kondor, thank you very much for taking time to chat with us. As we discussed ahead of the interview, the purpose of this discussion is to go over to the period during which replica symmetry breaking was formulated and developed. But to get to that, I have a few background questions I'd like to go over. In particular, in notes that you sent us ahead of this interview you mentioned that you hesitated between pursuing a career in piano and one in physics[1]. Can you first review what led you to be interested in physics and then to choose physics over music?

IK: I was interested in many things. Too many, possibly. At an early age—something like 12—I had this very remarkable teacher of chemistry in school, who created a fantastic enthusiasm not only in me but in basically the whole class. He must have been a very remarkable person. I'm not able to recall any specific trick he used. Some years later, I had a chat with an ex-classmate and asked him what the secret might have been. The guy hesitated, and then after a certain while said that [this teacher] was an authentic person. You have to understand the weight of such a description. That was the '50s and Hungary was in a full-fledged Stalinist terror regime[2]. To be an authentic person, as a teacher, was no trivial achievement under those conditions. That might have been part of the magic.

The guy did not even have a diploma or a degree. As a curiosity, we went to university simultaneously. [Earlier] he [had been deported to the countryside], because he was coming from the wrong sort of family—enemy of

[1] **IK**: My journey to science was less than a straight arrow, though. During my high school years I mainly played the piano, attended the conservatoire as an external student, going to concerts nearly every evening, and practicing up to four hours each day. I wanted to seek admittance to the Academy of Music, and decided for physics only at the last moment, a very sober decision that spared Hungarian music and myself a lot of frustration.

[2] Stalinist era of the Hungarian People's Republic (1949-1956): https://en.wikipedia.org/wiki/Hungarian_People%27s_Republic#Stalinist_era_(1949%E2%80%931956)

the people or that sort of thing—and therefore he was prevented from going to university. As it happened, we got admitted together. [Yet] he knew a lot, that's for sure, about physics and chemistry.

He fed all kinds of textbooks to me, including university textbooks. I regarded those as somehow holding the secret to Creation, that type of childish picture. I really took them seriously. I took notes out of those books, including an 800-pages university textbook[3]. Taking notes—if you're young, your memory is working fine—I learned the whole thing. It helped me a lot during my university years, because I had this intimate relationship with different materials, elements, salts, whatever. That was kind of unique among my classmates at university who, being physicists, looked down at chemistry, like a poor sort of kitchen science. I was regarded an expert on the basis of something which I learned at the age of 12.

Also, I had this key experience of building experiments, and checking that they work out as expected. Not automatically. You had to pay attention to details. Nevertheless, I had this little laboratory, and it was very reassuring[4]. It was more than reassuring. It was a revelation that there is one segment in life where this universe of lies—which was all over the country—was not present, and [where] you could trust what you read. It was a fantastic experience. Later, I found out that many of my colleagues in Eastern Europe, including the Soviet Union, were motivated by the same sort of thing. This may be behind the brilliance of certain [Eastern European] physicists.

PC: So you chose physics over music. Music also does not lie, by the way.

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[3] Lengyel Béla, Proszt János, and Szarvas Pál, *Általános és szervetlen kémia* [General and Inorganic Chemistry] (Budapest: Tankönyvkiadó, 1954). This book was the standard university textbook at the time.

[4] **IK:** The most spectacular experiment I did in my private laboratory was to make an electric arc. It required a fairly high current density, well beyond what you could get from the mains. I circumvented this difficulty by building a little contraption to manipulate the carbon electrodes immersed in an electrolyte. (I surmised [that with] the electrode in the electrolyte one would have much more charge carriers than in air.) This became a kind of key experience for me: in a world where vicious political lies penetrated every aspect of life, here I was, understanding something at the age of 12, able to predict the outcome of an experiment, and flooding the room with an incredibly bright light as a proof! From that point on I have regarded the hard sciences as the safe havens of truth. And this is also why I am very reluctant to believe in string theory or in any speculation that cannot be experimentally verified. By the way, the arc left behind some gooey tar at the bottom of the out-of-use aquarium that I could only remove by sacrificing my cherished CCl_4 as solvent, thus obtaining a deep purple solution. Knowledgeable friends told me later that this black tar might well have contained fullerenes, so I may belong to the (presumably large) group of people who encountered fullerenes without realizing what they were dealing with.

IK: [0:06:13] Yes. First, I was not good enough. That was rather clear to me. Besides, I found music to be an emotional burden. A Beethoven sonata is a high drama. If you take it seriously, then it does have an impact on you. I did not necessarily want to spend my life under this constant emotional pressure.

PC: You pursued a diploma thesis specifically in theoretical solid state physics, or condensed matter physics[5]. What drew to that?

IK: [0:07:08] [They were] many-body problems, not solid state; it was liquid helium related. I found superfluidity [to be] mesmerizing. It was a very interesting phenomenon. The person who was involved in that type of research at the Theoretical Physics Department at Eötvös University was a very serious scientist[6]. He was not a showman. His lectures were somewhat boring, but he paid conscientious attention to detail. He was able to make us understand all of it. Many-body theory is a technical subject, so you have to learn a lot: second quantization and all that stuff. He convinced me that this is a guy who takes himself seriously and takes his subject seriously. So I decided to join him, or [rather to] ask for a research subject from him.

The first thing he gave me was not related to bosons, but to fermions: Friedel oscillation and that type of things. Overscreening—if [people still] know what it is—[comes from] the random phase approximation producing an effective distribution of charge density. More charges are chased away from the neighborhood of a random ion than the actual density that exists in the [vicinity of that] ion. We made some progress, but I was not happy with it. I thought it would make a thesis, but a trivial one.

So we changed subject—rather late—and ended up with this problem, whose first appearance in the literature was due to Gavoret and Nozières[7]. [It was] a proof of the [coincidence of the] collective excitation spectrum and the one-particle excitation spectrum in condensed [bosonic] systems. My thesis advisor had a strong suspicion that this was not just something

[5] Imre Kondor, “Abszolut zérus hőmérsékletű Bose-rendszer elemi gerjesztéseinek leírása” [On the Elementary Excitation Spectra of a Bose System at Zero Temperature], Diploma Thesis, Eötvös Loránd University (1966).

[6] Péter Szépfalusy: https://hu.wikipedia.org/wiki/Sz%C3%A9pfalusy_P%C3%A9ter; Imre Kondor, “Kondor Imre: Szépfalusy Péter halálának első évfordulójára: A teljes tudományos életút áttekintése,” [Memorial speech of Imre Kondor on the occasion of the first anniversary of Péter Szépfalusy's death: An overview of his entire scientific life] *Fizikai Szemle* **66**(January), 2-7 (2016). http://fizikaiszemle.hu/archivum/fsz1601/tart1601.html (Consulted April 25, 2021.)

[7] J. Gavoret and P. Nozières, “Structure of the perturbation expansion for the Bose liquid of zero temperature,” *Ann. Phys.* **28**, 349 (1964). https://doi.org/10.1016/0003-4916(64)90200-3

which is valid at zero temperature in the long wavelength limit, but it may be due to the existence of the condensate, and so may be true all over the condensed regime. He posed a problem, and I managed to extend this theorem by Gavoret and Nozières. Later it became a paper[8].

PC: If I understand correctly, after that you didn't have to do a PhD thesis. Is that correct?

IK: [0:11:14] Well, it has its own story. The PhD as it is understood in the West, or as it is understood now also in Hungary, did not exist. There was a doctorate, which a university was able to confer on you, but it was not really much more than the doctorate of a lawyer or a medical doctor. It's some embellishment to your name, but it was very cheap to get it. Nevertheless, I decided to get such a title. But in order to be able to submit your application for a title like that, you needed the support of the local Communist Party. I happened to be on bad terms with the party secretary, and he told me that he would not support me. Since the whole thing did not really have any kind of significance, I decided not to bother with it. Parallel with university-given doctorates, we also had the Soviet system of scientific titles, or degrees, or whatever: Candidate of Sciences[9] and Doctor of Sciences[10]. [Those were] given by the [Hungarian] Academy of Sciences[11]. It was much more serious. By the time you got a Candidate of Sciences degree, you had to have something like a dozen papers to your name, at least, and in order to get the Doctor of Sciences degree you had to have some students, some school around you, and [the Doctor of Sciences degree was usually regarded as a condition for becoming] a full professor. I got those from the Academy of Sciences. When the political changes came at the beginning of the '90s, they decided that a Candidate of Sciences, and even more [so] a Doctor of Sciences, implied a PhD. So I have a piece of paper, but frankly I never had a proper PhD.

PC: As you were not a trainee, as we would have at least in a PhD program today, how did you choose research questions after your diploma thesis?

[8] I. Kondor and P. Szépfalusy, "On the connection between the one-particle Green's function and the density-density correlation function in a large bose system," *Acta Physica* **24**, 81-92 (1968). https://doi.org/10.1007/BF03159393
[9] Candidate of Sciences: https://en.wikipedia.org/wiki/Candidate_of_Sciences
[10] Doctor of Sciences: https://en.wikipedia.org/wiki/Doctor_of_Sciences
[11] Hungarian Academy of Sciences: https://en.wikipedia.org/wiki/Hungarian_Academy_of_Sciences

IK: [0:14:00] I had this thesis advisor in the Masters program, and when I got to graduation, he went to the US and became a member of Richard Ferrell's group[12], who introduced the dynamic scaling hypothesis. When he returned to Hungary, and when I was able to get the position in the same department as where he was working, I joined him and we tried to do some work on dynamical scaling. The microscopic theory of dynamical scaling is quite complicated, because in contrast to the static scaling, the vertices tend to have singularities due to hydrodynamic modes. We did not take that into account, so most of what we did on dynamical scaling was more or less nonsensical[13].

Then I decided to calculate critical indices in the framework of the 1/*n* expansion. That decision was motivated by, first, to become independent of my previous thesis advisor, and, second, critical indices were supposed to be universal so that you did not need day-to-day contact with a laboratory. You were after a universal number. So I had first one student, then later more, and together with them, we calculated [some] Feynman graphs up to six loops, which is a pretty hard job I would say[14]. We did the calculation by hand, you understand. There were 75 different Feynman diagrams to evaluate. It took some sweat. We pushed it just to the second order in 1/n, but we had to calculate Feynman graphs going up to the six-loop level. We had to use all kinds of fancy regularization tricks: Mellin transforms[15], dimensional regularization[16], and also this trick—which I learned from Kurt Symanzik[17] and Giorgio [Parisi]—of conformal invariance integration. Conformal invariance imposes some structure on the propagators, and if you have an irreducible Feynman diagram, which doesn't have self-energy insertions and nothing like that, then you can reduce the number of loops by this trick. The trick was due to d'Eramo, Parisi and Peliti[18]**.**

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[12] Richard Allan Ferrell (1926-2005), Department of Physics, University of Maryland (2005). http://www.physics.umd.edu/announcements/ferrell.html (Consulted April 25, 2021.). Joseph Sucher, Douglas Scalapino and Richard Prange, "Obituary of Richard Alan Ferrell," *Phys. Today*, 18 May 2006 (2005). https://doi.org/10.1063/PT.4.2323

[13] I. Kondor and P. Szépfalusy, "Dynamics of the phase transition in the weakly interacting Bose gas," *Phys. Lett. A* **33** 311-312 (1970). https://doi.org/10.1016/0375-9601(70)90156-8; I. Kondor, P. Szépfalusy, "Dynamic critical exponent of a Bose system to $O(1/n)$," *Phys. Lett. A* **47**, 393-394 (1974). https://doi.org/10.1016/0375-9601(74)90143-1

[14] I. Kondor and T. Temesvari, "Critical indices to O $(1/n^2)$ for a three dimensional system with short range forces," *J. Physique Lett.* **39**, 99-101 (1978). https://doi.org/10.1051/jphyslet:0197800390809900; "Calculation of critical exponents to O $(1/n^2)$," *Phys. Rev. B* **21**, 260 (1980). https://doi.org/10.1103/PhysRevB.21.260

[15] Mellin transform: https://en.wikipedia.org/wiki/Mellin_transform

[16] Dimensional regularization: https://en.wikipedia.org/wiki/Dimensional_regularization

[17] Kurt Symanzik: https://en.wikipedia.org/wiki/Kurt_Symanzik

[18] M. d'Eramo, L. Peliti and G. Parisi, "Theoretical predictions for critical exponents at the λ-point of Bose liquids," *Lett. Nuovo Cimento* **2**, 878-880 (1971). https://doi.org/10.1007/BF02774121; Kurt Symanzik, "On

Kurt Symanzik took the pain to write me, by hand, a long letter, showing that one of the graphs which we evaluated could be [calculated] much [more simply] via this technique. It was a fantastic gesture. Just imagine, the guy was world famous, an authority, and I was a nobody, and he took the time and effort to write a 12-page-long handwritten letter explaining how the results which I derived can be obtained much more easily. That was impressive. In '78, I also met Giorgio the first time.

PC: Before we jump to that I wanted to first talk about your first crossing of the Iron Curtain, I think, which was to go to the ICTP in 1972[19]. How important was that experience for you scientifically and personally? And the corollary to this is how isolated was the theoretical physics community in Hungary before you were able to travel?

IK: [0:19:20] Well, the Iron Curtain started to have some holes in it. Some people could go to the US on a Ford scholarship[20], like my thesis advisor.

Also, there was a contact with Vienna University in high-energy physics. The people were allowed to go for one single day for a seminar in Vienna. In fact, the first time I crossed the Iron Curtain was on the occasion of such a seminar. So I spent one day in Vienna.

Then, there was a summer school in Kiljava, Finland[21], to the north of Helsinki, and [the Hungarian authorities] graciously allowed me to take part. That was in '71, but that was a short visit, ten days or something. I did not have any money at all. My main memory of that school is that I was starving. We were swimming in the lake, we were having these endless sauna sessions, but I could not buy one piece of bread or anything. Nevertheless, that was an interesting school. That was when I first heard about Kenneth Wilson's renormalization group papers. I have to tell you that those two Phys. Rev. papers were so convincing that everybody at the school was

Calculations in conformal invariant field theories," *Lett. Nuovo Cimento* **3**, 734-738 (1972). https://doi.org/10.1007/BF02824349

[19] John M. Ziman, "The Physics of Condensed Matter at Trieste," *IAEA Bulletin* **19**(1), 36-39 (1977). https://www.iaea.org/publications/magazines/bulletin/19-1/physics-condensed-matter-trieste (Consulted April 26, 2021.)

[20] See, *e.g.*, John Krige, "The Ford Foundation, European Physics and the Cold War," *Hist. Stud. Phys. Biol. Sci.* **29**, 333-361 (1999). https://doi.org/10.2307/27757813

[21] VIth International Summer School in Theoretical Physics, "Topics in the Physics of Condensed Matter", July 28-August 7, 1971, Kiljava, Finland. Proceedings: *Physica fennica: Collected Reprints* **8**(2-3) (1973). https://books.google.com/books?id=53csAAAAIAAJ

convinced that the guy would pick up a Nobel prize for them[22]. We all knew about the renormalization group issue, but it was like some sort of black magic. Wilson filled it up with content, with physical content and the physical picture behind it. All of a sudden everybody was able to understand the basic idea. With that knowledge, we could go back to the original papers, [and figure out that] those [old] guys must have understood perfectly what they were doing. It was their audience that did not understand. It took Wilson to make it a revelation. Anyways, that happened in Helsinki.

The next winter, at the beginning of '72, I was allowed to go to the ICTP for a longer period. I think it was a three- or four-month winter school that was directed by John Ziman[23] and Norman March[24]. It was on theoretical solid state. That's where, for example, I made the acquaintance of Jona-Lasinio[25]. Jona-Lasinio was a big name and he invited me to Padova—he was in Padova at that time—and I gave a seminar. We had a chat etc. That was again a sort of uplifting experience. I went to Rome University "La Sapienza". I visited Ferdinando de Pasquale**,** Carlo di Castro[26]. I did not meet Giorgio [Parisi], because [then] he was working at Frascati and he was very young. These were the first international contacts I had.

PC: You were mentioning your first meeting with Giorgio, and that you had already started to correspond with him ahead of time. Can you tell me about how you got to interact with Giorgio, and then to meet him?

IK: [0:24:25] There was a conference, in Hungary[27]. Although it was on field theory or something, which was outside of my immediate experience, I learned that Giorgio would come, and [so] I went to the conference in order to meet with him. By that time, I had learned from Kurt Symanzik that Giorgio was one of the authors of the d'Eramo-Parisi-Peliti paper and I wanted to have a chat with him. (By that time we had [calculated] all of

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[22] Kenneth G. Wilson, "Renormalization group and critical phenomena. I. Renormalization group and the Kadanoff scaling picture," *Phys. Rev. B* **4**, 3174 (1971). https://doi.org/10.1103/PhysRevB.4.3174; "Renormalization group and critical phenomena. II. Phase-Space Cell Analysis of Critical Behavior," *Phys. Rev. B* **4**, 3184 (1971). (Both published November 1, 1971)

[23] John Ziman: https://en.wikipedia.org/wiki/John_Ziman

[24] "In Memoriam: Norman H. March," *ICTP News Highlights*, November 13, 2020. https://www.ictp.it/about-ictp/media-centre/news/2020/11/in-memoriam-march.aspx (Consulted April 26, 2021.)

[25] Giovanni Jona Lasinio: https://en.wikipedia.org/wiki/Giovanni_Jona-Lasinio

[26] "Carlo di Castro Profile," *Accademia Nazionale dei Lincei*. https://www.lincei.it/it/content/di-castro-carlo (Consulted April 26, 2021)

[27] **IK**: The conference was held around 1978 at the Hungarian Academy of Sciences Mátraházai Akadémiai Tudós Üdülő [Mátraháza Academic Scholar Resort], and the main organizer was Julius Kuti, but this is all I can remember.

the Feynman diagrams [you need to the second order in $1/n$, but the conformal invariant integration technique offered a great simplification].) Giorgio turned out to be extremely open and very helpful. We had a long discussion in which he [taught] me all the secrets of this conformal invariant integration. That was the first meeting. The correspondence came much later, when I was in Frankfurt, and I was making some attempts to derive an alternative symmetry breaking scheme to his.

PC: You mentioned in your notes that you first heard about replica symmetry breaking from Robin Stinchcombe[28], at a MECO meeting in March '78[29]. Was that your first introduction to spin glasses as well?

IK: [0:26:06] That was the first time I even heard about them.

PC: Can you tell us a bit more about this? Was this an important talk? Or was this just one of many?

IK: [0:26:16] It was important from the point of view that he was talking about the Edwards-Anderson model, about the de Almeida-Thouless instability, and necessarily about replicas. To be frank, I found this idea of making the analytic continuation from a discrete set of integers to the reals, absurd. Of course, there are additional constraints under which the analytic continuation is unique. The sufficient condition is called Carlson's Theorem[30]. It tells us about the necessary structure of the set over which the function is defined. It has to have an accumulation point; the integers have an accumulation at infinity**.** [So that condition] was met. It also puts a limit on the type of divergence the function may have along this set. It should not be faster than exponential. Whereas in the case of replicas it was $\exp(n^2)$. So it violated the sufficient condition. On top [of that], there were necessary conditions, like monotonicity and convexity and that type of thing. Richard Palmer and van Hemmen did work on this[31]. So [the analytic continuation] seemed to me arbitrary. You just write up a formula, and you decide that the formula is valid on the reals, [for a] formula [that] is written up on the integers. I was not surprised at all to learn that it did not work,

[28] Robin B. Stinchcombe: https://academictree.org/physics/peopleinfo.php?pid=82545 (Consulted February 17, 2021).

[29] **IK:** In 1978, I went to a workshop in Boszkowe, Poland, where I heard about replicas and the Almeida-Thouless instability from Robin Stinchcombe. See: Robin Stinchcombe, "Theory of critical effects in random systems," Fifth International Seminar on Phase Transitions in Solids and Liquids (MECO), Boszkowe, Poland, 20-22 March, 1978. Middle European Cooperation (MECO) in Statistical Physics: https://en.wikipedia.org/wiki/Middle_European_Cooperation_in_Statistical_Physics

[30] Carlson's theorem: https://en.wikipedia.org/wiki/Carlson%27s_theorem

[31] J. L. van Hemmen and R. G. Palmer, "The replica method and solvable spin glass model," *J. Phys. A* **12**, 563 (1979). https://doi.org/10.1088/0305-4470/12/4/016

that there was this instability. This was the first experience of mine with spin glasses.

The second came in '79 at a Trieste conference, where Giorgio displayed a poster[32]. Now, that poster was a peculiar one. It was just two pages torn from a copy book with some scribbles on them. He was very late to submit [his application for participation] to the organizers. That's why he was given this "high" position of presenting a poster. In any case, I listened to his explanation, and my impression of replicas became even worse as a result. Evidently, I could not understand the considerations behind. As I may have written to you, later I was putting some moral pressure on Giorgio to write up the considerations that let him to that replica breaking scheme. Normally people regard it as a flash of intuition or something. But that's not quite true. He did have an array of experiences from high-energy physics to quantum chemistry. The replica trick also comes up in the theory of polymers, it comes up in the theory of cyclic molecules and that sort of things, and he used all that information in formulating this outlandish idea about the structure of replica symmetry breaking. Well, I did not get the point in '79. My first real engagement with spin glasses came in Frankfurt.

PC: Did that motivate you to start following the literature, or did that also come later?

IK: [0:31:20] No. I was doing percolation theory and all kinds of other disordered systems, but spin glasses I started to learn them in Frankfurt in '81[33].

PC: You moved to work in Saclay after your time in Frankfurt, and you started collaborating with Cirano De Dominicis. Did you know Cirano ahead of going to Saclay?

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[32] **IK:** Possibly the Sixth International Seminar on Phase Transitions and Critical Phenomena (MECO) 26 - 28 March 1979, Trieste, Italy. But there were so many conferences at ICTP that it's far from certain.

[33] **IK:** We started to have some political difficulties in Hungary. My then wife got involved in the organization of the fledgling democratic opposition in the country and by 1980 got close to getting arrested. I tried to pull her out of this situation and started to look for some employment abroad. In the fall of 1980, I got two offers, both arriving within a short period of time: one from Frankfurt University to work on some vaguely defined subject related to disordered systems, the other from the Service de Physique Theorique at Saclay. I accepted the first immediately, and agreed with the Saclay group that I would go to France after my Frankfurt contract expires. In Frankfurt I was to join Heinz Georg Schuster's group. Upon arrival I was presented with a contract explicitly demanding me to write a report on spin glasses, in particular the application of supersymmetry ideas and replicas to them. I did not have much of a choice there: I did not have enough money in my pocket even to take a taxi back to the airport. So I signed up and decided to learn spin glass theory on the expenses of the German taxpayer. I found Giorgio's way of looking at the question highly challenging and did some experiments with alternative schemes, only to fall back on Giorgio's solution as the only viable one. I even exchanged some letters with him on the subject. My report submitted to the German grant-giving body described these attempts, but it was not worth publishing. Heinz Georg Schuster: https://de.wikipedia.org/wiki/Heinz_Georg_Schuster

IK: [0:31:58] Yes. That again has a background story. In '78, I wanted to attend the Les Houches school on Ill[-condensed] Matter[34]. (This was the school which Bernard Derrida was mentioning in his interview[35].) It was a key event for the whole community. I submitted my application, which was accepted by Roger Balian[36], but at the last moment a special unit of the intelligence or some other services [in Hungary], suddenly discovered that this school was being sponsored by NATO. They came to me—it was a Saturday—and told me: "Comrade Kondor, we are loosened up already, but have not yet sunken so deep as to accept sponsorship from NATO." It triggered a fantastic scandal, because all the people who overlooked this NATO label, and who forwarded my application up along the chain, were very severely reprimanded. My boss at the Theoretical Physics Institute wanted to interview me, because this guy from the Minister of the Interior said that: "If the Hungarian State finds your participation in this school important, they will find the means to pay for your scholarship." My boss went to the deputy minister of education and said: "Ok, I think it's an important school, so please come up with the money." Even before telling my name, the guy was going: "I know, Les Houches, NATO, Kondor." This is the last association I wanted to be involved in. In any case, I was forbidden to go there.

In '79 there was a conference in Hungary, and Cirano was one of the participants[37]. I told him about this adventure of mine. He decided to help me. He produced a fake invitation to Saclay for the summer of 1980, so that I can go to Paris, take the flight from Orsay to Ajaccio, and take part in the Cargèse school[38]. You have to know he was a deeply convinced communist, but he was not particularly enthusiastic about the form of communism practiced in Eastern Europe, so that he was very kind of helping me to get into contact with the Saclay group. This is what I really wanted to do, because we were involved in this $1/n$ expansion, and Édouard Brézin and Shinobu Hikami were doing these $1/n$ expansions[39]. [Also,] all kinds of

[34] Les Houches, Session XXXI, July 3-August 18, 1978, Les Houches, France. R. Balian, R. Maynard and G. Toulouse, Eds., *La Matière mal condensée/Ill-Condensed Matter* (Amsterdam: North-Holland Publishing, 1979).

[35] P. Charbonneau, History of RSB Interview: Bernard Derrida, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 23 p. https://doi.org/10.34847/nkl.3e183b0o

[36] Roger Balian: https://en.wikipedia.org/wiki/Roger_Balian

[37] **IK:** The conference was held in Visegrad, Hungary, in 1979. Other details are unknown.

[38] 1980 Cargese Summer Institute: Phase Transitions, Status of the Experimental and Theoretical Situations, 16-31 July 1980. Proceedings: *Phase transitions : Cargèse 1980,* eds. Maurice Lévy and Jean-Claude Le Guillou and Jean Zinn-Justin (New York : Plenum Press, 1982).

[39] See, *e.g.*, S. Hikami and E. Brézin, "Large-order behaviour of the 1/N expansion in zero and one dimensions," *J. Phys. A* **12**, 759 (1979). https://doi.org/10.1088/0305-4470/12/6/006

other very high-level field theory work was coming out of Saclay. I regarded the Saclay group as the pinnacle of European science in that field, and therefore I was desperate to come into contact with them. Cirano helped me to do this. This was our previous contact.

PC: In 1982, you went to Saclay and you started to collaborate more explicitly with Cirano. Can you give us some scientific context for that project? And in what did your previous work prepare you to attack these questions?

IK: [0:37:03] My previous work was to learn the Parisi replica symmetry breaking scheme. Already in Frankfurt, I came to the conclusion that this must be where the solution lies for this whole riddle. I decided to make an effort to understand it in depth, so I recalculated the first four or five papers of Giorgio[40]. This was a non-trivial thing because some of them were Phys. Rev. Letters with some limited supporting information. Besides, some of the equations were wrong, infested with typos, except the last ones. This was a fantastic experience. You are trying to use those equations, which are being published, as stepping stones. But it turns out that the stepping stones are very slippery, and you find yourself in the water. By the time you fight your way up to the last one, it turns out that that was solid and trustable. This is the way Giorgio works. I'll recap later these papers.

I got fascinated, to say the least, by this sort of approach to theoretical physics. Instead of laying down the ground work, deciding which concepts, which rules of the game to use, you focus on the physical ideas you have to express and you create the tools for that. This I found immensely deep and interesting. Perhaps it's a hyperbole to bring here the case of the prediction of the positron, but this sort of gesture or this sort of approach to the problem which Giorgio used, reminded me of Paul Dirac's approach[41]. He had four positive numbers, he wanted to take the square root of that expression, such that the result is linear in the four things under the square root. He did not ask the question: "Whether this has a solution within the framework of usual complex numbers or whatever." He asked the question: "What is the minimum space wherein I can implement such a trick?"

I think this is the main difference between mathematics and great theoretical physics. We don't necessarily play the game correctly. You are subtracting infinitely large quantities and coming up with something which

[40] See, *e.g.*, G. Parisi, "Toward a mean field theory for spin glasses," *Phys. Lett. A* **73**, 203-205 (1979). https://doi.org/10.1016/0375-9601(79)90708-4; "Infinite number of order parameters for spin-glasses," *Phys. Rev. Lett.* **43**, 1754 (1979). https://doi.org/10.1103/PhysRevLett.43.1754; G. Parisi and G. Toulouse, "A Simple hypothesis for the spin glass phase of the infinite-ranged SK model," *J. Physique Lett.* **41**, 361-364 (1980). https://doi.org/10.1051/jphyslet:019800041015036100

[41] Positron: https://en.wikipedia.org/wiki/Positron

agrees with the laboratory experiment to 10 or 12 digits. Later, some [lesser mortals] work out the details and convince the rest of humanity that that trick was alright. I don't know if I can express myself properly, but I found this sort of approach to theoretical physics immensely exciting.

Surely, it was queer mathematics. I mean it's not really about the 0x0 matrices everybody was mentioning. To get there, you had to go through vector spaces of indefinite metric, where [distances] or the length of a vector can be negative. Anything that you have in your mind—the picture of geometry—gets completely upended. So I found it interesting and a challenge, and I decided that the next logical step would be to see if this instability was present or not. Cirano had the same conviction, so it was very natural for us to join forces.

PC: Do you know what drew Cirano himself to spin glasses? Did you discuss that with him?

IK: [0:42:47] He came to all this via dynamics. There was always a trend in spin glass physics that somehow replicas have to do with dynamics. André Blandin wrote a fairly cryptic paper in which he formulated this assumption[42], and it took another decade or even more [for] Jorge Kurchan and Leticia Cugliandolo to put up a dynamical theory and see how it could be mapped into replicas[43]. Back at the beginning of the '80s it was not clear at all, but Cirano was convinced that this whole phenomenon was of a dynamical nature, and therefore required a dynamical approach. So his first contributions to spin glass theory were dynamical contributions. Together with Henri Orland and [Marc] Gabay, they wrote a paper where they derived the Sompolinsky solution to the mean-field theory[44]. He was also trying to count the number of solutions of the TAP equation[45]. Well, he was very active. [In contrast] all that I knew was coming from Giorgio's solution and my own failed attempt to go beyond that, which I did back in Frankfurt.

So I arrived [to Saclay]. There was this very liberal and liberating experience that you were not told what to do, but you were [encouraged] to go

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

[43] See, *e.g.*, L. F. Cugliandolo and J. Kurchan, "Analytical solution of the off-equilibrium dynamics of a long-range spin-glass model," *Phys. Rev. Lett.* **71**, 173 (1993). https://doi.org/10.1103/PhysRevLett.71.173

[44] Haim Sompolinsky, "Time-dependent order parameters in spin-glasses," *Phys. Rev. Lett.* **47**, 935 (1981). https://doi.org/10.1103/PhysRevLett.47.935; C. De Dominicis, M. Gabay and H. Orland, "Replica derivation of Sompolinsky free energy functional for mean field spin glasses," *J. Physique Lett.* **42**, 523-526 (1981). https://doi.org/10.1051/jphyslet:019810042023052300

[45] Cirano De Dominicis, Marc Gabay, Thomas Garel and Henri Orland, "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

around and look for [a] collaboration. You cannot do this now, because now there are grants assigned to a leading scientist and the leading scientist is recruiting collaborators. He is underwriting the check, so if you sign up, you are signed up to a certain subject and a certain person. But back in those times the lab was funded by CEA, [so] they did not need to fight for grants, and they were top notch even without that competition. Therefore, I arrived and they told me: "Okay, you look around the corridor and you decide whom to join." This is how I decided to join Cirano, because by that time I was sure that the next important problem in spin glass theory was to check the stability of the Parisi solution.

PC: What was the reaction to that program by the burgeoning spin glass community at that point?

IK: [0:47:24] The first reaction was from the editor of Phys. Rev. Letters, who could not see the point and therefore bounced [the manuscript] back. It came out in the Rapid Communication[46], but Cirano was really mad about it. (Much less important works were published in Phys. Rev. Letters, [but] never mind.) Then there was the reaction of the people in Saclay. I gave a seminar in April, I think, that was just when we finished the stability analysis. Of course, nobody could understand the details, but the reception was extremely encouraging. You have people like Claude Itzykson[47], Édouard Brézin [and] Jean Zinn-Justin[48], people of this caliber sitting in the audience and at least pretending to appreciate your work. It was tremendously encouraging. Then, there was this Les Houches summer school directed by Jean-Bernard Zuber[49]. Although it was not on a subject in which I was involved in any degree, I asked him for his permission to go there in order to meet Giorgio. Zuber agreed. I went to Les Houches and had this long discussion in the chalet which Giorgio and family were renting there. That summer school took place after our finishing the work on stability and after my short visit to London. I went [there] to see Peter Young and David Sherrington at Imperial, and also Tony Leggett[50] at Brighton[51]. I was telling them about this effort of ours and the result of the stability analysis. I also gave a talk in Les Houches with Giorgio in the audience. So we had a meaningful interaction there.

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

[47] Claude Itzykson: https://en.wikipedia.org/wiki/Claude_Itzykson

[48] Jean Zinn-Justin: https://en.wikipedia.org/wiki/Jean_Zinn-Justin

[49] Les Houches, Session XXXIX, Développements récents en théorie des champs et mécanique statistique, 2 August-10 September 1982, Les Houches, France. Proceedings: Jean-Bernard Zuber and Raymond Stora, eds., *Recent Advances in Field Theory and Statistical Physics* (Amsterdam: North-Holland, 1984).

[50] Anthony James Leggett: https://en.wikipedia.org/wiki/Anthony_James_Leggett

[51] University of Sussex in Falmer, Brighton: https://en.wikipedia.org/wiki/University_of_Sussex

In London, I learned about Mackenzie and Peter Young's measurements of the distribution of barrier heights[52]. The importance of that measurement is the fact that this was the first explicit statement about the emergence of macroscopic barriers. Everybody knew there were barriers. You could not go to a spin glass seminar without people dutifully drawing a romantic landscape. This was commonplace. But Peter and Mackenzie attached some quantitative measure to that. The exponents they came up with were not good. Later, it turned out that the details were wrong. Nevertheless, the idea that those barriers are macroscopic was an important one. As far as I can recall, I was the first person to inform Giorgio about this. I did not fully understand the significance of this, but if you have macroscopic barriers, then it means that ergodicity is completely destroyed. The system is fragmented in ergodic components. This is what Giorgio was able to take out immediately, and this is what led him to the interpretation of $q(x)$. It may be ridiculous, nevertheless, to be the postman of that message, to convey that message was an important function. Giorgio acknowledged that much. There is this acknowledgement at the end of that paper[53].

Then I went back [to Saclay], did some other work. With Cirano we did the stability analysis of the Sompolinsky solution[54] and all kinds of other things. Then, at the end of the year, I returned to Budapest. Our collaboration with Cirano was kept up[55]. We were exchanging letters. Ordinary letters, delivered by post and written by hand. We started to derive not only the eigenvalues, but the eigenvectors, and the inverse of the stability matrix, so the propagators[56]. At that point, Cirano was using a technique which was completely unknown to me. It was so much unknown that I could not even

[52] N. D. Mackenzie and A. P. Young, "Lack of ergodicity in the infinite-range Ising spin-glass," *Phys. Rev. Lett.* **49**, 301 (1982). https://doi.org/10.1103/PhysRevLett.49.301

[53] G. Parisi, "Order Parameter for Spin-Glasses," *Phys. Rev. Lett.* **50**, 1946 (1983). https://doi.org/10.1103/PhysRevLett.50.1946

[54] I. Kondor and C. De Dominicis, "The spectrum of fluctuations around Sompolinsky's mean field solution for a spin glass," *J. Phys. A* **16**, L73 (1983). https://doi.org/10.1088/0305-4470/16/2/005

[55] **IK:** Another problem I worked on in one of my regular visits to Saclay was related to the analytic continuation in the replica number. This work was an answer to a question raised by Bernard Derrida, and has been given a rigorous treatment recently by Francesco Guerra. A further problem that came up in discussions over coffee at the Saclay lab was that of chaos in spin glasses. The idea was mentioned in a footnote in one of Giorgio's papers, and I also had a discussion about it with David Huse, but the first RSB-based treatment with a definitive value for the chaos exponents was given in a paper of mine. Chaos and long range correlations are entangled in a curious way in RSB. Long range correlations are also evidently related to what Newman and Stein describe as chaos in the system size. See: I. Kondor, "Parisi's mean-field solution for spin glasses as an analytic continuation in the replica number," *J. Phys. A* **16**, L127 (1983). https://doi.org/10.1088/0305-4470/16/4/006; I. Kondor, "On chaos in spin glasses," *J. Phys. A* **22**, L163 (1989). https://doi.org/10.1088/0305-4470/22/5/005

[56] C. De Dominicis and I. Kondor, "Gaussian propagators for the Ising spin glass below Tc," *J. Physique Lett.* **46**, 1037-1043 (1985). https://doi.org/10.1051/jphyslet:0198500460220103700

understand it fully from his letters. So I decided to take an alternative route and produced the same propagators via a technique that you would use in inverting any big matrix. The was a primitive and long route, whereas Cirano was able to find some shortcuts, which I was not able to follow[57].

But I would say that this was rather characteristic of our collaboration, all through the times. We were collaborating for 18 years, which is a lot of time. This was a very strange sort of collaboration. He was very quick and very prone to make mistakes. Small mistakes, factors of 2, but those are the most vicious ones, because you're not able to figure out where a factor 2 can have been lost. A factor of pi would be much better. He insisted upon doing these calculations on the blackboard. I really hated that, because if you do a calculation of such length on the blackboard and you make a small trivial mistake somewhere, there's no way you are able to go back and find the origin of the mistake. What we ended up with was a solution like this. Each morning we would meet. We would decide what to do during the day, agree on the notation, and go our separate ways. At the end of the afternoon we would compare notes. This is a very strange sort of collaboration. Nevertheless, in the given context, it might even have been the proper method because those equations were extremely complicated. Systems of 14 integral equations—that type of thing—and each of them so long that we could not use normal paper. [So] we were using the scrap paper that

[57] **IK:** I could see no established way to do this, so I took a large sheet of paper and wrote out the Hessian matrix for one-step replica symmetry breaking, and colored the different regions having the same matrix elements with the same color. I thought I would be able to memorize the structure and carry it around in my mind in the hope that the mysterious subconscious processes that are running in our brain would bring about the enlightenment. And indeed, the idea struck me while driving to Saclay from our home in Antony. From there, it was relatively straightforward to discover the whole structure. If the space of 0x0 matrices was the functions over the unit interval, the space of 0x0x0x0 tensors turned out to be the functions defined on the unit cube. The structure of the "replicon" subspace where the most dangerous instabilities were expected to show up was relatively easy. The most difficult subspace was what people termed the anomalous sector, for want of any better name. I take the opportunity to mention here an interesting fact we never published: the integral equations for the eigenvectors in the anomalous sector could be transformed into the differential equations of a loaded string. Strings are everywhere! In hindsight, it is clear that what seemed to be a task with no rational approach to it, could have been resolved by invoking the Wigner-Eckart theorems for finding the irreducible representations of the residual group. This residual symmetry is widely called ultrametricity now, but it is a known object in group theory; it is called a wreath product, and the Parisi scheme is an infinitely many times iterated wreath product. When we did the diagonalization of the Hessian, we exploited this symmetry, but we managed to describe it in such an opaque manner in our paper that nobody could see through it. The paper by Mézard, Parisi, Sourlas, Toulouse and Virasoro revealing the physical meaning of this symmetry came out a good year later than our stability analysis. The question of irreducible representations was later taken up by Claude Itzykson; untimely death prevented him from finishing the analysis which was then performed by Bantay and Zala. See: M. Mézard, G. Parisi, N. Sourlas, G. Toulouse, and M. Virasoro, "Nature of the Spin-Glass Phase," *Phys. Rev. Lett.* **52**, 1156 (1984). https://doi.org/10.1103/PhysRevLett.52.1156; P. Bantay and G. Zala, "Ultrametric matrices and representation theory," *J. Phys. A* **30**, 6811 (1997). https://doi.org/10.1088/0305-4470/30/19/019

was coming out from the line printers. (Do you remember that? Mike Moore was mentioning this in his interview[58].) We started to use those because most of the mistakes you make originate from turning the page, when copying the last equation for the next page. If you have a larger piece of paper you don't need to turn the page that much. Besides it was a [roll] so that you did not need to turn the page at all.

I remember when I arrived in Saclay, I had a chat with Bernard Derrida. He told me that: "You know that there are people in this lab quite able to do 50 pages of algebra without a mistake. This is a standard against which you have to measure yourself." It was bloody serious. If you picture Édouard Brézin or Jean Zinn-Justin, they are the people who are able to do a calculation without a mistake for 50 pages. It turned out that I was also able to do 50 pages without a mistake. Otherwise, we would have never come to the end of this stability analysis and of inverting the Hessian matrix. (I have this very objective measure of becoming more and more stupid with age, because the number of pages without a mistake I am able to do now is two. There is a quantitative measure. There's no argument about it. One is getting stupid.)

I also would like to mention that the general atmosphere in Saclay was both challenging and very friendly. There were people who were frightened by figures like Édouard Brézin or Claude Itzykson, or Zinn-Justin: extremely intelligent, extremely quick, very dedicated people. They can inspire awe in some people… For example, Elizabeth Gardner[59], she was not frightened, but she was an extremely shy person. This was the major factor for which she decided to change subject. She arrived in Saclay with non-Abelian gauge theories in the background[60], with the recommendation of David Wallace[61] from Edinburgh. In fact, it was very natural for her to join Claude Itzkyson, and yet she was looking for someone else. We [were] these foreigners, in this extremely high-level laboratory [where] everybody is cleverer than you are. [So] we got along very well, and she consulted me about this problem. She [wanted] to change subject, [but] she did not know whom to join. I suggested she join Bernard Derrida, because Bernard has the really fantastic ability of formulating a very simple-looking problem, which is at the same time very deep. I thought that a clever girl like Elizabeth could join such a person because the questions Bernard was

[58] P. Charbonneau, History of RSB Interview: Michael Moore, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 26 p. https://doi.org/10.34847/nkl.997eiv27
[59] Elizabeth Gardner: https://en.wikipedia.org/wiki/Elizabeth_Gardner_(physicist)
[60] E. J. Gardner, "The Schwinger-Dyson equation for the gluon propagator in 2+ 1 and in 3+ 1 dimensions," *J. Phys. G* **9**, 139 (1983). https://doi.org/10.1088/0305-4616/9/2/007
[61] David Wallace: https://en.wikipedia.org/wiki/David_Wallace_(physicist)

asking were almost elementary. Basically, you need to know the concept of free energy and temperature, more or less that was the introductory knowledge you needed. Yet he was able to grasp extremely complicated and deep problems. I found that very impressive, and I thought that Elizabeth would find a good source of information and inspiration. That became a very fruitful collaboration.

PC: Did you two stay in touch after you left Saclay, or through the years after that, or was this really just a moment?

IK: [1:02:44] With Elizabeth, no. When they turned to neural nets, the storage capacity of the perceptron and things like that, I decided I wanted to switch off because I thought that if we lose focus on this field theory problem, then it would be totally hopeless to make any progress at all. I really wanted to stay within this extremely sharply-defined subject. With Elizabeth… Everybody has this survivor's guilt vis-à-vis her. She died very young, and she was really brilliant, and an extremely kind person. I met her in London some years later, but by that time she was already ill—she never told [me] about it, but looking back it's clear that it was going on. What else can I say…

PC: I was going to take you up to Manchester, but if there's something else you wanted to say about Saclay…

IK: [1:04:26] Yes. I started to say that Saclay was very challenging and friendly. For example, they had Mehta[62]—of random matrix fame—there. He was an extremely interesting personality. Very friendly. And he enjoyed looking like a *clochard*. He was almost provoking the environment. For people who did not know him, to display their racist attitudes, and then shaming them by revealing that he was a professor and a world-famous scientist. I liked him very much, and I enjoyed his stories. Whenever he saw me in the afternoon, going along the corridor, he would come out: "Come! I have a nice problem for you." He would pull me in his office, write on the blackboard some Ramanujan[63]-like identity. "Look! Can you prove this?" I could not, but I took a copy and worked it out in the night at home, which was only fueling his next attempt for it. "Come on! I have another very nice identity for you."

Then, Jacques des Cloizeaux. I had an old car with one of its headlights broken and I had to change it. Jacques learned about this and said: "Ok. I have a workshop in my garage. Why don't you come over and we can fix it

[62] Madan Lal Mehta: https://en.wikipedia.org/wiki/Madan_Lal_Mehta
[63] Srinavasa Ramanujan: https://en.wikipedia.org/wiki/Srinivasa_Ramanujan

together?" It turned out to take four hours, leaving us all covered in dirt and oil, but the car was resuscitated. Then, we went up from the garage, and it turned out to be [almost like] a smallish castle with his ancestors' pictures on the wall, period furniture, and that sort of things. Jean-Michel Drouffe helped me tuning the car, because some stupid mechanic screwed up the spark plugs—the guy permuted them and the car was objecting—so that we had to retune. Displaying very friendly attitude and real acceptance. I liked being there. On my part, I was not afraid of their being too clever, because I always liked the company of people who were much cleverer than I am, so I enjoyed that atmosphere a lot.

And in '84, we went to Manchester for two years.

PC: So you spent two years in Manchester, where Bray and Moore were. I think you never worked with each other. Is it because you had diverging perspectives on the problem at this point already?

IK: [1:08:57] The whole motivation to go to Manchester came at a conference, in Heidelberg, on spin glasses[64]. Moore gave a talk on the cubic field theory of spin glasses[65]. I pointed out that: "Ok. At the next perturbation step you're going to have the whole thing blowing up into your face." He decided that this was a meaningful piece of critic, and he invited me right away. [That] tells a lot about him.

He mentions in his interview that Alan Bray and himself were diverted off this field theory approach, because they could see how complicated the propagators, which we derived, were. I was not aware that I had such a big impact on them. But it's sure that they started to push this droplet and scaling idea in '84[66], after my arrival.

The atmosphere at the Manchester department was again a very special one, and friendly in a totally different way. In Rome and in Saclay, people

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[64] Heidelberg Colloquium on Spin Glasses, 30 May-3 June, 1983, University of Heidelberg, Germany. J. L. van Hemmen and I. Morgenstern, eds. "Heidelberg Colloquium on Spin Glasses," *Lecture Notes in Physics* **192**.
https://doi.org/10.1007/3-540-12872-7

[65] A. 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 **IK**: The conference was of Ref. 64, but Mike's talk is not included in the proceedings. Perhaps he did not submit it for publication, because the material it was based on had already been published in 1979. In that early paper, with Alan Bray, they derived a φ^3field theory for spin glasses. In Heidelberg, he presented results derived from it at the first order in perturbation theory. At that order there were a number of massless modes, and it was clear to me that those masses would all go negative at the second order, precisely due to the de Almeida-Thouless instability.

[66] See, *e.g.*, A. J. Bray and M. A. Moore, "Nonanalytic magnetic field dependence of the magnetisation in spin glasses," *J. Phys.* **17**, L613 (1984). https://doi.org/10.1088/0022-3719/17/23/005

were fighting over coffee about political issues. At a British university—at least at that time, maybe Brexit has changed that—you would not touch anything political. You were a bore if you were raising these questions. It took me almost half a year to figure out the probable position of the different members of the department on these issues, because there was this coded exchange of ideas, remarks and things. This was a high art, in a way. The whole department was having lunch together—we had these miserable sandwiches. We were meeting regularly each day, for lunch and usually for tea each afternoon. So I came to know all the people in the department. They were using some small personal story, some remark on the news on the tv, that type of chitchat, with an undercurrent of a conviction about larger things, but never ever going into details on those. That I found very remarkable in its own right.

As you say, we never worked together, but we were discussing a lot. I was trying to point out that long-range correlations in spin glasses, which were a fact—everybody was convinced about this—should make people extremely cautious about choosing boundary conditions. Rather than just imposing all plus on one end, all minus at the other, and looking at this torsion exponent—or whatever it's called—this way, on top in a very small system, 8^3.

My impression was that these are much more complicated issues and the presence of these long-range correlations is a major enigma or a big huge question mark in my mind ever since. You have a system with discrete symmetry, Z_2, and that system somehow turns out to sustain soft modes, Goldstone modes. How do you understand this? What's happening to the spins when these sort of Goldstone modes are propagating across the system? You have a very clear idea what's happening to the spins in a Heisenberg spin system**.** Slight tuning of the nearby spins. But in an Ising spin glass you cannot do that. You either flip or not. This is something [about] which we were having a lot of discussions, that there were all these Monte Carlo works, especially at the beginning of the scaling approach, or droplet picture, whether it was justified or not.

Also, John Ziman—the one who gave me this advice in Trieste: "You can build a nice career on glasses.[67]"—had this funny little journal called *Science Progress*. He was the chief editor and he wanted to publish a baby

[67] **IK:** My first encounter with the glass problem occurred in 1971. On an industrial commission, I wrote a report on phase transition in silicates. I observed that these phase transitions were invariably accompanied by a strong distortion of the elementary cell, with the oxygen atoms shared by two neighboring SiO_4 tetrahedra apparently having two possible positions and tunneling between them. In silicate glasses that have a random molecular structure this meant a collection of random two-level oscillators that would then determine the low temperature properties of glasses. In 1972, I was allowed to leave Hungary for

introduction to spin glasses. First, he asked Mike Moore, who did not feel like writing baby introductions, so that Ziman came to me and I did write it[68]. The motto of Ziman was: "Write it in such a way that a biologist will understand it," so spin glasses for biologists. This paper [had] an interesting fate, because everybody in the subsequent years who was taking on a completely green student, with no prior knowledge of spin glasses, assigned this paper to them as a first [read]. So somehow it became a very popular paper from America to Persia.

In a way, I liked England a lot. (I have to tell you that I liked Italy and I liked France, Germany and England as well. I did not like Hungary a lot.) We were even hesitating about staying on. Then, there was this consideration that we wanted our son to go to a Hungarian high school. If we wanted him to establish friendships—the type of friendships which you establish as an adolescent—then we have to move back. (As it happens, my son is a professor in Chicago[69], and all his friends are scattered over the world.) So we moved back in '86.

PC: You mentioned that you've kept on collaborating with Cirano over the years, and you brought in also your first student in that collaboration, Tamás Temesvári, over the years. You've already discussed how Cirano and you were complementary in your approaches. What was Tamás' synergies in this work?

IK: [1:19:00] Tamás is a person who checks everything a dozen times and he wants to make everything absolutely sure. It's on the level of an obsession. Not only being conscientious, but really someone dedicated to the last detail. If you start your research by evaluating 75 Feynman graphs going up to six loops, then you have to have this sort of capacity[70]. These [spin glass]

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the first time and participate in a Winter College organized by ICTP, Trieste. I had the opportunity to talk about these ideas to John Ziman, the director of the Winter College (see Ref. 19). He gave a very definitive advice: "Forget condensed Bosons and pursue this glass problem. You can build a life career on it!" Having returned to Hungary, I could not find either a theoretical or experimental group with whom I could cooperate on glasses. The typical reaction I got was: "This is not a physics problem, this is engineering." The paper by Anderson, Halperin and Varma on the low temperature behavior of glasses, as determined by two-level units of unspecified nature, appeared in the second half of that year. I regard this episode the greatest missed opportunity in my scientific career. See: P. W. Anderson, B. I. Halperin and C. M. Varma, "Anomalous low-temperature thermal properties of glasses and spin glasses," *Philo. Mag.* **25**, 1-9 (1972). https://doi.org/10.1080/14786437208229210

[68] I. Kondor, "An introduction to the theory of spin glasses," *Sci. Prog. Oxf.* **71,** 145-180 (1987). https://www.jstor.org/stable/43420673

[69] Risi Kondor: http://people.cs.uchicago.edu/~risi/ (Consulted April 26, 2021.)

[70] Temesvári Tamás, *Kritikus exponensek számolása térelméleti módszerekkel* [Calculation of critical exponents by field theory methods], Diploma Thesis, Eötvös Lorand University (1977).

propagators were given in an implicit form: definitions embedded into definitions and whatever, taking up three printed pages. In order to derive the long wavelength behavior for the different cases, it took a fantastic effort and a large part of that effort was due to Tamás.

Those efforts provided an understanding of the meaning of those propagators. Propagators are correlation functions and overlaps of correlation functions. You can have long-range overlaps—power-law-like overlaps—between valleys which are not very closely related to each other, so that the Parisi overlap between the spin configurations is small, and yet the propagators overlap between those two valleys are long-range. How can you understand such a thing? This violent infrared divergence is coming up even between valleys which have zero overlaps in the magnetization pattern?

I can only offer my own understanding of this. If you consider two independent pieces of literary work in the same language, you translate them into binary code or whatever and you calculate the overlap. Chances are the overlap will be small, because they are two independent pieces of writing. But if you calculate the correlation functions inside both and look at the overlap between these correlation functions, they must have a long-range overlap because those texts are in the same language. The structure of the language will come across in a high degree of overlap between the correlation functions, but not between the patterns.

This sort of picture motivated me when I was working on the chaos problem[71]. There again, you switch on a small external field and the magnetic pattern goes away immediately and will have zero overlap with the original one. Yet if you calculate the overlap of the correlation functions it turns out to be long-range. This is again the same with changing the temperature and other things which Newman and Stein pointed out—the chaos in volume, chaos in the number of particles.

Tamás came in with this sort of dedication to detail. In fact, the collaboration between Tamás and Cirano survived my collaboration with Cirano. I got involved in administration-type of work at the beginning of the ‘90s, building up new institutions, new department and things like that. Lot of waste of time, I have to admit. Tamás was clever enough to refrain from this sort of activity. Later, I went to work for a bank, so that was a completely different type of adventure again. Still, spin glasses followed me there too.

[71] See, *e.g.*, I. Kondor, “On chaos in spin glasses,” *J. Phys. A* **22**, L163 (1989). https://doi.org/10.1088/0305-4470/22/5/005

PC: We'll get to that. Before, as you mentioned, you had a field-theory program. This was what you pushed for a decade. What was the reaction of the community to the field-theory approach? (This was also Cirano's program. You were jointly involved in this.)

IK: [1:25:38] People were looking at it with some reservation, and the type of respect that you earn if you are mad. Édouard Brézin compared this whole effort to pyramid building, if you wish. It was not completely fruitless. Nobody really knows whether this type of hierarchical organization in phase space is present in physical dimensions or not. I tend to believe—together with the Rome group—that it's there. Not necessarily with very high barriers, but high enough to act as impenetrable at the levels of temperatures we are talking about. So I believe that this organization is there. And [assuming] this ultrametric organization is there, we were able to derive a Ward identity[72] from that, a Ward identity valid to any order in perturbation theory. I think this is a result[73]. This is something that at least should have an impression on people that the structure itself implies these long-range correlations.

In the meantime, I came to be convinced that long-range correlations are an integral part of any complex system. If there is any meaning I am able to assign to the slogan that a complex system is more than the sum of its parts, then it must mean that correlations are not finite-range. Otherwise the system would be able to be cut into finite sizes and it would not be more than the sum of its parts. I think that this whole spin glass story has an importance far beyond copper-manganese or any laboratory spin glass. It's a framework within which to think about complex systems and their long-range correlations. This is why I would like to understand [how] these long-range correlations emerge in the case of a discrete system. I think that this way of looking at problems has remained with me, independently of the context of spin glasses. The last paper we published together with the Vienna group—Stefan Thurner and company[74]—was about the fragmentation of society into these opinion bubbles, or whatever they are, and a possible mechanism which gives rise to these structures and again creating

[72] Ward-Takahashi Identity: https://en.wikipedia.org/wiki/Ward%E2%80%93Takahashi_identity

[73] I. Kondor and C. De Dominicis: Ultrametricity and zero modes in the short-range Ising spin glass, *Europhys. Lett*. **2**, 617 (1986), C. De Dominicis, T. Temesvari and I. Kondor, "On Ward-Takahashi identities for the Parisi spin glass," *J. Physique IV* **8**, Pr6-13 (1998). https://doi.org/10.1051/jp4:1998602

[74] T. Minh Pham, I. Kondor, R. Hanel and S. Thurner, "The effect of social balance on social fragmentation," *J. Roy. Soc. Interf.* **17**, 20200752 (2020). https://doi.org/10.1098/rsif.2020.0752

these landscapes and creating a lot of tragic consequences, as we could witness on the 6th of January[75].

PC: I'd like to draw you back still a bit in the past. In the mid-'90s, in July 1996, you organized a summer school, in Budapest, on Monte Carlo techniques[76]. As far as I know from your published work, you've never been involved in Monte Carlo simulations. Were you paying close attention to them? Was this something that you were involved at a distance?

IK: [1:30:21] I realized the importance of the approach. I was convinced about the very fruitful nature of this approach and its absolute necessity. But I also shared Sam Edwards[77]' or Mike Moore's conviction that without knowing the answer, you are in a hopeless situation if you just have numerical work at your disposal. Lev Landau[78] also had a maxim on this—apart from Monte Carlo: "Never start to try to crack a problem without knowing the answer[79]." Beyond jokes, it is absolutely important that you have a very good conceptual grasp of the problem. But in order to flush it out, to make it convincing for others, you need to do either laboratory work or numerical experiments. This is why I decided to organize that school.

There were extremely good people giving lectures there: Werner Krauth, Enzo Marinari and others. It turned out to be a program worth putting money on. At that point it was easy for me to organize these things, because I was the director of this college[80]. I had my own budget and I had the infrastructure. So it was natural to embark on this task.

PC: In notes that you sent us, you mentioned that your first got interested in structural glasses in the early '70s[81]. In the late '80s, there was a branch of spin glass ideas that moved into the structural glass world, through the work of Kirkpatrick, Thirumalai, and Wolynes[82]. Did you pay attention to this? Did that in any way rekindled your interest in structural glasses?

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[75] 2021 storming of the United States Capitol: https://en.wikipedia.org/wiki/2021_storming_of_the_United_States_Capitol

[76] Advances in Computer Simulation, Eötvös Summer School, Budapest, Hungary, 16-20 July 1996. Proceedings: Janos Kertesz and Imre Kondor, eds., "Advances in Computer Simulation," *Lecture Notes in Physics*, **501** (1998). https://doi.org/10.1007/BFb0105456

[77] Sam Edwards: https://en.wikipedia.org/wiki/Sam_Edwards_(physicist)

[78] Lev Landau: https://en.wikipedia.org/wiki/Lev_Landau

[79] **IK:** This citation may be apocryphal, and heard from a Soviet colleague.

[80] ELTE Bolyai Kollégium: https://hu.wikipedia.org/wiki/ELTE_Bolyai_Koll%C3%A9gium

[81] See Ref. 67.

[82] See, *e.g.*, T. R. Kirkpatrick and P. G. Wolynes, "Stable and metastable states in mean-field Potts and structural glasses" *Phys. Rev. B* **36**, 8552 (1987). https://doi.org/10.1103/PhysRevB.36.8552; T. R. Kirkpatrick and D. Thirumalai, "Mean-field soft-spin Potts glass model: Statics and dynamics," *Phys. Rev. B* **37**,

IK: [1:33:05] I took a very distant interest. I appreciated the work. I couldn't quite understand why one-step replica symmetry breaking was the same as mode-coupling [theory], so I was lost at a relatively early stage. Also, my own cursory meeting with the glass problem was about real glasses, like the one in the window, silicate glasses. That is a different problem from this fragile object which people treat with Gardner's transition and the fragmentation of phase space. This is a different issue. In a way, the real glass problem may be trivial, or may not be trivial but is less understood than these fragile glasses.

PC: You've mentioned already in a couple of occasions that you moved into a more economics or econophysics direction in the late '90s[83], and that spin glasses stayed with you[84]. How influential have spin glasses ideas remained in your research and in your life, since you've left the explicit study of spin glasses?

IK: [1:34:53] First, I realized that when I got involved in regulation, because I was elected to be the chairman of the Hungarian Association of Risk Managers[85], and in such capacity I got involved in the implementation of the European regulation in Hungary. I had to study the different national implementations of those European rules in different countries like Germany, England, France. These are non-trivial things. The body of the regulation was something like 40 pages of legalese, but the interpretation issued by the Bank of England was a big volume of 800 pages, explaining all the different aspects of that regulation. To equip myself with that knowledge, I was negotiating with the national bank, the bank supervision, the treasury in the country, people who did have a deep knowledge of all these legal arrangements.

What I could point out is that some of that regulation implied finding the ground state of a spin glass. It's both non-trivial and trivial, because spin-glass-like problems arise whenever you have a built-in contradiction into

5342 (1988). https://doi.org/10.1103/PhysRevB.37.5342; D. Thirumalai and T. R. Kirkpatrick, "Mean-field Potts glass model: Initial-condition effects on dynamics and properties of metastable states," *Phys. Rev. B* **38**, 4881 (1988). https://doi.org/10.1103/PhysRevB.38.4881

[83] *Econophysics: An Emergent Science, Proceedings of the 1st Workshop on Econophysics, Budapest, 1997*, eds. J. Kertész and I. Kondor (2002). http://newton.phy.bme.hu/~kullmann/Egyetem/konyv.html (Consulted April 27, 2021.) See also: Adrian Cho, "Econophysics: Still Controversial After All These Years," *Science* **325**(5939), 408 (2009). https://doi.org/10.1126/science.325_408

[84] See, *e.g.*, A. Gábor and I. Kondor, "Portfolios with nonlinear constraints and spin glasses," *Physica A* **274**, 222-228 (1999). https://doi.org/10.1016/S0378-4371(99)00387-8; I. Kondor, "Spin glasses in the trading book," *Int. J. Theor. Appl. Finance* **3**, 537-540 (2000). https://doi.org/10.1142/S0219024900000516

[85] Professional Risk Managers' International Association (PRMIA) Hungary Chapter.

the structure of a problem. If you have to optimize a portfolio [under non-linear constraints], and there are elements that are positively correlated and there are some other elements that are negatively correlated, essentially you are dealing with a spin glass problem.

Not only that, but in some other points I was able to detect built-in inconsistencies. So a regulation that has its purpose to avoid regulatory arbitrage, gave the chance to banks to game the regulation. In the effort to avoid this, they are building in by-laws and other little rules which lead to a regulatory arbitrage, to the breakdown of convexity and that sort of thing. I made some friends in different central banks in Europe and also in the US, at the Federal Reserve, and I was telling them about that. At a certain point I was even given an audience by the chairman of the banking supervision of the Senate[86], and I explained them that there is this piece of international regulation that is inspiring banks to have regulatory arbitrage, and that these pieces should be avoided in the next edition of these same rules. [The senator] looked at me and said: "My friend, financial regulation is like sausage making. Once you've seen how it is done, you would not touch the subject." That was the achievement I was able to [reach] in that field.

Nevertheless, I had this very interesting experiences with these people. Those people are not stupid at all. Those officials working for the Federal Reserve in Washington are absolutely brilliant people. Very well-equipped in mathematics and first-rate. But they are working under suffocating political constraints. The outcome is a sausage. You prepare the legislation to the best of your knowledge to be consistent and without internal contradictions etc. Then that goes in front of the decision makers, and the decision makers are representing different interests—different national interests or business interests etc.—and they are starting to screw up your regulation. However nicely you thought it out, you end up with something which is full of internal contradictions. This is what I learned.

PC: Cirano co-authored a book on random fields and spin glasses in the early 2000s[87]. At that point you had stopped collaborating because you had taken up this new branch of work. Was it a consideration for you to write this book with him?

[86] United States Senate Committee on Banking, Housing, and Urban Affairs: https://en.wikipedia.org/wiki/United_States_Senate_Committee_on_Banking,_Housing,_and_Urban_Affairs

[87] C. De Dominicis and I. Giardina, *Random Fields and Spin Glasses: A Field Theory Approach* (Cambridge: Cambridge University Press, 2006).

IK: [1:41:43] No. It had a kind of personal aspect to it. As I told you, Cirano was a deeply convinced leftist. He regarded banks in the spirit of Bertolt Brecht: establishing a bank is worse than robbing a bank[88]. He regarded my signing up with the bank as some sort of a treason. I was selling myself out in leaving science behind. Emotionally I could understand that, but of course I was not happy, especially because all my efforts both in the bank and outside the bank were focused on making the bloody system more stable. At the bank, I was in infinite fights with colleagues whose bonuses I was reducing by my objection to their grand ideas. I had veto power and I was using it. Everybody got a smaller envelope as a result. You can imagine that this is a conflict-laden position. In fact, I paid with some heart troubles etc. But Cirano was not able to see it in that light. [For him] this was just daylight robbing.

PC: During your time as an academic and elsewhere, did you ever get to teach (part of) a course on replica symmetry breaking or the replica trick? If yes, could you please detail?

IK: [1:44:11] Yes. I started doing that in '98 or [around that time]. I kept that course running up until the end of my employment with Eötvös University, in 2011. That was a special course, optional for the students, but you could earn credit for it. It was part of the curriculum, but it was elective. You may [wonder] what my experience was with students in those courses. They regarded it as some kind of exotic excursion into something which was half crazy, half challenging. But I did have students to whom I could teach how to apply replicas in a creative manner. So I had serious students [as well]. My description of the situation is such. Of course, the replica approach is not ordinary mathematics. There's no doubt about it. Nevertheless, you can formulate it in such a way that it sounds rational if you are willing to give up your addiction to mathematical axioms, and if you are able to somehow liberate yourself and have a fantasy or whatever, then it can even be amusing in a way. I have a number of students who were and are using this technique ever since. Some of them working for MSCI[89]. Another one working for the European Central Bank. So replicas are spreading over finance.

PC: Is there anything else you'd like to share with us about this epoch, about this era, that we might have missed along the way?

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[88] "What's breaking into a bank compared with founding a bank?" Source: Bertolt Brecht, *The Threepenny Opera* (London: Methuen Drama, 1979), 76. https://doi.org/10.5040/9781408163122.00000007; The Threepenny Opera: https://en.wikipedia.org/wiki/The_Threepenny_Opera

[89] MSCI Inc.: https://en.wikipedia.org/wiki/MSCI

IK: [1:47:24] Well, I think it is a remarkable episode in the history of statistical physics, because of the reasons I was trying to describe: this extremely imaginative and creative use of mathematics, and this attitude to mathematics. From this point of view it is [an entirely different issue] that, remarkably, Michel Talagrand[90] and Francesco Guerra were able to derive the results within the framework of consolidated, real probability theory. If you look back at the story of statistical physics, as it started with Ludwig Boltzmann[91], ergodicity was at the core. It was the first major stumbling block. It was a highly non-trivial problem that gave rise to a chapter in mathematics etc. Compared with the importance of the ergodicity question, it is already remarkable that [it was possible to insert] phase transitions into the framework of statistical physics. You have broken ergodicity in a phase transition. People figured out they are able to mimic the ergodic process by adding this infinitesimal external field etc. That is very remarkable. Now, you take one more step and you find non-ergodicity in an extremely complicated pattern, in a situation where you do not have this crutch of the ordering field. There is no external field that would mimic [the spin glass order]. Even if there were [such a field] at zero temperature, it would not be much of an advantage because of this chaos property. The fact that statistical physics was able to survive these challenges I find to be extremely remarkable. It's a fantastic advance along these lines.

PC: Did you follow the work of Guerra and Talagrand—the mathematical physics and the probabilistic work—as it was happening or is this a retrospective view?

IK: [1:50:43] I think I'm able to understand what Guerra did. Michel Talagrand is way above my head. Although I am using replicas now[92]—I am even using replica symmetry breaking occasionally—I never had this urge to build it on the solid basis of [mathematics]. I am quite happy with the heuristic use of it. I always have people who are able to run Monte Carlo simulations and find the numerical evidence, so I could just claim that I had a dream and this is the formula I dreamed up, forgetting about replicas. [Yet] those formulae are extremely non-trivial. In the finance context you may have six or eight order parameters, so it's a non-trivial structure. Nevertheless,

[90] Michel Talagrand: https://en.wikipedia.org/wiki/Michel_Talagrand

[91] J. Uffink, "Boltzmann's Work in Statistical Physics," in E. N. Zalta, ed., *The Stanford Encyclopedia of Philosophy* (Spring 2017) https://plato.stanford.edu/archives/spr2017/entries/statphys-Boltzmann/

[92] See, *e.g.*, F. Caccioli, S. Still, M. Marsili and I. Kondor, "Optimal liquidation strategies regularize portfolio selection," *Eur. J. Finance* **19**, 554-571 (2013). https://doi.org/10.1080/1351847X.2011.601661; **IK**: This example is replica symmetric. The example with RSB has not been completed. It is related to margin accounts, and the structure of the problem is similar to what Franz and Parisi found in the problem of the perceptron with a negative margin. See: S. Franz and G. Parisi. "The simplest model of jamming." *J. Phys. A* **49**, 145001 (2016). https://doi.org/10.1088/1751-8113/49/14/145001

the replicas lead you to these non-trivial predictions, which are borne out by simulations, and in some cases even by exact mathematics.

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

IK: [1:52:40] I'm pretty sure that we do not have too many documents left. We moved house more than once, and whatever I have left is in the garage, in folders which number something like 150 and which are collecting dust. What I can promise is that if I find something, such as the letters from Cirano…

PC: Or with Giorgio, or your correspondence from the '70s.

IK: [1:53:33] Sure. It also would be a great tribute to [Cirano's] memory. A great person he was. So if I find any such document, I will be happy to deposit it. Right at the moment, I do have some hope, but no tangible document.

PC: Thank you very much for your time and for this conversation.

IK: It was a pleasure. Listening to some of the previous interviews, I came to see how important these are. Not my contribution, but [those of] people like Bernard Derrida or Édouard Brézin, or Thouless, or Mike Moore. These are important players, and it's an interesting story how this replica symmetry breaking thing came about, and how it went on to inseminate countless different disciplines. If you manage to catch Giorgio, please try to get him to give an account of how on earth he got this idea.

PC: We will. Thank you.