History of RSB Interview: David Gross

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Location:
Over Zoom, from Prof. Gross' home in Santa Barbara, California, USA.

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PC: Professor Gross, thanks for joining us. We will be discussing the history of a replica symmetry breaking in physics, which me roughly bound from 1975 to 1995. But before we get to that we have a few questions leading up to this to enrich your biographical context. In your Nobel biography[1], you explain why you ended up at Berkeley but not really what drew you to particle physics. Was it then you only scientific interest in physics, or were there other subfields, such a solid state physics, that were on your radar?

DG: [0:00:43] No. At the time, I was interested in fundamental questions in physics. At that time—so different from today's time—particle physics was clearly the frontier. My exposure to solid state physics was minimal. The courses I took as an undergraduate—and as a graduate, by the way—were pretty bad and not very interesting, I must say. Band theory[2]… It didn't seem at all exciting. For many years, I showed absolutely no interest in what is now called condensed matter physics. That was reinforced by the culture at the time, certainly at Berkeley but just about everywhere. Berkeley was at that point the center of high energy physics. It had the largest accelerator in the world for the time I was there. There were experimental discoveries all the time. New particles! It was incredibly exciting, so there was really no question. I never even considered condensed matter physics. It was partly, as I said, the culture at the time. Solid state physics was referred to by Murray Gell-Mann[3], who was one of the leaders of the of the field at the time, as squalid state physics[4]. That

[1] D. J. Gross, "David J. Gross – Biographical," *The Nobel Foundation* (2004). https://www.nobelprize.org/prizes/physics/2004/gross/biographical/ (Consulted October 15, 2022.)
[2] Electronic band structure: https://en.wikipedia.org/wiki/Electronic_band_structure
[3] Murray Gell-Mann: https://en.wikipedia.org/wiki/Murray_Gell-Mann
[4] See, *e.g.*, Talk delivered at the Third International Symposium on the History of Particle Physics, June 26, 1992. D. J. Gross, "Asymptotic Freedom and the Emergence of QCD," arXiv:hep-ph/9210207 (1992).

seemed to be the general consensus among those who were regarded as the elite theorists. This was historically a great mistake, but it was rectified by the end of the '60s, mostly due to Ken Wilson's work[5], which really formed a bridge between quantum field theory and condensed matter physics [with] the renormalization group [for] phase transitions. Before that, I paid absolutely no attention.

PC: In your Nobel lecture[6], you mentioned that the work of Giorgio Parisi[7] on deep inelastic scattering was helpful in showing that there exist no asymptotically free field theories. Did you know Parisi at the time? Had you crossed paths?

DG: [0:03:36] Yes, indeed. I'm trying to remember when I first met Parisi. (I discussed this recently with Giorgio.) Giorgio was a postdoc at some point in Columbia. I think it might have been a bit later. I'm not exactly sure, but it was the early '70s maybe '70-'71. Certainly in '72-'73, we overlapped. He was in New York[8]; I was in Princeton. I used to visit Rockefeller. He visited Rockefeller, he visited Princeton. So, I got to know him in the early '70s. The work that you're referring to[9]… Parisi is an extremely broad, creative theorist. He has worked in many fields. At that time, I think he was mostly interested in high energy physics.

I had, at that point, a detailed plan about how to deal with the scaling observed in deep inelastic scattering, which seemed extremely paradoxical. My plan, which I think was discussed in that Nobel lecture, was essentially to explore whether quantum field theory could possibly explain this. The strategy was… At that time, it seemed that an obvious explanation of this scaling phenomena, or free field-ish phenomena as you went to higher and higher energies, was what we called later an asymptotically free field theory, where the coupling gets weaker and weaker as you probe phenomena at shorter and shorter distances, or higher energies. My plan was [first] to show that that was necessary in quantum field theory, that in order to explain scaling, you really have to have asymptotic freedom. There couldn't be some kind of coincidence, or

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https://doi.org/10.48550/arXiv.hep-ph/9210207

[5] Kenneth G. Wilson: https://en.wikipedia.org/wiki/Kenneth_G._Wilson

[6] D. J. Gross, "Nobel Lecture," *The Nobel Foundation* (2004). https://www.nobelprize.org/prizes/physics/2004/gross/lecture/ (Consulted October 15, 2022.)

[7] P. Charbonneau and F. Zamponi, *History of RSB Interview: Giorgio Parisi*, transcript of an oral history conducted 2021 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2022, 80 p. https://doi.org/10.34847/nkl.7fb7b5zw

[8] Giorgio Parisi spent the academic year 1973-1974 at Columbia University, in New York City.

[9] G. Parisi, "Deep inelastic scattering in a field theory with computable large-momenta behaviour," *Lett. Nuovo Cimento* **7**, 84–88 (1973). https://doi.org/10.1007/BF02728276

some kind of non-trivial fixed point that would exhibit the free field scaling that was observed. Then, the other part of the plan was to explore all possible… If that was the case, one could explore any quantum field theory for the vicinity of weak coupling, which was required in the ultraviolet and perturbatively explore whether you were driven towards weak coupling or away from week coupling. So that was the plan.

Part of the plan, which I regarded as the hardest part, was to really show that asymptotic freedom was a necessary condition, if you accepted the data as indicating free field behavior at SLAC. There weren't many people who believed in the SLAC data. I mean Ken Wilson, who was in a better position than anyone perhaps to understand the implications, was of the opinion that the data wasn't conclusive, you can't believe it. It's true the data was pretty crummy at the time. The energies were remarkably low to expect anything dramatic and the error bars were very big. He sorted of dismissed it because it didn't fall into his general philosophy, which I must say I was never that convinced by.

I forget exactly how it went, but Curt Callan[10], who I was working with on this project exploring what was necessary to explain the kind of SLAC results, and I were aware of this paper of Parisi, where he examined in a special case the behavior of what we called the anomalous dimensions, or the scaling behavior of operators in the operator part of the expansion that contributed to deep inelastic scattering[11]. What we did was to generalize that result and apply it to just about all quantum field theories—with some exceptions—to give an argument based on a kind of analysis that Parisi had done for a very special case, and that one of its consequences was that the theory had to have an ultraviolet fixed point at the origin, and that implied asymptotic freedom. We proved that for all quantum field theories with the exception of non-Abelian gauge theories, which in '71-'72 were rather new and unexplored. Our proof broke down for such things. We now understand that a lot better, I should say.

So that an important ingredient. It really completed part of my program, which was to show that asymptotic freedom was necessary in quantum field theory to explain deep inelastic scattering. The other part of the [program] was to explore all possible quantum field theories and see if any are asymptotically free. The simplest theories one could write down, one could easily show were not asymptotically free, notoriously quantum

[10] Curtis Callan: https://en.wikipedia.org/wiki/Curtis_Callan

[11] See, *e.g.*, C. G. Callan Jr and D. J. Gross, "Bjorken scaling in quantum field theory," *Phys. Rev. D* **8**, 4383 (1973). https://doi.org/10.1103/PhysRevD.8.4383

electrodynamics[12]. The phenomena of screening of electric charge in a charged medium was the opposite of asymptotic freedom. But what I wanted to do is prove this for any quantum field theory. In the end, Sidney Coleman[13], who was visiting Princeton at the time, helped with a crucial technical insight and I completed that with him, showing that they were no asymptotically free field theories at all, except non-Abelian gauge theories we didn't discuss[14]. Our proof didn't apply to that situation. In general, what I expected was…

Partly, this rhymes with my earlier interest as a young student at Berkeley being fascinated by my advisor, Geoffrey Chew's[15] belief that quantum field theory was not the right framework for understanding the strong interactions. What I expected to show was that that quantum field theory simply could not explain deep inelastic scattering. One needed something else. A negative result, that's what I expected. As would be the consequence, asymptotic freedom is necessary, there weren't any asymptotic free field theories, [so we must] look outside of quantum field theory or something else.

The final part, of course, was the discovery of asymptotic freedom. With my first graduate student[16], we looked at Yang-Mills and discovered it was asymptotically free[17]. Then, everything fell into place because that was the unique… What we effectively showed was that was the only quantum field theory in four dimensions that could explain deep inelastic scattering. If you believed that, you had no choice. QCD[18] immediately followed from that discovery plus everything else one knew about the strong interactions at that point. Anyway, Parisi's work was very helpful in giving a clue as to how one might prove—as we did—that asymptotic freedom necessary and not a coincidence.

PC: Following that work, you kept working trying to solve QCD, and then on what you described as "speculative physics". What was generally driving your selection of problems? What was the overarching program?

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[12] Quantum electrodynamics (QED): https://en.wikipedia.org/wiki/Quantum_electrodynamics
[13] Sidney Coleman: https://en.wikipedia.org/wiki/Sidney_Coleman
[14] S. Coleman and D. J. Gross, "Price of asymptotic freedom," *Phys. Rev. Lett.* **31**, 851 (1973). https://doi.org/10.1103/PhysRevLett.31.851
[15] Geoffrey F. Chew: https://en.wikipedia.org/wiki/Geoffrey_Chew
[16] Frank Wilczek, *Non-Abelian gauge theories and asymptotic freedom*, PhD Thesis, Princeton University (1974). https://catalog.princeton.edu/catalog/9916560323506421; https://en.wikipedia.org/wiki/Frank_Wilczek
[17] D. J. Gross and F. Wilczek, "Ultraviolet behavior of non-Abelian gauge theories," *Phys. Rev. Lett.* **30**, 1343 (1973). https://doi.org/10.1103/PhysRevLett.30.1343
[18] Quantum chromodynamics (QCD): https://en.wikipedia.org/wiki/Quantum_chromodynamics

DG: [0:14:13] The overarching problem was—[it is] still incomplete, I must say… When we had QCD, it seemed like it was consistent with everything that was known about strong interactions, and it could be tested precisely with deep inelastic scattering, so that was great. But there were a few technical outstanding problems, and there was one deep conceptual problem. That was quark confinement. Confinement was necessary in some sense. Why didn't one see quarks? How come quarks weren't being produced? That was the overriding question that was worrisome, because it didn't make any sense to have a theory which was based on quarks, but there aren't any quarks. How does that happen?

At the beginning, there was zero understanding of that, although we suggested… Frank and I had [proposed] that the coupling becomes strong[19]. The so-called infrared singularities, the opposite phenomena as you go to larger distances, the coupling anti-screens or paramagnetic behavior of the quantum vacuum—now, we have a quite a good physical picture of that—because of the existence of charged gluon magnetic dipoles, which made it quite different than QED, where the glue, the photons, are neutral under gauge theory. This, I thought, was crucial. I wanted to make sure that the theory made sense, agreed with experiment, at least to explain why you never produced quarks.

It became very clear at the beginning that in order to explain the behavior of the proton and neutron, the hadrons, you had to dynamically generate the mass. That was, again, something that was very new: to have a theory with no masses, no scale. Scale variance is broken by quantum effects, and that can, in the regime of strong coupling produce a mass, a mass gap, and potentially in effect an infinite mass for the quarks. I felt it was very exciting to make a discovery, to propose a theory of the strong interactions, but there was this conceptual and technical problem which is paramount. That is what I devoted myself to for about 10 years. There were some other problems with QCD to begin with: the U(1) problem, etc. There are a few technical issues, but they weren't extreme. Quark confinement, however, was [and is] still an unsolved problem. We just started a Simons Collaboration on QCD and confinement, and QCD strings[20]. That remains an unsolved, which might not be completely solved to our satisfaction in this century, but we hope so. In any case, that's what I devoted myself entirely to.

[19] D. J. Gross and F. Wilczek, "Asymptotically free gauge theories. I," *Phys. Rev. D* **8**, 3633 (1973). https://doi.org/10.1103/PhysRevD.8.3633

[20] "Simons Collaboration on Confinement and QCD Strings" https://simonsconfinementcollaboration.org/ (2022). (Consulted October 15, 2022.)

Condensed matter physics was already a subject of great interest to me, because of Wilson's work with the renormalization group, whose applications was mostly at the beginning to… His motivation for the application of these ideas was largely to phase transitions in condensed matter physics, so that had a very important historical effect of reuniting the field of quantum field theory and high-energy theory and condensed matter theory with remarkable benefit to both sides.

PC: It's in that context that you spent you spend the academic year '83-'84 on sabbatical leave at the Laboratoire de Physique Théorique of ENS and the Service de Physique Théorique of the now CEA. What led you to these groups in particular?

DG: [0:19:49] Over the years, I had a lot of contact with the group at École Normale, Bouchiat's group[21], which had originally been at Saclay, but then moved to ENS. I forget exactly when. When I was postdoc at Harvard, as junior fellow[22], and later at Princeton, anytime [I could] I would go to spend the summer in Europe, often at CERN[23] for a bit and at École Normale, which had a summer program every summer. They had a group of people who would come. It wasn't very organized. It wasn't like a school. It was just a gathering, but it was a gathering in Paris of really excellent people: Tini Veltman[24] was always there, Sidney Coleman, Shelly Glashow[25], … I was by then good friends with Edward Brézin[26], who during my time at Princeton was a postdoc for two years, I think, just at the time this revolution was happening both in phase transitions with the renormalization group and—at the same time he was there—with QCD. So Édouard and I were good friends.

At Princeton, I always took advantage of the fact that it was very easy to go on sabbatical every three years or so. Princeton being so rich, we had a lot of sabbaticals, and the experimentalists couldn't use them so the theorists could easily abuse the system. And I did. So, I planned to go to Paris and also to Israel for a full sabbatical year. I guess it was '83-'84.

Aside from just wanting to spend a semester in Paris and work with my friends, one of the main reasons was that by that time my involvement in

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[21] Claude Bouchiat : https://en.wikipedia.org/wiki/Claude_Bouchiat
[22] Harvard Society of Fellows: https://en.wikipedia.org/wiki/Harvard_Society_of_Fellows
[23] CERN: https://en.wikipedia.org/wiki/CERN
[24] Martinus J. G. (Tini) Veltman: https://en.wikipedia.org/wiki/Martinus_J._G._Veltman
[25] Sheldon Glashow: https://en.wikipedia.org/wiki/Sheldon_Glashow
[26] See, *e.g.*, P. Charbonneau, *History of RSB Interview: Édouard Brézin*, transcript of an oral history conducted 2020 by Patrick Charbonneau and Francesco Zamponi, History of RSB Project, CAPHÉS, École normale supérieure, Paris, 2021, 20 p. https://doi.org/10.34847/nkl.9573z1yg

trying to solve QCD had petered out to some extent. Although a lot of progress had been made and in developing to QCD in many aspects, it was just too hard.

The other thing was that I was more and more interested in unification[27]. The standard model, by that time, it was clear, was leading us to think about unifying all the forces. The extrapolation of the particle content of the standard model suggested a unification on scale very high in energy, very close to the so-called Plank length, where gravity becomes important. So, I was drawn, as were many of my colleagues, to think about unification of gravity and the other forces of nature. That rekindled my interest in string theory, which I'd been interested in initially when it was discovered back in 1968 and had worked[28] on very early on at Princeton with John Schwarz[29] and two young French postdocs, Joël Scherk[30] and André Neveu[31]. They were there. That was very exciting. The dual resonance bond, the one-loop amplitude, and some renormalization. I gave that up because I realized it wasn't going to solve the problem [that] I was most interested [in], which was deep inelastic scattering and the structure of the strong interaction. But André and I were good friends and colleagues. We worked on problems famously right after QCD, on a two-dimensional model which is very illustrative of how you dynamically generate mass in asymptotically free field theories[32]. In any case, André was a close colleague and friend and a string theorist and remained so over the years.

I decided in ’82-83 to get back into string theory, which was a pretty neglected subject. It really was just Green[33] and Schwarz and a few others who were still doing, by that time, supersymmetric string theory. It was very interesting, but… My idea was to go to Paris, and André and I were going to work together on string theory. So, this is all set up. I had arranged this visit, and then André accepted a position at CERN. He was offered at that point a five-year position at CERN. CERN was very attractive, and he went to CERN. That was very disappointing to me, but I still was going to go to Paris anyway. But there almost no string theorists [in Paris]. There

[27] Unification: https://en.wikipedia.org/wiki/Unification_(physics)
[28] See, *e.g.*, D. J. Gross, A. Neveu, J. Scherk and J. H. Schwarz, “Renormalization and unitarity in the dual-resonance model,” *Phys. Rev. D* **2**, 697 (1970). https://doi.org/10.1103/PhysRevD.2.697; “The primitive graphs of dual—resonance models,” *Phys. Lett. B* **31**, 592-594 (1970). https://doi.org/10.1016/0370-2693(70)90703-3
[29] John H. Schwarz: https://en.wikipedia.org/wiki/John_Henry_Schwarz
[30] Joël Scherk: https://en.wikipedia.org/wiki/Jo%C3%ABl_Scherk
[31] André Neveu: https://en.wikipedia.org/wiki/Andr%C3%A9_Neveu
[32] Gross-Neveu model: https://en.wikipedia.org/wiki/Gross%E2%80%93Neveu_model
[33] Michael Green: https://en.wikipedia.org/wiki/Michael_Green_(physicist)

were almost no string theorists in the world. But Paris was Paris. I guess one of your questions is about random surfaces.

PC: I think this is the first problem you tackled there[34].

DG: [0:26:17] That was a way of trying to construct a different approach to string theory. String theory is a theory of random surfaces, there were all sheets. [What] a string maps out, as it moves in time, is described by a surface. Anyways, this was work directly related to string theory.

PC: How did that collaboration with Alain Billoire and Enzo Marinari begin[35]?

DG: [0:26:47] I was thinking about how to sum over random surfaces of path integral in two-dimensional geometry. These guys were computer experts. Computers at that time were pretty trivial compared to today, but I was not at all adapt in that, so I enlisted them in this project. But I was at École Normale and it was buzzing with all sort of stuff. So, I started hearing about spin glasses. It seemed to me awfully interesting. This was exactly the time when Parisi had put forward the mean-field theory replica symmetry breaking solution, which absolutely intrigued me. This was something I had never heard of. It seemed totally crazy and bizarre. When I learned about it from listening to talks…

PC: You had not kept in touch with Parisi since his time in New York?

DG: [0:28:03] I'm trying to remember. Parisi has a remarkable talent for noticing interesting ideas that occur here and there and amplifying them. For example, we overlapped in a whole variety of different areas. The **Iguchi-Kuwai** was one of them. (I forget whether that was before. It was probably before that example.) I would always notice what Parisi did. But no, I had never… Unfortunately, Parisi doesn't travel that much, especially to the United States. At KITP[36], I tried to get him to come to Santa Barbara so many times. So many times, it was this close, but in the end, I think he's never been here. So, we didn't overlap that much, [only] now and then.

What attracted me was just this crazy of symmetry breaking in replicas, which were kind of this mathematical trick. It was just fascinating stuff. I wanted to understand it much better. It seemed very complicated.

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[34] D. J. Gross, "The size of random surfaces," *Phys. Lett. B* **138**, 185-190 (1984). https://doi.org/10.1016/0370-2693(84)91897-5

[35] A. Billoire, D. J. Gross and E. Marinari, "Simulating random surfaces," *Phys. Lett. B* **139**, 75-80 (1984). https://doi.org/10.1016/0370-2693(84)90038-8

[36] Kavli Institute of Theoretical Physics: https://en.wikipedia.org/wiki/Kavli_Institute_for_Theoretical_Physics

The way I understand things – like, for example, the work I did with Neveu[37] – was to find some kind of large *N* limit, some kind of limit where the number of degrees of freedom becomes very large. That often means that you have a classical limit of some kind for a small parameter, and you can often solve things exactly. Again, that was one of the things I learned about when condensed matter physics and high energy physics began to talk to each other, because this had been an idea that was often used in condensed matter physics, starting with Heisenberg spin models, Stanley[38]'s work[39] and others' [work]. And I used it. I was happy to apply it to every possible hard problem that one can think of – and have over the years – from two-dimensional QCD, which was a great example of using the large *N* limit to solve and it was a great calculable example of quark confinement, to the so-called Gross-Neveu model[40], which I talked about before. Anyway, it seemed to me: "Why not try to take the spin glass, which is such a simple Hamiltonian, and find some kind of large *N* limit instead of just having Ising spins on each site of the spin glass?" I realized that continuous spins don't work[41], but maybe some other kind of model, like the Potts model, would work.

I started discussing this… Parisi was in Rome. If he had been in Paris, I would have gone to Parisi, but instead I talked about this with Marc Mézard[42]. Marc was great to work with. He started to look at the Potts model as a function of *p*, the *p*-spin Potts model, where you have *p* variables on each site. [We] pretty rapidly realized that [in] that limit one could actually solve the theory. One could explicitly examine Parisi's replica symmetry ansatz for large *p*, where it simplified dramatically. Instead of having this complicated hierarchical order parameter, one just had a two-step order parameter. Everything was calculable. Marc was also one of the experts on spin glasses. He had worked on spin glasses a lot. He was part of this group around Parisi, Virasoro, Toulouse and Sourlas[43]. He

[37] D. J. Gross and A. Neveu, "Dynamical symmetry breaking in asymptotically free field theories," *Phys. Rev. D* **10**, 3235 (1974). https://doi.org/10.1103/PhysRevD.10.3235
[38] Eugene Stanley: https://en.wikipedia.org/wiki/H._Eugene_Stanley
[39] *n*-vector model: https://en.wikipedia.org/wiki/N-vector_model
[40] See Ref. 32.
[41] See, *e.g.*, 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
[42] D. J. Gross et M. Mézard, "The simplest spin glass," *Nucl. Phys. B* **240**, 431-452 (1984). https://doi.org/10.1016/0550-3213(84)90237-2
[43] See, *e.g.*, M. Mézard, G. Parisi, N. Sourlas G. Toulouse and M. Virasoro, "Nature of the spin-glass phase," *Physical Rev. Lett.* **52**, 1156 (1984). https://doi.org/10.1103/PhysRevLett.52.1156; "Replica symmetry breaking and the nature of the spin glass phase," *J. Physique* **45**, 843-854 (1984). https://doi.org/10.1051/jphys:01984004505084300

taught me a lot about the TAP[44] approach etc. We just went pretty fast, because it's pretty simple once one started doing this. We showed that we could understand the solution from the TAP equation as well. Not only that, but it also remarkably agreed with the random energy model[45] that Derrida had postulated. It wasn't a microscopic model of spin glasses. It was a very cute idea of trying to construct a model where the energies are taken from some random sample. The fact that the random energy model was equivalent to the large p spin Potts model made the random energy model *real*, in a sense, and also gave a beautiful picture of what the spin glass looks like. I really liked this work a lot. I love soluble models that illustrate deep principles, and this was one of them.

PC: What was the reaction to this work? Was it immediately appreciated? Or was it only the cognoscenti who liked it?

DG: [0:35:07] The people who were into this game were again very few. It's always good to be in such a field. There are new, very deep ideas, but only a handful of people know about them. That was the case then. It was all new and complicated and mysterious, and maybe wrong. So, the group in Paris I knew about, was marginally excited about it, I guess. This was a period when there were all sorts of avenues to explore, and people were exploring them. My friends in high-energy physics knew zero about all of this and had no connection to it.

The second half of that year I went to Israel. I was visiting Hebrew University. There, I came right after this work, I was very excited about it. I gave some lectures and sort of interacted with a whole group of quantum field theorists there, and some condensed matter physicists who were intrigued. Really, I felt that I brought this to them. One of them has was Sompolinsky[46]. (Sompolinsky had worked on spin glasses before.) I was very focused on this, very excited by Parisi's breakthrough. There was another group of people, who were more quantum field theorists, who were interested in general quantum field theory, replica symmetry, and, in particular, in the Hopfield model[47]. They went off and had distinguished careers. In the end, Sompolinsky as well—in neuroscience—applying these ideas to the Hopfield model of memory, which was effectively a spin glass. Sompolinsky was very interested in this Potts model example. He said: "Why don't we look at it for finite p?" That has led to this work with his

[44] D. J. Thouless, P. W. Anderson and R. G. Palmer, "Solution of 'solvable model of a spin glass'," *Philo. Mag.* **35**, 593-601 (1977). https://doi.org/10.1080/14786437708235992
[45] Random energy model: https://en.wikipedia.org/wiki/Random_energy_model
[46] Haim Sompolinsky: https://en.wikipedia.org/wiki/Haim_Sompolinsky
[47] Hopfield model: https://en.wikipedia.org/wiki/Hopfield_network

graduate student, Ido Kanter[48]. He was at that time in Bar-Ilan University. I was at the Hebrew University, but Israel is a small place. Haim became a very good friend and colleague. I enjoyed that work with him. It was kind of remarkable that we could argue that this didn't really require p goes to infinity, but there was a phase transition as a function of p, already for p=4. That made it much more realistic. I guess I could have gone on in this game…

PC: Before we move on, I have a couple of other questions. During your time in Paris, did you cross path with Elizabeth Gardner[49], who was working with Bernard Derrida at the time?

DG: [0:38:51] No. Derrida was rather aloof and removed from this[50]. There was a group of Mézard and Sourlas, who were very interconnected, but Derrida was off doing his stuff, and not really part of this group. It's a different style of physics. It's very special and wonderful, but no I don't remember… I might have met Gardner, but I have no recollection.

PC: In your Nobel lecture you refer to the early of renormalization as the *renormalization trick* period. Was there a parallel in your mind between the renormalization trick and the replica trick, meaning that there were lots…

DG: [0:39:58] Renormalization was a much deeper problem, in a sense, than dealing with quenched disorder. Renormalization was regarded by many as a trick. Famously, Feynman talked about it even though it was crucial to his work in quantum electrodynamics. I forget the quote, maybe I quoted it in that lecture. It was a trick. There was this feeling that I learned by taking courses in quantum field theory as a student, that renormalization was kind of a trick. It was a way to circumvent these ultraviolet divergences and it worked. It was highly nontrivial to prove that it worked, and if didn't work you were gone, so you had to only work with so-called renormalizable field theories. Very different the way we look today at quantum field theory. There was never, when I learned quantum field theory, any understanding of what was behind this trick. What did it mean? What was the physics? That changed completely. It was the Wilsonian revolution that changed it. And, to a large extent, QCD. QCD was the first

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[48] D. J. Gross, I. Kanter and H. Sompolinsky, "Mean-field theory of the Potts glass," *Phys. Rev. Lett.* **55**, 304 (1985). https://doi.org/10.1103/PhysRevLett.55.304

[49] Elizabeth Gardner: https://en.wikipedia.org/wiki/Elizabeth_Gardner_(physicist)

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

example, at least in four dimensions, of a theory which had no ultraviolet divergence.

PC: In the context of the early days of replicas symmetry breaking–at a different scale, admittedly–did it have something a bit magical that sidesteps difficulties…

DG: [0:42:11] Clearly, it was a mathematical trick, and it still is. Replica symmetry as a trick, I guess, Edwards and Anderson invented it to deal with quenched disorder, how to average over the log of a partition function[51]. How to regard the log as a limit of a power is a mathematical trick, for sure, although some mathematical tricks acquire a physical meaning of their own. Some kind of meta world. It has enormous applications because of [the ubiquity of] disorder in condensed matter systems. Nowadays, in quantum gravity and holography[52], in string theory[53], the trick is invoked in order to calculate entropies, which are better regarded as a sort of limit of Rényi entropies[54], which are kinds of replicas. Just in the last year or two, these tricks have enabled one to solve very deep problems in quantum information and black holes.

At the time, it was regarded by many people—certainly the more mathematically inclined people—as a trick and as a dangerous trick. It's kind of evident why it's so dangerous, especially for spin glasses, because that's taking the number replicas *n* to zero, which doesn't seem to make any sense and there are all sorts of issues. And the perturbation theory around this mean-field theory was full of nasty divergences. It didn't seem at all normal, and still [doesn't]. Although now there are rigorous proof of the existence of this phase, it still seems a bit dangerous, and is indeed dangerous in many circumstances. It's slightly different. I don't know of any way to understand in some physical context the existence of these replicas. It seems totally a trick to some extent, although I must say that the use of replicas in geometry has deepened the power of this trick. There, one is taking not *n* goes to zero, but *n* goes to one, typically, in order

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

[52] See, *e.g.*, G. Penington, S. H. Shenker, D. Stanford and Z. Yang, "Replica wormholes and the black hole interior," *J. High Energy Phys.* **2022**, 205, (2022). https://doi.org/10.1007/JHEP03(2022)205; Z. Dong, X. L. Qi and M. Walter, "Holographic entanglement negativity and replica symmetry breaking," *J. High Energy Phys.* **2021**, 24 (2021). https://doi.org/10.1007/JHEP06(2021)024

[53] See, *e.g.*, C. Callan and F. Wilczek, "On geometric entropy," *Phys. Lett. B* **333**, 55-61 (1994). https://doi.org/10.1016/0370-2693(94)91007-3; A. Prudenziati, "A perturbative expansion for entanglement entropy in string theory," *Nucl. Phys. B* **943**, 114628 (2019). **https://doi.org/10.1016/j.nuclphysb.2019.114628**

[54] Rényi entropy: https://en.wikipedia.org/wiki/R%C3%A9nyi_entropy

to calculate the von Neumann entropy[55] as a kind of limit of replicas of the system—or the Rényi entropies—but not in such a dangerous limit as taking the number of replicas to zero. One is better than zero. Still, to some extent, I'm not sure whether there is more to put it on some firmer physical setting.

PC: You were mentioning that upon your return from Jerusalem, your enthusiasm for these models shifted.

DG: [0:46:42] I still enormously liked [these models]. Two things happened. One, I returned to Princeton and Parisi's breakthrough was really centered in Paris and in Rome. It had just spread a bit outside of that. Second, what I was really interested in—the second thing which I continued to do in Paris—was getting back into string theory. When I returned, which was spring of '84, there was a big breakthrough of Green and Schwarz[56] that summer, which was very exciting and made much more realistic the possibility of constructing a consistent theory of supergravity, superstring level 10-dimensions chiral matter. That was even more exciting, maybe a different order of magnitude, at least for me. So, I only followed very peripherally the exciting developments that came out of replica symmetry breaking. There were many. It really was amazing. But I was an observer. I was totally focused on string theory.

PC: At Princeton and around, there was Phil Anderson[57] and the Bell Labs crowd. Did you discuss with them at all and that point?

DG: [0:48:44] Phil, of course, was the Anderson of Edwards-Anderson. He invented, together with Edwards, replica symmetry. He was absolutely fascinated, not technically in the sense that he got into it, but he found… He got me to give a colloquium on spin glasses[58]. He was interested. Phil was not somebody, unfortunately, I could collaborate or work with. It was often very hard to understand him. And he wasn't that much into spin glasses at the time, although he followed it and was very positive about it.

[But] this was the superstring revolution. This was enormously exciting. We discovered that it had a lot of strength. It seemed that we had

[55] Von Neumann entropy: https://en.wikipedia.org/wiki/Von_Neumann_entropy
[56] Green-Schwarz mechanism: https://en.wikipedia.org/wiki/Green%E2%80%93Schwarz_mechanism
[57] Philip W. Anderson: https://en.wikipedia.org/wiki/Philip_W._Anderson
[58] **PC**: Anderson is then reported to have said: "I introduce David Gross the condensed matter theorist." R. H. McKenzie, "Colloquium on 2021 Nobel Prize in Physics," *Condensed Concepts* (October 28, 2021). https://condensedconcepts.blogspot.com/2021/10/colloquium-on-2021-nobel-prize-in.html (Consulted October 14, 2022.)

tantalizingly close to a complete unified theory of everything. I didn't have much time to do anything else.

FZ: What was the subject of this colloquium that you gave in Princeton?

DG: [0:50:22] This was a colloquium for the general physics department, so this was about spin glasses, and replicas and replica symmetry breaking, and also the simplest spin glass, because that's the simplest case for the random energy model. I have the notes somewhere. Those were the days where one had transparencies. I have those plastic things somewhere. It was a good lecture. It's a beautiful subject, the ultrametric nature of [spin glass] and its implications. It was a general talk on spin glasses, but maybe the simplest spin glass was an explicit illustration, but it was a general talk on spin glasses.

PC: In your Nobel lecture, you described American physicists as “inveterate pragmatists”, which affected how they initially responded to quantum field theory, for instance. Do you have that same impression about their response to replica symmetry breaking? In other words, do you see a difference between European and American physics in that context?

DG: [0:51:55] It used to be more. Not was much anymore. A bunch of things have happened in high-energy physics, of course. The time I was discussing then, was before we had real theories. Now, we have incredibly successful theories. I think I was referring more to a time where European theoretical was not as strong and very formal mathematically. It was dominated by people like Haag[59] and Jost[60], and others who were mathematical physicists. Then, there were phenomenologists, especially at CERN, but there weren't the kind of intermediate quantum field theorists—which I regarded myself as—not particularly interested in mathematical rigor, because one knew what was true somehow, even if we couldn’t prove it. That, I think, has changed by now.

At that time, there were certainly people in France who were very skeptical about replicas symmetry breaking, because it seemed so mathematically bizarre and unproven and non-rigorous. [It was also a] mean-field theory with infinite range. There was enormous skepticism about whether it applied to the real world, to real spin glasses in [three] dimensions etc. That has taken a long time and a lot of mathematical rigor to dissipate, but it's still not totally clear.

[59] Rudolph Haag: https://en.wikipedia.org/wiki/Rudolf_Haag
[60] Res Jost: https://en.wikipedia.org/wiki/Res_Jost

I think it was true in the earlier period that American physicists were less concerned with mathematical rigor and more used to bold assumptions and approximations, because one had experiments one could continue to interact with, whereas the European tradition was a little more formal and rigorous.

PC: You mentioned earlier that as director of the ITP, you tried to bring Parisi over many times. Is there any other way in which you tried to promote or encourage ideas of replica symmetry breaking while in that position?

DG: [0:55:18] Not particularly by that time. It didn't need [it]. We had a very good program that I did organize[61]. Trying to get Parisi to come was part of that, which he had agreed to but in the end didn't. Then, Marc Mézard—my collaborator and friend—helped [me] organize [it]. That was an extremely exciting program at the time.

I don't think the field needed support. It was going strong in so many different directions. It really is fascinating how deep ideas in theoretical physics have applications even outside of physics. That's certainly been one of the trusts of this Rome-Paris axis over the years.

PC: Is there anything else you would like to share with us about that era that we may have missed?

DG: [0:56:52] At that time, from the point of view of European physics, that was a time where this Rome-Paris connection was very strong and very fruitful. It's sad nowadays because Italian physics is suffering so much. It's so underfunded. Any young Italian physicist at some point—early on—leaves Italy. École Normale, I also don't think is necessarily what it used to be. It's kind of sad, because at that time, the École and the group in Rome were incredibly strong. They had carved out their own area and dominated it. It's just a pity to see it suffer as it is now. (Both places, but more in Italy.)

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

DG: [0:58:41] I'm sure I do, but they're not very well organized. I don't have any well-formulated plans, whether at Santa Barbara or Princeton or whatever.

[61] *The Future of Physics*, David Gross, Kavli Insitute of Theoretical Physics, University of Santa Barbara, California, USA, 7-9 October 2004. https://online.kitp.ucsb.edu/online/kitp25/ (Consulted October 14, 2022.)

I suppose I should start thinking about that. One tends not to until it's too late.

PC: Professor Gross, thank you very much for your time and this conversation.