Edouard Brézin

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Have I Really Been a Condensed Matter Theorist? I'm Not Sure, but Does It Matter?

Edouard Brézin

Laboratoire de Physique de l'École normale supérieure (ENS), CNRS Université Paris Sciences et Lettres, Sorbonne Université, Université de Paris, F-75005 Paris, France; email: brezin@lpt.ens.fr

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Abstract

My life as a physicist has been a blend of field theory, statistical physics, and condensed matter physics over half a century.

EARLY DAYS

How did I become a physicist? I am embarrassed to admit it when I compare my trajectory with the fate of young people nowadays, who have to wait for so long before they know whether they might stay in an academic career. I was offered a permanent position in the theory group at Saclay much before I got a Ph.D. Don't imagine that this is because I was particularly brilliant or anything but simply in the early sixties, France was trying to attract young people into research. In fact, the slightly more senior people in this group—Claude Bloch, the head of the group, Albert Messiah, Marcel Froissart, Roger Balian, Raymond Stora—didn't even bother writing a Ph.D.[1] They had all been hired because they graduated from the École polytechnique at the top of their class. It happened that in addition they were very good, a lucky "accident."

Yet, the education in that school at the time was not inspiring to say the least. Fortunately, a friend, who understood everything with ease and depth, Claude Itzykson, mentioned a new book by someone named Albert Messiah, which I (difficultly) managed to buy.[2] The discovery of the quantum world was a revelation, although I must confess that I was foolishly more impressed initially by things like Hilbert spaces or group theory than by the mere quantum wonders. A few weeks before graduation, we had a number of conferences by various representatives of organizations interested in hiring some of us, and the CEA (Commissariat á l'énergie atomique—the atomic energy commission of France) had sent Messiah himself. . . .

After a few years at Saclay, I understood that it could be advisable at some stage to get a Ph.D., and I undertook to present to that effect a French version of articles that I had written with Itzykson on quantum electrodynamics (nowadays one can write a Ph.D. in English). One of them was great fun. We were visited by an engineer working for a laser company who wanted to understand the prospect of pair creation e^+e^- by a powerful laser. We knew of Schwinger's calculation for the case of a constant electric field (1; see also the **Supplemental Literature Cited**), which is one of the most beautiful papers that I have ever read; neglecting the frequency of the laser, this led to a negative answer for years to come. But we also knew of the atomic physics phenomenon of multiphoton ionization. Cracking the vacuum would then require $n = 2mc^2/\hbar\omega$ photons, with probability proportional to α^n, and then it looked much less unlikely. We worked out the crossover between the zero-frequency limit and the perturbative result (2). Alas, as long as $\hbar\omega/mc^2$ remains small, the probability of pair creation is still very small, and in fact 40 years later it is still one of the Graals for the gigantic lasers built nowadays for fusion research.

Supplemental Material >

Before leaving my early days, I feel that I need to add a few words given that I belong to the vanishingly small tail of people who have lived through World War II. My parents were recent Jewish immigrants from Poland, totally secular and agnostic, but the Germans didn't care about such "details." My father volunteered in the French army; he was taken prisoner in May 1940, but he succeeded in escaping before being taken to a German camp. We lived relatively quietly after that, under false names, in Brive, a town in the so-called free zone, i.e., under Vichy administration. In November 1942, this zone was also occupied by the German army and, although we did not wear yellow stars, the situation became dangerous. I was hidden in farms, but my parents had to go back to town to make a living; my father was a tailor. So, I am indebted to several people who, often at the expense of their own safety, helped us. Half of the foreign Jews in France survived the war (three-fourths of the French Jews) (3, 4): We fell on the lucky side of the coin. Many members

[1] Cirano De Dominicis was an exception: After his undergraduate studies at Polytechnique, Cirano had gone to Birmingham and done a Ph.D. with Rudolf Peierls.

[2] Alas, Claude passed away in 1995.

of our family in Poland or France didn't. Since then, I have enjoyed a free life in which I was offered all the opportunities of the public system, without limitations other than my own.

THE SEVENTIES

In 1971, my wife K[3] and I decided to spend time abroad with our two children. K went to the biology department of Rutgers University, and Princeton University graciously opened its doors to me for a visiting position. The theory group was very impressive and led by senior members Murph Goldberger and Sam Treiman. One could talk occasionally with the legendary figures of Eugene Wigner[4] and John Wheeler, who still organized the physics colloquium. The Department counted brilliant young professors such as Curtis Callan, David Gross, John Schwartz, and an elegant young Canadian Less Saunders (who unfortunately died soon after from Hodgkin disease). I shared an office with a postdoc, Alan Guth, a few years before he understood cosmic inflation. In my recollection, John Hopfield was the only theorist in condensed matter or statistical physics at Princteon University at the time. The big event, which marked science for years to come, was Ken Wilson's renormalization group (RG). Ken, who was on leave from Cornell University visiting Princeton's Institute for Advanced Studies (IAS), had been invited by the university to give a talk on his new paper entitled "Renormalization group and critical phenomena. I. Renormalization group and the Kadanoff scaling picture" (5). [In fact, I had read for myself Leo Kadanoff's now famous article on block spins (6). The idea was very appealing, but I had the unwise reaction of trying to check this picture for the 2D Ising model. I didn't get anywhere, and I quit.] Wilson said that he couldn't explain his ideas in just one talk, and David Gross had the insight to tell him that he could come and lecture as much as he wanted. So Ken ended up giving fifteen lectures starting in the spring of 1972. In fact, I first listened to Ken in December 1971. The IAS had organized a small high-level workshop that was finally open to local scientists. I remember Steve Adler gave a talk on computing the fine structure constant from the Gell-Mann–Low RG, and Ken, a young and largely unknown physicist, somewhat aggressively stated that "he was going to cure that mess." I am not sure any of us understood from Ken's talk what he had meant. Now we all do.

Most people in the area attended Ken's lectures; almost all of them were particle theorists. Somehow, I was one of the few attendants who knew that critical phenomena was not something that Ken had invented but was one of the main open problems in theoretical physics. I remember attending Paul Martin's lectures at the Les Houches Summer School in 1967. Paul had started by listing what he regarded as the main open questions—five or six, if I remember right—such as strong and weak interactions, turbulence; critical phenomena was in the list. So, I did pay attention carefully to Ken's lectures on his new interpretation of the RG, but I found them very mysterious and even hard to believe. At the end of the second or third lecture, I even said loudly that I needed to check what Ken had said and added, "Is anybody interested?" A postdoc from Scotland, David Wallace, a student of Peter Higgs, reacted immediately, and we started making various alternative calculations, such as higher point functions and different cutoff, and of course in all cases we could just witness that Ken had been right. Clearly, Ken was not surprised, not even relieved of any weight, when we reported our findings. Finally, instead of checking, we decided to take the amazing new tools, such as the ϵ or large n expansions, to compute new phenomena. Discussing with Ken,

[3] My wife's first name is Colette, but she is known by friends as "Kouni," hence the K.
[4] Wigner used to come at tea time with formidable questions, which started invariably with, "Explain to a poor chemist why. . . ."

we started with the equation of state, i.e., in magnetic language, the relations among the applied field h, the magnetization M, and the temperature $\tau = T/T_c - 1$. The scaling theory developed by Ben Widom, Kadanoff, and others claimed that h/M^{δ} was a universal function of $\tau/M^{1/\beta}$. To our amazement, the explicit calculations with these new tools confirmed completely all the predictions of the scaling and universality picture that had been developed phenomenologically over the years and provided indeed an equation of state (7, 8).

If Ken Wilson was awarded the Nobel Prize ten years later for his work on critical phenomena, his motivation came from a new way of looking at high-energy physics and the influence of his ideas there too has been essential. Ken argued that the correct physical approach for particle physics consisted of starting with some completely unknown theory, apart from basic symmetries, at short distance and examine its behavior at larger distances. Then the concepts of critical surface, fixed points, and relevant and irrelevant operators led to some effective theory at large distance. So renormalizability was not simply a lucky accident but a consequence of the irrelevance of some coupling constants. Furthermore, he argued that a theory with an IR-stable fixed point at weak coupling could only be effective. If one tried to continue it down to vanishing length scales, it would have to be a free theory. This was a big blow against conventional views, and its influence on further developments such as asymptotic freedom and quantum chromodynamics (QCD) was on its way.

In the fall of 1972, we returned to France; K was eager to go back to her excellent lab at the Institut Pasteur. By that time, people were aware that something had happened around the RG and critical phenomena and, to my embarrassment, I was invited to give lectures for our Saclay group on the new theory. However, even if I knew how to use it, the justifications were still very foggy in my mind. Fortunately, Jean Zinn-Justin was also back from a year at Stony Brook University, where he had produced with Ben Lee a proof of renormalizability of non-Abelian gauge theories through functional integrals. He was also required to lecture on the renormalization of gauge theories. We discussed a lot after our lectures, often joined by Jean-Claude Le Guillou. We were aware of new contributions to the same topic, the RG, in renormalized field theory due independently to C. Callan (9) and Kurt Symanzik (10). They had realized that, even if a classical field theory was scale invariant, such as massless electrodynamics, the quantum fluctuations broke that invariance. The Ward identities for broken-scale invariance were indeed the RG equations for correlation functions. We convinced ourselves that, with the Callan–Symanzik equations, we could understand and justify what Wilson had established. We did use the standard language of renormalized field theory, which directly provides the scaling limit of the theory (11). The scaling limit corresponds to distances much larger than the microscopic length scales, say, the lattice spacing a. The limit $x/a \to \infty$ can be taken at large x, or at fixed x by letting a go to zero, which is exactly what the renormalized theory does. We were accused of "taking the wrong limit" by people who in fact did not understand what the RG meant. However, some high-energy theorists had the kindness to say that our approach had helped them to understand Ken's RG.

Early in 1973, I attended a meeting at Temple University organized by Mel Green. K. Wilson, Michael Fisher, and many others attended the meeting, in particular L. Kadanoff who, after being one of the pioneers of scaling and universality, had shifted for a while to urban studies. The excitement was tangible. For instance, Paul Martin was so enthusiastic that he declared the problem of critical phenomena to be solved and said, "If someone came up with a solution of the 3D Ising model, I wouldn't even look at it!" I remember telling Ken at that meeting that we had tried with Zinn-Justin the large n limit for gauge theories, and I had not finished my sentence before Ken said, "It doesn't work." It's only after Gerard 't Hooft's celebrated paper (12) that we all understood that Ken and ourselves had only considered planar diagrams, and thus we had missed the large n-reduction to planarity.

During the summer of 1973, I organized a summer school in Cargèse on critical phenomena. Among the lecturers were Callan, Elliot Lieb, Symanzik, Wilson, Zinn-Justin, and the young Giorgio Parisi, who had done calculations with an early version of conformal bootstrap. Interestingly, Wilson chose to present his RG numerical solution of the Kondo model (13). Indeed, he didn't like perturbative methods such as the ϵ or $1/n$ expansions, in spite of the fact that he had invented them. In his view, interesting problems did not possess any small parameter[5] and the RG was a tool designed to handle such problems. The Kondo problem was a textbook example. But Ken's interests had already shifted: He had just realized that one could write a gauge-invariant theory of quarks and gluons on a lattice, transforming the understanding of the bound state spectrum of QCD into a problem of statistical mechanics (14). This is still an active field in physics, requiring considerable computer resources. In spite of Ken's encouragement, I didn't feel that I could be useful there, and there were so many things to do with the RG approach!

Before the end of 1973, I made my first trip to the Soviet Union for a $M \cap \Phi$ meeting. I met for the first time Sasha Migdal, who had done work with Vladimir Gribov on the effective potential (i.e., equation of state) in a fashion very similar to ours.[6]

In the spring term of 1974, I was invited to give the Loeb lectures at Harvard University and ended up giving a few lectures on our Callan–Symanzik approach to critical phenomena. I remember discussions with Sydney Coleman and also Steve Weinberg, who was very kind, but would not accept (at the time) that the beautiful electroweak theory that he had invented was merely an effective theory. Soon after, he published a set of notes in which he had reconstructed Wilson's RG from field theory. One day, Sheldon Glashow, out of friendship, offered me some kind advice on how to conduct one's research, saying something like "Edouard, either you do quarks or DNA, nothing in between." I didn't follow the advice. Apparently, Murray Gell-Mann, who didn't seem to appreciate condensed matter physics, complained in a similar tone, maybe jokingly, that his brilliant former student Wilson had taken the wrong path.

In the summer of 1975, J. Zinn-Justin organized an RG school at Les Houches, one of the greatest in the glorious history of this school. Callan, Ludwig Faddeev, Gross, Martinus Veltman, and others lectured; I spent the whole six weeks there in spite of the fact that I was only required to be there for two weeks for my own lectures. Ed Witten was a student, who was obviously extremely bright, among many others who soon after became well-known theorists. In the whole history of Les Houches, the 1975 proceedings of that school (15) have been one of the most popular books with students.

Up to that point the predictive power of the RG was sufficient to understand scaling and universality, but not quite up to expectations in terms of quantitative analysis. Soon it became clear that the ϵ expansion was divergent. If one computed one more term in the expansion at $\epsilon = 1$, the approximate agreement without including the new term collapsed. So, the accuracy of the theory looked a priori limited.

Fortunately, a beautiful experiment for which the theory was free of approximations settled, in my mind at least, the validity of the approach. But first the theory: In a remarkable paper,

[5] Many theorists believe that the ultimate goal is to find exact solutions, as can be found for integrable models. Ken's point of view was just the opposite, and the Kondo model served as an illustration. The irony is that later Pavel Wiegmann and Nathan Andrei gave independently a Bethe ansatz solution of the model.

[6] I had previously met Sasha's father, Arkady Migdal, from the Landau Institute, a well-known physicist and a man with many talents, like making underwater movies, popular science television programs, and sculpture, and collecting Orthodox medallions. He was also a courageous man who, successfully, fought against the authorities who had banned his Sasha and his famous schoolmate Sasha Polyakov, from Moscow University, in the name of an unwritten but effective *numerus clausus*.

Larkin & Khmel'nitskii (16), from the Landau Institute, considered ferromagnets (ferroelectrics in their original paper) with strong dipolar interactions. As we know from Heisenberg's classic work, the dominant magnetic interaction between electronic spins in a solid is the exchange force, which is a short-range interaction induced by the Pauli principle. The dipole–dipole interaction is negligible in comparison. But the authors wondered what would happen in a material in which dipolar forces were dominant. Their article contained a number of surprising statements: (*a*) They claimed that the critical singularities in this theory in three dimensions was similar to an unusual short-range model in four dimensions and (*b*) they stated as if it was well-known that mean-field theory was valid at criticality above four dimensions. It seemed that this was common knowledge at the Landau Institute at the time but was not in my corner of the world. (*c*) They said that mean-field theory was violated right at four dimensions by logarithms, and they computed those violations from parquet diagrams. For instance, the specific heat, instead of a simple discontinuity as in mean-field theory, would diverge like $|\log(|T/T_c - 1|)|^{1/3}$. (*d*) Therefore, with strong dipolar forces, one should observe those logarithmic deviations to scaling in a real 3D experiment.

The first experiment to check that prediction was conducted at Bell Labs by the team of Günther Ahlers (17). They took a material with low T_c and large Landé factors, and they did find a log-divergence of the specific heat with a power of the logarithm 0.34 ± 0.01 instead of the predicted 1/3. I always liked this experiment because the theory there is free of any uncontrolled expansion or numerical scheme: If the RG is right, the prediction is unambiguous. In some sense it is similar for critical phenomena to the early tests of QCD based on logarithmic deviations to naive scaling for deep inelastic scattering (18). On the theoretical side, examining the extent of the validity of the dimensional reduction from four to three due to dipolar interactions remained. Some claimed that it would hold in any space dimension, but the original argument was just based on a one-loop calculation. So with J. Zinn-Justin, we examined the dipolar problem and showed that beyond one loop the dimensional reduction didn't hold. The next-to-leading critical singularity involves a log–log correction for the short-range problem in four dimensions and for the dipolar problem in three dimensions but they have different coefficients (19). As G. Ahlers once told me, nothing looks more like a constant than a log–log; there is no hope to check this more detailed prediction.

INTERACTING WITH THEORETICAL PHYSICS IN THE SOVIET UNION

In the summer of 1975 at the International Conference on Statistical Physics in Budapest, Sasha Migdal presented what is known nowadays as the Migdal–Kadanoff scheme. It is based on block-spin RG ideas, within a bond moving approximation, and it can be carried out in two dimensions or more. In his paper, Migdal compared his results with a yet unpublished article by Sasha Polyakov based on nonlinear interactions of Goldstone particles. The two Sashas were young stars in the extraordinary school of theoretical physics and mathematics in the Soviet Union. In particular, the Landau Institute, under the leadership of Isaac Khalatnikov, had succeeded to attract some of the most famous condensed matter theorists, mathematicians, and field theorists of the time, but the country had several remarkable institutes in Leningrad, Moscow, and all over the country. Both Sashas had played, at very young ages, with the idea of conformal invariance at criticality and designed an early formulation of conformal bootstrap, which was later recognized as equivalent to a fixed-point equation.

As a student, I had benefited from the famous Landau and Lifshitz series of books covering theoretical physics. The French version published by the official Mir editor was bound, well printed, and very cheap, but somewhat funny because the translators were not scientists! The

Landau Institute was located in Chernogolovka, which is not far from Moscow, but foreigners were forbidden to visit during the Soviet period. I discovered the institute only in the middle of the 1990s; it was a small building, with only a few half-ruined offices. So much great science had been linked to that institute, it was quite a shock to see where it came from.

Before leaving the evocation of theoretical physics in the Soviet Union, I would like to mention the Sunday seminars with the refuseniks; most of them had lost their jobs in retaliation for applying for exit visas. Initially, it was organized by Mark Azbel, who had been expelled from the Landau Institute. Under international pressure, Azbel was allowed to emigrate to Israel but, contrary to the expectations of the authorities, the Sunday seminars didn't stop. At the time I participated, during the 1980s, they took place in the apartment of Viktor Brailovsky. Once, after my talk, one of the participants asked me whether I had noticed X in attendance, and he told me, "He spies on us for the KGB." I asked why they did not kick him out and he answered, "We prefer to know who is reporting."

In the fall of 1975 with Jean Zinn-Justin, we tried to figure out, without the article yet, what Polyakov could have done (his method is now known as the nonlinear sigma model; 20). We understood its renormalizability in two dimensions and the possibility of establishing a systematic expansion in powers of $(d-2)$ of the critical properties, which is analogous to the ϵ expansion around four dimensions, whenever continuous symmetry was spontaneously broken (21). The coupling constant of the model is proportional to the temperature, which is of course a relevant parameter. Therefore, it has to be an IR-repulsive fixed point, i.e., a UV attractor, which is indeed what the nonlinear sigma model exhibits above two dimensions. The critical behavior is not always governed by IR-fixed points.

During the 1970s, one trip to Moscow sticks in my memory. In 1976, Zinn-Justin and I met the two Sashas again; we discussed sigma models beyond one loop. They mentioned a spectacular result by Lev Lipatov on the behavior of the coefficient of g^n for n large, in the beta function of $g\phi^4$ in 4D (22). Back in Saclay, we realized that indeed instanton methods could handle such problems. We applied the method to various problems in quantum mechanics and field theory (23). We knew that, within the so-called minimal subtraction scheme, it was sufficient to know the RG trajectories in four dimensions to obtain the ϵ expansion. Hence, if it was increasingly more cumbersome to compute the successive terms of perturbation theory, given the number of Feynman diagrams and the number of integrations, one could obtain an estimate of the coefficient of ϵ^n for n large, a number of the form $(-1)^n n! a^n n^b$. I presented our results in Rome at a meeting attended by Gerard 't Hooft. During the question session, he announced that he had noticed an effect of renormalization: Some individual diagrams of order n could grow like $n!$ and is now better-known as the renormalons singularity. This remark invalidated the Lipatov estimate in dimension four. I still don't know whether our large-order estimate of the ϵ expansion suffers from the same problem. In any case, Zinn-Justin and Le Guillou developed an extensive program of computation of the critical exponents using the perturbative series, transformed by summation procedures based on the large-order estimates. The safest starting point follows a proposal by G. Parisi to compute directly the perturbation series in 3D away from T_c. There the large-order estimates are not spoiled by renormalons. They obtained thereby RG values for the critical exponents that were more precise than those obtained by any previous estimate at the time (24).

MORE TOPICS

We all know that the flux of new ideas in research is not a constant of motion. Theoretical physics in the early 1970s happened to be blessed by deep paradigmatic changes, in both high-energy and statistical physics: The combination of the RG and of the renormalizability of non-Abelian gauge theories that led to the electroweak theory and QCD opened a considerable number of new routes for many of us. The world did not stop there, and my own trajectory led me to new areas, but it became much less linear and straightforward. The reader will have to forgive me for scattered and disordered recollections of collaborations that meant a lot to me.

A few articles with Cirano De Dominicis[7] allowed me to appreciate the algebraic imagination and the perseverance of Cirano, which is so manifest in his work on spin glasses. In particular in 1975, we applied, and sometimes also with Jean, the field theory techniques to the RG of critical dynamics and computed the exponents related to the theory of critical slowing down (25).

In 1978, a new postdoc joined our group at Saclay, Shinobu Hikami, with whom I have continued to collaborate for over forty years. Shinobu had done his Ph.D. in Kyoto on the $1/n$ expansion. Shinobu has a unique talent for understanding physics and mathematics, even when they seem to me very exotic, and he is a remarkable calculator. At the time, we worked with Jean on a version of the sigma model with gauge invariance appropriate to a field theory description of Anderson localization by disorder in the vicinity of two dimensions (26).

Although I'll return soon to that time in 1978, let me mention a few topics that are also part of the critical phenomena descent. In 1982, while visiting Tel Aviv University, I was wondering if finite-size scaling, a powerful method for fitting experimental and numerical data, was a mere ansatz or a consequence of the theory. I was pleased to realize that it was a straightforward consequence of renormalization theory (27). Sometime later with Zinn-Justin, we decided to demonstrate that it was indeed part of the RG theory by computing explicitly the finite-size scaling functions themselves within the standard techniques of ϵ or $1/n$ expansions (28).

I spent the summer of 1980 at the Kavli Institute for Theoretical Physics in Santa Barbara (UCSB). I was still trying naive mean-field ideas for lattice gauge models. It began with an external matrix field coupled to one plaquette. With David Gross, we solved this external field problem in the large N limit, which turned out to be a nice mathematical exercise (29). It was a generalization of the one plaquette gauge model solved a bit earlier by Gross and Witten. However, the mean-field ideas are not the right approach to gauge theories.

Critical wetting, a story full of events, started with my student, Stan Leibler. In 1983, Bert Halperin visited Paris and Saclay (followed by my visit to Harvard during the spring term of 1984). Stan was a brilliant student, interested in both performing experiments and studying the theory: He raised the question of wetting of a surface by one component of a dual mixture. We derived an effective model of delocalization at the wetting critical temperature of the interface between the two components of the mixture. The RG solution was richer than usual: Exactly soluble in three dimensions, i.e., a 2D interface, several regimes were present and the critical wetting exponents were predicted to vary continuously with the interfacial tension amplitude (30, 31). This complex scenario was soon confirmed through a functional RG approach by Daniel Fisher and David Huse (who corrected a small algebraic error in our calculation). However, soon after, Michael Shick and coworkers pointed out that we had assumed short-range forces, but in fluid mixtures long-range forces would dominate and lead to a simple mean-field behavior. Furthermore, Kurt Binder and colleagues soon after undertook a numerical simulation for an Ising-like model with short-range forces and concluded that mean-field theory was valid there, too. With Tim Halpin-Healy, a postdoc from the United States, we argued that the numerical simulation was much too limited to explore the critical region for wetting. More recent numerical simulations by Binder

[7] For many years, our late friend used the French spelling Cyrano popularized by the play *Cyrano de Bergerac*, a fictionalized account of the life of a 17th-century author. Toward the end of his life, he returned to the original Italian spelling Cirano.

and colleagues report a non-mean-field behavior. The pendulum seems to have switched back to our side, but the story might not be quite finished.

In 1984, we received an elegant article by Franz Wegner, who had considered a 2D electron gas under a strong magnetic field, thus confining the system to the lowest Landau level. Franz was concerned with the effect of scattering by impurities and, if the transport properties remained difficult to obtain, he gave a derivation of the density of states based on a diagrammatic sum over Eulerian trails (32). We discussed the problem with Claude Itzykson and David Gross, who was visiting Paris; we realized that the system had effectively a hidden supersymmetry, from which resulted a dimensional reduction to a $(d-2)$-dimensional problem (33); we then recovered easily Wegner's solution and some generalization to non-Gaussian impurities, but we failed to deal with transport properties through the same techniques.

Many other collaborations over this period meant a lot to me. With David Nelson at Harvard and André Thiaville, a student at the time, we considered the effect of fluctuations on the transition from a normal metal phase to a type II superconducting phase under a field H_{c2}, i.e., the melting of the Abrikosov lattice of vortices (34). We concluded that, contrary to the classical Landau–Ginzburg theory, fluctuations drove the transition to first order. We returned to the problem a little later that year with Ian Affleck (35), who was spending a year at Saclay.

MATRIX MODELS

In 1978, G. Parisi was visiting Paris and we discussed all kinds of subjects, which was always an amazing experience with Giorgio, who is so imaginative. Packed in a small office with Giorgio, Claude Itzykson and Jean-Bernard Zuber, we were dreaming in front of a blackboard of finding some solution for the large N limit for a theory like SU(N) gauge theory, in which the field is an $N \times N$ matrix. We knew from 't Hooft's work that the solution consisted of summing all planar Feynman diagrams. (In the well-known vector case, it consists simply of summing a geometric series of bubble diagrams, leading to an easy explicit solution.) Despairing to find any way to sum all the planar diagrams, we started wondering how many such diagrams there were. Then, we realized that this counting problem was simply given by an integral over $N \times N$ random Hermitian matrices with a weight that generalized Wigner's Gaussian ensemble to non-Gaussian terms. The large N limit was given by a saddle-point equation, which turned out to be a simple Wiener–Hopf integral equation. After this zero-dimensional success, we moved on to one dimension, i.e., quantum mechanics, for a wave function depending on N^2 matrix variables. The Schrodinger equation for invariant states, i.e., wave functions depending simply on the eigenvalues of the matrix, combined with the Vandermonde measure, reduced to a problem of independent fermions in a potential well, providing an easy solution in the large N limit. However, we did not succeed to solve the planar limit in any higher dimension. The account of this limited success resulted in a highly cited article (36). This article opened a new area, which is still quite active, for tackling problems in combinatorics by the mean of matrix integrals. However, its main use came 10 years later after the invention of matrix models for 2D quantum gravity.

EVOLUTION IN MY DUTIES

Let me add, before returning to science, a few words about my life as a teacher. I started teaching at the École polytechnique in 1974. My Saclay position did not require anything but research. For several reasons, I decided to take this part-time teaching position. The main one was probably the fear that I could go through a sterile period in my research, which of course did happen, and I thought that, through teaching I could at least be useful for something. Should I be ashamed to confess this weakness? I was struck to read in his memoirs that Richard Feynman himself was offered a position at the IAS after his Los Alamos days, and he chose Cornell University for a similar reason and because he believed that teaching contributed considerably to the maturation of his own ideas. I have liked teaching, those moments in the lectures at which one succeeds to capture the interest of the students, which one can easily apprehend in their eyes or from the quality of the attention. In fact, when I turned 68 and became emeritus, i.e., without teaching duties, I realized that I missed the students and I started accepting invitations to give several series of lectures in China, Rome, Hong Kong, and Japan, which I found quite rejuvenating.

In 1986, the physics department at the École normale supérieure (ENS) in Paris was looking for a chairman. I was perfectly happy at Saclay, but I realized that maybe a new environment, a new challenge, could be good for me. I took this position for four years, and I think that it did bring me a lot scientifically outside of my usual meadow. I've remained at the ENS since then as a Professor (emeritus now for more than 10 years). I have occasionally taken other part-time duties in my life. For instance, from 1992 to 2000 I chaired the Board of Trustees of the Centre National de la Recherche Scientifique (CNRS); at the time it was a nonexecutive position, with little leverage in between the CEO and the government. Although it was heavily time consuming, I never stopped teaching or researching during that period. I had the hope of influencing the joint organizations of CNRS and universities in a more integrated way: The CNRS supports laboratories located in universities. Such a lab has permanent members from CNRS and from the university. I thought that this defined an amount of research duties and teaching obligations that should be shared within the team. My efforts were in vain, and my life with CNRS has been largely a waste of time. I have had occasional external duties, in particular at the French Academy of Sciences. I was president-elect in 2004 when the science community was in turmoil in the whole country after years of mediocre public support for research in comparison with other European developed countries. Convinced that the Academy should support the "save research" movement, I ended up as a rapporteur at a general assembly in Grenoble in 2004. In the end, this popular action improved the situation in our labs for a while.

I have had all along a belief in science for peace, which some would call naive. I have worked for years with the Israeli–Palestinian science organization, which has supported projects led by principal investigators on both sides. I am still a member of the scientific council of the International Centre for Theoretical Physics in Trieste, which has done so much on behalf of developing countries, and I am still involved with the Cyprus Institute, an international research organization with strong regional significance.

MORE ON MATRIX MODELS

In 1985, several researchers (37–39) realized that integrals over random matrices could be a representation of discretized 2D gravity. In two dimensions, Einstein's theory is very simple because, from the Gauss–Bonnet theorem, the integral of Riemann's curvature over the manifold is a topological invariant related simply to the number of holes in the 2D manifold. So, classically the theory is trivial, but quantum mechanically the genus of the manifold is a fluctuating dynamical parameter. One knew that one can enumerate the discrete triangulations of a random surface of genus h as the coefficient of $1/N^{2h}$ of the expansion in powers of $1/N$ of a non-Gaussian integral over $N \times N$ random matrices. A discretized version of a continuum theory is normally meant for numerical treatment. However, for 2D gravity it turned out that it led to analytic solutions. In the fall of 1989 with Volodya Kazakov, a brilliant former student of Sasha Migdal in Moscow, who had recently joined the ENS as a permanent professor, we considered the scaling limit of continuous triangulations of random surfaces of arbitrary genus (the theory of bosonic closed strings in Polyakov's formulation). We were quite pleased to find the analytic solution to the continuum limit; our work was ready to be sent to a journal when one Friday evening, we received by fax a preprint along similar lines by Gross and S. Migdal (TeX and e-mail were not yet in use in physics circles). We were quite depressed to have been scooped until we realized that we had a disagreement with their string equation. So, we rushed to the only post office open in Paris on Sundays to send our manuscript to *Physics Letters*. In fact, a third group working at Rutgers, Michael Douglas and Steve Shenker, had almost simultaneously pointed out to their Princeton colleagues this difficulty, which they had soon corrected, and finally the three papers were published almost simultaneously (40–42). A period of intense activity on the so-called matrix model approach to quantum gravity followed, during which I enjoyed working with Kazakov, Douglas, Shenker, Herbert Neuberger, Enzo Marinari, Parisi, Alyosha Zamolodchikov, and Hikami.

Indirectly, the burst of papers in this area led to the arXiv: Joanne Cohn, a postdoc at the IAS at the time, maintained an electronic list for sharing the flood of preprints in the field. In 1991, Paul Ginsparg proposed to help, and he invented the automated system for sharing e-prints, the arXiv, which has so deeply changed life for the whole science community. In particular, it has saved the lives of many poorer institutes who could not afford the price of journals, as for instance Eastern European countries after the fall of the Berlin Wall.

I have spent after that many years studying random matrices. We did a lot of work with Tony Zee when he visited the ENS. I was happy when we succeeded in showing that the level spacing distributions were universal (43), i.e., independent of the probability distribution if it were, instead of a mere Gaussian, an invariant exponential of some polynomial in the matrix (this had been conjectured by Wigner and Dyson). My collaborations with Hikami have extended over nearly forty years, and they are still going on with multiple reciprocal visits to Paris or Japan. We have done a lot of work on the use of random matrices for problems in the geometry of curves on Riemann surfaces. This work is too mathematical to be discussed here, but we were often quite excited by our findings. We have summarized some of them in a small book (44).

STATISTICAL PHYSICS VERSUS FIELD THEORY NOWADAYS

I have written of my own scientific life up to the present. It opens only a small window toward the wealth of interactions between a priori disconnected areas of knowledge such as statistical mechanics and quantum field theory. Let me underline other areas that have been and are still tremendously important, which justify in my mind an education without blinkers. One of the most influential articles ever on field theory and statistical physics was written in 1984 by Belavin, Polyakov & Zamolodchikov (45). Let me quote the first sentence:

> Conformal symmetry was introduced into quantum field theory about twelve years ago due to the scaling ideas in the second-order phase transition theory. (45, p. 333)

The descent of this work is everywhere from string theory to statistical mechanics. Recently, Slava Rychkov and coworkers have shown that conformal symmetry, plus ideas from particle physics unitarity and crossing symmetry, led to a conformal bootstrap, i.e., a way to corner critical exponents in models such as Ising in three dimensions, which is more precise than any previous method, including the RG (46, 47).

Any reader who has reached this line has noticed that, unforgivably, I have failed to mention a number of wonderful topics. Of course, it is not simply that my memory is evaporating. I feel disappointed in myself for not having been able to contribute to beautiful subjects such as topology in condensed matter. The increasing importance of topological concepts over the years has been finally acknowledged by the 2016 Nobel Prize to Duncan Haldane, Michael Kosterlitz, and David Thouless for their discoveries of topological phase transitions and topological phases of matter. I haven't touched the subject of quantum phase transitions on which, from Subir Sachdev to Alexei Kitaev, there is so much exciting work nowadays. It is therefore without any hesitation that I would recommend to a contemporary theory student to engage in a path that would strive to maintain a broad view over the horizon.

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