Personal recollections about the first three years of string theory

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I first want to warmly thank the organizers for inviting me to talk at this conference, although I found it a bit difficult at first to become part of history while still alive...

Since this conference is an interdisciplinary exercice between philosophy and science, for the philosophers the message I would like to convey through this talk is the following: Although at the level of published work the evolution of scientific knowledge is generally rather smooth and *a posteriori* natural, upstream of the discoveries the process appears to me rather erratic in the details on who actually makes such or such discovery and when, depending on sometimes strange coincidences. When I look back at my involvement in the subject, this is what strikes me most. And over the decades, I have witnessed several other examples of such coincidences. So, this talk is in some sense the opposite of Freeman Dyson's article[1] "Missed opportunities" in which he describes contributions he did not make because such coincidences which in all likelihood should have occurred actually did not occur.

In 1968-1969, I was working with Joël Scherk on our research work for our Ph.D. in Orsay under the guidance of Claude Bouchiat and Philippe Meyer. The subject was electromagnetic and final state interactions corrections to non leptonic kaon decays. We were classmates in our last year as students at the École Normale and good friends. We enjoyed a lot working together. While we were finishing our thesis work, we got much interested in the explosion of activity which followed the original Veneziano paper[2], together with Claude Bouchiat and Daniele Amati, who was spending a sabbatical year in Orsay. We were particularly attracted by the mathematical beauty which we felt lying in this new structure. For example the changes of variables which guarantees the cyclic symmetry of the multiperipheral representation of the N-particule generalization[3] of the Veneziano formula. Or the proposal by Kikkawa, Sakita and Virasoro[4] (who gave a seminar in Orsay that year) to go beyond the narrow resonance approximation. We were puzzled by the exponential divergence which was discovered[5] in the loop diagrams when the correct level structure was taken into account, but not pessimistic like other physicists who considered that divergence natural (and fatal) for a theory with such an exponentially growing particle spectrum and arbitrarily high spins.

Now, for the year after, we were both much interested in going to the States, and continue working together. There was one fellowship in Princeton for a former student of the École Normale (endowed by Procter of Procter and Gamble). We knew about the existence of NATO fellowships, but General de Gaulle had just pulled France out of NATO, so we thought we were ineligible. Not true: France had only left the military part of NATO, not the cultural part. This we discovered totally by chance during a train ride back from Orsay to Paris. We happened to be seated facing two scientists discussing precisely the stay in the States which one of them had just done with a NATO fellowship. When we asked him about that, he gave us this information together with the address where to apply. This is the first coincidence. Result: I got the Procter fellowship and Joël a NATO fellowship and we were both set for Princeton. At that time, I had already heard (in very positive terms) about Pierre Ramond from Jean Nuyts (then in Orsay), with whom he had already signed the papers (without having met, if I remember) on crossing symmetric partial waves amplitudes which formed the basis of his Ph.D. in Syracuse with A.P. Balachandran as adviser.

With the Procter fellowship came a Fulbright travel grant. Having the choice, I chose the ship "France" for my first transatlantic crossing. The ship had a small and pleasant library with a few desks. I was spending many hours there, studying in detail the latest preprints on dual resonance models, as they were called. Now for the second coincidence: One afternoon, leaving all my material spread on the desk, I walked out of the library, called by an urgent need. . . Precisely during these two minutes when I was absent, Pierre walked in, and looked around for a vacant desk. There appeared to be only one, mine. He walked up to it, realized that it was not really vacant, but was shocked to see on it the Fubini-Veneziano paper[6] on the factorization of dual resonance models, the very same paper he was studying at that moment! He quickly went back to his cabin to make sure that what he had just seen was not his copy! Reassured about his sanity, he came back to the library, wondering on the way about who could be the fellow interested in such an esoteric topic. By which time, I, too, was back. You can imagine easily the next hours. This is how we became friends. After spending his summer vacation in France, he was on his way to the National Accelerator Laboratory (now called Fermilab) for his first postdoc. Together with Louis Clavelli and David Gordon, also postdocs, they formed the entire theory division of Fermilab.

In Princeton, Joël and I immediately realized that being an alumnus of the École Normale did not mean much, which was rather stimulating! We ended up sharing a corner of the attic of the old Palmer Lab, and it was a great luck, at least for me, that we were two together to face this relative solitude. We quietly pursued our collaboration on dual resonance models. After a few weeks, thanks to our mathematical training and to the properties of elliptic functions, we had understood how to handle the superficially catastrophic divergences of the planar one loop diagrams of the theory. During the afternoon tea time of the physics department, we could see by what they were writing on the blackboard that David Gross and John Schwarz were also interested in these divergences, and we were amused to see them trying things which we had tried much before and knew could not work. When we showed them what we had found, our situation improved dramatically: they proposed that we should work all four together, we were treated as colleagues, and we moved to a nice office in the brand-new Jadwin Hall.

I was chosen by the flip of a coin to present our results at the weekly joint informal seminar of the University and the Institute for Advanced Study a few weeks later. When I wrote the famous formula for the Jacobi imaginary transformation applied to the partition function (in a form that would make it as impressive as possible: a young postdoc of 23 had to impress the big names in the audience!):

$$f(w) \equiv \prod (1-w^n)^{-1} = w^{1/24} \left(-\frac{\mathrm{Ln} w}{2\pi}\right)^{1/6} \exp\left(-\frac{\pi^2}{6\mathrm{Ln} w}\right) f\left(\exp\left(\frac{4\pi^2}{\mathrm{Ln} w}\right)\right),$$

Barry Simon couldn't refrain from exclaiming: "This is impossible!" Coming from him, this gives you an idea of the state of our mathematical knowledge in those days…But it is clear that the electrostatic analog of the Koba-Nielsen[7] formula ment that it was only a matter of weeks before somebody else would have discovered these elliptic functions in dual loop amplitudes and their consequences.

After that year, Joël went back via Berkeley to Orsay where he made his very important contribution about the zero slope limit of dual amplitudes, showing that the model after all shared all the good properties of quantum field theory, and more. As for myself I obtained the same NATO fellowship to spend another year in Princeton. In the fall, we received Claud Lovelace's preprint[8] with the first appearance of the critical dimension 26, but, like everybody else knowing Claud, we did not take that point seriously! Simultaneously, after many other people, John Schwarz and I got interested in the problem of introducing spin 1/2 in the dual model. We came across the Bardakçi-Halpern[9] paper on their attempts to build ghost-free models with fermions. In retrospect a very interesting paper containing what I believe are among the first if not the first examples of affine Lie algebras in the Sugawara construction. A pionneering paper, much too sophisticated for John and me, and we put it aside as too complicated for us.

Meanwhile, Pierre Ramond and I had kept in touch regularly. At Fermilab, he had been asked to work on the design of the sewers for the main building. He refused, with consequences 1) that his three-year contract was not extended, 2) that he discovered the Ramond model[10]. He sent me that paper personally, which turned out to be very important. Indeed, in the Princeton University Physics department, there was no preprint library. No need was felt for it: All the important people there received the important preprints themselves, and then spread around the important news. Would a short and partly speculative paper by a still relatively obscure postdoc at Fermilab have been considered important? And reached me? Perhaps, but probably after too long. So, it was most fortunate that I had met Pierre on the ship! With John we quickly discovered that from a fermion line emitting three pseudoscalar "pions", we could factorize the first pole in the fermion-antifermion channel and obtain the Lovelace-Shapiro[11] formula. Since this meant bosonic trajectories with both integer and half-integer intercepts, it was not hard to introduce half-integer anticommuting modes and it took us only three or four weeks from there to build the bosonic sector of the Neveu-Schwarz-Ramond model. We were really naive; at first, we had not even clearly realized that a symmetry algebra larger than the Virasoro algebra was needed to get rid of the ghosts introduced by the new anticommuting modes. We were just lured by the elegance of the superconformal algebra and went ahead without further thinking. And this was most fortunate for us. How the superconformal algebra kills the ghosts was at first too clever for us and we had to discover "experimentally" that they were absent before we could understand the ghost-killing mechanism.

Then, around Easter, stopping on the way in Fermilab to visit Pierre, I went to Berkeley, where Miguel Virasoro was. There, I met Korkut Bardakçi, Marti Halpern, Stanley Mandelstam, Charles Thorn, Mike Kaku, who all made my stay most enjoyable, and so I made new lifelong friends. I take this opportunity to thank them in public for their warm welcome. I told them about our model, and with Charles[12] we found the way the ghosts were eliminated through the introduction of the "F_2 formalism". Marti Halpern showed me the thick pile of notes about his and Korkut's attempts at introducing spin. Indeed, the Neveu-Schwarz vertex for "pion" emission appeared rather early in those notes, but they discarded it. They were after a Virasoro algebra enlarged with commutators as the ghost-killing mechanism, not a superalgebra. Supersymmetry did not exist then. However, by the time of my visit, they had become aware of Pierre's paper, and by conversations I had with Charles, it is clear to me that it would have been only a matter of weeks before they would have discovered that that vertex worked after all!

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