Reminiscences of Collaborations with Joël Scherk

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Abstract

I had the privilege of collaborating with Joël Scherk on three separate occasions: in 1970 at Princeton, in 1974 at Caltech, and in 1978-79 at the Ecole Normale Supérieure. In this talk I give some reminiscences of these collaborations.

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1 Our First Meeting

In 1969 my duties as an Assistant Professor in Princeton included advising some assigned graduate students. The first advisees who came to see me were a pair of Frenchmen, André Neveu and Joël Scherk. I had no advanced warning about them, and so I presumed they were just another pair of entering students. In fact, they had achieved the equivalent of a Ph.D. in France and were attending Princeton on some special Fellowships. Because their degrees were not called PhDs, the Princeton bureaucracy classified them as graduate students, and so they were assigned to me.

At our first meeting, I asked the usual questions: “Do you need to take a course on electrodynamics?”, “Do you need to take a course on quantum mechanics?”, etc. They assured me that they already had learned all that, and it wouldn’t be necessary. So I said okay, signed their cards, and they left.

2 Loop Amplitudes

Veneziano discovered his famous formula for a four-particle amplitude in 1968 [1]. In 1969 various groups constructed $N$-particle generalizations of the Veneziano amplitude [2] - [6] and showed that they could be consistently factorized on a well-defined spectrum of single-particle states as required for the tree approximation of a quantum theory [7] - [10]. In those days the theory in question was called the dual resonance model. Today we would refer to it as the bosonic string theory. Knowing the tree approximation spectrum and couplings, it became possible to construct one-loop amplitudes. The first such attempt was made by Kikkawa, Sakita, and Virasoro [11]. They did not have enough information in hand to do it completely right, but they pioneered many of the key ideas and pointed the way for their successors. Around this time (the fall of 1969) I began studying one-loop amplitudes in collaboration with David Gross, who was also an Assistant Professor at Princeton. (We had collaborated previously when we were graduate students at Berkeley.)

A couple months after our first meeting, Joël and André reappeared in my office and said that they had found some results they would like to show me. They proceeded to explain their analysis of the divergence in the planar one-loop amplitude. They realized that by performing a Jacobi transformation of the theta functions in the integrand they could isolate the divergent piece and propose a fairly natural counterterm [12]. I was very impressed by this achievement. It certainly convinced me that they did not need to take any more quantum mechanics courses! Essentially the same calculation was carried out independently and simultaneously by Susskind and Frye [13]. The modern interpretation of

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the result is that viewed in a dual channel there is a closed string going into the vacuum. The divergence can be attributed to the tachyon in that channel, and its contribution is the piece that they subtracted. The same result can also be obtained by analytic continuation techniques. This interpretation explains why in a model without tachyons such divergences would not occur. The cancellation of the milder divergences due to dilaton tadpoles that can appear in superstring theories became an important consideration in later years.

Since Neveu and Scherk were working on problems that were closely related to those that Gross and I were studying, we decided to join forces. One of the important things that the four of us discovered \cite{15} was that the nonplanar loop amplitude contains not only the expected two-particle threshold singularities but also new and unexpected singularities. This was also discovered independently by Frye and Susskind at about the same time \cite{16}. In both of these works the dimension of spacetime was assumed to be four, and the Virasoro subsidiary constraints were not implemented on the internal states circulating in the loop. As a result the singularities were found to be unitarity-violating branch points. We wanted to identify the leasing Regge trajectory associated to these singularities with the Pomeron, since they carried vacuum quantum numbers, but clearly something wasn’t quite right.

About a year later Lovelace observed that if one allows the spacetime dimension to be 26 and supposes that the subsidiary conditions imply that only transverse oscillators contribute, then instead of branch points the singularities would be poles \cite{17}. As we now know, these are the closed-string poles in the nonplanar open-string loop. This calculation showed that unitarity requires that one choose the critical dimension and the intercept value for which the Virasoro conditions are satisfied. It was supposed that the unphysical Regge intercept value of two – implying the existence of a massless spin two particle as well as a tachyon – would somehow be lowered to the desired Pomeron value of one in a more realistic model.

As I recall, Lovelace’s paper was quite a shock to everyone, since until then nobody considered allowing the dimension of spacetime to be anything other than four. We were doing hadron physics, after all, and four was certainly the right answer. Before long, most people were convinced that this theory required 26 dimensions. However, it had a tachyon and no fermions, so it was unphysical anyway. It was natural to suppose that a better theory would require four dimensions. We did find a better theory soon thereafter. However, it turned out to require ten dimensions, not four.

3 String Theory for Unification

In 1971 Scherk returned to Paris, and in 1972 I moved to Caltech. At Caltech, Murray Gell-Mann put ample funds at my disposal to invite collaborators of my choosing. One of
them was Joël Scherk, who spent the first half of 1974 visiting Caltech. I am certainly happy that he did!

Prior to coming to Caltech, he had visited NYU, where he had written a very elegant review of string theory [19].

The hadronic interpretation of string theories was plagued not only by the occurrence of massless vector particles in the open-string spectrum, but by a massless tensor particle in the closed-string spectrum, as well. Several years of effort were expended on trying to modify each of the two string theories so as to lower the leading open-string Regge intercept from 1 to 1/2 and the leading closed-string Regge intercept from 2 to 1, since these were the values required for the leading meson and Pomeron Regge trajectories. Some partial successes were achieved, but no fully consistent scheme was ever developed. Furthermore, efforts to modify the critical spacetime dimension from 26 or 10 to four also led to difficulties.

By 1974, almost everyone who had been working on string theory had dropped it and moved to greener pastures. The standard model had been developed, and was working splendidly. Against this backdrop, Joël and I (stubbornly) decided to return to the nagging unresolved problems of string theory. We felt that the theory has such a compelling mathematical structure that it ought to be good for something. Before long our focus shifted to the question of whether the massless spin two particle in the spectrum interacted at low energies in accordance with the dictates of general relativity, so that it might be identified as a graviton. Several years earlier Joël and André Neveu had studied the massless open-string spin one states and showed that in a suitable low-energy limit they interacted precisely in agreement with Yang–Mills theory [20, 21]. Now we wondered about the analogous question for the massless spin two closed-string ground state. Roughly, what we proved was that the theory has the gauge invariances required to decouple unphysical polarization states. Then it followed that the interactions at low energy must be those of general relativity.

Once we had digested the fact that string theory inevitably contains gravity we were very excited. We knew that string theory does not have ultraviolet divergences, because the short-distance structure is smoothed out, but that any field-theoretic approach to gravitation necessarily gives nonrenormalizable ultraviolet divergences. Evidently, the way to make a consistent quantum theory of gravity is to posit that the fundamental entities are strings rather than point particles [22]. Adopting this viewpoint meant that the length scale of string theory had to be identified with the Planck scale rather than the QCD scale, which represented a change of almost 20 orders of magnitude. So, even though the mathematics was largely unchanged, this was a large conceptual change.

Convinced of the importance of this viewpoint, we submitted a short essay summarizing the argument to the Gravity Research Foundation’s 1975 Essay competition [24]. Regret-
tably, the judges were not impressed. (We received ‘Honorable Mention’.)

Having changed the goal of string theory to the problem of describing quantum gravity rather than hadronic physics, it became natural to suppose that the Yang-Mills gauge interactions that string theory also contains should describe the other forces. This means that one is dealing with a unified quantum theory – an explicit realization of Einstein’s dream. (Actually, it’s not clear that Einstein wanted his unified theory to be quantum, but that’s another story.) Moreover, the existence of extra dimensions could now be a blessing rather than a curse. After all, in a gravity theory the geometry of spacetime is determined dynamically, and one could imagine that the extra dimensions form some kind of compact space (spontaneous compactification). We attempted to construct a specific compactification scenario in a subsequent paper \[25\]. From today’s vantage point, that work looks rather primitive.

In Japan, Yoneya independently realized that the massless spin two state of string theory interacts in accordance with the dictates of general relativity \[26\]. I would claim, however, that Scherk and I were only ones to take the next step and to propose seriously that string theory should be the basis for constructing a unified quantum theory of all forces. In any case, the recognition of that possibility represented a turning point in my research. I found the case convincing and was committed to exploring the ramifications, which is what I have been doing ever since.

I still do not understand why it took another decade until a large segment of the theoretical physics world became convinced that string theory was the right approach to unification. (There were a few people who caught on earlier, of course – most notably Lars Brink, David Olive, and Michael Green. I also received encouragement throughout this period from Murray Gell-Mann. By the early 1980s Edward Witten and Bruno Zumino were also very supportive.) One of my greatest regrets is that Joël was not alive to witness the impact that this idea would eventually have.

4 Spacetime Supersymmetry

Supersymmetry arose in string theory and in field theory separately. However, for the first several years, the supersymmetry considered by string theorists only pertained to the two-dimensional world-sheet theory. I will not review that story here, since I will be speaking about it at a conference in Minnesota in October. In any case, Joël and I became interested in supersymmetry theories, and we each did some work on supersymmetric field theories. One work by the two of us and Lars Brink was the first construction of supersymmetric Yang-Mills theories in various dimensions \[27\]. When this work was done, Brink and I were
at Caltech and Scherk was in Paris. We communicated by mail. (Email was not an option.) We found that the requisite gamma matrix identity required by these theories $\gamma^m_{(ab} \gamma^m_{cd)} = 0$ could be satisfied in dimensions 3, 4, 6, and 10. (The fact that the number of transverse dimensions is 1, 2, 4, or 8 suggests a connection with the number fields: real, complex, quaternionic, octonionic. However, the precise way this should work is not clear even today.)

At about the same time as the work by Brink, Scherk, and me, Scherk also did some very important work with Gliozzi and Olive [28, 29]. It gave further explanation of the results about super Yang–Mills theory, and took the next major step. This was to conjecture a connection with the RNS string theory. Specifically, they noted that if the spectrum of the 10-dimensional theory was restricted in a specific consistent way (GSO projection) that then the number of bosons and fermions at each mass level would be exactly the same. Moreover, since the spectrum contains a massless gravitino in the closed string sector this truncation is essential for consistency. This constituted powerful evidence (though not a proof) that the GSO-projected RNS string has local 10-dimensional spacetime supersymmetry. I was very delighted by this result. Finally we had a theory that was tachyon-free and seemed to make sense as a starting point for a unified theory. I was committed to exploring it more deeply.

There was much that needed to be learned before it would be possible to really dig into superstring theory. For example, one needed to understand supergravity, which was just starting to be developed. One of the most beautiful results ever found in supergravity was the action of 11-dimensional supergravity by Cremmer, Julia, and Scherk [30]. That said, I must admit that when it first appeared I was a bit baffled. My problem was that I knew that supergravity could not give a consistent quantum theory by itself. So I felt that the only supergravity theories worth studying were those that might arise from string theories at low energy. However, since superstring theory only allowed ten dimensions, this did not seem to leave a role for 11-dimensional supergravity. I viewed it as a 10% error. As we now know, what I failed to consider was the possibility that in (Type IIA) superstring theory an 11th dimension might be generated nonperturbatively!

5 Supersymmetry Breaking

I spent the Academic Year 1978–79 visiting the Ecole Normale, on leave of absence from Caltech. I was eager to work with Scherk on supergravity, supersymmetrical strings, and related matters. He was struggling with rather serious health problems during that year, so he wasn’t able to participate as fully as when we were in Caltech five years earlier, but he was able to work about half time. On that basis we were able to collaborate successfully and to obtain some results that I think are interesting.
After various wide-ranging discussions we decided to focus on the problem of supersymmetry breaking. We wondered how, starting from a supersymmetric string theory in ten dimensions, one could end up with a nonsupersymmetric world in four dimensions. The specific supersymmetry breaking mechanism that we discovered can be explained classically and does not really require strings, so we explored it in a field theoretic setting. The idea is that in a theory with extra dimensions and global symmetries that do not commute with supersymmetry ($R$ symmetries and $(-1)^F$ are examples), one could arrange for a twisted compactification, and that this would break supersymmetry. For example, if one extra dimension forms a circle, the fields when continued around the circle could come could back transformed by an $R$-symmetry group element. If the gravitino, in particular, is transformed then it acquires mass in a consistent manner. We called this type of local supersymmetry breaking “spontaneous”. Whether this is an appropriate use of that term can be debated. The important thing is that the symmetry breaking is consistent and sensible.

An interesting example of our supersymmetry breaking mechanism was worked out in a paper we wrote together with Eugen Cremmer [33]. We started with maximal supergravity in five dimensions. This theory contains eight gravitinos that transform in the fundamental representation of a USp(8) $R$-symmetry group. We took one dimension to form a circle of radius $R$, and examined the resulting four-dimensional theory keeping the lowest Kaluza–Klein modes. The supersymmetry-breaking $R$-symmetry element is a USp(8) element that is characterized by four real mass parameters, since this group has rank four. These four masses give the masses of the four complex gravitinos of the resulting four-dimensional theory. In this way we were able to find a consistent four-parameter deformation of $\mathcal{N} = 8$ supergravity. (Most good ideas from previous years have reappeared, sometimes in new guises, in modern superstring theory. This construction is an example of one that has not reappeared yet, so I wonder if there may be an opportunity here.)

Even though the work that Scherk and I did on supersymmetry breaking was motivated by string theory, we only discussed field theory applications in our articles. The reason I never wrote about string theory applications was that in the string theory setting I could not see how to decouple the supersymmetry breaking mass parameters from the compactification scales. This was viewed as a serious problem because the two scales are supposed to be hierarchically different. In recent times, people have been considering string theory brane-world scenarios in which much larger compactification scales are considered. In such a context our supersymmetry breaking mechanism might have a role to play. Indeed, quite a few authors have explored various such possibilities in recent times.
6 Conclusion

When I left Paris in the summer of 1979 I visited CERN for a month. There I began a collaboration with Michael Green. During that month we began to formulate a plan for exploring how the spacetime supersymmetry identified by Gliozzi, Scherk, and Olive is realized in the interacting theory. However, it took almost a year for us to achieve definitive results. Therefore, in the fall of 1979, when I spoke at a conference on supergravity that was held in Stony Brook, I reported on the work that Scherk and I had done during the preceding year [34]. At the same conference, Jöel gave a talk entitled From Supergravity to Antigravity [35]. He was intrigued by the fact that in string theory graviton exchanges are accompanied by antisymmetric tensor and scalar exchanges that can cancel the gravitational attraction. I felt that the use of the term antigravity was a bit too sensational. In any case, nowadays we understand that the effect he was discussing is quite important. For example, parallel BPS D-branes form stable supersymmetric systems precisely because the various forces cancel.

The Stony Brook conference was the last time that I saw Jöel. I was very shocked and saddened a few months later when I was informed that he had passed away. As you know, the ideas that he had helped to pioneer have been very influential in the subsequent development of our field. It is a pity that he was not able to participate in these developments and to enjoy the recognition that he would have received.
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