String from Veneziano model

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Abstract

This article is about my memories from the discovery[1, 2, 3, 4] that the Veneziano model[5] described in fact interacting strings. I came to the understanding that the dual or Veneziano model is really a model of strings independently of Susskind[3] and Nambu[4]. A characteristic feature of my approach [1, 2] was that I used thinking of very high order “fishnet” or planar Feynman diagrams as the way of at first describing the development of the strings. A chain of constituents lead to planar diagrams dominating when only neighbours on the chain interact significantly. The article also mentions the works of Ziro Koba’s and mine[10, 12, 13] about extending the Veneziano model first to five external particles - as Bardakci and Ruegg[6], Chan Tsou[8] and Goebel and Sakita[9] also did - and subsequently to an arbitrary number \( n \) of external mesons.

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1 Introduction

In a roughly published preprint[1, 2] with the title “An almost physical interpretation of the $n$-point Veneziano model” I - independently of Nambu [4] and Susskind[3] - was one of those who proposed that the dual model or Veneziano model[5] was indeed a model describing the scattering of strings against each other. In fact there was even an earlier preprint version in which the word “almost” was not in the title.

It is the purpose of the present contribution to describe my recollections as to how I came to understand that the Veneziano model is a model describing the scattering of strings. The background that leads to the string idea was the works that developed dual models first of all to an arbitrary number external particles together with Ziro Koba[10, 12, 13]. This work is described in Section3. It would, however, be natural in such a reminiscence to first describe what for me was very crucial, namely the seminar by Hector Rubinstein given at the Niels Bohr Institute. Hector was clearly very excited about the very great discovery of Veneziano. These earliest memories will be presented in 2. The line of thinking that ended with the insight that the Veneziano model describes interacting strings is described in Section 5. In my case this insight came from using my pet idea of very high order Feynman diagrams for the purpose of studying strong interactions and is presented in section4. I would also like to write about my recollections of the discussions, section 5.1, with people at various conferences such as the Lund Conference and subsequently my correspondence with David Fairlie.

2 On the Veneziano model

Historically Veneziano [5] started by formulating a scattering amplitude for the four mesons $\pi^+, \pi^0, \pi^-$ og $\omega$. This means then that by the analyticity properties of the Mandelstam representation it should actually be useful for the description of the following physical scattering processes

- 1

$$\pi^+ + \pi^0 \rightarrow \pi^+ + \omega$$

- 2

$$\pi^- + \pi^0 \rightarrow \pi^- + \omega$$

- 3

$$\pi^+ + \pi^- \rightarrow \pi^0 + \omega$$

- 4 and then the decay process for the unstable $\omega$ to the three pions

$$\omega \rightarrow \pi^+ + \pi^- + \pi^0$$

The above list of processes can be extended by taking into account time reversal symmetry which for example implies the process $\pi^+ + \omega \rightarrow \pi^+ + \pi^0$ given the process $\pi^+ + \pi^0 \rightarrow \pi^+ + \omega$. 
2.1 (Introduction to the) Mathematical Properties of the Veneziano Model Amplitude

The monumental problem that Gabriele Veneziano succeeded in solving was to find a mathematical function of the Mandelstam variables $s$, $t$ and $u$ (of which only two are needed, since the third one is just a linear combination of the other two) with the property that there were no other singularities than the poles corresponding to the resonances connected with the Regge poles. Furthermore, in his construction the asymptotic behavior was as it should be according to the theory of Regge poles.

Now it must be pointed out that these invented Regge poles were not at first known experimentally although one already knew about the $\rho$-meson Regge-trajectory. Prior to Venezianos breakthrough, other physicists had used the idea of inventing further trajectories (the $\rho$-trajectory was the only one known experimentally) in order to get a self consistent amplitude with the combined Regge exchange and Regge resonance properties. The work preceding Veneziano must have been an extremely important source of inspiration for the Veneziano model. Veneziano then went on to invent infinitely many Regge trajectories.

Let me also explain that the first case for which Veneziano wrote down the “Veneziano model” had in fact been considered by others in their efforts to have consistency of the exchange in various channels. However Venezianos first case choice was a very clever because the isospin properties of the amplitude ensures that the amplitude must be totally antisymmetric under permutations of the four momenta of these three pions. (The pions are isovectors that must couple to the $\omega$ meson isosinglet).

Venezianos choice is implemented by contracting the (totally antisymmetric) Levi-Civita symbol $\epsilon_{\mu\nu\tau\sigma}$ with three of the linearly independent four-vectors - i.e., three out of the four external four momenta - and the polarization four vector for the $\omega$-particle which is a spin one meson that has its polarization described by a four vector $\epsilon^\mu$. This means that the scattering amplitude must contain a factor

$$\epsilon_{\mu\nu\tau\sigma}p_\nu^\mu p_\tau^\sigma p_\sigma^\pi.$$  \hspace{1cm} (5)

It is this unavoidable factor that we seek to multiply by a function of the Mandelstam variables $s$, $t$, and $u$. It is upon this function that analyticity properties are imposed in such a way as to ensure having only the poles connected with the Regge trajectories as speculated upon in the Veneziano model. It was the discovery of this non-trivial function of the Mandelstam variables multiplying (5) - i.e., the surprisingly simple Veneziano model function (given in fact by the Euler Beta function) - that was the great progress made in this Veneziano model.

We may regard this idea of considering a process in which there was only the possibility for just one factor (5) as just a detail that was used for getting a simple problem with only one amplitude as a function of the Mandelstam variables. One could alternatively have chosen to consider simply scalar particles, without isospin complications scattering on each other. The latter was the technology used by Koba and me and also by other groups including Chan Tsou, Bardakci-Ruegg, Sakita, etc\cite{6, 8, 9} to get a simple problem appropriate for looking for a generalization to five and more external particles of the
Veneziano model. But by choosing scalars one departs from the real hadron scatterings of the most important particles. The lowest mass mesons are indeed pseudoscalars and vector mesons and they have mostly nontrivial isospins.

After the extraction of the factor (5) the remaining factor in the amplitude should be symmetric under the permutation of the three external $\pi$-mesons. This means in reality that Veneziano had to make this remaining factor be a sum of three terms coming from each other by the permutation of these $\pi$-mesons. But the really interesting question was how these three terms looked one by one. In fact each of the three terms has only Regge poles in two out of the three “channels” $s$, $t$ and $u$. So for example it could be that one of the terms had resonances say only in the $s$- and the $t$-channel while there were no resonances corresponding to particles in the $u$-channel. It might be good to have in mind that an arrangement with the assymetry that only the $t$-channel but not the $u$-channel has resonances would be impossible if there were in the $s$-channel only resonances with odd spin as would at first be phenomenologically suggested by considering the $\rho$-meson Regge trajectory in the $s$-channel. That would namely suggest that there would only be a sign difference when we permute the $t$- and the $u$-channels. However, there is often the phenomenon of “exchange-degeneracy” meaning that there exist another trajectory with resonances of even spin together with the one with odd spins. By interference of the two trajectories with respectively even and odd spins it becomes possible to arrange the asymmetry between the $s$- and the $u$-channels. So for the Veneziano model it was rather important to have this exchange degeneracy.

The reason I have mentioned these problems of needing to assume exchange degeneracy and to have the several amplitudes summed up for symmetry is because I believe that this was one little technical detail needed to get a relatively simple solution when seeking to generalize the Veneziano model to an arbitrary number of external particles.

This gives me then an excuse for recollecting my cand scient thesis in which I as a main theme worked on studying the self consistency of three meson vertices described in the quark model. This may have strongly inspired me to think of the idea of processes in which the mesons are assumed to be composed of different pairs of quark and antiquarks just so arranged as to guarantee that there would only be resonances of non-exotic mesons in some of the channels: for example in the $s$- and $t$-channels but not in the $u$-channel as in the case just described.

3 Developing dual models with arbitrary number of external particles with Ziro Koba

At the time that Hector gave his for me sensational seminar on the Veneziano model[5] I was rather pressed to finish my cand. scient thesis[14]. Nevertheless I managed to include a figure about the Veneziano model in the thesis. However, first after I had finally delivered the thesis and could do no more about it did I become so relaxed that I could begin to concentrate on the Veneziano model. The first work I did concerning the Veneziano model was done together with Koba. We did the natural generalization to a five point function. The Veneziano model was only for a four point scattering amplitude.

What was needed was an integral formula over two independent variables that should
then obey some algebraic relations so as to insure the wanted pole and Regge pole behavior at the desired places in the amplitude as a function of the Regge pole variables $\alpha(s_{ij})$. These $\alpha(s_{ij})$-variables were put into the exponents of the dependent variables $u_{ij}$. Here I like to stress a recollection of a little accident that may in fact have been of great significance for me in getting into the project in a fruitful way. At that time I taught as an instructor - as I also had done during my “eternal studies - a third year course in geometry - especially projective geometry - at the mathematics department. In that course we had a problem about a pentagon in the projective plane in which the students and the instructor had to prove a series of algebraic relations between anharmonic ratios for this pentagon. Interestingly enough these relations were exceedingly well suited for getting the relations between the dependent variables that could give the right properties for the generalized dual model for the scattering of five scalar particles analogous to the Veneziano model four point function. At that time the most important for my physics studies and research was the relatively regular meetings with Ziro Koba and some younger people in a study group. At these meetings we presented ideas as they came to us. Koba was indeed very good in inspiring the students to be active. I remember also that he once “uninvited me to participate in order that an even younger group of students should feel more free to express themselves without feeling embarrassed by the presence of slightly more advanced people.

In generalizing from the five to the n-point case I think that I first attempted something like the generalization of the pentagon formulation. There is indeed such a possibility, but it is more complicated than the simplified version which Koba discovered. It was simpler to just have the points on a line in the projective plane, really the Koba-Nielsen variables.

When we talked about this work of the n-point Veneziano model at CERN, Koba gave the main talk and I was assigned to talking about the application of the Haar measure technology needed to make sense of the expression which would otherwise be divergent.

4 Ideas that arose in developing string theory

The attempts that led to strings from the Veneziano model were for me based on the idea of treating strong interactions by very high order Feynman diagrams. It is of course a most natural thought that, if the coupling constants are strong, the higher the order of the diagram the more dominant the diagram should be. At least the dominant diagrams are expected to be of huge order. So I set about visualizing such diagrams.

If on the other hand the Veneziano described hadronic scatterings, should the high order Feynman diagrams not give the Veneziano model?

Then I began to investigate whether I could get the limits in which one gets the Veneziano model integration variables with the correct asymptotic behavior and poles from considering very high order diagrams described mainly as some gross shape structures of the diagrams. It turned out that approximating the diagrams by a speculative central limit theorem that rendered everything Gaussian - when we convolute sufficiently many propagators - was equivalent to taking the propagators as Gaussian functions of the four momenta. It did not quite work out as hoped however unless a little extra assumption was made. This little extra assumption was that the diagrams should only be the planar ones.
Assuming this is of course easily argued to be equivalent to assuming that the particles, the propagators of which were used in the high order diagrams, tend to sit in long chains. They really formed strings. Or rather I had to assume that they did sit in such string chains in order that the summation over the diagrams could lead to the Veneziano model with the summation over the different diagrams being identified with the integrations in the Veneziano model integral representation.

One question that I had to treat was how to evaluate how the external momenta from the external lines would get guided through the diagram dominantly, i.e. from which range in the loop integration space came the dominant contribution. This is analogous to the conduction of current through a network of resistances. Concerning this question I had some discussion that lead to improvements in my correspondence with David Fairlie.

My thinking was that if we have a very big - in numbers of loops and vertices - Feynmann diagram, then there is an integration over a huge number of loop four momenta and that therefore the external four momenta, which of course are led through the diagram (formally in some arbitrarily specified way), will in some sense only give a small correction relative to the internal loop four momenta. This leads to the idea that there is some dominant region in the space of all the loop momenta around which one gets the main contribution to the diagram. It is only in this sense of thinking about the dominant or central point in the integration region over the loop momenta that one can ask about the “way that the external momenta are led through the diagram” in a physical or mathematically sensible way. If one knows the point in the integration region that is the center of the dominant region one can then ask: how in this center have the contributions to the propagator momenta been changed compared say to the case when the external momenta were zero. The change could then be called the way the external momenta are led through the diagram.

If one now plays around with the external four momenta the effects of changes can be expected to be very small compared to the loop momenta and it is obvious to imagine making a Taylor expansion in these small external momenta. In the biggest part of the Feynmann diagram structure you can indeed simply imagine that because there are so many ways the external momenta could find their way through the diagram that it is natural to expect that the external four momenta can be thought of as being led through in a distributed way. So only a tiny fraction goes through each of the internal propagators. Thinking in this way a Taylor expansion would indeed be suggested as a good approximation. So even though it might in general not be a good approximation to approximate the propagators by Gaussian functions of the four momenta as was suggested in my string paper [1, 2], at least in very high order complicated diagrams it should somehow be a good approximation anyway because in the end the Taylor expansion in the small external four momenta is what counts.

In this way of arguing one might see some similarity to the argumentation that in later years has been central in my pet theory of random dynamics: Often you can Taylor expand so that you get essentially the same result regardless of the true underlying theory. So perhaps the true theory does not matter at all in the end.
5 Review of my string paper

My preprint(s)[1] were poorly distributed. There was a first very badly distributed version with the title “A physical model for the Dual model”. In the second slightly better distributed version the word “almost” appeared in the title “An almost physical model for the Dual model” as a result of discussions with Knud Hansen who kindly discussed with me. The concept of a string was put in by the assumption that the dominant very high order Feynman diagrams were planar. In reality I expressed this assumption by calling them “fishnet diagrams” and drew simply diagrams with lots of squares as if it were a $\lambda \phi^4$ theory. Of course you can easily argue that if the constituents of an object - in this case a hadron or rather a meson - sit sequentially along a chain forming a string, the only interaction will be between neighbors on this chain. The Feynman diagrams that are important would be obtained by first drawing the series of propagators representing the propagation of the constituents (particles) sitting side by side on the chain and subsequently connecting them by exchanges that predominantly are between neighbors only.

Next it is suggested to approximate these diagrams by approximating the propagators by Gaussian expressions,

$$\text{prop}(p^2) \approx C \exp(\alpha p^2).$$

(6)

If one imagines after a transformation to have made it effectively into an Euclideanized theory and you use the $+ - - -$ metric, there needs to be a minus sign in front of the “$C$” in the Gaussian. Here $C$ is of course just a constant as is $\alpha$.

Gaussian loop integrals can be imagined to be calculated exactly. Even in practice it is not so difficult to get an estimate of the dependence on the external momenta, which is the important thing. I had correspondence with David Fairly who provided important mathematical insights leading to good analogies for this type of calculation. Really it is analogous to studying electrical networks with resisters. Then the approximation of the fishnet diagram by a planar homogeneous conductor - approximating a conducting fishnet - becomes quite obvious. One of the important points - also with inspiration from David Fairly - is that in a two-dimensional network the flow through of the external momenta is calculable even without knowing the resistance. There is conformal invariance so that you can conformally deform the conducting surface without it having any consequences for the result of the diagram. The external momentum-dependent factor for the diagram being an exponential of a quadratic expression has though a coefficient proportional to $\alpha$ (identified with the resistance).

There are divergence problems connected with leading terms in the external momenta through point attachments to the conducting disc. But the really important thing is that the variation of the positions on the edge of the disc - or the conformally deformed disc - do not matter because of the conformal invariance. The result is the integrand in the multiple meson scattering amplitude in the formulation of Koba and me.

The summation over various large planar diagrams should then hopefully have led to the integration measure to be used in the formula by Koba and myself, but that part of the derivation - the correct measure - did not come (convincingly) out of my technology for obtaining the dual model.
5.1 Discussions with many people

During the time that I developed the string interpretation of the Veneziano model and especially when I was nearly finished I met with and discussed with many people many of whom I thank in acknowledgements. At the Lund Conference - where my official talk was about the multiparticle Veneziano model of Koba and me - I talked with several people\(^1\) in the lobbies and of course also with colleagues at the Niels Bohr Institute.

At one meeting in Copenhagen I should have talked about the string theory but began to tell the story somewhat privately - I think especially to Sakita - while the audience was more or less present. So I gave the talk in this half private way and did not really get to give the formal talk. Sakita clearly must have learned my account very well because at the next big conference in Kiev he gave a special talk \(^2\) and subsequently made the first well published work about string theory using my approach with fishnet diagrams, etc. Neither Susskinds nor Nambus string theory versions were represented in Kiev and I believe Sakita had never heard about them.

I remember that I first became aware that Susskind was up to something similar from a note added at the end of an article by him telling or at least alluding to that he was making a string theory. Then I immediately got my mother to send a copy of the preprint to him. I pressed her so much to hurry that she made remarks about it that some receiver of preprint were especially favorably treated.

Nambu should have talked about the string at a Sinbi-conference at the Niels Bohr Institute but got stuck in a desert and could not come to deliver his talk. But he did deliver his manuscript.

I also remember that I was honored by being asked to give a talk about strings on the occasion of the visit of Heisenberg to The Niels Bohr Institute. There were two talks that day: mine and that of Heisenberg. I do not think though that I managed to make Heisenberg extremely enthusiastic about strings.

6 Loop correction

Together with David Fairlie \(^{18}\) I wrote an article in which we found that also the loop correction to dual models could be understood in terms of string theory so to speak.

7 The vortex line string

The vortex line, I suppose, should really not be counted as part of the \textit{beginning or birth} of string theory since we came to think of vortex lines - Poul Olesen and I - only after both having already worked on and known about the string. So we should rather say that we attempted to make a model behind the string model. The idea of vortex lines were at that time already known in the non-relativistic case of superconductivity, but we were not familiar with this work. I personally believe to have learned about this work when

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presenting our work at CERN. I illustrated a soliton by means of the necklace of the wife of Masud Chaichans - and became rather inspired to make a theory behind the string.

8 Memories from CERN and understanding the 26 and 10 dimensions

Following our formulation of string theory as described above, the period during which I was most occupied with string theory was during a nine month stay at the theory division of CERN. Here there was indeed a very good group engaged in working on dual models or Veneziano models. I had especially strong contact with Paul Frampton with whom I also visited many good restaurants. Paul worked at that time on his book about dual models. With Lars Brink I published among other things a paper which I think provided the nicest way of seeing what was so special about the critical dimension 26 for the simple string with only the geometrical degrees of freedom and 10 dimensions for the Neveu-Schwarz-Ramond string having further fermionic degrees of freedom.

The idea of Lars Brinks and mine [19] for understanding the critical dimensions started from the by then well known point that the first excitation of the string had only the transverse polarizations and therefore was forced to be massless. But the point of our idea was to calculate the zero-point energy contribution to the energy of a string that is not excited but in its ground state. This contribution to the mass squared of the string state became - after a renormalization of the speed of light, as we got it in our formulation - for the simple string the number \( d - 2 \) of true, transverse, degrees of freedom of the position fields \( X^\mu \) multiplied by \(-1/24\) of the distance in mass square between successive bunches of string states. Here we denoted the number of “transverse modes” by \( d - 2 \) so that the number of space-time dimensions would be \( d \). The zero point energy actually shifts the mass squares for all the string states by the same quantity \( -\frac{d-2}{24} \alpha' \) where \( \alpha' \) is the mass square spacing between successive levels. In order that the level, once it becomes excited, gets down to having zero mass (square) as required for consistency, it is necessary that the shift down \( -\frac{d-2}{24} \alpha' \) be just equal to the needed shift \( -\frac{1}{\alpha'} \). This gives the famous value for the space time dimension \( d = 26 \).

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