Reminiscence on the Birth of String Theory

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Abstract

These are my personal impressions of the environment in which string theory was born, and what the important developments affecting my work were during the hadronic string era, 1968-1974. I discuss my motivations and concerns at the time, particularly in my work on loop amplitudes and on closed strings.

1 Introduction

It is not unusual in theoretical physics for conceptual frameworks to ride roller-coasters, but few have had as extreme highs and lows as in the history of string theory from its beginnings in 1968 to the present. In fact, string theory was so dead in the mid to late ’70’s that it is a common assumption of many articles in the popular press, and of many younger string theorists, that the field originated in the ’80’s, completely ignoring the period we are celebrating here, which is primarily 1968-74.

So it was pleasantly surprising to be invited to reminisce about the early days of string theory. Research results from that era have been extensively presented and reviewed, so I will try to give my impression of the atmosphere at the time, and what questions we were trying to settle, rather than review the actual results.

2 The Placenta

In the mid ’60’s, the framework for understanding fundamental physics was very different from what it is now. We still talk about the four fundamental interactions, but we know that the weak and electromagnetic interactions are part of a unified gauge field theory, that strong interactions are also described by a gauge field theory which might quite possibly unify with the others at higher energy, and that even general relativity is a form of gauge field theory.

1Invited Contribution to “The Birth of String Theory” Commemorative Volume
In the 1960’s things were very different. Not only were the four interactions considered to be of completely different natures, but for the most part the physicists who worked on them were divided into groups by the interactions on which they worked. Of course, every budding particle theorist learned QFT and how wonderfully successful it was in treating QED. But one also learned how these perturbative methods could not be used for strong interactions because the coupling constant was too large, and that for the weak interactions one could only work at the Born approximation, because all existing field theories for the weak interactions were non-renormalizable. So particle theorists were divided into separate groups: one working on strong interactions, one on weak interaction phenomenology, and one doing high order, esoteric QED calculations. Each group had very different techniques and styles.

Even more removed from the world of a strong interaction physicist was the fourth interaction, gravity, which was studied, if at all, by general relativists. When, as a very naïve graduate student who knew nothing of the fields of physics research (I had just received my ScB in Applied Mathematics), I was asked by my future advisor what I might be interested in, I replied “unified field theory”. Nonetheless, it was never suggested that I take a course in general relativity!

So the context into which string theory was born was not so much theoretical fundamental physics or even particle theory, but rather strong interaction theory/phenomenology. The principal recent successes in that field had been in searching for patterns and fitting simple models to scattering data. Scattering cross-sections were dominated by resonance peaks and the high energy asymptotic behavior described by Regge trajectories. A huge number of particles and resonances had been found and were listed in the particle data tables. The organization of these particles into (flavor) SU(3) multiplets was the most impressive thing understood about the strong interactions.

The quasi-stable hadrons and the resonances fell beautifully into patterns which could be understood by treating baryons as if they consisted of three quarks and mesons of a quark and an antiquark. Even though this very successfully described the dominant experimental observations, theorists were very reluctant to think of the quarks as real constituents of hadrons.

Fits to the data were done by treating the scattering amplitude as a sum of resonance production and decay, together with an additional contribution due to the exchange of the same

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2 How separated general relativity and particle physics were in the '60’s is discussed by David Kaiser [20]. He argues that funding cuts in particle theory in the late '60’s and '70’s played a large role in the subsequent bringing together of particle theorists and general relativists.

3 I am using “model” and “theory” with a distinction that is perhaps not generally accepted. To me, a theory is a comprehensive approach to explaining part of physics in a way which will at least have features which are fundamentally correct, while a model tries, with less ambition, to fit aspects of the data, but cannot be taken as the fundamental truth, even as an approximation of the truth. Thus QED, QCD, and general relativity are theories, even though the last clearly needs modification to include quantum mechanics, while the interference model, DHS duality [12], and my thesis are models. The Dual Resonance Model might be taken to have evolved into a theory when we started calculating unitary corrections in the form of loop graphs.
particles in the form of Regge poles to describe the high energy behavior. This sum was called the interference model. But the experimentalists kept finding more and more resonances, and they were joined by phase-shift analysts. It soon appeared the sequence of resonances continued indefinitely to higher masses and spins, in what clearly looked like linearly rising Regge trajectories. In fact, my Ph. D. thesis\[40\] was a very naïve non-relativistic model using PCAC, which rather successfully explained the experimental\[38\] π-nucleon decay widths of a tower of five Δ resonances with spins ranging from 3/2 to 19/2. Unfortunately the top two of these resonances have subsequently dissolved\[54\]. This infinite sequence of resonances suggested the idea of duality\[12\], that the amplitude could be described either in terms of a sum of resonances or in terms of a series of Regge poles. The possibility that a scattering amplitude $A(s, t)$ could be given as an sum of resonant poles in $s$ or alternatively as a sum of Regge poles in $t$ caused great excitement, but also skepticism that such a function could exist.

### 3 Conception and the Embryonic Period

Thus it seemed miraculous when Veneziano\[51\] discovered that Euler had given us just such a function in 1772, to describe the $\pi\pi \rightarrow \pi\omega$ scattering amplitude. This paper arrived at the Lawrence Radiation Lab in Berkeley in the summer of 1968 while I was away on a short vacation, and I returned to find the place in a whirlwind of interest. Everyone had stopped what they were doing, and were asking if this idea could be extended to a more accessible interaction, such as $\pi\pi \rightarrow \pi\pi$. I suggested the very minor modification necessary to remove the tachyon,

$$\frac{\Gamma(-\alpha(s))\Gamma(-\alpha(t))}{\Gamma(-\alpha(s)-\alpha(t))} = \frac{\Gamma(1-\alpha(s))\Gamma(1-\alpha(t))}{\Gamma(1-\alpha(s)-\alpha(t))},$$

and Joel Yellin and I investigated whether this could be taken as a realistic description\[48\] for $\pi\pi$ scattering. It had a lot of good qualitative features, including resonance dominance, regge behavior, and full duality. We were forced to have exchange degeneracy between the $I = 0$ and $I = 1$ trajectories, which was well fit by the data. We noticed the problem that such an amplitude can wind up with ghosts, with a negative decay width for the $\epsilon'$, the $0^+$ partner of the $f$, but also that this problem disappeared if the $\rho$ trajectory intercept exceeded 0.496, very close to the value of 0.48 which we got from fitting the low energy phase shifts. A much more serious problem was that we predicted a $\rho'$ degenerate with the $f$, which seemed to be ruled out by experimental data. That the simplest function did not produce a totally acceptable model was discouraging, especially to Yellin, although we realized that there was no compelling reason not to add subsidiary terms to the simple ratio of gamma functions, except that to do so removed all predictive power! This convinced Yellin that he didn’t want to coauthor the fuller version\[41\] of our paper. But Lovelace\[28\], who independently discovered the same amplitude, managed to do a favorable comparison to experiment.

There were a number of papers attempting to do phenomenology with dual models, mostly describing two-body scattering processes. In general the results had, as did our
paper, nice qualitative features but unsatisfactory fitting of the data. At the same time, the
formal model was becoming much more serious, as great progress was made in extending the
narrow resonance approximation amplitude, first to the 5 point function [5] and then the
$n$-particle [6] amplitudes. A very elegant formulation of these amplitudes was given
by Koba and Nielsen[26, 34, 27], in which the external particles correspond to charges given
by their momentum, entering on the boundary of a unit circle, and the amplitude is given
by an integral, over relative positions of the particles, of the two-dimensional electrostatic
energy. Here the conformal invariance was seen to play a crucial role, and in particular the
Möbius invariance explained the cyclic symmetry. From the $n$-point amplitude for ground
state particles one could factor in multiparticle channels to extract the scattering amplitudes
for all the particles which occurred in intermediate states, determining all amplitudes in what
could be considered the equivalent of the tree approximation in a Lagrangian field theory.
We took the attitude that the particles of the theory should be all and only those which arose
from $n$-point scattering amplitudes of the ground state particles, as intermediate states in
the $n$-point tree function. The amplitude for an arbitrary particle $X$ connected to $p$ ground
states could be found by factoring the $p + q$ ground state amplitude [4], and amplitudes
involving more arbitrary states could come from factoring that. Thus one could determine,
in the tree approximation, the arbitrary $n$-particle amplitude. In a sense, this was a form
of bootstrap, as the set of particles generated as intermediate states were added to form a
consistent set, with the same particles as intermediate states as were considered external
states.

4 Birth of String Theory

Of course a set of tree amplitudes is not a unitary theory. In perturbative field theory,
the Feynman rules are guaranteed to implement unitarity by specifying loop graphs whose
discontinuities give the required sum over intermediate states, because these all come from
a Hermitian lagrangian. The possibility of advancing dual models to a unitary theory be-
came possible once we had the tree amplitudes for arbitrary single particle states, as one
could sew together the loop graphs to give a perturbative (in the number of loops) theory
satisfying unitarity. In perturbative quantum field theory, loop graphs give the appropriate
contributions to the optical theorem, satisfying the unitarity of the $S$-matrix. Bardakçı,
Halpern and I (BHS) [3] defined the one loop graph by the requirement of two-particle uni-
tarity. An earlier attempt (KSV) [25] defined the planar loop graph by extending duality
to the internal legs, which gave most of the factors in the loop integrand. But to get the
full expression, the one-loop amplitude for $n$ ground-state particles $\sigma$ should be required to
have the correct two particle discontinuity, a sum over all possible two-particle intermediate
states. Starting with the tree amplitude for $n \sigma$’s plus $X(p)$ plus $X(-p)$, and summing over
all possible states $X$ and momentum $p$, as shown by the stitch marks here, one is summing not only over $X$ but also over all particles in the left arm, because those are all included in the tree graph.

Of course we called this process “sewing”, which led to an amusing battle with Sy Pasternack, the editor of Physical Review, on a subsequent paper [17]. Pasternack thought he needed to uphold a certain formality, and was responsible for “pomeronchukon” rather than “pomeron”. He wrote us a very witty letter [35] arguing that “sewing” would lead inevitably to “weaving”, “braiding”, “darning”, “knitting” and “sew-on”. We objected, however, that the actual thread lines were shown in the figures. Redrawing figures in those days was a major undertaking. That won the argument.

I should point out that at the time, our description of the intermediate states and the amplitudes was quite clumsy, using the rather messy techniques of the Bardakçı-Mandelstam factorization [4]. While we were working on deriving the loop graph, Nambu [31], and Fubini, Gordon and Veneziano [13] were developing the elegant operator formalism, in which the states of the system are described by harmonic oscillator excitation operators $a_{n}^{\mu\dagger}$, $n = 1...\infty$ acting on a ground state $|0\rangle$. Here $n$ corresponds to nodes on a string and we have a Lorentz index $\mu$. The amplitudes can then be written as a matrix element with vertex functions for each external particle, and propagators integrated $\int_{0}^{1} du$ over an internal variable $u \sim e^{-\tau}$, where $\tau$ acts like a time describing how long an intermediate state propagates. Thus resonance poles in a tree, or two particle intermediate states in a loop, come from the $\tau \to \infty$, $u \to 0$ limit for the corresponding propagator. This formalism made the calculations much easier. It enabled the authors of KSV to discover independently from us the extra factors that get two particle unitarity correct, except for spurious states. The new formalism was so superior [2] that few people were encouraged to read our paper, and I am still grumpy about that.

The operator formalism made clearer two problems that had already been vaguely seen. In this formalism, the amplitudes appear to lose the Möbius invariance, but the amplitudes do not, due to the existence of Ward identities. That is, there are spurious states, combinations of excitations which decouple from all $n$ ground state amplitudes, and therefore by our philosophy should not be included among the states. Secondly, the time-like creation operators create ghosts, particles with negative widths, which clearly should not be there. The set of these ghosts produced by the lowest node operator, $a_{1}^{0\dagger}$, were precisely those that could be exorcised by those Ward identities known at the time. Much of our effort in BHS involved excluding these spurious states from the loop. Of course the higher time-like modes $a_{n}^{0\dagger}$, $n > 1$ also produce ghost states, but these were also exorcised by the Ward identities found later by Virasoro [53].

One unpleasant feature of the planar loop amplitude we constructed from two-particle unitarity was the presence of additional factors $\prod_{r}(1-wr)^{-D}$, where $w = \prod u_{j}$ is the product of the $u$ factors of all the internal propagators, and $D$ is the dimension of space-time, which
at that point we simply wrote as $4$. The discontinuities we were building into the loops come from several intermediate state $w$’s going to zero, so only the $w \to 0$ endpoint contributes, but the natural integration range for $w$ was from 0 to 1. Of course at $w = 1$ this factor has extremely bad behavior. Eliminating the one set of spurious states known about at the time eliminated just one power of $(1 - w)^{-1}$, which didn’t help much. Later, Virasoro [51] discovered that if the Reggeon intercept $\alpha(0) = 1$, there was an infinite set of generators of spurious states, and eliminating those gets rid of all the ghosts (for $D \leq 26$), and one full set of $\prod_r (1 - w^r)^{-1}$. Still, there is a very serious divergence as $w \to 1$, which will turn out to be connected to the Pomeron/closed string. Before that was realized, there was speculation about whether this divergence was removable, and whether the two particle discontinuity had the expected two-Reggeon cut asymptotic form [47] as $s \to \infty$.

It should be mentioned that this effort to raise dual models to the same level of legitimacy as perturbative QFT was a departure which made many uncomfortable. Strong interaction theorists had been divided into field theorists and S-matrix folk, and dual models were generally considered the domain of S-matrix types, but here they were adopting the moral values of a field theorist, even if the context was different. The phenomenologically inclined thought it would be better to simply assign imaginary parts to the regge trajectories in the dual amplitudes to go beyond a narrow resonance fit. As there had been no real data-fitting successes, many had great skepticism about the value of dual model research. One such skeptic asked me why I would work on something so unlikely to be the real physical truth. I recall saying that even though the probability that dual models would be part of the real answer was small, perhaps 10%, at least there was a chance of working towards the truth, while fitting elastic scattering data to Regge poles, to me, seemed not to have any chance of leading to fundamental physical understanding.

I mention this because in 1987, in Aspen where string theory was the superhot theory of everything, I asked some of the younger researchers what their estimate was, of the probability that string theory would be part of the true theory of physics, and was rather astounded to hear answers upwards of 50%.

Anyway, let’s get back to the construction of loop graphs for a complete, unitary dual resonance theory. This was a very active field. Neveu and Scherk [32] used their superior French mathematical education to express the planar loop in terms of elegant Jacobi $\theta$ functions, enabling them to extract the divergent behavior. The operator formalism [14, 13, 50] made tractable the calculation of nonplanar loops [24, 16, 21] and multiloop amplitudes [22]. Abelian integrals were used by Lovelace [29, 11, 30], who suggested that experimentalists deprived of funding for higher energy machines could “still construct duality diagrams in tinfoil and measure induced charges” as their contribution to understanding particle physics.

**Closed Strings**

My second post-doc appointment was at the University of Maryland, which had a pleasant and active high-energy theory group, but no one doing dual models. I felt quite isolated, and while I was able to write some technical papers [42, 44], I missed the stimulating environment.
I had had in Berkeley. In particular, the first of these papers was a rather misguided attempt to get rid of the spurious states given by the Ward identity with $L_1$, before I became aware that Virasoro had found, for the “unrealistic choice of $\alpha(0) = 1$”, that there was an infinite set of such Ward identities, enough to get rid of all the ghosts produced by $\phi_{n\dagger}^0$. Fortunately I was free to visit Berkeley and Aspen during the summers. During my Berkeley stay in 1970 I spent a day at SLAC, where in a discussion with Nussinov and Schwimmer, they asked me a very interesting question. At the time, the $n$-point Veneziano formula was best described by the Koba-Nielsen picture of external charges (or currents) on the circumference of a disk. The integrand could be interpreted as the electrostatic energy of the charges or as the heat generated by the currents, inside the disk. There was also much interest in this being an approximation of very complex Feynman diagrams called fishnets within the disk. The question Nussinov asked is what would happen if the external particles, instead of residing on the circumference of a disk, where on the surface of a sphere. Nussinov answered his own question with “I bet one would get the Virasoro formula”. This is because, with the particles integrated over the surface, there is no cyclic order constraint, and any collection of particles are free to approach each other and produce a singularity in $P^2$, where $P^\mu$ is the sum of their momenta. This is what happens in the Virasoro formula$^{[52]}$.

Should the fishnets, or electric fields, or currents, fill the ball, or should they be confined to the surface with the external particles? In my view, the new Virasoro identities were associated with the local conformal invariance of analytic functions, a much richer group than conformal transformations in Euclidean spaces of higher dimension. Thus filling the three dimensional ball was unlikely to work, but putting fishnets on the surface might be very interesting. So the three of us began to work out electrostatics on the surface of the sphere.

In the Nielsen approach one needs the electrostatic energy of a configuration of point charges at arbitrary locations, and then integrate over the charges’ positions. We can solve Poisson’s equation for each configuration of charges on a 2-sphere, but we cannot define the electrostatic energy as the integral of $(\vec{\nabla}\phi)^2$, because that includes the infinite self-energy of each charge. Instead we might define $E = \frac{1}{2} \sum_{i\neq j} q_i \phi_j(\vec{r}_i)$, but to do so one needs to be able to find the electrostatic potential of a single charge. We cannot have a source of electric flux without a sink, and we seemed to hit an impasse and let the matter drop. Several weeks later, after we had all gone our separate ways, while I was (I think) in Aspen, I decided to look at this again, and I realized that putting an arbitrary sink for all the fields would do no harm. After all, for two-dimensional electrostatics one takes $\phi \propto \ln |\vec{r} - \vec{r}_0|$ without worrying about the flux which goes off to infinity, and in two dimensions conformal invariance makes infinity no different from any other point. As the sphere is conformally equivalent to the complex plane, the potentials are just logarithms of $|(z_i, z_j, a, b)|$, the absolute value of the cross ratio, including the arbitrary sink point $b$ and a point $a$ at which we can set the potential to zero. The Möbius invariance, now extended to three complex parameters, again permits the positions of three charges to be fixed arbitrarily, and the others integrated over the sphere, or complex plane. We have consistency only for $\alpha(0) = 2$, but there we have
a consistent n-point function for closed string scattering. I speculated that this was equivalent to the Pomerons which appeared as a problem in the loop graphs of open strings, and later, with Clavelli, I showed that this is indeed the case.

There was at the time not much interest in closed strings, which have no ends. All the semi-successes of dual model phenomenology were based on Harari-Rosner diagrams being incorporated by Chan-Paton factors, which required string ends on which to attach quarks. Even I postponed looking at factorization and loop graphs in this model in favor of a paper showing that nonorientable graphs do not enter theories with $SU(n)$ flavors incorporated à la Chan-Paton. But the following summer, in Aspen again, with the factorization having been done by others, I addressed the one-loop diagram. The propagators now have an integral over the length of the tube and the angle of twist to get to the next particle, and the complex variable $w$ has $|w| = -\ln T$, where $T$ is the combined times of propagation, but $w$ also has a phase given by the angle of twist in sewing the initial end of the tube to the final end. The amplitude involves

$$\int_{|w| \leq 1} d^2 w \prod_{r=1}^{\infty} |1 - w^r|^{-2(D-E)},$$

where $D$ is the dimension of space-time and $E$ is the number of factors assumed to mysteriously disappear if one removes the spurious states. $E$ had already been shown to be 1 in general $D$, but we were hoping, as was found true later, that $E = 2$ in the right $D$. Still, $D - E$ is a positive number, and $1 - w^r$ vanishes for $w$ any integral power of $e^{2\pi i/r}$, so we have a terrible divergence at every point of the unit circle for which the angle is $2\pi$ times a rational number.

Fortunately, by the time I was looking at this, I had read the elegant reformulation of the open-string loop in terms of Jacobi theta functions. This encourages us to look at $\tau = (\ln w)/(2\pi i)$. Of course the integrand is invariant under $\tau \to \tau + 1$, because $w$ is unchanged, but it is also invariant under the Jacobi imaginary transformation, $\tau \to -1/\tau$, provided we have the magic dimensions $D = 26, E = 2$. The world sheet of a loop of closed strings is a donut, conformally equivalent to a parallelogram with sides 1 and $i\tau$, with opposite sides identified. While a Hula hoop and a Mayflower donut may not look the same, multiplying the parallelogram by $-i/\tau$ maps one into the other. These transformations generate the modular group, so invariance shows that in integrating $w$ over the unit disk we are including an infinite number of copies, while we wanted, for unitarity, only one copy of the region around $w \approx 0$. Thus the right thing to do is restrict our integration to the fundamental region, which is $|\tau| \geq 1, -\frac{1}{2} < \text{Re } \tau \leq \frac{1}{2}$. In terms of $w$, this is a subset of $|w| < 0.0044$, so we stay far away from the horrible divergences.

\footnote{For some bizarre reason, I referred to the dual models in terms of what we would now call their world-sheets, as strip and tube models for what we would now call open and closed strings.}
If closed strings aroused little interest, loops of them really aroused none. The figure shows the number of citations to [45] each year as listed by Spires. Interest in dual models as models of the strong interactions was fading fast. Firstly, the evidence for partons, pointlike constituents of hadrons, found in deep inelastic scattering starting in 1969, was inconsistent with the soft, extended object picture of strings. Secondly, non-abelian gauge theories were proven renormalizable (’t Hooft, 1971 [19]), explained neutral currents in a unified electroweak theory, and gave quantum chromodynamics as a theory of the strong interactions. This greatly improved the appeal of conventional field theory at the expense of string theory. And within string theory, the inclusion of fermions by Ramond [37] and Neveu-Schwarz [33] was more exciting than loops of Pomerons.

In the fall of 1971 I started an Assistant Professorship at Rutgers in a new high energy theory group headed by Lovelace, and including Clavelli as a postdoc. Lovelace had a very ambitious program for describing arbitrary multiloop diagrams, and Clavelli and I looked at how the closed string intermediate state in the nonplanar loop interacts with the ordinary (open-string) states. Then I struggled with understanding renormalization [46], an effort which would have been totally trivial if I had only realized that $\sum n = -1/12$, but unfortunately I had taken too many pure math classes to recognize this fact.

The decision on my tenure was coming up, and dual models did not seem the best way to prove my worth, so I reluctantly got into several other endeavors, and was rather slow to get back into strings when they arose like a phoenix, or perhaps like a fire storm, in 1984. But that is after the period we are considering here.

**A Comment on Impact**

I want to say a few words about how this field was perceived within the physics community. In recent years there have been numerous attacks from some in the high energy theory community, and from experimentalists, that strings are, like the Pied Piper, leading the bright young theorists astray. String theory was not quite so dominant in the 1969-1974 era, though it did absorb the attention of a very large fraction of the young theorists. It did not get a similar acceptance by most of the more established people, though I think Europe was more receptive than America. In particular Phys. Rev. Lett. published very few articles in this field, but Physics Letters had many of the important papers.

The field did attract quite a bit of attention. In fact, in the early ’70’s I was interviewed by a sociologist who wanted to do a study of what attracted so many people to working on dual models. Unfortunately she followed a narrow set of prepared questions which seemed totally off the mark. Her focus was on which experimental data encouraged me to continue working in the field. I don’t think anything came of that study, and I haven’t heard
of studies done when the field became even more rabid in the mid '80's. But this is still an important question: Is there any real physics in string theory, and should so many people be working on it. Undoubtedly there will be a shift towards more applied high energy theory as LHC starts giving us more data to work with. It will be very interesting to see where the field goes.

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