On the Early Tension between String Theory and Phenomenology*

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Abstract

Today there is a very noticeable tension between string theory and phenomenology in particle physics. It is interesting to muse over the roots of this situation in the very early days of string theory. In this personal account of a small part of the ancient history we review some developments in the string theory groups at Fermilab and Rutgers University in the early 1970's.

1 Introduction

To some degree the present conflict between string theory and phenomenology is more easily understandable than that of the early days. Today it is primarily a debate over the relative federal funding that the two areas should properly receive. Among phenomenologists there is the perception that progress in string theory has slowed noticeably while still attracting a disproportionate fraction of students and post-doc positions. On the other side there is the feeling that the killing of the Superconducting Supercollider (SSC) project has led to a pronounced drought in the qualitatively new experimental discoveries that could feed phenomenological work. An exception is the confirmation of neutrino masses with well-measured mass splittings and mixing angles.

In the beginning, however, string theory (or dual model theory) was totally devoted to the building of a theory of strong interactions that could directly deal with the large amount

*Invited contribution to "The Birth of String Theory" commemorative volume of data on hadronic phenomena being accumulated at Brookhaven, CERN, and Serpukhov. One might have, therefore, expected a long honeymoon between dual model theorists and phenomenologists. This however never materialized.

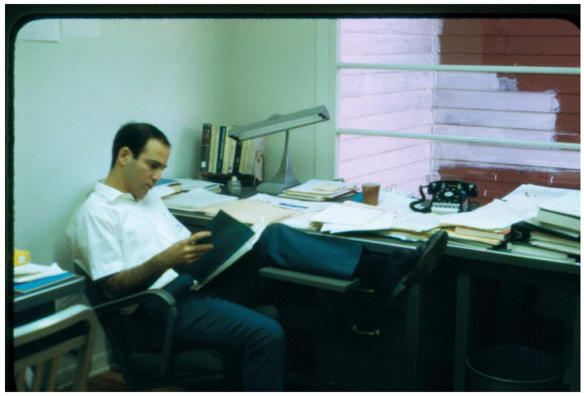

Figure 1: Posing in my office next to the Curia in October of 1970. Note the half painted wall outside.

In the rustic environment of Fermilab (then called the National Accelerator Laboratory, NAL) there were, from Fall of 1969 to Summer of 1971, three theorists excited about dual models, myself, Pierre Ramond, and David Gordon. Pierre has given a valuable account of those days at this string history meeting and I have presented elsewhere my personal recollections. Around the world in physics theory circles there was, in the years leading up to the dual models, a feeling of pessimism about the prospects of dealing with the rapidly increasing number of known hadronic resonances in terms of the simple lagrangian models of the type that had been studied. Lagrangian field theory was at its nadir and had been widely discarded. Hadronic physics was dominated by Geoff Chew's ideas of democracy among resonances in an "S matrix bootstrap". Veneziano's remarkable model which burst upon the field in 1968 and its N point generalization that rapidly followed seemed to have magical properties. However, what we considered fascinating and somewhat magical, others considered strange and other-worldly. We heard that Sam Trieman had said that no one working on string theory would ever get tenure at Princeton.

I composed a list of twenty phenomenological reasons to pursue the dual models as a theory of hadrons. These are still valid but progress in this direction has come to a standstill. We are no closer today to a predictive theory of hadrons in agreement with the observed trajectories and couplings of Regge poles than we were in 1970.

Today's younger string theory experts may not even be too sure of the meaning of some of these. Collectively, they represent a distillation of hundreds of now forgotten experimental and theoretical papers of the 1960's. Without explaining each of them in detail let me list them here:

1. Resonances build Regge behavior

2. Linear trajectories

3. Suppression of exotics via exchange degeneracy
4. Explicit crossing symmetry and analyticity
5. Finite energy sum rules
6. Ghost free saturation of superconvergence relations
7. Quantization of masses and intercepts
8. Smooth connection from Regge to resonance region
9. Adler zeros
10. N point generalization, S matrix bootstrap
11. Wide angle behavior, P_T cutoff
12. Factorization, particle interpretation
13. Exponential degeneracy
14. Ghost free, relativistic harmonic oscillator spectrum
15. Wholesale treatment of resonances
16. Generation of the Pomeron, half the slope and twice the intercept
17. f dominance and modified Wu-Yang behavior of the Pomeron
18. Quantization of the dimensionality of space-time
19. Abnormal duality in Pomeron-particle scattering
20. Finite mass sum rules

The experimental staff at Fermilab was well aware of the properties of hadronic scattering although they were too absorbed in building the machine to worry about whether the dual models were going to be the ultimate answer. The laboratory management on the other hand seemed to think we should have been working on more short term solutions. Ned Goldwasser, the deputy director, had told us of the Curia's preference (i.e. Director Bob Wilson's preference) that the theory group be actively involved in analysis directly related to the experimental program which at that point was a year or two away. He did, of course, temper his remarks with the concession that, if we made some formal advances of great significance, that would be OK also. Ned had previously assured us that we five theorists were on the same appointments as the experimental staff and that any decisions on our future at the lab were at least five years away.

Pierre and I had been working on generalizing the Veneziano model. Having dissected the group theoretical basis of the model we were each investigating possibilities involving anti-commuting oscillators though from different points of view. The patience of the lab, however, came abruptly to an end. In the Fall of 1970 we were summarily dismissed with the statement "..we had hoped that considerably stronger interactions would develop between you and the experimental physicists than has been the case... It is pretty clear that our experiment has not been a total success, and it would be foolish to pretend otherwise."

Even Pierre's remarkable construction of a supersymmetry on the string world sheet was not considered sufficiently relevant phenomenologically nor sufficiently significant as a formal advance. Nevertheless, Pierre's supersymmetry spread rapidly at more enlightened institutes around the world. Bunji Sakita, one of the active string theorists of that day, brought Pierre's work to the attention of Bruno Zumino and Julius Wess at CERN. The supersymmetric Wess-Zumino model that resulted brought supersymmetry to life as a field theory independent of string theory.

We left Fermilab in August of 1971 replaced by theorists of a more phenomenological bent: Henry Abarbanel (from Princeton), Steve Ellis, Manny Paschos, and Tony Sanda. Pierre took up a position at Yale and I moved down the coast from him to Rutgers.

On the banks of the Raritan a very strong effort in string theory had been initiated a year earlier. Our leader was Claud Lovelace who had surprised (and initially amused) string theorists around the world with the observation that the dual pomeron would be a factorizable trajectory if space-time had 26 dimensions. Although his background was South African and not Indian, Claud was an admired guru in string theory complete with long beard and vegetarian food preference. When I arrived he was still living in a motel and driving a rental car; presumably he had no time for mundane matters.

Also at Rutgers, fresh from very productive post-docs at Berkeley and Maryland was Joel Shapiro. Joel was already well known for the Lovelace-Shapiro model and the Shapiro-Virasoro model.

The Rutgers physics department was located in a relatively new but already cramped facility in Piscataway north of the river from the main campus. I shared an office with Frank Wong but, naturally, began working with Claud and Joel on strings.

In the Fall of 71, a war of wall posters broke out between Lovelace and Bogdan Maglich. Maglich posted a challenge to the theorists to stop working on strings and tell him what they would see at Fermilab. Claud responded with a pictorial suggesting that Maglich would see a jagged cross section while the rest of the world would see a smooth Regge behavior. Maglich had become well known for reporting that the A_2 resonance had a pronounced dip in the center. This result, which was initially confirmed by another experiment and which had triggered a barrage of theory papers, later evaporated.

Joel, Claud, and I began work on the double twisted loop that contained the Pomeron behavior. Claud gave us voluminous notes on a dual Reggeon calculus but for the most part we never saw him except at seminars. Claud worked mostly at night sometimes slipping notes under our doors about his results. Meanwhile Joel and I, following the harmonic oscillator formalism, began diverging from Claud's path. We attempted to keep Claud abreast of our progress and we always assumed we were going to write a joint paper. However, when the manuscript, ultimately 80 pages long, began to take form in the Winter of 1973, the gap between Claud's methods and ours was too great to bridge. Claud claimed that no equation in our paper followed from any other which we, of course, denied. Joel and I had labored well over a year by then and if we were to try to combine Claud's methods with ours, it would have certainly required at least another year.

The Nuclear Physics paper that came out with only Joel and me as authors was called "Pomeron Factorization in General Dual Models" but the results were much more general than might be suggested by that title. The paper survived the revolution under which string theory came to be considered a theory of the fundamental interactions rather than a theory of hadronic resonances and has continued to be referenced up to the present time.

The main result was a technique by which a string theory trace could be replaced by a simple expectation value of some rotated oscillators. This allowed us to demonstrate the factorization of arbitary dual models including the Neveu Schwarz model and the dual quark models of Bardakci and Halpern. John Schwarz expressed to me his previous belief that those models would not have a factorizable Pomeron. The factorization of the Pomeron in the general model implied the existence of an operator, Υ, that changed a closed string into an open string. This allowed one to replace the Pomeron exchange graph with a tree graph. I later foolishly spent a year using the still complicated method to calculate inclusive reactions in the double Pomeron region. Inclusive reactions were the final attempt to use dual models for hadronic physics. Veneziano devoted quite some time to this effort. The result was that the tree level dual amplitudes gave a fairly good description of the exponential cutoff in transverse momentum of the produced particle at intermediate transverse momenta but could not reproduce the parton model power law cutoff observed at still higher transverse momenta. I finally arrived at a result from the double Pomeron contribution to the six point function but still found no parton model behavior.

The paper with Joel on Pomeron factorization also noted how the extra dimensions of the dual models could be traded for anti-commuting degrees of freedom. The critical dimensionality of the Veneziano model could be reduced in a ghost free way from 26 to 4 by adding 22 scalar quarks and 22 scalar antiquarks of the Bardakci-Halpern type. This later became crucial in the heterotic string making the critical dimensionality of the Veneziano and Neveu-Schwarz-Ramond sides equal.

The twist operator for the general model was also presented. Ironically, both Joel and I forgot this result years later and, when the generalization of the Neveu Schwarz model to dimensions lower than ten was under consideration in the next decade, it initially appeared that there would be no possibility of writing such a model in four or eight dimensions. Zvi Bern and David Dunbar eventually showed how to perform this reduction using the correct twist operator from Joel's and my paper (which they had independently rediscovered).

The first incarnation of string theory ended about 1974. After this time there was little work on hadronic strings or on strings as fundamental interactions. The triumph of field theory inspired by the success of the standard model was complete. For the most part, Pierre and I and the vast majority of other string theorists began working on what Pierre referred to as four-dimensional physics. It was the end of an era. Only a few hardy souls, notably Mike Green and John Schwarz, kept the theory alive until the renaissance of 1984.