

NAL's First Theory Group

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The National Accelerator Laboratory (NAL), today's FermiLab, was born with the clear mission to build the most energetic collider of its time, but no such clarity existed for its theory group.

Two proposals were considered; the first, championed by Bob Marshak, would clone CERN's large theory group, while the second proposal, shepherded by Robert Serber, required few theorists. Bob Wilson, whose opinion of theorists was lukewarm at best, compromised: the NAL theory group would start with five junior postdoctoral theorists, but no senior theorist.

The Sputnik-induced plethora of academic positions was coming to an end and the 1969 postdoctoral "draft" was brutal, leaving many fresh PhDs without an academic job. I was one of the lucky five leftovers chosen for the first NAL theory group.

NAL looked like a summer camp rather than a laboratory. I had worked at Bell Labs/Holmdell, where lots of people with ties and suits worked in imposing buildings with impressive scientific equipment inside. At NAL, no ties, no suits, just small scattered houses with the exception of a half-buried structure with magnets inside that served as a tornado shelter. The last theorist to arrive at the NAL, I was given the smallest bedroom in the back of the "Theory House".

This sounded like fun and most importantly I had a job and time to prove myself as a theoretical physicist.

I soon met my fellow theorists, David Gordon (Brandeis), Jim Swank (Illinois), Lou Clavelli (Yale), and Don Weingarten (Columbia). I had met Lou in Trieste earlier in the summer. Don was the most famous, having been pictured smoking cigars in the office of the president of Columbia University!

In Trieste Jean Nuyts and Hirotaka Sugawara had infused me with the beauties of the Veneziano model. David Gordon had written an important paper on this subject. David and I published together a forgettable paper (TH-1, perhaps).

Lou and I struck a life-long personal and physics friendship. We were soon at work on using the Gliozzi-Thorn $SU(1,1)$ symmetry to construct dual amplitudes. It took us most of one year, resulting in two publications (TH-5, and TH-6). The second broke new ground with the construction of the vector vertex. I believe this was the first original contribution of the NAL theory group.

In early 1970, Director Bob decreed from the Curia “all theorists must go to Aspen”. I had never heard of Aspen nor did I want to go: my wife Lillian, an electrical engineer, who had given up her IBM job to come to Illinois, had secured a new job at Bell Labs/Western Electric and could not go with me. The director was persuasive and NAL theory went to Aspen where, in between music and volleyball, I started to think of dual models in terms of an entity with particle-like properties with its own generalized momentum, position and Klein-Gordon equation. I submitted the “interpretation” paper (TH-7) and began extending the approach to the Dirac equation. It was a time of intellectual joy as entirely new theoretical concepts were emerging. The string Dirac equation established the foundation of superstring theory, and introduced a new symmetry, (world sheet) supersymmetry. These concepts entered the domain of fundamental physics from NAL’s first theory group.

In early November, my analogy paper was rejected and a letter from Ned Goldwasser terminated our appointments effective Summer 1971. We were all devastated at this breach of promise¹ ; only David was spared. The thought of being on the job market reduced me to a nervous wreck. I lost my intellectual focus and it took me forever to finish the fermion paper (TH-8) which was sent out just before Christmas 1970.

I am very proud of the achievements of NAL’s first theory group. It started as a pressure-free adventure², even though it ended on such a sour note. On the other hand, it is where I met Lou, my friend and collaborator, who introduced me to the great Nambu, and where Bob Wilson led me to the Aspen Center for Physics, that became a life-long passion. Finally it is where I did my best work, to which the early NAL atmosphere surely was a contributing factor.

¹To compensate for the lack of senior theorists, the Curia had told us to expect five-year appointments

²See <http://lclavelli.people.ua.edu/physics-at-Weston.html> for Lou’s account of early NAL