Standard Model

In the summer of 1971, A. Pais came back from the EPS (European Physical Society) meeting in Amesterdam. I asked him what new in that conference and he show me 2 papers by G. 't Hooft and M. Veltman on the renormalization of Yang-Mills theory. They claimed that they can show that Yang-Mills theory is renormalizable. This problem of how to renormalize Yang-Mills theory has been an outstanding problem ever since it was first written down by Yang and Mills in 1954. This problem is connected with quanitzation of the theory with non-abelian local symmetry. Every few years there will be some claim that these problems have been solved and later truns out to be not the case. When I show these 2 papers to a senior thoertican in our group he said that every few years there will be a claim like that and in particular Veltman is not such a reliable person and 'Hooft is just a graduate student. His advice is to not take them seriouly and ignor these two papers.

Pais was originally from Holland and he felt some obligation to help out his countrymen. So he invited the young graduate student 't Hooft to give series of talk about his work. But due to the limit of time only two talks were scheduled. But Pais conveiently got out of time to avoid any awkward situation. The talks were scheduled in Novermber at seminar room in Rockfeller university which is not very large. The words got around that some very important important results will be discussed. So people came from all different directions, Columbia, New York University, Steven Institute of Technology,$\cdots$ *etc* to listen to this event. Unfortunately, right at beginning people jump on the speaker asking questions like , Is the Yang-Mills theory really renormalizable? how do you handle the overlapping divergences,$\cdots$ etc. Some of the questions are really nasty and some are really very technical and not appropriate for an one hour seminar. t' Hooft spend so much time fending off all these unfair questions that he did not get to the second sentence of his note. In the second seminar next day the same thing repeated and 't Hooft did not get to give his seminar. This is the first time I experience what unruly audience can do to the seminar. I was very impressed by 't Hooft's persistence in keeping his confidence and not yielding to screaming and shouting.

After this he toured the courtry giving talks here and there, still he did not win over many believers. This probably has to do with the arrogance of the senior theorists who look down on some young graduate student claiming such important results. In the meantime, a theorist Benjamin Lee understood what H' tooft 's work is all about because he is quite familiar with the theory of renormalization and spontaneous symmetry breaking. So Ben Lee wrote a series of papers with collaborator Zinn-Justin to explain 't Hooft's work. Since Ben Lee was a fairly well-known field theorist people are more willing to accept their work. Then all of sudden the renormalization of non-Abelian gauge field theory with spontenous symmetry breaking spread like a wild fire. many people drop what they were doing and jumped into the study of this type of theories.

In one of the original papers by 't Hooft, he applied the renormalization scheme to the original Weinberg's wrok on weak interaction of leptons. The

results are fine without the hadrons. The trouble with hadrons is the presence of the strangeness changing neutral current which is forbidden from the non-obervation of the process $K_L \to \mu^+\mu^-$. This problem is solved later when a new quark c, called charm quark was introduced by Glashow, Iliopoulos and Maiani to cancel this strangeness changing neutral current. By then the structure of the weak interaction is more or less determined.

In the development for the theory of weak interaction one of the important milestone is the Weinberg's paper of leptons. In this paper, published in 1967, Weinberg combined the spontaneous symmetry breaking with the local $SU(2) \times U(1)$ symmety in the fromulation of weak interaction. Unfortunately, not many people realizes the significance of this paper. I think one of the reasons is the lack of experimental evidence for this model because the acceralator in Chicago is still under construction. By 1971 the accerlator was finished and discovered the neutral current reactions. Everybody start to belive this model. But Abus Salam claimed that he had a similar idea of using spontaneous symmetry breaking and local symmetry to construct a model of weak interaction and should deserve some credit for it. So he wrote to all the workers in the field to request them to refer this model as Weinberg-Salam model. This name was in use for a while until more experimental evidences had been accumulated and was replaced by "Standard Model".