PROCEEDINGS OF SCIENCE

Aspects of Héctor's research

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I will discuss the ideas developed for describing strong interactions in the Sixties, Seventies and Eighties and some aspects of the research carried out by Héctor in those years. Finally, I will briefly discuss my interaction with Héctor.

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1. Theoretical ideas in the Sixties

During the years after the end of World War II and in the Fifties, quantum electrodynamics (QED), the theory of electrons, positrons and photons, was actively studied and it was understood how to get rid of ultraviolet infinities by means of the perturbative renormalization procedure. In this way, one could compute corrections to the Dirac theory of the electron, as for instance the Lamb shift in the levels of the hydrogen atom and the anomalous magnetic moment of the electron and it turned out that the theoretical predictions agreed with the experimental data. It was the triumph of quantum field theory.

The Sixties, instead, were characterized by the discovery of a large number of hadrons in the newly constructed accelerators and, given its success with QED, it was natural to use quantum field theory also for strong interactions. But, assuming that the pions and nucleons were elementary objects, the strength of the pion-nucleon coupling constant ($\frac{g^2_{\pi NN}}{4\pi} \sim 14$) did not allow for a perturbative expansion. Furthermore, given the large number of mesons and baryons, it did not seem appropriate to have a Lagrangian containing all of them.

Two schools developed in the Sixties: the symmetry school and the S matrix or analyticity school, as Héctor used to call them.

The symmetry school classified the hadrons in terms of C,P, T, $SU(2)$ isospin, $SU(3)$ flavour, that were exact or approximate symmetries of the Lagrangian (whatever it was) describing the hadrons. Since the mesons were in the product of 3 and $\bar{3}$ of $SU(3)$ and the baryons in the product $3 \otimes 3 \otimes 3$, it was suggested by Gell Mann and Zweig that the hadrons were made of more elementary objects, called quarks. But, since the quarks were not observed as free particles, there were some people who thought that they only were mathematical entities and not real observable particles. These ideas led to the quark model and to current algebra that was abstracted from the free quark Lagrangian. In particular, the quark Lagrangian with two flavours and small quark masses had an approximate chiral $SU(2) \otimes SU(2)$ symmetry that was assumed to be spontaneously broken into $SU(2)$ vector. From this assumption low energy theorems for the pions were derived and they were in good agreement with experiments.

Given the difficulties in writing a Lagrangian for hadrons, it was proposed for the hadrons to forget it and write directly the S matrix. In fact, unlike the Lagrangian, the S matrix was directly observable in the experiments: this was the analyticity or S matrix school.

The S matrix was supposed to satisfy properties as analyticity, crossing symmetry, unitarity and Regge behaviour. But there was no precise recipe on how to construct it. The bootstrap principle was mentioned for constructing it, but it was not precisely formulated. The breakthrough came from Dolen-Horn-Schmidt duality between the resonances exchanged in the s-channel and the Regge poles exchanged in the t-channel and from the analysis of the so called Finite Energy Sum Rules (FESR). They put such strong restrictions on the S matrix for mesons that led to the Veneziano model.

Although there was a certain polarization between the two approaches, many people considered them as two complementary tools to understand strong interactions. In some formulation, however, they were alternative. For instance, the quark model considered the hadrons as made up of more fundamental constituents, the quarks, while the S matrix approach assumed that there was no state more fundamental than the others. The properties of the hadrons can be established by starting from any set of them and deducing the properties of all others by using analyticity and consistency conditions. This was called at that time nuclear democracy.

In the Sixties Héctor used both approaches to understand strong interactions.

In 1967 he, together with F. Scheck and R.H. Sokolow, published an important paper in which they derive the electromagnetic properties of the hadrons in the quark model.

Immediately after, using the FESR, he wrote together with Ademollo, Veneziano and Virasoro the fundamental analysis that led to the Veneziano model.

After the construction, done by Veneziano in 1968, of the four-point amplitude for mesons, Héctor worked with several collaborators on the construction of the five and six-point amplitudes, comparing them with the experimental data and finding impressive agreement. In 1969 a consistent N-point amplitude, generalizing the four-point Veneziano amplitude, was finally constructed that, however, turned out to be free from negative norm states, forbidden by unitarity, only for the value of the intercept of the Regge trajectory $\alpha_0 = 1$. Unfortunately, this value was not consistent with the value of the intercept that was extracted from the experimental data that was close to $\alpha_0 \sim \frac{1}{2}$.

In 1968 Lovelace wrote a more realistic model for the $\pi\pi$ scattering:

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

where $\alpha(s) \sim \frac{1}{2} + \alpha' s$ was the correct ρ Regge trajectory. Many attempted to generalize it to the scattering of N pions, but that has been possible only with the value $\alpha_0 = 1$ for the Regge trajectory, as in the Neveu-Schwarz model.

Lovelace applied this model to $N\bar{N}$ annihilation at rest in 3π, where the $N\bar{N}$ pair can be approximated with a massive pion. Héctor with Altarelli extended the previous model including "satellites" that do not destroy the central depletion of the Dalitz plot and found even better agreement with experiments.

2. The Seventies

In 1969, evidence for the existence of point-like objects (quarks) inside the proton was found in deep inelastic experiments. The N-point dual amplitude, discussed in the previous section, besides of having a wrong value for the intercept of the Regge trajectory, was also unable to explain the large momentum behaviour found in deep inelastic experiments and therefore the previous approach was abandoned and people went back to field theory.

In 1973 Quantum chromodynamics (QCD), the theory of quarks and gluons, was formulated. Non-abelian gauge theories were proved to be renormalizable by ' t Hooft and Veltman and the Standard Model in the present form was constructed.

It turned out that QCD was an asymptotically free theory implying that quarks behave as free particles at short distances and explaining in this way the success of the parton model in explaining deep inelastic experiments. However, when one tries to pull the quarks apart, there is a linear potential that confines the quarks inside the hadrons, thus explaining why the quarks cannot be taken out of the hadrons. Although one had finally constructed a theory for strong interactions, it was, however, not so easy to compare QCD with what was observed in experiments. Héctor being a person who liked to use theoretical ideas for explaining experimental data, he suffered in this period from the difficulties in comparing the theory with experiments.

In this period Héctor worked on isospin violation effects, production and decays of charmed particles and other things.

With the construction of the Standard Model it was again the triumph of quantum gauge field theories.

3. The Eighties

In 1979 Shifman, Vainshtein and Zakarov formulated the QCD sum rules. In this approach, assuming that the only physical states are colour singlets, one can extract the properties of the hadrons from relations involving both perturbative and non-perturbative quantities of QCD. Héctor got immediately very interested in this approach. This can be clearly seen in the introduction of the Physics Report that he wrote in 1985 together with Reinders and Yazaki and that is a standard reference on QCD sum rules.

They start the article saying: "Hadrons are, beyond reasonable doubt, bound states of quarks. It is far from clear however how to generate the observed spectrum and its properties, even if one accepts the idea of confinement. The reason for the complexity of the problem and the ensuing frustration is that asymptotic freedom and confinement imply that the theory must have a very complex infrared structure. It is therefore a big challenge to extract information on the spectrum from the rather simple Lagrangian of QCD".

Héctor had a very fruitful collaboration with Reinders and Yazaki writing more than 10 papers with them. They computed masses and couplings of charmonium, of beauty states and also of hadrons made up of light quarks. Non-perturbative quantities as the gluon and quark condensate were also be determined in this way.

Then, with Banks, Horsley and Wolff, he computed the gluon condensate using the existing Monte Carlo data and found agreement with the value obtained from QCD sum rules.

I stop here discussing his scientific activity by only adding that the presence of Héctor in Sweden has been very positive for the Swedish environment because, among other things, he strongly contributed to the creation of a strong astroparticle physics group in Stockholm.

4. Héctor and me

I have known Héctor since the beginning of the Seventies, but I got to know him better when I moved to Nordita in Copenhagen in 1974 because he, being the Editor of Nuclear Physics B that had its headquarter there, was often coming to Copenhagen. In the beginning I felt a bit uneasy with his strong and sometimes aggressive statements and I was a bit intimidated by him. But then when I came back to Nordita in 1986 and especially when he moved to Sweden, I got to know him much better from my visits to Stockholm and Uppsala and since then, it was always a great pleasure to meet him and to discuss various things with him. Therefore, any time I visited the Stockholm area, it was always a must to go and talk to Héctor listening to his, most of the time, unusual but stimulating points of view and discussing various scientific and other issues.

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Héctor has always strived for scientific excellence and when he moved to Sweden he was very critical of the situation there and in the Nordic countries. He had very clear ideas on what was good physics and bad physics and he expressed his ideas directly without any diplomacy and in an extreme and sometimes aggressive way. But, if one was able not to be too disturbed by his direct way of saying things, one could see that there was always some truth in what he said and, if one was open to a serious discussion with him, one could learn a lot from his criticism. Of course he was not always right, but, unlike other people, he had the good quality of openly admitting it.

When Nordita was in the middle of a storm around 2004-2005 and it was not clear what would happen, I heard that Héctor was making very strong comments on the scientific activi at Nordita. During that period I came to Stockholm for a Nordic meeting and, as usually, I visited Héctor in his office. When he saw me, he said: are we friends or enemies? I answered: Héctor, how can we be enemies? and then he said: OK, let's sit down and discuss. Then I said: Héctor, do you really mean that what we do at Nordita is completely useless. He replied that he was not saying this and then we discussed for about two hours, but at the end he was still against moving Nordita to Stockholm.

Nordita moved to Stockholm on January 1st 2007 and I started to stay longer and longer in Stockholm and I was talking to Héctor very often. He saw how Nordita was developing and when he came to Nordita for a program, he admitted that he was wrong not wanting Nordita in Stockholm. He could now see that Nordita was an enrichment for the Stockholm area. Unfortunately he entered a period in which he started to have more serious problems with his health. After a period in which he felt worse and he could not walk very much, he entered a period in which he felt better again. I hoped that the worst was gone, but in August 2009 I got an email from Gabriele Veneziano with Héctor in the subject. I remember that I was afraid to open it and when after a couple of minutes I opened it, I got the sad news that Héctor had passed away. Now I miss not being able to talk or to have lunch with Héctor when I go to the fifth floor in Albanova and I think many people there also miss his presence.

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