Celebrating 50 Years of Hadron Collider Physics

Sebastian White, Paola Catapano July 25, 2019

In a bit over a year we will arrive at the 50th anniversary of the launch of Hadron Colliders (ISR start January 1971). We launched another hadron collider roughly every 10 years -completing 5 in all.

In 1973 my thesis advisor, Leon Lederman, sent me to the ISR for my Ph.D. research and landed me in the middle of the "November Revolution".

Like many colleagues in physics I tend to prefer small collaborations but, nevertheless, I now realize that I have done experiments at all 5 Hadron Colliders.

I cannot think of anyone else that has done so.

But this 50th anniversary has special significance.

Our community has no other Hadron Collider lined up on this 10 year time scale. We are now talking on the scale of 40-50 years and are well aware of the "communications challenge" to convince funding agencies, politicians and even our peers in other rapidly developing fields of science that the investment in the "Energy Frontier" is worth it.

At the same time this sense of crisis is ironic. The Higgs discovery at LHC really successfully captured the public imagination and we may be guilty of lack of follow-through by not capitalizing enough on this interest.

It is easy to collect impressions from across the spectrum of non-scientists and gather that LHC and the Higgs figured large in conversations around the water cooler starting around 10 years ago.

One impactful article was the photo essay by Kurt Andersen and Todd Eberle in the January 2010 issue of Vanity Fair- a nice example of CERN successfully opening its doors and sharing material with organizations like VF.

In collaboration with Paola Catapano of CERN, we have been collecting first-hand accounts of the 50 year history in which CERN has had such a central role. Our intention is to collect this “oral history” and at the same time develop a dialog about the uniqueness of the opportunity for the next step at the “Energy Frontier”.

This step is really carried out in the spirit of enhancing the archive now in CDS and adding balance to the “talking heads” material on people that are currently active. In other words, this material is collected as a resource for scholarship in the spirit of oral histories collected by other institutions such as Rockefeller University or Scuola Normale.

We do also have in mind that this material will enable at least one documentary- perhaps in the style of Ira Flatow’s excellent “Transistorized”- and assume that this would best Be developed with resources not available at CERN.

We list below some of the interviews up to now:

1) Luigi DiLella- he tells the story (with amazing attention to detail) of the start of the ISR experimental program and the relevance of Lederman’s BNL experiment leading to one of the ISR discoveries- hi-pt physics.
2) Igor Dremin- the focus here is on the other ISR discovery (the rise of the total cross section) and its relevance to new results from the LHC. He also provides a

perspective on initial collaboration of the former Soviet Union with CERN and the US.

3) Fritz Caspers- for accounts of stochastic cooling- tested at ISR and the basis of the 2^{nd} successful hadron collider.
4) Tatsuya Nakada- for the wild west days when there was much uncertainty of how best to do B-physics at a hadron collider leading to the now successful LHCb.
5) Jim Virdee- who has a very articulate account of the transition from UA1 to CMS- both in the challenges of the detector needed for getting at the Higgs and in the management style.

We, of course, have other interviewees lined up but have interrupted the interviews at Matthew's request pending discussion of whether this involves any significant CERN resources.

Carlo Rubbia is an obvious next candidate and he has already said he would be willing.

To capture some of the lesser known historical figures we Have had advice from Ugo Amaldi and others.

Particularly important for filling in the US side has been advice from John Peoples and Chris Quigg.

In addition to the above materials, Paola and I have filmed interviews with the following people:

Jack Steinberger, Dick Garwin, TD Lee, Freeman Dyson, Maurice Goldhaber, Alfred Goldhaber, Murray Gell-Mann, Willis Lamb and others- parts of which may be useful for this project.

{Excerpt of transcript from DiLella interview added below}

(discussing the beginnings of an ISR experimental program and the "Split Field Magnet")

Luigi DiLella(LDL):The magnetic field configuration that was chosen was such that there was no magnetic field at 90 degrees. There was no analyzing power at 90 degrees.

It was a clever magnet just to do forward physics and this magnet was built. It is called the Split Field Magnet and it was filled with the newly invented Charpak Chambers-multi wire proportional chambers. So it was a frontier detector. With a frontier magnet. The magnet was very cleverly built in order to have the maximum bending power in the two forward directions at zero degrees with respect to the two beams- neglecting completely the possibility that interesting physics may occur at large angles.

So that was the end of '68. At the end of 1968 my fixed term CERN appointment finished and I moved to Columbia University.

There at Columbia University high energy physics the experimentalists are not housed in downtown Manhattan. They are at a laboratory along the Hudson about 20 miles north of New York City, which is called the Nevis laboratories and there the Director of the lab was Leon Lederman. He had just finished an experiment at the Brookhaven AGS.

It was one of those original and somewhat unconventional ideas by Lederman. He had taken the full beam at thirty GeV from the AGS and he had smashed it into uranium- a thick uranium target. Then there was a wall of iron to filter away all particles that were interacting strongly and then he was able to detect only muons coming out with scintillation counters and measured their energy by range in additional iron that was put there.

So he was looking for muon pairs with that detector. He had (if you like) a very high luminosity. He had a tremendous number of collisions. The full proton beam against the thick uranium target. But, what is called today, the "mass resolution" (namely the possibility of seeing narrow states) was not really optimal because he had only coarse measurement of the track directions and the energies of the muons. Nevertheless, he found a continuum of di-muon mass which was totally unexpected.

He also found a kind of structure at nearly the end of phase space which was later interpreted as the J/psi decaying to two muons- four years before its official discovery in 1974 (or five years).

But somehow they didn't believe in that signal.

S.White (SNW): But they believed the di-muons were a real signal?

LDL: They believed in the di-muon signal. When I saw these results for the first time I had been already a few months at Columbia and so that was in the middle of 1969. Then finally they published in 1970 the final results because the analysis of those events was not simple.

Now I remind you also that in 1968 in Stanford they had done this famous deep inelastic scattering experiment using electrons from the 20 GeV linear accelerator and they had found that when you look for inelastic interactions it looks as though the electron has hit- not really the proton- but something point-like inside the proton.

So in other words that the proton had inside it objects behaving as if they were point-like. Now this was totally unexpected ..

SNW: You could say that it was analogous to the Rutherford scattering experiment.

LDL: It was kind of analogous to the Rutherford scattering. In fact, I remind you that Rutherford discovered the nucleus inside the atom by just sending alpha particles onto an atom and showing that there was a small fraction of alpha particles scattered at large angles, which was totally unexpected from a model of the atom made of a uniform charge of distribution.

So that's how we discovered the nucleus and at SLAC they did more or less the same thing using however electrons and they found that when these are. Then when these electrons scatter at a large angle they produce hadrons and they behave as though they had hit point-like particles. ….