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The discovery of the W & Z, a personal recollection

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1 Preamble

It is such a pleasure to be back at CERN after four years of absence, on such a happy occasion, and to be able to meet again so many dear friends. I am very grateful to Luciano Maiani and to those who helped him with the organization of the event for having given me such an opportunity.

At the occasion of the twentieth anniversary of the W/Z discovery several articles have appeared in the press. Some are excellent, as that of Daniel Denegri, a former member of the UA1 Collaboration who reminds us in the CERN Courier of the spirit of discovery in those times. Unfortunately some others are mediocre, as the piece of gossip taken from Gary Taubes' *Nobel Dreams*, published and endorsed by Physics World in January. Such an article does no service to the history of science, it only retains a collection of anecdotes selected for their ability to seduce the general public, but this is not what history is made of. As a result it gives a completely distorted and misleading account of what had been going on. Worse, it makes no service to science by mistaking research for a horse race and scientists for bookmakers. The author, who had spent a few months with UA1, reminds me of the kid who was taken to the theatre to see a Shakespeare's play and who only remembered the shining uniform of the fireman on duty at the emergency exit without having grasped a single word of what was going on stage.

Each of us remembers only part of the story and our memories are always biased, whatever effort we devote to giving them documented support. We saw what was then the present through our own eyes and such are the images that we try later on to recall from our memories in order to reconstruct the past. What looked important to us was largely dependent on what we knew and on what we were unaware of at the time when it occurred. It is the work of the historian to put these various recollections together and to try to make a sensible story out of that material. I hope that this personal recollection can be used by him as a useful testimony of those times.

I have selected some topics among those that have been most grossly distorted by accounts such as that published in Physics World.

Pierre Darriulat

2 An announced discovery

The decade between 1967 and 1976 witnessed an impressive sequence of experimental and theoretical discoveries that have changed the vision we had of the world. To list just a few of the main milestones I may quote the prediction of electroweak unification in the lepton sector (Weinberg and Salam 67–68), the discovery of deep inelastic electron scattering at SLAC (69) immediately followed by the parton ideas and models (Feynman, Bjorken), the prediction of charm (Glashow–Illiopoulos–Maiani 70), the proof of the renormalizability of spontaneously broken gauge theories ('t Hooft 71), electroweak unification in the hadron sector (Weinberg 72), the discovery of neutral currents (Gargamelle 73), asymptotic freedom and QCD (Gross–Wilczek–Politzer and Gell-Mann–Fritsch–Leutwyler 73), the measurement of R at SLAC in electron–positron annihilations and the J/ψ discovery (74) followed in 76 by the discovery of naked charm (again at SLAC).

In 1976 the Standard Model was already there, ready to confront experiments, and it was clear that a new accelerator was required to explore the electroweak unification sector where the weak gauge bosons, W and Z, were ex-

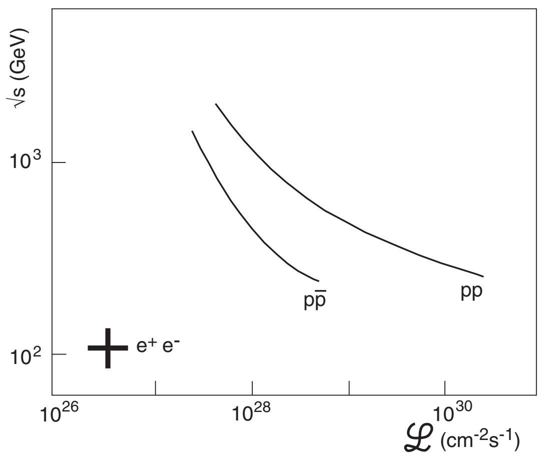

Fig. 1. Energy-luminosity requirements for the production and detection of 10 Z^0 (e^+e^-, $\mu^+\mu^-$) per year (from [4], page 758)

pected with approximate masses of 65 and 80 GeV/c^2 respectively: the arguments for LEP were present and strong (Fig. 1). I remember having been asked by John Adams to convene the LEP study group in April 1976 and to edit the report. In practice it meant listening and learning from John Ellis and Mary K. Gaillard all the beautiful new physics that was waiting for us, putting together some documents on the feasibility of the machine that were available following Burt Richter's seminal paper, and wrap it all up as fast as possible together with some bread and butter experimental comments. It took only seven months to get it all done, to the satisfaction of John Adams who wanted to push the LEP project in the wake of the success of the SPS that was just due to start operation at that time.

Moreover, it is worth recalling that supersymmetry blossomed between 1971 and 1974 and that, in principle at least, the arguments for LHC, a machine to explore the Higgs and low mass SUSY sectors, were also there. In practice, however, it took another few years before they could be expressed with some clarity.

3 The proton–antiproton choice

The 1976 situation sets the context in which the proton–antiproton decision was made.

The pressure to discover the W and Z was so strong that the long design, development and construction time of the LEP project left most of us, even the most patient among us, unsatisfied. A quick (and hopefully not dirty) look at the new bosons would have been highly welcome. But when *pp* colliders such as MISR or SCISR were proposed in this spirit, they got killed in the egg by the management with the argument that they would at least delay, or even worse, endanger the LEP project. This was accepted as a serious argument even by the proponents of such colliders. I remember having preached for SCISR, together with other ISR colleagues and with Maurice Jacob as our spokesman, and having been sent packing dryly by John Adams and Leon van Hove. They found it improper and somewhat irresponsible to make any noise that might divert CERN from the LEP party line and I must confess that I thought that, after all, they were right.

The same argument did not apply to the proton–antiproton collider that was not requiring the construction of a new collider ring and could be proposed as an experiment. One might object that this sounds like a bad joke because it implied the construction of an antiproton source that turned out later to include a collector/accumulator complex (AA/AC), but it remains true that the existence of the SPS, that was soon shown to perform extremely well, has obviously been an essential element of the success of the proton–antiproton project, not enough acknowledged in my opinion, and for which John Adams has to be credited. It is also true that John Adams found it difficult to swallow that his newborn baby should be pottered about with at such a young age and turned into a collider that had only little chance to work. This was indeed the feeling of the vast majority of machine experts at the time and much of the merit of Carlo Rubbia is to have pushed his ideas with such an untiring determination and in such an adverse context. Not only with determination but also with a clear vision of what they turned out to lead to and with a deep understanding of the machine physics issues at stake.

But another argument made it possible for the proton–antiproton project to break the LEP taboo. Most likely, if CERN hadn't bought Carlo's idea, he would have sold it to Fermilab. This threat was clear and had a very strong weight in the taking of the decision. In spite of the fact that the Fermilab machine was not performing well enough at the time to be used as a proton–antiproton collider, it very effectively accelerated the well known sequence of events that followed the publication of the 1976 paper by Rubbia, McIntyre and Cline. In 1977, after the proposal had been made to CERN and Fermilab to produce W/Z with existing machines, a feasibility study was undertaken by Bonaudi, van der Meer and Pope that led to the AA design, a detector study was initiated under Carlo that led to the UA1 design and the Initial Cooling Experiment (ICE) was proposed to the SPSC. Its success was demonstrated in June 1978 and the UA1 approval followed immediately. Only six months later was UA2 also approved.

It is very difficult to rewrite history, all events are so intricately linked to each other, but I strongly believe that, if it had not been for Carlo, there would have been no proton–antiproton collider physics in the world for a long time, maybe ever. Whether the weak bosons would have been discovered at LEP or at SLC or at some kind of a CBA is another matter, but it would have taken another six years at least. One might argue that six years is not that much after all, but the top quark would not have been discovered either (other than indirectly from radiative corrections at LEP) nor would we have learnt from the vast and rich amount of strong and electroweak physics data that have been collected at the SPS and Tevatron colliders. Not to mention the low energy LEAR physics, antihydro-

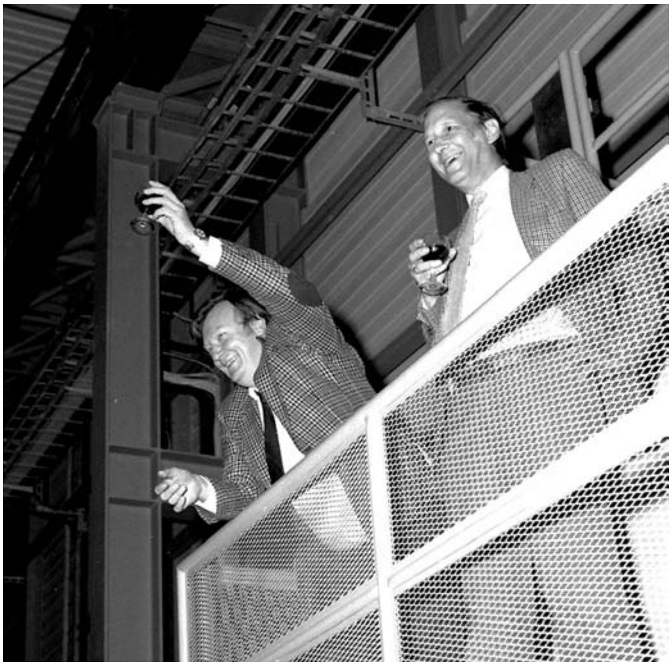

Fig. 2. Photograph of Carlo and Simon celebrating their Nobel Prize (Reference 523-10.84 from the CERN collection). As soon as it became known that the 1984 Nobel Prize was awarded to Carlo Rubbia and Simon van der Meer a celebration was organized in a CERN experimental hall, at LSS5. The happiness that they radiate was shared by the crowd of participants to the proton–antiproton project who attended the event and drank a glass in their honour. Undoubtedly, this has been one of the happiest days in the CERN history, maybe the happiest

gen, glueballs, CP violation, antiprotonic helium atoms, etc. If the Nobel Committee were to rewrite today the caption of the 1984 award to Rubbia and van der Meer (Fig. 2), they would undoubtedly say something like "for their decisive contributions to the large projects which led to the discovery of the field particles W and Z, communicators of the weak interaction, to the discovery of a sixth quark, the heaviest of all particles known to us today, to the exploration of the strong and electroweak interactions up to masses approaching the electroweak symmetry breaking mass scale, to the identification of new mesons such as glueballs and hybrids and to remarkable advances in atomic physics." I am fully aware that there is some irony to credit Carlo for contributions to the discovery of the top quark when one remembers some well known UA1 hiccups on that chapter, but I do mean what I just said.

4 Physics in the limelight and physics in the shade

Gossip only knows about what was going on in the limelight but history should also learn about what happened in the shade. Lacking such knowledge leads to oversimplifications and to distortions of the truth.

Such an oversimplification is the statement that before the W/Z discovery "CERN had been losing out on big discoveries to less conservative labs". It took a quarter of a century for Europe to reconstruct fundamental research after World War II. It has been a long and painful process that required tremendous efforts of many outstanding people. Learning about that history is both fascinating and extremely instructive. Those who take today too lightly actions that are detrimental to research and to science should learn how harmful they may be from the lessons of the history of this revival. Sentences such as the one I just quoted make so little of that history that they give a completely false account of the reality.

I do not mean to recall here the discovery of neutral currents in Gargamelle, this has just been done brilliantly by Dieter Haidt, but to say a word about the CERN Intersecting Storage Rings and the seminal role that they have been playing in the success of the proton–antiproton project. The ISR was the first hadron collider ever built in the world, the machine on which the young generation of machine physicists who designed, built and operated the antiproton source and the proton–antiproton collider (and later on, may be to a lesser extent, LEP) had got their hands in, had learned their experience and gained their expertise. It worked superbly, exceeding its design goals in both energy and luminosity. It is the machine on which van der Meer's ideas on stochastic cooling were tried for the first time, where they have been studied and understood. It is also the machine where a generation of physicists learned how to design experiments on hadron colliders. When the first ISR experiments were being designed the strong interaction was still a complete mystery, when the machine was finally shut down Quantum Chromo Dynamics was there. I do not mean to say that it is ISR physics that has taught us about QCD, but it has contributed to the development of several of its ideas and it has helped us greatly in drawing a clear picture of hadron collisions without which we would not have been able to design so effectively the UA, CDF and D0 experiments. A picture in which the soft $\log s$ physics and the hard parton interactions were separately described in simple terms. We, in UA2, were particularly indebted to the ISR where many of us had been previously working and for whom this experience had been an essential asset in designing a good detector.

It is not always clear what makes the spots of the limelight point to this physics rather than to that other. There is no doubt that they did point to the W/Z discovery that rightly appeared to be as emblematic of the progress of the new physics as had the J/Ψ discovery eight years earlier. In principle, there is no less beauty in QCD than in SU(2)×U(1) but one cannot name such an emblematic experiment in the strong interaction sector. Yet, from the deep inelastic electron scattering experiments at SLAC in 1969 to the studies performed at LEP of quark–antiquark and quark–antiquark–gluon(s) final states, there has been a quarter of a century during which the strong interaction theory and experiments have progressed hand in hand to a state of near perfection. Incidentally, I take this opportunity to express my admiration of an experiment that had been running in the shade of the proton–antiproton project just before the UA2 detector had been rolled into the ring, the streamer chamber experiment UA5 that, despite the very short data taking time that was made available to it, succeeded in giving experimental $\log s$ physics much of its most important results.

I should not like to close this chapter without recalling the extraordinary concentration of outstanding talents that the proton–antiproton project succeeded to attract. One reason was of course that between the SPS and LEP projects, one completed and the other still in the egg, its timing was in some sense ideal. But the other reason, possibly more important, was the challenging nature of the project that was proper to attract to it extremely bright engineers and physicists, both machine physicists and particle physicists. The challenge of designing, constructing and assembling the antiproton source and the detectors, and of getting them to work in such a short time, was enormous. As was that of digging and equipping the large experimental halls that were required for housing the new detectors that had to be alternately rolled in and out between collider and fixed target periods. As was that of making the transformations implied by the operation of the SPS as a collider. The amount of ingenuity that went into all these achievements was truly outstanding. My best memory of those times may indeed be the good fortune it was for me to work with so many talents, and, in the particular case of UA2, to enjoy collaborating with such bright colleagues, senior physicists, postdocs, students or physicists of the same generation as mine. CERN as an institution, and more generally the whole European particle physics community, were rightly proud of the success of the proton–antiproton project: it had indeed been the result of a very coherent and efficient collective effort.

5 The UA1/UA2 competition

In presenting the W/Z discovery as a race between UA1 and UA2 Taubes has shown that he did not understand well what had really been going on. There had been a race indeed, but it was at a higher level, between Europe, with CERN, and the United States, with Fermilab and SLAC. No doubt, the competition between UA1 and UA2 was real and lively, but it was relatively unimportant in comparison, it was anecdotic rather than historic, it was more a kind of a game, and we had a lot of fun in playing it.

There was no doubt that Carlo was the king of the proton–antiproton kingdom and was recognised as such by all of us. Undoubtedly, he would have had to take the blame if the proton–antiproton project had been a failure, but as it turned out to be a success he deserved to take the fame. Personally, I had been working in Carlo's group for six years or so, mostly on K physics, I had joined him as a postdoc in the mid sixties, coming from nuclear physics, and I had learned from him the bases of experimental particle physics. I had always been impressed by his brightness, by the readiness of his mind and by his far-reaching vision and I respected him then, as I do today, as someone of a clearly outstanding stature. To respect him as the king did not mean to belong to his courtship and we in UA2 were particularly keen at detecting occasions on which we could proclaim that the king was naked. Such occasions were very rare, the king was usually dressed splendidly, so they were the more enjoyable.

UA2 had been approved in order to create a competition to UA1 that was meant to provide a constructive and coherent emulation, and it served that purpose very well. We usually enjoyed a very friendly, helpful and even sometimes protective attitude of the management during the design and construction period, in particular from the research and accelerator directors, Paul Falk Vairant, Sergio Fubini, Erwin Gabathuler and Franco Bonaudi. Most of the time the management had the elegance to treat UA1 and UA2 on an equal footing, or at least to pretend to do so, and we were thankful to them for playing that game. There have been instances when the management did not have this elegance, I remember in particular having been called to the office of van Hove, together with Luigi Di Lella and Jean-Marc Gaillard, to pass a kind of examination before UA2 was approved (and therefore Sam Ting's proposal rejected). Van Hove wanted to check that we were not clowns. I also remember, the day when Carlo gave his W seminar at CERN in January 1983, namely the day before Luigi gave the UA2 seminar, to have found a routing slip on my desk from "the other" director of research ("our" director was out of CERN) stating that "if UA2 had anything to say that would contradict the statements made by Carlo, you should come and tell me beforehand". Clearly he did not care a damn about what we had to say, what mattered to him was only that we should not mess around and spoil the beauty of the UA1 results. Such inelegances were rare but were cruel to the collective self-respect of the members of UA2. Much more cruel than the tricks that UA1 may have been playing on us and that we were accepting as being part of the game. Indeed Rubbia himself has always considered UA2 with much respect, starting from the time when the experiment was being proposed. And the relations between the members of the two collaborations have always been excellent. The members of each collaboration were usually having several old (or less old) friends in the other and the senior members of both collaborations paid much attention to maintain this friendly atmosphere. We all were very indebted to Alan Astbury for having played a particularly constructive role in this respect.

The design of the UA2 detector had been a success and its construction and running-in went extremely smoothly. We were rightly proud of it. For a cost that was only one third of the UA1 cost (a condition to our approval was that the cost should be significantly lower than the UA1 cost) we managed to build a detector that was ready on time, that saw the W and Z as soon as the collider luminosity made it possible (and at the same time as UA1 did), that measured the W and Z masses more accurately than UA1 did and that was better than UA1 at detecting and measuring hadron jets. It was easier to design UA2 than UA1 because UA2 did not have to be a multi-purpose

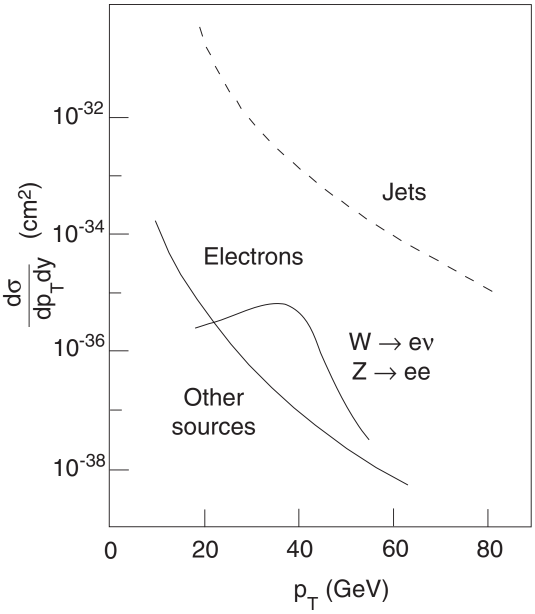

Fig. 3. The cross-section for the production of electrons from W and Z decays and from other sources is compared to the jet cross-section. A clear W/Z signal could be expected as long as the misidentification of hadronic jet faking a lepton could be kept below the 10^{-4} level. The bulk of the total cross-section, ~ 60 mb, 7 orders of magnitude above the W/Z cross-section, was easily eliminated at trigger level on the basis of the transverse energy deposition. In both UA1 and UA2 electron identification relied on the observation of a track having a good match to a calorimeter energy cluster, both track and cluster exhibiting features characteristic of an electron (from [4], page 760)

detector and could afford to simply ignore some of the physics, in particular to be blind to muons. The main asset of the UA1 detector was its central detector, that of UA2 was its calorimetry (Figs. 3 to 6).

A difficulty in making the right design had been to have a good judgement of how well the machine would perform, how long it would be to take off, how noisy and hostile an experimental environment had to be expected. Sam Ting's detector could have run in almost any background conditions but could only see muons, the UA1 central detector was requiring very clean conditions, UA2 was somewhere in between. The collider turned out to be an exceedingly clean machine and we had grossly underestimated how fast its luminosity would increase. In particular we had left an open wedge in our calorimeter, instrumented with a magnetic spectrometer, to do quietly, so we thought, some exploratory measurements while the machine would be being tuned and run in. The wedge did not stay open very long, the performance of the machine was progressing at high speed, and we were able to tackle the first high luminosity run with full calorimetric coverage.

It is sometimes said that UA1 was better than UA2 at detecting neutrinos. I do not think that this is true. What is certainly true is that UA1 did put much emphasis (and rightly so) on the virtue of using momentum imbalance as a W signature. But both UA1 and UA2 were well aware of the importance of measuring the lack of transverse energy balance in order to reveal the presence of neutrinos, this is beyond any doubt. It was indeed the main issue at stake in the SPSC discussion of the UA2 proposal that followed a DESY note written by Branson and Newman where they were ignoring what we were calling "background rejection by p_{T} balance". Moreover, ideas about neutrino detection from lack of p_{T} balance had been in the air for a long time and the 1976 report of the LEP study group (that I mentioned earlier) was already giving them due consideration (of course in the easier environment of an e^+e^- collider, but the idea was the same). What was not known was exactly how much the underlying soft secondaries would smear out the measurement accuracy of the p_{T} balance (very little it turned out to be). Moreover it took us some time to digest QCD and to realise that the W and Z (and for that matter any high mass structure in the final state) could be produced with large transverse momenta: the

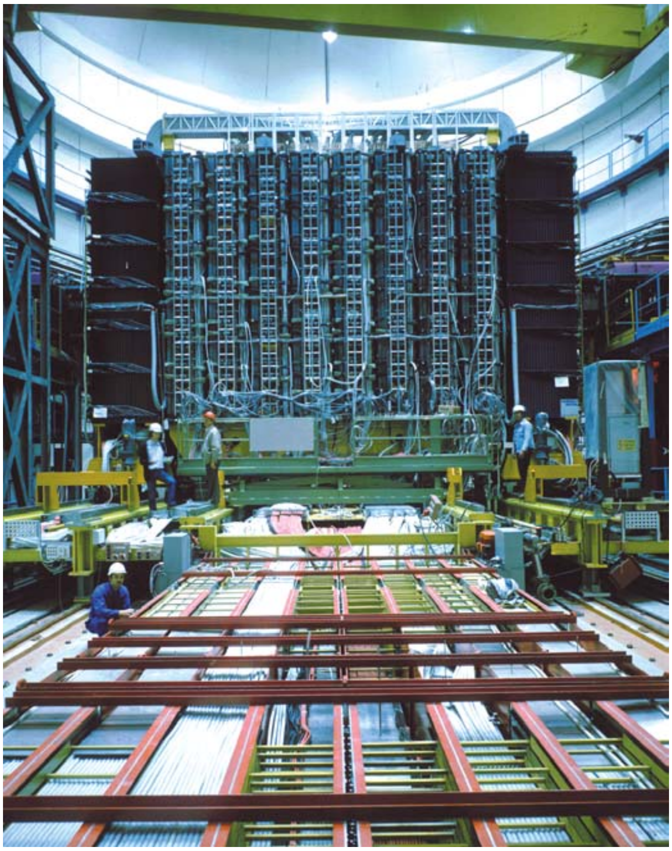

Fig. 4. Photograph of the UA1 detector (CERN reference X.595.04.81). The UA1 detector, shown here in its garage position, was a multi-purpose detector. It covered as large as possible a solid angle and was able to detect hadron jets, electrons and muons. This universality had been obtained at the price of compromises on the performance of its individual components: The 0.7 T dipole magnetic field was generated by a shoebox magnet segmented for hadron calorimetry, electromagnetic calorimetry was made in semi-circular lead-scintillator sandwiches (the "gondolas") surrounding the central detector

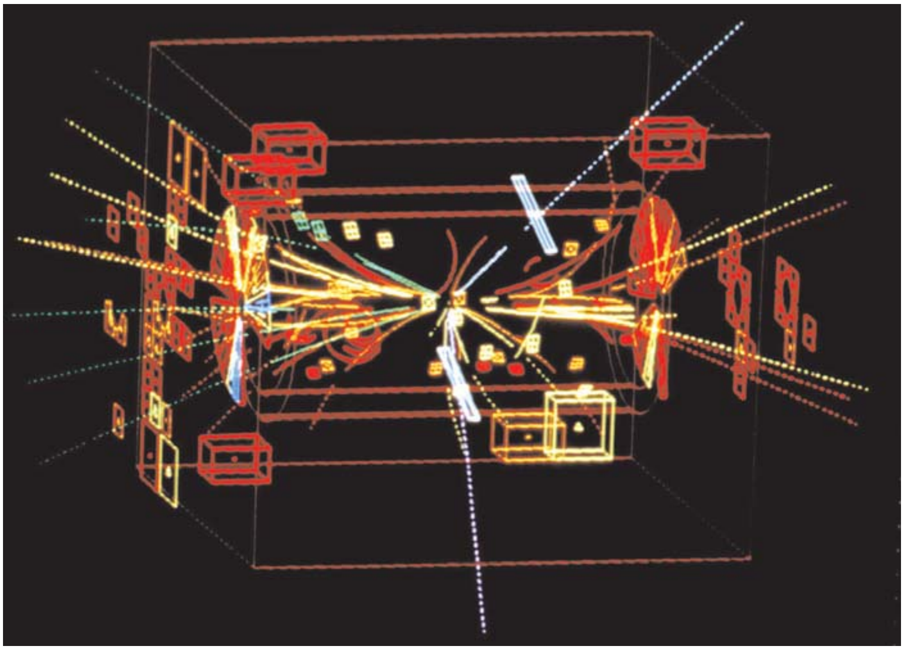

Fig. 5. Photograph of a UA1 Z^0 event in the electron channel (Reference X542.11.83 from the CERN collection). The main asset of the UA1 detector was a large volume, high-resolution central tracking detector, of an original and high performance design. It made it possible for UA1 to detect muons and tau mesons, to make precise checks of lepton universality and of the $V-A$ nature of the W-coupling, to detect muons in the vicinity of hadron jets, giving early evidence for BBbar mixing

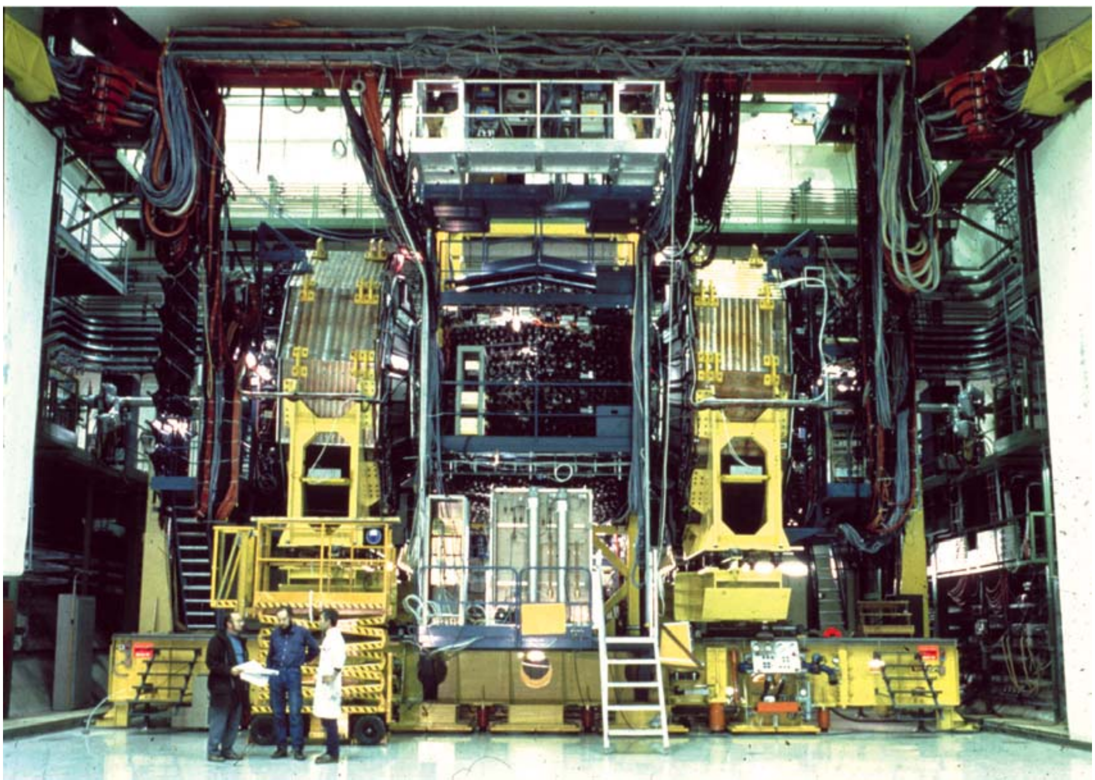

Fig. 6. Photograph of the UA2 detector (Reference X.559.3.83 from the CERN collection). The UA2 detector had a more limited scope than the UA1 detector: it could detect electrons but not muons, it focussed on the central rapidity region, it could not measure particle charges except for limited regions where the W decay asymmetry was maximal. But what it could do, it did better than UA1. It provided the most accurate measurements of the W and Z masses and its excellent jet detection capability, as illustrated by the identification of W/Z decays into two jets, gave important contributions to jet physics and to the study of the strong interaction sector. Its main asset was the fine granularity and projective geometry of its calorimeter design, with segmentation perfectly matched to the job. Tracking in the central region was done efficiently in a very limited space around the beam pipe

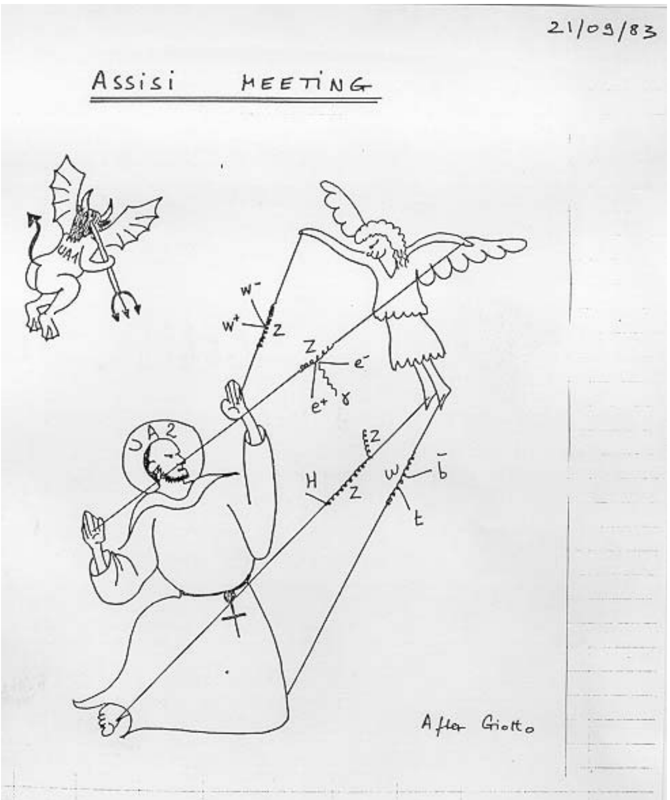

Fig. 7. The upgrade of the antiproton source into a double ring, accumulator and collector, was an opportunity to also upgrade the detectors. How to do it best was difficult to decide and the UA2 Collaboration met in Assisi in order to reach an agreement on the final design. The above drawing was sketched on that occasion in the hope of getting inspiration from such a mythic meeting place. The cute little devil representing UA1 is a good illustration of the omnipresence of the UA1/UA2 competition in our minds and, at the same time, of its ludic rather than dramatic nature

UA2 proposal had been written with the assumption of a Gaussian distribution, 1.5 GeV/c on average, therefore strongly damping the power law tail predicted by QCD. But being aware of the importance of a good neutrino detection was not sufficient. In fact both UA1 and UA2 were mediocre in terms of hermeticity. UA2 was suffering from a lack of coverage at small angles and UA1 from imperfections of the central calorimeters (gaps, insufficient segmentation and non projective geometry). In practice however, both experiments were hermetic enough for detecting in excellent background conditions weak bosons produced with not too high a transverse momentum and both UA1 and UA2 did it very well, each making optimal use of the background rejection power of the p_{T} imbalance signature of W production. But it became insufficient in studies of "monojet" events as UA1 called them, or when searching for the top quark, and the main purpose of the upgrades (Fig. 7) that both UA2 and UA1 proposed after two years or so of operation was to improve hermeticity (the UA1 upgrade never got implemented).

I do not wish to repeat here the often told stories about the first seminars and the first publications reporting the UA1 and UA2 discoveries of the weak bosons. But I wish to comment on how we perceived these events. As I already said, we were all expecting to see the weak bosons, we had no competition to fear from other laboratories and there was no question of UA2 "scooping" UA1 in the sense of stealing a Nobel prize or whatever as Taubes has been suggesting. I repeat that there was no question in our minds that Carlo (and of course Simon, but this is not what I am talking about) deserved the whole credit for the success; that what had been a real outstanding achievement was the production of the weak bosons, not their detection; that without Carlo and Simon there would have been no proton–antiproton collider but that without UA1 and UA2 there would have been other experiments that would undoubtedly have done as good a job; that the success of UA2 was largely due to the quality of many physicists who had been working together very efficiently and with an excellent team spirit and that it was impossible to single out a few of them as deserving a larger part of the credit. Of course there was competition, of course we enjoyed being faster or more clever than UA1 whenever we could afford to be, as when we were first at reporting to the 1982 Paris Conference the observation of very clear hadron jets, a breakthrough in the history of strong interaction physics. But this was not the dish, it was just the spices. The dish was serious business. It was reporting to the physics community what we had been finding. It was writing papers that would stay forever as important documents in the history of science. For years we had learned that this implied intellectual rigour and honesty, that it should resist biasing influences such as theoretical preconceptions, to make it short that it had to obey the ethic of scientific research. We surely were not to forget that in such an outstanding occasion. In retrospect I am proud that we resisted the pressure that was exerted on us to publish faster than we thought we had to. It would have been stupid and childish to give in and would not have shown much respect for science. In fact this pressure made us almost overreact and, in the case of the Z, it caused a delay of nearly two months between the UA1 and UA2 publications because we preferred to wait for the imminently coming new run and collect more statistics before publishing. There was virtually no dissenting opinion in UA2 that we should have behaved differently, we were all feeling quite strongly about it, in particular the wiser and more experienced members of the Collaboration (I mean the generation before mine) were giving their full support to this line. It is obvious today that there would have been no point in making a fuss about an event detected in 1982 that was most likely a Z but had one of its decay electrons not identified because it was hitting a coil of our forward spectrometer magnets. It is obvious today that we had been wise to wait for more statistics before publishing the Z results. The issue at stake was not to bet on the truth (as I explained already there would have been no pride in making the right bet) but to behave as if we had been the only experiment. There was no hurry from a purely scientific point of view, and there was no glory in taking any risk. Of course we had no reason to doubt that the events we were seeing were W's and Z's, what else could they have been? But this was not an argument to be taken into consideration, in our opinion at least. As in UA1, several of our W and Z candidates had some peculiar features, usually instrumental, sometimes real, like a $Z \to e^+e^-\gamma$ event that had been collected very early. Understanding all that was asking for some statistics and I do not regret that we decided to wait for the coming run. I am not at all trying to criticize UA1 for having published too early, this is not for me to judge. I am just trying to explain that this was not a very important issue, it was only the kind of media pressure and excitement that was prevailing in the community at that time that made it appear important. Anyone who followed these events knows well that both experiments had very similar data and that there was no scientific argument for one to publish before the other. At least this is how we felt in UA2. We thought that time would damp the noise and help in having a more serene look at the history of those happy days. This is why I find it so disappointing that a journal like Physics World, that has some pretension at being scientific, does exactly the opposite.

Scientists of my generation are very fortunate to have witnessed such amazing progress in our understanding of nature, in phase with our own scientific life. It is remarkable that this has not only been the case in particle physics but also, and may be to an even greater extent, in astronomy – in particular astrophysics and cosmology – and in life sciences – in particular genetics, molecular biology and neurosciences. While many questions remain unanswered in each of the three fields, none can be left aside any longer as being a mystery inaccessible to science. Our vision of the world has changed drastically. Having had a chance to contribute to this progress, however modest our contribution may have been, is a very happy fortune. May science be smiling at the next generation as kindly as it did to us with the new physics that LHC is soon going to reveal.

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7. L. Bertocchi et al., *Report of the Study Group on Physics with Antiprotons, Deuterons and Light Ions*, CERN, ISR Workshop, 4–15 October, 1976, 76-F-1