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Sheldon Lee Glashow

Comment on the occasion

I welcome this opportunity to celebrate the marvellous accomplishments of CERN, which is arguably the most successful of all international organizations and a show-piece of world-wide cooperation. I have been a visitor here on so many occasions: first as an NSF fellow in 1959-60, then as a paid visitor for a semester, as a frequent summer drop-in, conference participant, and member (now "old-boy") of the Science Policy Committee. These experiences have been central to the evolution of my own career in theoretical physics. Many of the experimental discoveries underlying our Standard Model have taken place here, among them the discoveries of neutral currents, weak intermediaries, and the many precision tests of the electroweak model carried out at LEP. But of equal importance is the fact that so many crucial developments in fundamental theory were either initiated, nurtured or perfected at CERN, by both its resident and visiting theorists. This is certainly so for me, as I am certain it is for many of my distinguished theoretical colleagues. CERN has always been, and must continue to be, the place where the action is, the Grand Central Station of particle physics, the crossroads of thousands of individual physicists' lives.

Today's Standard Model is a successful theory of almost everything. Although it has so far met every experimental test, many important questions remain unanswered, especially concerning the origin of electroweak symmetry breaking. More than ever before, the world-wide community of particle physicists is dependent on CERN, and in particular, on its timely construction, deployment and instrumentation of the Large Hadron Collider. Indeed, if not for the LHC and its enormous discovery potential, our discipline, already in distress, would be facing imminent demise. But we must also look beyond the LHC. As my colleagues and I have argued elsewhere, CERN must strive to preserve and transmit to future generations the hard-won art and know-how underlying our discipline. Only then can CERN continue to contribute, as it has done so magnificently in the past, to a better Europe and a better world.

Donald H. Perkins

A comment on perturbative QCD in early CERN experiments

We have heard today of how neutrino experiments and those at the proton–antiproton collider led to the discovery of neutral currents and the W and Z bosons, so validating the electroweak theory. I just wanted to remark here that these same experiments gave some of the first quantitative support for perturbative QCD, that other component of the Standard Model.

The first graph (Fig. 1) shows some results from 25 years ago on nucleon structure functions from the Gargamelle neutrino experiments at the PS and those in the BEBC chamber (Bosetti *et al.* 1978) and by the CDHS counter experiment (de Groot *et al.* 1979) at the SPS. Taking the difference of neutrino and antineutrino charged-current cross-sections measures the non-singlet structure function, that is the distribution in momentum fraction x carried by the valence quarks. Perturbative QCD makes a very simple prediction: the moment of the x-distribution varies as $1/\log q^2$ to a certain power, called the anomalous dimension, which depends on the order of the moment, the SU(3) nature of the colour symmetry and the spin of the gluon. Hence if one plots two different moments against each other on a log–log scale as q^2 varies over the range from a few GeV^2 to about 100 GeV^2, one should get a straight line with a slope equal to the ratio of the two anomalous dimensions. In fact the observed and calculated slopes agreed to within the errors of 5–10%, and verified the vector nature of the gluons. Scalar gluons – the

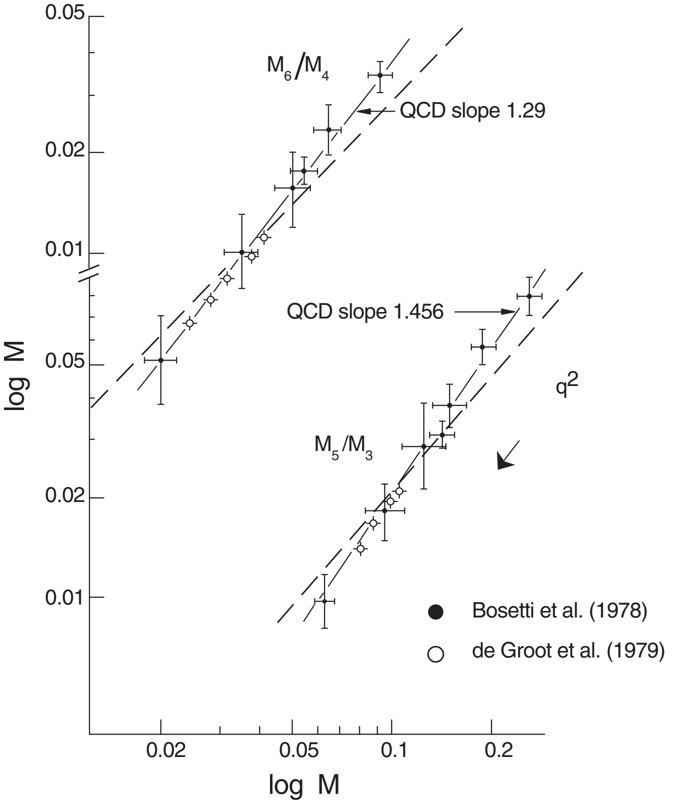

Fig. 1. Moments of non-singlet structure functions

dashed lines – were excluded at the 4σ level, long before the three-jet analysis at PETRA gave the same result.

Both the UA1 and UA2 experiments analysed the distribution of two-jet events at large angle, as a quark, antiquark or gluon from the proton scattered from one from the antiproton. The second graph (Fig. 2) shows the CMS angular distribution $d\sigma/d\Omega$ of these events in UA1 (Arnison et al. 1984), expected to vary roughly as $(1-\cos\theta)^{-2}$ (or as Rutherford would have said, $\mathrm{cosec}^4\theta/2$), corresponding to a $1/r$ potential mediated by vector (gluon) exchange. Again scalar gluons are excluded. It is interesting to compare that distribution with the one found by Geiger and Marsden exactly 75 years earlier (1909), for scattering of α-particles by gold and silver foils, again for a $1/r$ Coulomb potential mediated by vector (photon) exchange, shown by the dashed line. There are three differences. First, in the collider experiment there is a θ, $(\pi-\theta)$ ambiguity in the jet direction, so the distribution for $\theta > \pi/2$ has been folded into that for $\theta < \pi/2$. Secondly there are quark spin effects which do not apply for the spinless alpha-particles, which affect the distribution at large θ. Finally, while the Rutherford cross-section varies as α^2 which is essentially constant at the values of $q^2 \sim 0.2\ \mathrm{GeV}^2$ involved, the quark–quark scattering is proportional to α_s^2 which runs significantly over the relevant q^2 range of 100's to 1000's of GeV^2, so that at smaller angles and momentum transfers, the points deviate upwards from the line. But despite these differences, the similarity between these distributions strikes me as remarkable and a nice demonstration of unity in particle physics.

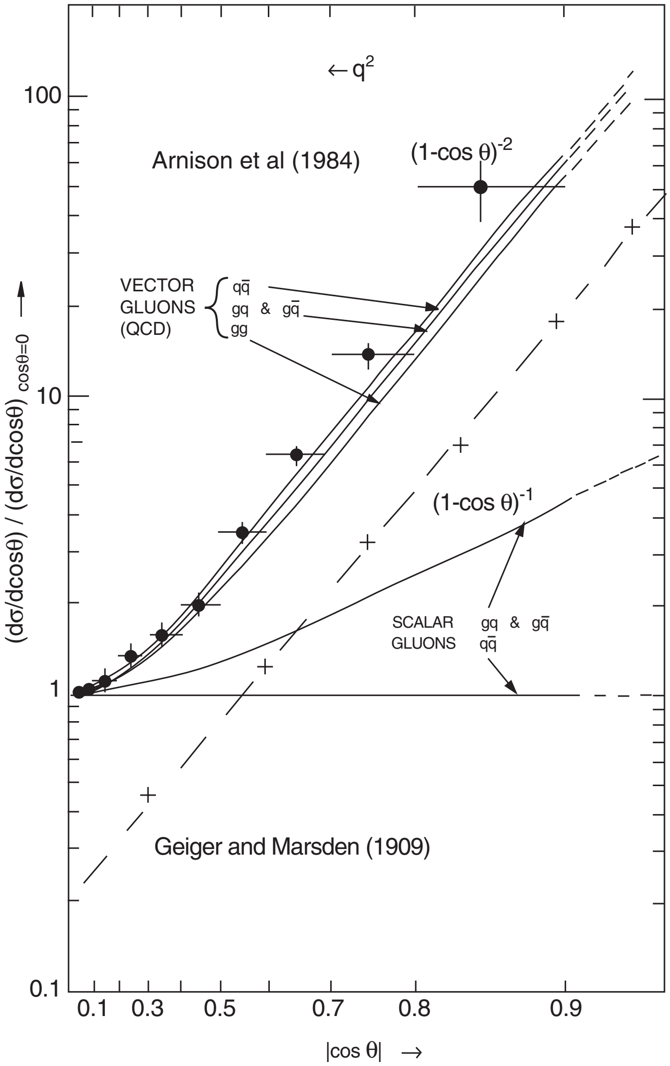

Fig. 2. Centre of mass, angular distribution of two jet events as measured by UA1, in 1984

Antonino Pullia

Let me make some personal remarks about the Neutral Current discovery in Gargamelle.

General remarks

I would like to remind you that during the 1960's there were good reasons to disbelieve the existence of neutral currents. Processes such as:

$$K^+ \to \pi^+ + \nu + \bar{\nu}$$

were highly suppressed [1]: the branching ratio for this kaon decay mode was less than 5×10^{-5}. Many experiments placed other, similar upper limits on strangeness-changing neutral currents. Since there was no reason at the time to believe that any relevant distinction existed between strangeness-changing and strangeness-conserving neutral currents, the reasonable conclusion that many physicists reached was that neutral currents simply did not exist.

Furthermore experimental limits were established also on strangeness-conserving neutral currents processes [2, 3]. I would like to remind you that the original Weinberg–Salam theory concerned only leptons and that quarks played no role at all. The experimenters were furthermore and correctly attracted by the new discoveries on partons at SLAC and by the opportunity to measure their quantum numbers by the interactions with neutrinos.

So in such a framework the search for neutral currents was not a high priority in the experiments in the world.

At the beginning of the 1970's theorists took a new interest in neutral currents; let me recall:

- the very important work of Glashow, Iliopoulos and Maiani [4] postulating a mechanism invoking a fourth quark and suppressing the strangeness-changing neutral currents, but allowing the strangeness-conserving ones;
- the work of 't Hooft providing the renormalization proof of the Weinberg–Salam theory [5];
- the calculation of the experimental consequences on the semileptonic neutral currents induced by neutrinos of Paschos and Wolfenstein [6] and Pais and Treiman [7].

Remarks on "Gargamelle"

I remember well a colloquium held in the small library of Gargamelle's building at the beginning of 1972 with Paul Musset and Bruno Zumino, Jacques Prentki and Mary K. Gaillard. Zumino explained to us the theoretical fascination of the new renormalizable theory of Glashow, Salam and Weinberg, suggesting the search for the muon neutrino and antineutrino scattering on electrons.

We (The Milan group and P. Musset, D. Haidt et al. at CERN) were already engaged in the study of the semileptonic neutral currents (see the meeting of the Gargamelle Collaboration in Paris in March 1972 where, on behalf of the Milan group, I offered preliminary evidence on the neutral currents existence [8]).

Just for fun, I remember that people of the Milan group found themselves out of their offices at the via Celoria, as students had occupied the Institute (Students Protest!). We had therefore an internal meeting in my house to prepare the Paris meeting of the Collaboration!

The very important problem of the neutron background is very well treated in the talk of D. Haidt in this Symposium. Probably he forgot to mention that the main author of the effort in this direction was himself. In early January 1973, the Aachen group found the famous candidate for a $\bar{\nu}_\mu + e^- \to \bar{\nu}_\mu + e^-$scattering.

The background for this process was really very small and at this point the whole Collaboration was excited and the search for neutral currents then stood at the center of everyone's attention.

Let me finally recall the strong pressure applied by A. Rousset and A. Lagarrigue to finalize the analysis of the events (done mainly by J.P. Vialle) and to publish a letter (July 1973).

I personally believe that without their strong belief, the collaboration would have delayed the publication of this very important discovery.

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