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Guido Altarelli and the evolution of QCD

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Summary. — I describe the contributions of Guido Altarelli to the development of Quantum Chromodynamics from the discovery of asymptotic freedom until the end of the $S p\bar{p} S$ collider era, 1973–1985.

1. – Introduction

I have been asked to write an appreciation of Guido Altarelli's role in the evolution of QCD. By evolution, I mean not only the evolution of the parton distributions, for which Guido is justly famous, but also the evolution in our ability to calculate with the QCD Lagrangian. In the Autumn of 1972 I arrived in Rome as a second-year graduate student, having been granted leave of absence from the University of Oxford where I was enrolled in the Theoretical Physics Department. My motivations for leaving Oxford were not entirely scientific, (see [1]), but Rome La Sapienza turned out to be a wonderful department, with an astonishing array of talent. As well as Guido Altarelli, there were Franco Buccella, Nicola Cabibbo, Raoul Gatto, Giorgio Parisi, Giuliano Preparata, and Massimo Testa and fellow students Roberto Petronzio and later Guido Martinelli. The nearby institutions hosted Sergio Ferrara, Etim Etim, Mario Greco, Luciano Maiani, Giulia Panchieri and Bruno Touschek.

As always in Italy, the organisational details were a little fuzzy and real progress was only to be made by exploiting personal relationships. I was formally assigned to study with Raoul Gatto, but the only office available for me was the office of Giuliano Preparata in a small corridor next to the office of Guido. I spent much of the first year in Italy, learning Italian and adapting to the new way of life. I had tried to work on a topic offered by Giuliano Preparata, but made little progress and when Giuliano left to take up a position at CERN, I started to work on a project suggested by Guido and Luciano Maiani. Thus began a 12-year collaboration with Guido. He was my mentor, collaborator and friend. After I moved to Fermilab in 1984, we remained close friends until his untimely demise in 2015, although we never collaborated again.

This intense period of our relationship corresponded with the establishment of QCD, initiated by the discovery of asymptotic freedom. How QCD evolved from a Lagrangian with the property of asymptotic freedom to a sophisticated tool for the calculation of high energy proecsses is the subject of this note. I would like to identify five periods of great change punctuating the evolution of the theory of the strong interactions, called Quantum Chromodynamics.

- 1970–1972, the pre-QCD years.

- 1973–1974, the discovery of asymptotic freedom and the first applications.

- 1976–1977, the Altarelli-Parisi equation, the demise of the k_T-cutoff, factorization and infra-red safety.

- 1979, the Drell-Yan mechanism and the beginning of QCD corrections to hadronic processes, and factorization beyond the leading logs.

- 1983–1984, the discovery of the W and the Z and the conclusion of the S$p\bar{p}$S era.

Guido himself has written his own perspective on these years [2]. Like that document, this note is not a professional history, but rather a sketch of these years as I remember them.

2. – Pre-QCD

The years 1973–1974 were watershed years for particle physics. Prior to 1973 there were models of strong interactions, but no real candidate theory. Although approximate scaling was established in deep inelastic scattering, the explanations for it were somewhat baroque. One explanation was that the commutator of the electromagnetic currents, comported itself as a free field theory on the light cone, but somehow conspired to be strongly interacting off the light cone. Another explanation was the parton model [3,4], which required an *ad hoc* cutoff on the transverse momentum of the partons to explain the scaling behaviour. The state of the theory was nicely summarized in the book of Feynman [5], written as a reaction to the data presented at the Cornell conference [6].

Guido spent 1968–1969 at NYU and academic year 1969–1970 as a Fulbright fellow at Rockefeller University. Early in the spring of 1970, the group of Leon Lederman presented preliminary results on the production of muon pairs at the meeting of the American Physical Society. Two months later Drell and Yan produced their paper [7] proposing the quark antiquark annihilation mechanism.

$$(1) \qquad \frac{\mathrm{d}\sigma}{\mathrm{d}Q^2} = \frac{4\pi\alpha^2}{3Q^2}\frac{1}{Q^2}\mathcal{F}(\tau)$$
$$= \frac{4\pi\alpha^2}{3Q^2}\frac{1}{Q^2}\int_0^1 \mathrm{d}x_1 \int_0^1 \mathrm{d}x_2 \delta(x_1 x_1 - \tau)\sum_a \lambda_a^{-2} F_{2a}(x_1)F'_{2\bar{a}}(x_2);$$

F_2 are (the quark and anti-quark components of) the deep inelastic structure functions. In a modern formulation there would be an additional factor of 1/3, due to the fact that *e.g.* a red quark can only annihilate with an anti-red quark.

The final data of the Lederman group [8], published in September 1970, showed a rapidly falling continuum spectrum in the mass Q of the muon pairs with a shoulder in

the region $Q \approx 3$–4 GeV. With the benefit of hindsight the shoulder can be ascribed to the J/ψ and ψ' observed at low mass resolution.

Altarelli, Brandt and Preparata (ABP) followed the results of Lederman's group closely, (perhaps too closely) and in September 1970 published a paper, based on a quark model with a scattering cross section having Regge-like properties which produced a shoulder in the region of 3 GeV. The model of ABP predicted non-scaling behaviour,

$$(2) \qquad\qquad \frac{\mathrm{d}\sigma}{\mathrm{d}Q^2} \sim \left[\frac{1}{Q^2} F_1(\tau) + F_2(\tau) \right].$$

Although, ultimately the model of Drell and Yan gave the correct description, the familiarity with the muon-pair production process would prove to be important for future research.

3. – Asymptotic freedom

The years 1973–1974 were years of great change for both theoretical and experimental particle physics. The papers indicating that non-Abelian gauge theories were asymptotically free were published in June 1973 by Politzer [9] and by Gross and Wilczek [10]. 1974 started with the prediction that charmed quarks should have masses less than 5 GeV [11], based on the cancellations inherent in the GIM mechanism [12]. Subsequently, in November 1974, the J/ψ was discovered [13,14]. The ADONE accelerator at Frascati was able to raise the energy to 3.1 GeV to produce the J/ψ [15], and private communications with the experimenters indicated an observed forward backward asymmetry in the muon pairs produced on resonance. This apparent asymmetry gave rise to two papers from the Rome group, the first [16], interesting but incorrect, claiming the particle discovered was the Z-boson, the second [17], (presumably) correct, but by the time it appeared uninteresting (because the large asymmetry had gone away), calculating the forward-backward asymmetry in the presence of a vector resonance.

In January of 1975, Guido and other members of the Rome group returned to consider the Drell-Yan process, with an explicit model of the parton distributions [18], this time taking into account the production of the J/ψ.

3`1. *The first applications*. – The acceptance of QCD as the correct theory of the strong interactions was slow. For a time many papers (including ours) began with tentative phrases, such as *The gauge theory of colored quarks and gluons (QCD) is at present the best candidate for a theory of the strong interactions* [19]. Many influential people, felt that when a correct theory was found, it should instantly make sense out of a disparate range of experimental data. Thus, as late as 1976–1977, Feynman and Field, proposed their black box model to explain inclusive scattering data [20]. This despite the fact that reliable lowest order cross sections for parton-parton scattering became available in 1977 [21]. The energies were not high enough for the $1/p_T^4$-behaviour characteristic of Rutherford-like scattering to reveal itself.

In addition to the insufficient energy, the other reasons for this theoretical hesitation are not hard to understand, and were presented by Guido in his write-up of the 1983 session of the Moriond conference. *"Alhough QCD essentially imposes itself as the only theory of the strong interactions within reach of the weapon arsenal of conventional quantum field theory, yet QCD is still the less established sector of the standard model. Testing QCD is in fact more difficult than testing the electroweak sector. In the latter*

domain perturbation theory can always be applied. Also the leptons and the weak gauge bosons are at the same time the fields in the lagrangian and the particles in our detectors. Instead QCD is a theory of quarks and gluons while only hadrons are observable. Moreover perturbation theory can only be applied in those particular domains of the strong interaction where approximate freedom, which is only asymptotic, can be reached" [22].

But the excitement of having a candidate theory of the strong interactions was lost on no-one. The first applications were limited to processes governed by the operator product expansion. In addition to deep inelastic scattering, which was treated shortly after the discovery of asymptotic freedom by the protagonists themselves [23,24], Ken Wilson had suggested [25] that the $\Delta I = 1/2$ rule might be explained by strong interaction effects, but now one had a theory with which one could calculate. The challenge was taken up by Altarelli and Maiani [26] in Italy and and Gaillard and Lee [27] in the United States. It was found that in the standard model, the strong interactions did indeed give an enhancement of about 4–6 in amplitude, too small to explain the whole observed amplitude enhancement of 20. My thesis project was to calculate whether there was a similar pattern of enhancement in Parity Violating processes in Nuclei which proceed via the weak interaction [28]. However at this point the real challenge was to find a way of going beyond the operator product expansion.

4. – The Altarelli-Parisi equation

The precursors of the paper on the Altarelli-Parisi equation were presented at two back-to-back winter conferences at Flaine in the French Alps in 1976 by Altarelli [29] and by Parisi [30]. The first paper deals with the translation of deep inelastic data, especially neutrino data, from the language of the operator product expansion into scale-dependent parton distributions. The second paper contains an early form of the AP equation; versions of the splitting functions are presented, but some of the details are wrong. I am acknowledged in the paper by Parisi, but only because I helped to correct the English.

In September 1976, frustrated with the progress of his career in Rome, Guido went on sabbatical to ENS in Paris. As luck would have it Giorgio Parisi was also visiting Paris. The paper on the evolution of the parton distributions was written there. The Altarelli-Parisi (DGLAP) equation [31,32] changed the way that we thought about deep inelastic scattering. It made it quite clear that the scale dependence of the parton distributions was processs independent. In the paper the splitting functions were calculated from the branching probabilities, without reference to any particular hard scattering process. This simple branching picture only holds in a physical gauge in which only the transverse degrees of freedom of the gluon field propagate. The splitting functions were calculated in old-fashioned perturbation theory, in which manifest Lorentz invariance is lost, but unitarity is simpler. Importantly, the calculation thrust the attention back on the Feynman diagrams, rather than the operator product expansion, the proofs of which were not widely understood. Before the publication of their paper there was little understanding of the diagrams resummed by the renormalization group in the operator product treatment of deep inelastic scattering.

Guido worked on the manuscript of the paper [31] through the winter of 1976–1977, and I passed through Paris and was shown a copy of the manuscript. He was concerned that the paper would not be accepted for publication, because although the paper introduced a new language, for the unpolarized structure functions the results were simply the inverse Mellin transform of results previously obtained by Christ, Hasslacher and

Mueller [33] for the Abelian case, and Gross and Wilczek [24] and Georgi and Politzer [34] for the non-Abelian case. To be sure it was an inverse Mellin transform that required the introduction of a new quantity, the plus distribution, similar in spirit to the delta function of Dirac, but neverthless it was just an inverse Mellin transform. To forestall any possible objection by a referee, calculations of the polarized splitting functions, which had recently been calculated by others [35, 36] were included.

The treatment of Altarelli and Parisi raises the question of how QCD and the parton model could be reconciled for the Drell Yan process. In the summer of 1977 Politzer wrote his first paper on factorization [37] in hard processes treating the specific case of muon-pair production.

5. – Drell Yan and the K-factor

The first order of business was to determine whether the transverse momentum of the muon pairs, was limited as predicted by the naive parton model, or whether it grew with Q^2 at fixed $Q/\sqrt{S}$ as required by QCD. The issue was clouded by the fact that an experiment running at fixed $\sqrt{S}$, could not directly investigate this scaling. Detailed calculations [19, 38] showed that the average value of the transverse momentum was expected to be approximately constant at fixed $\sqrt{S}$ and increasing Q, as the perturbative growth with Q was balanced against the fall-off of the structure functions.

The next challenge was to calculate higher order corrections to the Drell-Yan process. From an operational point of view, parton distributions measured in leptoproduction were used to predict cross sections for muon pair production. In our theoretical approach [39] we followed a similar logic, finessing the issue of how much was factorized into the parton distribution by comparing the perturbative results for deep inelastic and Drell-Yan. Thus parton distributions were defined beyond the leading order in terms of the radiatively corrected deep inelastic structure function F_2. As a consequence of the Adler sum rule, which has no perturbative corrections, this choice had the nice feature that the number of valence quarks in the proton remains fixed at all orders in QCD perturbation theory. To perform these calculations a regulator is required to control the divergences associated with soft and collinear parton emission. In our first paper this was achieved by taking the quarks slightly off their mass shell. This was doable, but quite cumbersome. The second paper [40] regulated the singularities using dimensional regularization, which was much more efficient. This is now a textbook calculation, and established the method used to calculate all higher radiative corrections to hard processes in QCD. The corrections to the Drell-Yan turn out to be large at the values of Q which were probed at that time. In fact they were so large that one might doubt the validity of perturbation theory. The data [41] also showed an excess over the tree level prediction of the Drell-Yan model by a factor of about two. This was dubbed the K-factor by Guido [42]. Particularly significant was the data with antiproton beams, which in the Drell-Yan picture proceeded via the annihilation of two valence quarks. The occurrence of the same K-factor in this case showed that ignorance of the true size of the sea quark distributions, could not be responsible for the effect.

In 1979 I moved from MIT to Caltech. On arrival I was pleased to discover that Feynman and Field were repeating the calculations that we had done and published [40, 43] the previous year. The fact that Feynman felt that our calculations were important enough to repeat, was a big psychological boost for me; Guido's leadership had placed us in the major league.

6. – S$p\bar{p}$S collider: the discovery of the W and Z

The S$p\bar{p}$S collider was an opportunistic project to exploit quark antiquark annihilation, with the explicit goal of discovering the W and the Z. Given our history with the Drell-Yan process we were perfectly poised to exploit the physics of the S$p\bar{p}$S collider, which in the main was the physics of quark-antiquark annihilation. To be sure, we were able to write authoritative papers on the total cross section for W and Z production, but also for the the p_T and rapidity distributions for the produced bosons [44, 45]. Our theoretical prediction for the p_T distribution of the W-bosons is quoted in the Nobel lecture of Carlo Rubbia [46].

As an historical aside, I note that the discovery of the W and Z caused consternation in America, since the bold strategy of building the S$p\bar{p}$S at CERN initiated the transfer of the leadership in Experimental High Energy physics to Europe. This loss of primacy was especially bitter at Fermilab since the proton anti-proton machine had been originally proposed for the Fermilab main ring [47]. Although it might be a *post-facto* rationalization, the experiment would probably not have worked at Fermilab. "The vacuum system was terrible, so beam lifetime (number of antiprotons) and, worse, luminosity lifetime (beam size due to multiple scattering) would have been very poor. At injection (8 GeV) the beam lifetimes were measured in seconds, which was made worse by poor magnetic field at injection. As one accelerates those problems become less severe, but the top energy was limited by the magnet design. The Main Ring magnets could not be operated DC at more than, probably, 200 GeV because they would burn up" [48].

After the great success of the S$p\bar{p}$S collider in discovering the W and Z bosons and confirming the standard model, there was a desire to exploit the machine to the utmost. Could it also give signatures of physics beyond the standard model? At the Bern Conference in 1984, Carlo Rubbia presented evidence for five events with greater than 40 GeV of missing energy [49], and, in my recollection, declared the Standard Model to be dead.

The following year, directly after the presentation of Rubbia at the Saint-Vincent conference [50], Guido stated his opinion that a cocktail of standard model processes plus a few cracks in the detector could explain the monojet events. Our theoretical understanding of the p_T distributions of produced W, Z bosons, gave us great confidence in the predicted rates for monojets coming from Z+jet events, with the Z decaying to neutrinos. As stated by Roy Schwitters in his conference summary [51], "The basic point is that by a combination of improbable but conventional processes such as Z^0 + gluon where the Z^0 decays to neutrinos, single W production followed by tau decay, and measurement errors, one may be eventually able to explain all the mono-jets. At the workshop this became known as Altarelli cocktail". The Altarelli cocktail was the beginning of the end for beyond-the-standard-model explanations of the monojet events.

7. – The man himself

Guido was a masterful conference summarizer, managing to take stock of all the important issues with great clarity. His presentations were colourful and presented in an idiosyncratic English that left no doubt about his Mediterranean origins. I remember him with his tall frame leaning slightly toward his interlocutor, his long elegant hands gesticulating to emphasize his point.

As Guido's student I was given various pieces of advice. One was to cultivate "il senso del gioco", an understanding of the strategy of the game, a concept taken from professional soccer. Throughout his professional life Guido displayed a consummate

understanding of the game, a keen understanding of what was important, and what was not. I may not have mastered "il senso del gioco" but I certainly learnt from Guido's sense of fun, his wry sense of humour and his playful use of the Italian language.

When my time as a post-doc in Rome came to an end, it was time to get letters of reference. Nicola Cabibbo was the most famous person in the department, with whom I had contact. So I summoned up my courage and asked him for a letter of recommendation. Nicola's answer was that if I wrote the letter of recommendation, he would sign it! (Nicola was not a man to trouble himself with things that did not interest him). I tried to draft such a letter, but found out impossible. When informed of my dilemma, Guido's response was immediate and predictable, "Write a letter which says that Keith Ellis is more talented than Nicola Cabibbo, and see if he signs it".

8. – In conclusion

It has been one of the most rewarding periods of my professional life to work with Guido Altarelli. With his leadership, we were able to play our part in turning the fledgling theory of QCD into the sophisticated calculational engine that it has become today. As a recapitulation of what was achieved in those early years, I can do no better than quote Guido himself, and so here is a sentence from his plenary talk [52] at the Bari conference in 1985. *"The beautiful naive parton model of Bjorken, Feynman and others has by now evolved into the QCD improved parton model. This powerful language has become such a familiar and widespread tool for everyday practice in high energy physics that one is led to take all its new successes as granted and in a way obvious."*

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