QCD, from its inception to its stubbornly unsolved problems

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Whenever one has witnessed some event and then sees it reported in the media, one’s reaction is the same: it was not quite like that. It is in this spirit of a frequent first-hand witness that I write this article. I discuss a few selected points which—to my judgement—illustrate well the QCD evolution (in time) from the theoretical, phenomenological and experimental points of view.

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1. Foreword

This is not a review of QCD, of which there are so many that simply trying to select some of them to be quoted would be quite an endeavor. Instead, this article is a version of a colloquium at CERN written at the request of the publisher.

Adapting to a fashion in novels and films, the text repeatedly jumps from the past to the future and back. This is not a choice of style, but an attempt to organize the document by subject matters.

My having witnessed a good fraction of the QCD developments I describe may make the text vivid, but somewhat self-serving. Yet, my eminent collaborators—who do not need any extra praise—may perhaps appreciate it.

2. The prehistory of quarks

Back in 1949 Enrico Fermi and Chen Ning (Frank) Yang published an impressively prescient paper. They posited the notion that some of the few particles then known might not be elementary, but composite. They assumed that pions are made of a nucleon and an antinucleon. Treating the $\pi^0$ as a $p\bar{p}$ bound state they figured out that its (then unknown) parity ought to be opposite to that of the proton. With the two constituents spinning in the same direction, they foretold the existence of the $\rho$. They also discussed how the mass of the pion may be so much lighter than
that of a couple of nucleons if the latter pair were placed in a very deep square-well potential. Fairly good for a five-page-long paper!

Also in 1949 Jack Steinberger published a calculation\textsuperscript{3} of the lifetime of the $\pi^0$, which—besides giving a good estimate—anticipated the study of “triangle diagram” anomalies in quantum field theory, as well as the predictions of the rate of gluon fusion in the production of a Higgs boson and the width of the $H \rightarrow \gamma\gamma$ decay. Not bad for someone trained as a chemist. Soon after his first steps as a particle theorist, Steinberger moved to the University of California at Berkeley, where he became an experimentalist, but continued to study neutral pions. Fermi and Steinberger are examples of the then much weaker dichotomy between experimentalists and theorists.

### 3. The first non-abelian gauge theory

Unbeknown to most, the Apollo 11 astronauts actually did something useful, other than testing Moon boots, as in Fig. 1. In 1969, they placed the first passive laser reflector on the Moon. After many years of sporadic lunar ranging measurements, some of the length parameters describing the lunar orbit are known with millimeter precision. In a considerable improvement of Galileo’s supposed experiment at the Leaning Tower of Pisa, the Earth and the Moon are measured to “fall” towards the Sun with the same acceleration with a precision of $\sim 2 \times 10^{-13}$. This is called the Nordtvedt test of the Equivalence Principle\textsuperscript{4}

The gravitational self-mass of a uniform extensive body is $\Delta M \propto G_N M^2 / R$. More precisely, this quantity is $\Delta M_\oplus \approx -4.6 \times 10^{-10} M_\oplus$ for our planet, and $\Delta M_\mu \approx -0.2 \times 10^{-10} M_\mu$ for its satellite. If these self-mass contributions were attracted by the Sun differently from the bulk of the mass of the two bodies, differing accelerations would result\textsuperscript{5} In actual numbers, we know that the Earth’s bulk acceleration and that of its self-gravity are equal to a precision of $2 \times 10^{-3}$.

Einstein’s General Relativity (GR) is akin to a non-Abelian Yang-Mills theory in the sense that gravitons gravitate. The diagrammatic translation\textsuperscript{6} of the previous paragraph is shown in Fig. 2. What all this means is that we know the triple-graviton coupling to be what it should be, to 2 thousands. This is better than the precision to which we know the “triple-gauge” couplings of intermediate vector bosons or gluons, or the equality (up to group-theoretical factors) of the colour charges of quarks and gluons. There is a long way to go before we have tested Yang-Mills vector-boson theories to an astronomically satisfactory accuracy!

For the purist I ought to add that in drawing and interpreting Fig. 2 I seem to have assumed that gravitons exist (no reason to doubt it). What the Nordtvedt test really tests is that the energy-momentum tensor of the Coulomb-like gravitational self-energies of the Earth and the Moon couple to an external gravitational field with the strength predicted by GR for this non-abelian coupling. Much as one would derive the Coulomb potential from the Fourier transform of a Feynman diagram involving a known-to-exist photon... I have drawn and interpreted Fig. 2.
Neil Armstrong and Buzz Aldrin, the astronauts of Apollo 11 who set foot on the Moon, also left a seismometer on its surface. The lunar seismometer, measuring moon-quakes, inaugurated the study of the inner properties of celestial bodies other than the Earth. Later Apollo crews brought with them more precise instruments of these and other types.

One moral to be extracted from the above is that once upon a time fundamental physics—such as testing the Equivalence Principle—was considered sufficiently important to include a fairly massive light reflector on the first Moon landing. No need to emphasize how difficult and significant such a “sacrifice” of weight-to-be-lifted must have been.

4. Who invented quarks?

A tricky question. The official history is that they were invented by Gell-Mann and Zweig, in that chronological order: Gell-Mann’s published paper [7] was received by Physics Letters on January 4th 1964, while Zweig’s unpublished work is a CERN yellow report [8] dated January 17th of the same year. But, as Napoleon is said to have said: History is the version of past events that people have decided to agree upon.

According to the same un-trustable source of the previous quote (Internet) Gell-
Mann’s paper was originally rejected by Physical Review Letters. Allegedly untrue. None of this would significantly change the chronological order, which is anyway fairly irrelevant since the dates were so close. Concerning dates, it must be recalled that Gell-Mann wrote: “These ideas were developed ... in March 1963; the author would like to thank Professor Robert Serber for stimulating them.” For Serber’s recollections, see Ref. [10].

According to Serber’s autobiography[10] he knew how to build representations of SU(2) from the fundamental spinor one. In March 1963 he was trying to extend this construction for SU(3) and he mentioned all this to Gell-Mann, who promptly realized that the components of the required 3 and 3 bar representations of SU(3) would have to have fractional charges. Their existence would be a strange quirk of nature, and quirk was jokingly transformed into quark[10].

A point in the official history[11] is lacking. André Petermann published a paper (in French!)[13] received December 30th, 1963, shortly before the dates quoted in the first paragraph of this section. In this paper he discussed mesons as made of a spinor/anti-spinor pair and baryons as composed of at least three spinors. Concerning the delicate issue of their charges, Petermann delightfully writes: “if one wants to preserve charge conservation, which is highly desirable, the spinors must have fractional charges. This fact is unpleasant, but cannot, after all, be excluded.

Fig. 2. The Earth and the Moon being attracted to the Sun. The triple-graviton vertex represents the pull by the Sun on the gravitational self-energy of the Earth.
There are other unofficial issues concerning this chapter of the history of science. Was Zweig forbidden to have a preprint typed and to give a talk at CERN at the time? If so, by whom? I shall not answer these questions, but another one which I have been challenged to answer: why was the publication of Petermann’s paper delayed for a year? Alas, nobody has found the original CERN preprint, yet. So, one cannot disprove something evil I have been told, namely that Petermann had plenty of time to change the paper before publication. That was not his style. Not only did he publish this paper in French –guaranteeing a dearth of readers– but, though he always drove a Porsche and on occasion coached the Swiss ski team, he was extremely slow at anything else, including bothering to correct the proofs of an article.\footnote{Having been a coauthor of Peterman’s, I know this first hand. Notice also that I have spelled his name differently here. He did not even care to sign all his papers with the same name.}

5. Searching for free quarks

Gell-Mann upheld, perhaps for longer than anybody else, the view that quarks were mere mathematical objects. After all, the notion that hadrons are made of parts – but cannot be taken apart– is not that easy to accept, even grammatically. But none of this deterred experimentalists from looking for unconfined quarks and dozens of searches were performed.\footnote{Amongst the many experiments hunting for free quarks, perhaps the most original one was based on analyzing oysters; it was conducted by Peter Franken and collaborators. The liver of an oyster, it is stated, is one of the best filters anywhere. Every day it processes an amount of water some thousand times its weight. In so doing, it accumulates all sorts of peculiar substances that are preserved in its growing shell. An atom or molecule containing one extra quark would have a fractional electrical charge and should have a very peculiar chemistry. It would presumably be sieved by the oyster’s liver. Alas, these experimentalists did not find any quarks. But, in the process of studying a barrel of New England oysters a day, they probably gained some weight. Not all experiments failed in finding evidence for fractionally charged objects. Perhaps the most notorious “successful” search was the one performed by William Fairbank and collaborators. It employed the venerable Millikan technique, using Niobium-coated Tungsten balls, and found one with charge $(0.337\pm0.009)e$. Needless to say (now) the finding could not be reproduced.}

6. Other quark mysteries, also unveiled

The naive (constituent) quark model was impressively successful in its understanding of hadrons made of $u$, $d$ and $s$ quarks and in predicting the existence, decay
pattern and mass of the \( \Omega^- \). But quarks and their confinement remained mysterious, more so because of the complementary evidence in SLAC’s deep-inelastic electron-scattering experiments for charged constituents of protons\(^{15}\) Feynman’s partons\(^{10}\) with “point-like” interactions with photons: Bjorken’s scaling\(^{20,21}\).

Since it has been done so very many times, I shall not discuss the original literature on Yang-Mills theories\(^{22}\) QCD\(^{23–25}\) the electro-weak standard model\(^{26–29}\) the necessary existence of charmed quarks\(^{30}\) and the renormalizability of non-abelian gauge theories\(^{31,32}\).

The discovery of strangeness-conserving neutral currents in neutrino scattering by the Gargamelle bubble-chamber collaboration at CERN\(^{33}\) made experimentalists, and the world at large, aware of Yang-Mills theories, much as the 1971 work of ‘t Hooft\(^{31}\) and ‘t Hooft and Veltman\(^{32}\) immediately attracted attention from (field) theorists to the same subject. For the hypothetical young reader I must emphasize that, at the time, the fact that the Standard Model had all the chances of being “right” was only obvious to an overwhelmed minority of field-theory addicts.

The understanding of how quarks behaved when probed at short distances had to wait for the discovery\(^{6}\) of QCD’s asymptotic freedom\(^{34,35}\). At the time David Politzer’s office was next to mine at Harvard. David Gross and Frank Wilczek were at Princeton. The Harvard/Princeton competition was acute\(^{36}\) and productive.\(^{6}\) To characterize it, suffice it to say that Harvard’s motto is VERITAS (Truth), while Princeton’s is DEI SVB NVMINE VIGET (God went to Princeton).

In a talk reproduced in Ref. 37, Howard Georgi recalled how everybody, in years long past, knew Harvard as the place not to be. He was the seventh of a long list of applicants. The first six had chosen “better” destinations. One year later, I was to share Howard’s honour. It turns out that I was not quite at the right place at the right time but I was, literally, next door. Indeed, when QCD’s asymptotic freedom was discovered David Politzer had the office next to mine at Harvard. He was a Junior Fellow and I a lowly post-doc. Some of the outcasts that gathered in this back-door way changed physics (and Harvard) forever. Now Harvard is again a place where to be, but for far far more formal reasons.

In the late ’60s, it seemed perfectly ridiculous for the strongly interacting partonic constituents of protons to do what they do: exhibit a “scaling” free-field behaviour\(^{20,21}\) in deep inelastic scattering experiments\(^{13}\). Thus, though the full rationale for a rather low-energy “asymptotia” remained obscure for a while, the discovery of asymptotic freedom was received with a great sigh of relief by field-theorists.

\(^{b}\)The fact that QCD’s asymptotic freedom was first noticed by ‘t Hooft, Symanzik and perhaps others made me emphasize “discovery” which, when not italicized, includes the realization of how important something may be.
7. A renewed call for leniency

Hereafter I am going to cite papers in an unbalanced way, with a large fraction of references to articles authored or coauthored by me. Part of this is a proximity effect, I am writing as a witness and a $1/r^2$ law is inevitable, a price to pay for personal recollections, often more colorful than “official” histories.

As Golda Meyer put it: *Don’t be humble... you are not that great.* That is correct in my case, but it does not apply to any of my to-be-cited coauthors, as the reader will easily recognize.

8. $\alpha_s$ and $\Lambda_{\text{QCD}}$

The first concrete predictions of QCD\[38-41\] concerned the deviations from an exact scaling behaviour. But the electron scattering and $e^+e^-$ annihilation data of the time\[18\] covered momentum transfers, $Q^2$, of not more than a few GeV$^2$. Nobody (yet) dared to analyze these data in the “asymptotic” spirit of QCD. And that is how some people not affected by dataphobia—a morbid condition of the brain (or brane?) that turns theoretical physicists into mathematicians—set out to exploit the only data then available at higher $Q^2$.

By the early ’70s, the proton’s elastic form factor had been measured\[42\] up to $Q^2 \sim 20$ GeV$^2$. To bridge the gap between the QCD predictions for deep inelastic scattering and the elastic form factor, two groups\[43,44\] used (or, with the benefit of hindsight, slightly abused) the then-mysterious “Bloom–Gilman duality”\[47,48\] relating the deep “scaling” data to the elastic and quasi-elastic peaks. I prefer the paper containing Fig. 3 and beginning: “Two virtues of asymptotically free gauge theories of the strong interactions are that they are not free-field theories and they make predictions that are not asymptotic\[c\] to conclude “The results agree with experiment but are not a conclusive test of asymptotic freedom.”

Not atypical of the Harvard/Princeton competition of those times, the papers I just quoted\[43,44\] were received by the publisher within a one-day interval (mine was the late one). These are the first two papers on QCD phenomenology. They were written a full year after the discovery of asymptotic freedom. It is quite an amazing coincidence that they were so well synchronized.

Being a bit more inclined to data analysis than my Princeton competitors—and wanting to be the first theoretical physicist \[ (\star) \] to extract from observations a fundamental constant of nature— I obtained a value and an error range for $\Lambda \equiv \Lambda_{\text{QCD}}$, while David Gross and Sam Treiman simply chose a reference value for this quantity (which they called $\mu$), perhaps because their results —based on an analysis slightly different from mine—neither fitted the data nor subtracted from their confidence in the theory\[44\].

In Fig. 4 I show recent and very sophisticated results\[45\] on QCD’s fine-structure

\[c\]The fact that one felt obliged to emphasize all this means that it was not at all obvious to the community at the time.
“constant”, \( \alpha_s(Q^2) \), as well as my original three-flavored leading-order result, \( \alpha_s = 12/[25 \pi \log(Q^2/\Lambda^2)] \), and its error range. The vertical green double arrow shows, that the central value of the original determination of \( \alpha_s \) must be reduced by \( \sim 33\% \) to get close to best current results. This level of discrepancy between leading-log QCD results and more sophisticated ones is quite typical.

One may also recall that dimensional transmutation\(^{46}\) was also very hard for many to accept. It is the ab-initio astonishing renormalization-group fact that, in an asymptotically free theory, a dimensionless coupling defined by its value at a given momentum scale (two parameters) is equivalent to a one-parameter expression at different momentum scales: \( \alpha_s(Q^2/\Lambda^2) \) as quoted above, in this leading-log case.

9. Bloom–Gilman duality (BGD)

Is Bloom–Gilman duality\(^{47,48}\) a prediction of QCD? In spite of recent efforts\(^{49}\) it is not (yet). That would require a complete understanding of bound-state production. But perturbative QCD explains BGD, in its QCD-improved realization.\(^{50}\) If you trust me, do not read this technical section, but for the rest of this paragraph. Two of the results of Ref. 50 are worth recalling. The first is that our analysis of deep inelastic electron scattering resulted in \( \Lambda = 500 \pm 200 \text{ MeV} \), in agreement with

\[ G_M(q^2) \leftrightarrow \nu W_2(\xi_p, q^2) \]

\[ \alpha_s \equiv \frac{g_s^2}{4 \pi} = \frac{12}{25 \pi \ln[q^2/\Lambda^2]} \]

Fig. 3. The proton form factor \( G_M(q^2) \), divided by the usual dipole parametrization \( D(q^2) \). Un-normalized results for \( G_M(q^2) \) would show the agreement between theory and data over a much more extended range on the vertical axis.
the results of Ref. 43 and Fig. 4. The second is that we also extracted results to Next-to-Leading-Twist (NLT). These are characterized by a mass $M_0$ that ought to be of $O(\Lambda)$. We obtained $M_0 = 375 \pm 25$ MeV. As data on other processes become more precise, a revival of NLT analyses may become mandatory.

BGD is the observation\textsuperscript{47,48} that at low $Q^2$ a structure function shows prominent nucleon resonances, which “average” to the “scaling” function measured at some higher $Q_0^2$, and snugly slide down its slope as $Q^2$ increases. As shown in Fig. 5 this happens if the chosen scaling variable in not Bjørken’s $x = Q^2/(2m_p\nu)$, but contains a “target mass correction”, $1/\omega' \equiv x' = Q^2/(2m_p\nu + m_p^2)$. All this was considered at the time, in Californian style, as mystifying as a myth.

In \textit{Demythification of Electroproduction Local Duality and Precocious Scaling}\textsuperscript{50} Howard Georgi, Politzer and I argued that BGD is a consequence of QCD, inevitable if scaling is “precocious”, as it must be for small $\Lambda$ (a fraction of a GeV).

The scaling variable to be used, as we insisted in Ref. (51), is not $x'$, but the one implied by a full use of QCD’s operator-product expansion, i.e. Nachtman’s variable\textsuperscript{52} $\xi = 2x/[1 + (1 + 4m_p^2x^2/Q^2)^{1/2}]$, which takes care of the target–mass “higher-twist” effects of order $m_p^2/Q^2$. In studying the $Q^2$-evolution of the $n$-th moment of a structure function, weights $\xi^n$ and not $x^n$ ought to be used. And the entire structure function is to be $\xi^n$-weighed, including the elastic contribution at $\xi = \xi_p \equiv 2/[1 + (1 + 4m_p^2/Q^2)^{1/2}]$. The QCD duality is shown in Fig. 6.
The crucial point is that the customary logarithmic QCD evolution of structure functions has higher-twist corrections. The next to leading-twist ones are of the form \((1 + n a_n \Lambda^2/Q^2)\), with \(|a_n| \simeq 1\), as the data allowed us to check in Ref. [50]. Consider taking the \(n\)-th \(\xi\)-moment of a structure function measured at a relatively large \(Q_0^2\), where the resonant peaks are barely observable. Next, evolve this moment perturbatively down to a lower \(Q^2 \ll Q_0^2\). Since the \(a_n\) are not perturbatively calculable, the perturbative prediction for the \(n\)th moment at the scale \(Q^2\) has a relative uncertainty of \(\mathcal{O}(n \Lambda^2/Q^2)\), where the factor \(n\) is crucial.

Our claim\([50]\) that Bloom-Gilman duality and precocious scaling were consequences of QCD must have looked too good to be true, for it met with an immediate barrage of theoretical papers attempting to prove us wrong\([53–55]\). In Ref. [56] we defused all this artillery and, more constructively, we used the parton-model language to interpret the field-theoretic operator-product expansion in successive twists while developing an intuitive physical interpretation of twist, illustrated in Fig. 7. We did also reanalyze the “paradoxes” in Ref. [55] in parton language and detailed how to deal with the production of hadrons containing quarks heavier than the proton. Finally, we resolved the question of nonperturbative effects in the analysis of elec-

Fig. 5. Bloom-Gilman duality\([47]\) at a fixed \(e\)-scattering angle and varying energies (or \(Q^2\) values).
To extract information on local duality from $n$ available moments, consider a polynomial $P_n(\xi) = \sum_{m=0}^{n} C_m \xi^m$. One can find, for a given $n$, the $C_m$'s corresponding to a best fit to a “window function” that can be used to test duality “locally”, i.e. in a chosen interval $\xi_1$ to $\xi_2$, see Fig. 8. Given a predicted set of moments with uncertainties of $\mathcal{O}(n \Lambda^2/Q^2)$ one expects a more local and precise QCD duality the smaller $n/Q^2$ is. That is precisely what is observed\textsuperscript{[50]} QED\textsuperscript{[40]}. The preceding detailed discussion justifies a posteriori the analysis of Ref. 43 based on BG local duality in an interval enclosing the elastic proton contribution $\propto G^2_M \delta(\xi - \xi_p)$. It also explains why these initial attempts at QCD phenomenology resulted in reasonable and consistent values of $\Lambda$.

\textsuperscript{d} The actual analysis of QCD duality is a bit more elaborate, since one expects slightly different precisions for window functions centered at different $\xi$'s.\textsuperscript{[50]}

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**Fig. 6.** Dashed line: the perturbatively evolved\textsuperscript{[50]} proton structure function $\nu W_2(\xi, Q^2)$. Continuous line: a fit to actual data. $\xi_p$ is the position of the elastic contribution $\propto G^2_M \delta(\xi - \xi_p)$. A few data points are also shown.
10. Progress on the determination of $\alpha_s$

In this section I rely heavily on the contribution by Bethke, Disertori and Salam in the Review of Particle Properties.\textsuperscript{57}

As a function of a scale $\mu_R$, the renormalization group improved perturbation expansion of $\alpha_s$ reads:

$$\mu_R^2 \frac{d\alpha_s}{d\mu_R} = \beta(\alpha_s) = -\left( b_0 \alpha_s^2 + b_1 \alpha_s^3 + b_2 \alpha_s^4 + b_3 \alpha_s^5 + \ldots \right),$$  \hspace{1cm} (1)

where all the quoted $b_i$ have been calculated and the minus sign is of asymptotically free fame ($b_0 > 0$ for fewer than 17 flavors).

![Diagram](image-url)

Fig. 7. Operators and parton-language diagrams at various twists. (a) shows typical twist-2 effects, corresponding to a parton-model picture with no communication between struck quarks and spectator quarks. (b) shows twist-4 effects. (c) shows an even higher-twist effect.
The quantity $\alpha_s(Q^2)$, measured at a specified momentum scale is measurable. In a limited sense that is, for the coefficients $b_i$, $i \geq 2$ in Eq. (1) are scheme-dependent, a first sign of discomfort. One of the most precise measurements of $\alpha_s(Q^2)$ relies on the illustrious ratio $R$ and the precise knowledge of $R_{EW}$, its value for a free-quark ansatz with $\alpha_s = 0$:

$$\frac{\sigma(e^+e^- \to \text{hadrons}, Q)}{\sigma(e^+e^- \to \mu^+\mu^-, Q)} \equiv R(Q) = R_{EW}(Q)[1 + \delta_{QCD}(Q)].$$  

(2)

The perturbative series for $\delta_{QCD}(Q)$ is:

$$\delta_{QCD}(Q) = \sum_{n=1}^{\infty} c_n \left[ \frac{\alpha_s(Q^2)}{\pi} \right]^n + O \left( \frac{\Lambda^4}{Q^4} \right)$$

(3)

The coefficients $c_1$ to $c_4$ have been calculated. The series converges slowly, perhaps due to “renormalon” divergences, related to non-perturbative contributions and behaving as $n!\alpha_s^n$, another current limitation.

The values of $\alpha_s$ at $Q^2 = M_Z^2$, extracted in various ways, are summarized in Fig. 9, the references cited therein can be found in Ref. 45. It is somewhat surprising and gratifying for theorists that the precision of the lattice determinations competes so favorably with that of the experimentally “assisted” ones.

11. Back to the past: the irony of scaling in neutrino scattering

The consensus that the observed scaling deviations smelled of QCD was not triggered by theorists, but by an analysis of neutrino data by Don Perkins et al. This test of QCD was anything but severe. One reason is the neglect of higher twists. Furthermore it is not possible, event by event, to measure the neutrino energy. Thus, in an unintended implementation of Bloom–Gilman duality, a measured structure function, $F_\nu(x,Q^2)$, is significantly blurred in $x$ and $Q^2$. This erases, at the low $Q^2$ of a good fraction of the data, the resonance bumps that must be there, as

![Fig. 8. Best fits to a window function from $\xi_1$ to $\xi_2$ with polynomials $P_n(\xi)$, for $n = 4$ and 7.](image-url)
in Fig. 6 the total $\nu$ cross section increased linearly with $E_\nu$ and was related to the naive, constituent-quark expectation from electroproduction by a famous $18/5$ factor, relating weak to electromagnetic quark charges. 

Had the energy resolution of neutrino experiments been as good as that of their electron-scattering counterparts, the cross section rise would not have been linear at all, the nucleon-resonance bumps in the structure functions would have been clearly visible, and the data analysis would have had to be quite different. With a pinch of poetic license one could assert that, early on, many concluded that QCD was quite precise, but only because the data were not.

12. The QCD evolution of structure functions

Elaborating on work by Giorgio Parisi, Georgi, Politzer and I explicitly worked out the $Q^2$ evolution of structure functions at fixed $x$, large $Q^2$ (for which $\xi \simeq x$). The simplest results are for $x F_3(x, Q^2)$ (the C-odd neutrino scattering structure function), shown in Fig. 10. A compilation of theory and HERA data for the electron scattering $F_2(x, Q^2)$ is shown in the same figure.

The results shown in the left panel of Fig. 10 were to become heavily used... and systematically referenced to authors of later papers. While yowling, I plead guilty to having learned much later that the simple underlying physics had been understood elsewhere: the renormalization-group picture of seeing partons within partons was drawn by Kogut and Susskind, the “physical gauge” diagrammatic image of a parton dissociating into others is due to Lev Lipatov and its vintage QED analogue is none else than the Weiszäcker–Williams equivalent-photon approximation.

\[
\alpha_s(Q^2 = M_Z^2)
\]

Fig. 9. Values of $\alpha_s(M_Z)$. 

\[S(Q^2 = M_Z^2)\]
As it is extremely well known the QCD evolution became a hit with the 1977 publication of *Asymptotic freedom in parton language* by Guido Altarelli and Giorgio Parisi (AP), which significantly simplified matters by avoiding the explicit use the Mellin transforms of structure-function moments and directly using, instead, quark and gluon parton density functions (PDFs). Discussing the relation of this paper with the ones in the previous paragraph, Giorgio taught me something very wise. To wit, one should compare Columbus to people having previously set foot in America, such as the Vikings, not to speak of the Amerindians... to conclude that the important thing concerning discoveries is not to be the first, but the last.

With time, “the West” discovered that papers somewhat similar to AP’s were published in the Soviet Union. One, also in 1977, by Dokshitzer and a much earlier one by Gribov and Lipatov. The QCD evolution of structure functions and parton PDFs thus became synonymous to DGLAP.

The article by Gribov and Lipatov predates the discovery of asymptotic freedom, does not deal with composite hadrons and is not a predecessor of AP in the same sense as the other articles I have cited. The article of Dokshitzer is a close contemporary of AP (they were received by the respective journals one week apart) but does not contain the simple AP equations.

![Fig. 10. Left: Evolution of a normalized $\nu$ structure function $F_3(x, s)/F_3(x, s_0)$ at fixed $x$. The trend has been corroborated in detail by a multitude of experiments (and theorists). Right: HERA electron-scattering data.](image-url)
13. Progress in deep inelastic scattering

Once again, in this section, I rely heavily on the contribution by Bethke, Disertori and Salam in the Review of Particle Properties. In the customary notation, the parton model statement of Bjorken scaling is:

\[ F_2(x, Q^2) = x \sum_q e^2_q f_q/p(x), \quad F_L(x, Q^2) = 0, \]  

where the longitudinal structure function vanishes for point-like spin 1/2 constituents such as quarks. The full QCD perturbative result is:

\[ \mu^2 F \frac{\partial f_{q/p}(x)}{\partial \mu^2} = \sum_j \frac{\alpha_s(\mu^2)}{2\pi} \int_x^1 \frac{dz}{z} P_{i\to j}(z) f_{q/p} \left( \frac{x}{z}, \mu^2 \right), \]  

where the unspecified term reflects higher-twist corrections. The coefficient functions \( C_{n,i}^{(n)} \) depend on two arbitrarily chosen scales, a renormalization scale and a “colinear” factorization scale: a boundary between parton emissions taken care of by the coefficient functions and those included in the splitting functions.

Since \( F_2(x, Q^2) \) in Eq. (6) is an observable, the dependence on \( \mu_R^2 \) and \( \mu_F^2 \) should in principle disappear (in the absence of renormalon contributions) from a completely summed series, or at least become weaker and weaker as more terms are added. Not currently close to this ideal situation, the practice consists in defining a “theoretical uncertainty” as the range of results in the brackets \( \mu_R^2/2 < \mu_F^2 < 2\mu_R^2; \mu_F^2/2 < \mu_R < 2\mu_F^2 \). It is not clear how to do any better.

An sketch of deep inelastic scattering (DIS) is shown in Fig. 11, which illus-

trates the concept of the factorization of the process into two steps. The first, involving the nucleon’s constituents, occurs at a “short distance” of \( O(1/\sqrt{-Q^2}) \). The second is the hadronization of the scattered and spectator partons, occurring at inter-parton distances of \( O(1/\Lambda) \). Factorization is the hypothesis that the short- and long-distance processes do not interfere. This is augmented by the unitarity argument that the semi-final-state partons have no choice but to end-up –with 100% probability– confined within the outgoing hadrons.

Collins, Soper & Sterman have analyzed in great detail the extent to which the previous paragraph’s arguments are defensible. They find them to be correct, even beyond leading twist, in \( (\Phi^3)_6 \), the 6D renormalizable asymptotically free theory of scalars with a cubic self-interaction. But for realistic processes in the Standard Model this is not the case. Not surprisingly, the best understood process is DIS, even for higher-twist contributions from multiparton processes.

Another item of interest concerns photons as partons. Progress has been made from an early The photon constituency of protons to a decisive How bright is the proton.
14. Drell-Yan-like processes

The production in $pp$ collisions of a single intermediate vector boson, or a couple of them, are amongst the processes akin to the original “Drell-Yan” process $pp \rightarrow e^+e^- + X$. In analogy with Eq. (6), the cross section for single-$W$ production, for instance, is written as:

$$\sigma(h_1 h_2 \rightarrow W + X) = \sum_{n=0}^{\infty} \alpha_s^2(\mu_F^2) \sum_{i,j} \int d x_1 d x_2 f_{i/h_1}(x_1, \mu_F^2) f_{j/h_2}(x_2, \mu_F^2) \times \sigma^{(n)}_{i,j \rightarrow W + X}(x_1 x_2 s, \mu_F^2) + O\left(\frac{\Lambda^2}{M_W^4}\right),$$

(7)

Here, the caveats concerning the use of the two scales $\mu_F$ and $\mu_R$ are the same as the ones we have already commented upon. What is worse, for these processes the infrared divergencies in higher-twist contributions do not cancel beyond the one-loop level.\[68,70,80–82\]

Quite welcome –but somewhat surprising– is the fact that, in spite of the above admonitions, theory and data agree to their current level of precision. This is shown for CMS data in Fig. 12 and for Atlas data in Fig. 13. The perturbative NNLO calculations, as well as the data are reaching a precision at which it will no longer be safe to ignore Next to Leading Twist effects.

15. The November Revolution

In the 1970’s the pace of discovery was so fast that (lazy) journalists decided to prepare a “matrix article”, wherein only the details of each specific discovery had

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Fig. 11. Factorization and hadronization in DIS.
to be filled in at the last minute. The NYT matrix article and its filling in November 1974 are shown in Fig. 14.

Ten days of November 1974 shook the world of physics. Something wonderful and almost unexpected was to see the light of day: a very discreetly charmed particle, a hadron so novel that it hardly looked like one. Two score and five years later it is not easy to recall the collective “high” in which this discovery and others to be made in the two consecutive years, submerged the particle-physics community. In my opinion, a detailed account that reflects well the mood of the period is that by Riordan. In a nutshell, the standard model arose from the ashes of the November Revolution, while its competitors died honorably on the battleground. A couple of survivors and many of the casualties are shown in Fig. 15. All of them but the last two were published, unrefereed, in the same issue of PRL.

Burt Richter was also caught in the November avalanche. On a short visit to Harvard, and with a healthy disrespect for theory, Burt told us that the electron spent some of its time as a hadron. In answer to a question by Applequist, he explained that sufficiently narrow resonances would escape detection in $e^+e^-$ colliders. Nobody around was aware of the possibility of catching the devil by its radiative tail

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\footnote{The “usual Russian suspects”, so often ahead of Westerners, did not on this occasion have the time to contribute. This is in spite of the fact that one of them obtained permission from the Soviet authorities to give me a phone call to catch up on the news.}
(the emission of photons by the colliding particles widens the observed resonance on its $\sqrt{s} > M$ side). Our vain discussions came to an abrupt end; a rather urgent

Fig. 13. Comparison of Atlas data with theory. Notice the results for single and even double gauge boson production.

**SCIENTIFIC DISCOVERY MATRIX ARTICLE**

Recently, it was announced at press conferences that Prof. Ting and Prof. Richter and collaborators have established the existence of a new Particle.

This is an important discovery corroborating the theory of Prof. Glashow and collaborators, who had predicted the existence of such a Particle as a consequence of the existence of Charmed Quarks.

Other schools of thought interpret the new Particle as a manifestation of the long sought Whatchamacallit.

Yellow entries filled by newspapers on November 6th and 7th, 1974.

Fig. 14. The Scientific Discovery Matrix Article. The November 74 Revolution; fully filled version.
call summoning Burt back to SLAC delivered us from his scorn for theorists.

The experimental papers of the Brookhaven group led by Samuel Ting and the SLAC one led by Richter were sent for publication within an interval the reader should by now be familiar with: one day. Their statistical evidence for a discovery was more than sufficient to remind one of a contemporary dictum by Val Telegdi: *If you need statistics to prove your point, you may not have one.*

In the early Fall of ’74, Tom Appelquist and David Politzer had been looking leisurely at how asymptotic freedom could imply a positronium-like structure for the $c\bar{c}$ bound states of a charmed quark and its anti-particle. In those days, both QCD and charm were already “established” at Harvard. Since Americans are often short of vocabulary, my first contribution to the subject was to baptize their toy charmonium. David and Tom’s first charmonium spectrum was so full of Coulomb-like peaks that they could not believe it themselves. They debated the problem long enough for the experimental avalanche to catch up with them. It was a heavy price to pay for probity.

For an object of its mass, the $J/\psi$ is four orders of magnitude narrower than a conventional hadron resonance, and one order of magnitude wider than a then hypothetical weak intermediary. It could not be either. Of the multitude of theoretical papers of Fig. 15, only two attributed the narrow width to asymptotic freedom, one by Tom and David, who had intuited the whole thing before, the other one by Sheldon Glashow and me. I recall Shelly storming the Lyman/Jefferson lab

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**Fig. 15.** Immediate interpretations of the $J/\psi$, with their titles. PRL is Phys. Rev. Lett. 34, Jan. 6th, 1975. The last two papers are in Lett. Nuovo Cim.
corridors with the notion of the feeble three-gluon hadronic decay of the $J^P = 1^-$ \textit{orthocharmonium} state, and I remember Tom and David muttering: “Yeah”. Our paper still made it to the publishers in the auspicious November, but only on the 27th, a whole week after the article of our Harvard friends. The paper by Cesareo Dominguez and Mario Greco\cite{89} also singled out charmonium as the interpretation.

Glashow and I did a lot in our extra week.\cite{87} \textit{Abusus non tollit usum} (of asymptotic freedom) we related the hadronic width of the $J/\psi$ to that of $\varphi \to 3\pi$, to explain why this hadronic resonance \textbf{had to be} so narrow. For its dominant decay into hadrons, we estimated a width:

$$\Gamma = \frac{3}{2} \frac{M_{J/\psi}}{M_{\varphi}} \left[ \frac{\alpha_s(M_{J/\psi})}{\alpha_s(M_{\varphi})} \right]^6 \Gamma(\varphi \to 3\pi) = 42 \text{ keV},$$

with $\alpha_s(M_{J/\psi})$ QCD evolved from the three-gluon estimate:

$$\alpha_s(M_{\varphi}) = \frac{3}{2} \left[ \frac{9 \pi \Gamma(\varphi \to 3\pi)}{10 (\pi^2 - 9)M(\varphi)} \right]^{1/6}.$$  \hspace{4cm} (9)

It is fun to quote QCD results from the times they were so simple.

The result of Eq. (8) is to be compared with the currently measured hadronic width, $\sim 59$ keV. A rather good prediction, particularly considering that it depends on $\alpha_s(M_{J/\psi})/\alpha_s(M_{\varphi})$ to an error-enhancing sixth power. Its well known basic input is shown in Fig. 16, involving the same considerations as the ones about Fig. 11.

In Ref.\cite{87} we did also correctly estimate the yields of production of truly charmed particles in $e^+e^-$ annihilation, $\nu$-induced reactions, hadron collisions and photoproduction. Our mass for the $D^*$ turned out to be 2.5% off, sorry about that. We even

$$J/\Psi \text{ as CHARMONIUM}$$

$$d \sim \frac{1}{m_C}$$

$$d \sim \frac{1}{\Lambda_{QCD}}$$

Fig. 16. Annihilation of a $c\bar{c}$ pair and subsequent hadronization.
discussed mass splittings within multiplets of the same quark constituency as hyperfine, a fertile notion. In discussing paracharmonium \((J^P = 0^-)\) we asserted that “the search for monochromatic \(\gamma\)’s should prove rewarding”. Finally, we predicted the existence of \(\psi'\), but this time it was our turn to be overtaken by the pace of discovery.

16. Charmonium spectroscopy

I have a few vivid printable recollections of the times I am discussing. One concerns the late night in which the existence of \(P\)-wave charmonia hit my head: we had been talking about \(L = 0\) states without realizing (we idiots!) that a bunch of \(L = 1\) charmonia should lie between \(J\) and \(\psi'\) in mass. Too late to call Shelly, I spent hours guessing masses and estimating the obviously all-important \(\gamma\)-ray transition rates. At a gentlemanly morning hour I rushed on my bicycle to Shelly’s office, literally all the way in, and attempted to snow him with my findings. I was speechless, out of breath and wits. Shelly profited to say: “I know exactly what you are trying to tell me, there are all these \(P\)-wave states etc., etc.” He had figured it all out at breakfast. I hated the guy’s guts.

In no time, David and Tom gathered forces with Shelly and me to produce an article\(^{90}\) on Charmonium spectroscopy. Physical Review Letters was fighting its usual losing battle against progress (in nomenclature, \(\rightarrow\)) and did not accept the title. Neither did PRL accept a similar title by our friendly Cornell competitors.\(^{91}\) The predicted spectra and the current experimental situation are shown in Fig. 17.

We, the Crimson, estimated the energy levels as “half-way” between those of a Coulomb and a harmonic oscillator potential. Indeed, a linear potential –adequate for confined \(c\bar{c}\) states– is in its dependence on distance somewhat half-way. Our Cornell friends, the Carnelian, borrowed a linear-potential program from Ken Wilson and got similar results. Except for the all-important \(\gamma\)-ray transition rates, for which the Carnelian predictions were much better than ours.

17. Hadron masses in a gauge theory

Early in 1975, Georgi, Glashow, and I wrote a paper\(^{92}\) whose style reflects how high we rode, as well as how unorthodox QCD still was. But for the added parentheses, here is how it began:

Once upon a time, there was a controversy in particle physics. There were some physicists who denied the existence of structures more elementary than hadrons, and searched for a self-consistent interpretation wherein all hadron states, stable or resonant, were equally elementary (the bootstrap). Others, appalled by the teeming democracy of hadrons, insisted on the existence of a small number of fundamental constituents (quarks) and a simple underlying force law (QCD). In terms of these more fundamental things, hadron spectroscopy should be qualitatively described and essentially understood just as are atomic and nuclear physics.
To the non-relativistic quark model, we added chromodynamic interactions entirely analogous to their electrodynamic counterparts. We shall see that to this day it is not totally clear why the ensuing predictions were so good. Our paradigmatic result was the explanation of the origin and magnitude of the $\Sigma^0-\Lambda$ mass difference. The two particles have the same spin and quark constituency, their mass difference is a hyperfine splitting induced by spin–spin interactions between the constituent quarks. A little later, the “MIT bag” community published their relativistic version of the same work.

In Ref. 92 we also predicted the masses of all ground-state charmed mesons and baryons and (me too, I’m getting bored with this) we got them right on the mark. Predictions based on an incredible SU(4) version of the Gell-Mann–Okubo SU(3) mass formula, and also the more sensible bag results, turned out to be wrong. Only one person –indeed, again a Russian– trusted a “QCD-improved” constituent quark model early enough to make predictions somewhat akin to ours: none less than Andrei Sakharov.

17.1. Good News at Last

While theorists faithfully ground out the phenomenology of QCD, experimentalists persistently failed to find decisive signatures of our Trojan horse: the charmed quark.

Fig. 17. The spectra of charmonia. From left to right: Cornell’s (squeezed), Harvard’s (mirror reflected) and observed (with the inclusion of some non-radiative decays).
At one point, the upper limits on the $\gamma$-ray transitions of charmonia were well below the theoretical expectations. Half of the $e^+e^-$ cross-section above $\sqrt{s} = 4$ GeV was due to charm production, said we. Who would believe that experimentalists couldn’t tell?

In the winter of ’75 we saw a lone ray of light. As Nick Samios recalls in detail in Ref. [95], a Brookhaven bubble-chamber group pictured a $\Delta S = -\Delta Q$ event, forbidden in a charmless world, and compatible with the chain:

$$\nu_\mu \ p \rightarrow \Sigma_c^{++} \mu^-; \ \Sigma_c^{++} \rightarrow \Lambda_c^+ \pi^+; \ \Lambda_c^+ \rightarrow \Lambda \pi^+ \pi^+ \pi^-.$$ 

Two charmed baryons discovered in one shot! This was a source of delight not only for us, but also for the experimentalists involved. They deserve a reproduction of their event: Fig. 18, and a quotation:

*The total recoiling hadron mass ($\Lambda \pi^+ \pi^+ \pi^-$) [is] 2426 ± 12 MeV.*

This mass is in reasonable agreement with the value predicted by De Rújula, Georgi and Glashow for the lowest-lying charmed-baryon states of charge +2, 2420 MeV ($J^P = \frac{3}{2}^+$, $I = 1$, $\Sigma_c^*$) ... There are three $\pi^+$'s and thus three mass differences derivable form this event; these are observed to be 166 ± 15 MeV, 338 ± 12 MeV, and 327 ± 12 MeV. The first of these differences is in remarkable agreement with the 160 MeV predicted for the decay of a spin-$\frac{1}{2}$ charmed baryon $\Sigma_c$ decaying into a charmed $\Lambda_c$.

This is almost precisely the way I feel experimentalists should write papers. Only “almost” because the agreement between 2426 ± 12 and 2420 MeV seems to me to
be a bit better than “reasonable”.

What made QCD part of the now generally accepted Standard Model? Even the most formal theorist or the most cable-connecting experimentalist understands positronium and hydrogen. These objects are not so very different from their QCD analogs, charmonium and charmed particles. This may be why it took asymptotic freedom and a fourth quark to have the Standard Model become the standard lore.

18. Back to the present: charmed baryon masses

By now the masses of many mesons and baryons have been precisely post-dicted in lattice QCD. It is perhaps instructive to look at an instance: charmed baryons. This is done in Fig. [19] where a collection of lattice results [97] is shown, along with the observed values (blue lines) for the positive parity baryons we shall discuss. Also shown in the figure are the predictions of Ref. [92] made after the discovery of the $J/\psi$, but before that of open charm. This limited information made us (over)estimate a common uncertainty of $\pm 50$ MeV – reflected as the red ellipses – around the central values of the masses.

The QCD-improved naive quark model predictions [92] still compete with the lat-
tice results. The moral is that there is an element of “truth” in the naive model: unresolved by a short-distance probe, a baryon consists of three quarks (and some glue). This is strongly corroborated by the quenched lattice results\footnote{This happens in attempts to describe confinement by methods based on Bethe-Salpeter or Schwinger-Dyson equations. I am indebted to Pilar Hernandez and Carlos Pena for discussions on this topic.} shown as unfilled ellipses in Fig. 19: adding $q\bar{q}$ does not significantly modify the lattice predictions.

Presumably, if latticists could device a gauge-invariant way to characterize a confined constituent-quark propagator, it would be found to peak at a “constituent mass” some 300 MeV larger than a “Lagrangian” or chiral-model mass\footnote{This happens in attempts to describe confinement by methods based on Bethe-Salpeter or Schwinger-Dyson equations. I am indebted to Pilar Hernandez and Carlos Pena for discussions on this topic.} Similarly, they might discover one gluon exchange dominance for the mass differences between baryons of the same quark constituency but different spin or quark-spin alignments.

19. Lattice predictions

Two areas of QCD have witnessed an enormous progress over the years, as summarized, for instance, in Ref. \cite{101}. One of them is the non-perturbative first-principle lattice calculation of many relevant observables. These include exotic hadron masses and decays, glueballs, form factors in $K$, $D$, $B$, $\Lambda_b$ and $\tau$ decays, moments of structure functions, $K \to \pi\pi$ amplitudes, the hadronic vacuum polarization contribution to $g_\mu - 2$, long-distance contributions to $K \leftrightarrow \bar{K}$ mixing and rare $K$ decays.

Not having ever been active on the subject, I shall give only one recent example of a lattice prediction that turned out to be very relevant. That is a result for $|V_{ub}/V_{cb}|$, the ratio of two entries in the CKM matrix and the corresponding unitarity triangle. To understand the data on the ratio of the decay rates $\Lambda_b \to p \mu^- \bar{\nu}_\mu$ and $\Lambda_b \to \Lambda_c \mu^- \bar{\nu}_\mu$, integrated over given ranges in $q^2$ (with $q$ the momentum transfer between the baryons) it appeared to be necessary to invoke right-handed weak currents\footnote{This happens in attempts to describe confinement by methods based on Bethe-Salpeter or Schwinger-Dyson equations. I am indebted to Pilar Hernandez and Carlos Pena for discussions on this topic.}, absent in the Standard Model. When the matrix elements $<p|j^\mu|\Lambda_b>$ and $<\Lambda_c|j^\mu|\Lambda_b>$ for vector and axial currents, $j^\mu$ – required to extract $|V_{ub}/V_{cb}|$ – were calculated in the lattice\footnote{This happens in attempts to describe confinement by methods based on Bethe-Salpeter or Schwinger-Dyson equations. I am indebted to Pilar Hernandez and Carlos Pena for discussions on this topic.} the problem disappeared. Yet another Beyond the Standard Model (BSM) result that goes down the drain.

19.1. The $\Delta I = 1/2$ rule, quite briefly

The fly in the lattice ointment is the $\Delta I = 1/2$ rule, an issue that lattice calculations have not solved. But this is a different type of BSM result: it is a Before the Standard Model problem.

20. Beyond $q\bar{q}$ and $qqq$

A currently very active endeavor is the analysis of hadrons with a larger quark constituency than the consuetudinary old one. The theoretical prehistory of this subject
QCD, from its inception to its stubbornly unsolved problems

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dates back to the mid 70’s. Perhaps the first data analyses with Molecular Charmonium in mind were those of Refs.\cite{107, 108}. In the first of these papers we concluded: *It seems very likely to us that four-quark molecules involving a $c\bar{c}$ pair do exist, and have a rich spectroscopy. Our conjecture that the 4.028 GeV and perhaps the 4.4 GeV peaks in $e^+e^-$ annihilation are indeed due to the production of these molecules is more speculative. If it is true, then nature has provided us with a spigot to a fascinating and otherwise almost inaccessible new “molecular” spectroscopy full of experimental and theoretical challenges. This conclusion is still unaltered, but for our lack of prescience in the almost inaccessible stipulation. That the situation would be very messy, even in a narrow energy domain in $e^+e^-$ annihilation, could already be concluded from Fig. 20.*

There have been vivid discussions on whether the observed tetra- and penta-quark resonances are atom-like—that is, “bags” containing all the quarks—or molecule-like—that is, bound states of two “sub-bags”. Studying the mesons $X$ and $Z$ containing two heavy quarks ($Q$) and ordinary $QQq$ baryons, Maiani, Polosa and Riquer\cite{109} have recently proposed that these objects are colored molecules, respectively analogous to the $H_2$ molecule and the $H^+_2$ ion. This is an interesting twist in the atom vs molecule debate: a third possibility.

Amongst the scores of theoretical articles on tetra- and penta-quarks there is another one deserving in my opinion an explicit mention. Marek Karliner and Jonathan

![Fig. 20. The spectrum of molecular charmonium in $e^+e^-$ annihilation above 3.6 GeV.](image-url)
Rosner predict that a doubly-bottom tetraquark, $T^{(bb\bar{u}\bar{d})}$, with $J^P = 1^+$ and $M = 10389 \pm 12$ MeV would be stable under strong and electromagnetic interactions and can only decay weakly. (It would be) the first exotic hadron with such a property.

21. Back to the past again

In the summer of '75 –after a year of upper limits incompatible with the theoretical expectations– evidence finally arose for the $P$-wave charmonia. The DESY experimentalists did not refer to the theorists who suggested their search; they are hereby punished: they do not get a reference, and they will remain eternally ignorant of my juicy version of the story of their competition with SLAC.

The discovery of the positronium-like $c\bar{c}$ spectrum of Fig. 17 started to convert many infidels to the quarker faith. And the charmed quark, not yet found unaccompanied by its antiparticle, was to continue playing a crucial role in the development and general acceptance of the standard lore.

21.1. R and yet another year of lank cows

The quantity $R \equiv \sigma(e^+e^- \to \text{hadrons})/\sigma(e^+e^- \to \mu^+\mu^-)$ used to be so familiar to physicists that it was unnecessary to tell the younger ones that $R$ is not only Ricci's scalar curvature. The current observational situation for $R$ is shown in Fig. 21, a beautiful summary of a lot of particle physics’ history. One example: between $\sqrt{s} = 1$ and 3 GeV the green dotted line is the naive quark model prediction for $R$, with three quark flavors and the usual fractional charges. The agreement with observation was a reason for equally naive theorists to believe in quarks.

In Fig. 22 we see an artist’s rendition of the results, in 1976, of measurements at SLAC of $R$. They showed a doubling of the yield and structure aplenty as the $\sqrt{s} \sim 4$ GeV region is crossed. Much of the jump had to be due to the production of charmed pairs, which were obstinately not found. Howard Georgi and I innocently believed that a serious sharpening of the arguments would help.

In the space-like domain, $s < 0$, QCD predictions for $e^+e^-$ annihilation are insensitive to thresholds, bound-state singularities and hadronization caveats. For years, theorists had been unjustifiably applying the predictions to the time-like domain wherein experimentalists insist on taking $e^+e^-$ data. In a paper whose rhythmic title Finding Fancy Flavours Counting Colored Quarks was duly censored, we transferred the $e^+e^-$ data, via a dispersion relation, to a theoretically safer space-like haven. This somersault allowed us to conclude that the old theory with no charm is excluded, the standard model with charm is acceptable if heavy leptons are produced, and six quark models are viable if no heavy leptons are produced. Thus, anybody listening to the other voice in the desert (that of Martin Perl, who was busy demonstrating that he had discovered the $\tau$) had no choice but charm.

Our work was improved by Enrico Poggio, Helen Quinn and Steven Weinberg, who realized that one could, in the complex $s$-plane, work in a contour around the real axis where perturbative QCD can still be trusted, whilst the distance from the...
dirty details of real life is judged safe. The work of Enrico, Helen and Steve further strengthened our conclusion: the measured total cross section, analyzed on firm theoretical grounds, implied the existence of charm and of a new heavy lepton.

\[ \text{Or perhaps not, Helen recently told me that their paper was not right.} \]
Imagine that some theorists, analyzing LHC data with the current Standard Model—with its six quarks and three charged leptons—and on the basis of a shipshape analysis with a statistical evidence so strong that there was no need to count $\sigma$’s, proved that an extra quark and an extra charged lepton were being produced. There is no doubt that the community would conclude that the cited theorists had discovered these particles. But the social power of preconceptions cannot be overestimated. Prior to 1976, the Standard Model—then having three established quarks and two observed charged leptons—was not yet accepted as “part of the truth”. Thus, to be considered believable, the analysis in Ref. [115] had to wait for the explicit discovery of open charm and the $\tau$ lepton.

21.2. Charm is found

No amount of published information can compete with a few minutes of conversation. The story, whose moral that was, is well known. For the record, I should tell it once again. Shelly Glashow happened to chat with Gerson Goldhaber in an airplane. Surprisingly, the East Coast theorist managed to convince the West Coast experimentalist of something. There was no way to understand the data unless charmed particles were being copiously produced above $\sqrt{s} = 3.7$ GeV. The experimentalists devised an improved (probabilistic) way to tell kaons from pions. In a record 18 days two complementary SLAC/LBL subgroups found convincing evidence for a new particle with all the earmarks of charm. The charmonium advocates at Cornell had been trying for a long time to convince the experimentalists to attempt to discover charm by sitting on the $\psi(3440)$ resonance, or on what would become a “charm factory”: $\psi(3770)$. Alas, they initially failed.

The observation of charmed mesons ought to have been the immediate happy ending, but there was a last-minute delay. The invariant-mass spectrum of recoiling stuff in $e^+e^- \rightarrow D^0(D^\pm) + \ldots$ had a lot of intriguing structure, but no clear peak corresponding to $D^0\bar{D}^0$ or $D^+D^-$ associated production. Enemies of the people rushed to the conclusion that what was being found was an awful mess, and not something as simple as charm, as in Fig. 23.

But we had one last unspent cartridge. We expected $D\bar{D}$, $D\bar{D}^* + \bar{D}D^*$, and $D^*\bar{D}^*$ production to occur in the “spin” ratio 1:4:7 (thus the $D\bar{D}$ suppression). We trusted our prediction $m(D^*) - m(D) \simeq m(\pi)$, which implies that for charm production close to threshold, the decay pions are slow and may be associated with the “wrong” $D$ or $D^*$ to produce fake peaks in recoiling mass. Finally, we knew that the charged $D$’s and $D^*$’s ought to be a little heavier than their neutral sisters. Consequently, the $D^*$ decays had to be very peculiar: $D^{*0} \rightarrow D^+\pi^-$ is forbidden, $D^{*0} \rightarrow D^0\gamma$ competes with $D^{*0} \rightarrow D^0\pi^0$, etc. On the basis of these considerations (and with only one fit parameter) we constructed the recoil spectra shown in Fig. 24. Case closed!
Fig. 23. $\bar{D}D^*$ pair-production. Invariant masses (IM) are measured as recoiling from $K\pi$ (as in the figure), or $K\pi\pi$ ensembles. The decay $D^* \rightarrow D\pi$ is allowed, forbidden or suppressed, depending on the particle's charges. Close to the open-charm threshold all observed hadrons are “slow” and fake invariant mass peaks consequently occur.

Fig. 24. Predicted and observed invariant-mass spectra, recoiling against neutral and charged $D$’s. The theoretical curves are a one-parameter description.
21.3. Seeing (gluons) is believing

Quarks have not been seen and may never be. But their manifestation as quark jets was apparent since 1976. Kogut and Susskind argued that the gluon could show up in the same way: the elementary process $e^+e^- \rightarrow \bar{q} q g$ may result in three-jet final states.

Further work on QCD jets was often based on “intuitive perturbation theory”, an appellation perhaps meant to admit a fundamental lack of understanding. Decorum was regained by the work of Sterman and Weinberg, Georgi and Machacek, and Farhi, who exploited the fact that in QCD, as in QED, there are “infrared-safe” predictions, not sensitive to the long-distance dynamics that, in QCD, are intractable in perturbation theory.

One infrared-safe observable is the “antenna” pattern of energy flow in an ensemble of hadronic final states in $e^+e^-$ annihilation, properly reoriented event by event to compensate for the vicious quantum mechanical penchant for uncertainty. We foretold the pattern, binned in “thrust” to be that of Fig. 25. This three-jet structure and the QCD-predicted details of the angular or energy distributions played an important role in the “discovery of the gluon”, a subject whose denouement (that gluons are for real) has been described in detail by James Branson in Ref. 132.

21.4. The triple gluon coupling

Once upon a time, a measurement of the triple gluon coupling was thought to be nearly impossible. But that was before the advent of LEP. Recall that $C_A = N_c = 3$ is the triple-gluon “Casimir” color factor associated with gluon emission by a gluon, $C_F = (N_c^2 - 1) = 4/3$ is the one for gluon emission by a quark and $T_R = 1/2$ is for a gluon to split into $q\bar{q}$. Fixing $T_R$, $\alpha_s$ and the number of flavors, it is possible to extract $C_A$ and $C_F$ from the analysis of event shapes and other observables at LEP. The result of combining all experiments as in Fig. 26 is $C_A = 2.89 \pm 0.03$ (stat) $\pm 0.21$ (syst); $C_F = 1.30 \pm 0.01$ (stat) $\pm 0.09$ (syst).

The above results, impressive as they are, do not compete in precision and cleanliness with the corresponding measurement in General Relativity, discussed in Section 3.

22. Confronting reality in full detail

I have not been able to find the originator of the intimidating drawing in Fig. 27. Its green parts (hadronization and the “underlying” event) are not understood at the quite satisfactory level of its hard scattering and QCD shower parts. They are treated by “phenomenological” methods requiring a back-and-forth zitterbewegung between predictions and observations, to tune parameters and/or coach a machine learning program. The hadronization issue is tackled by non-quantum-mechanical methods, such as stretching frangible strings or gathering colorless clusters. For a
Fig. 25. Examples of predictions for three-jet distributions at different fixed thrusts, $T$. The left column contains the leading perturbative QCD predictions; in the middle one the results are smoothed for “hadronization” effects. More often than not, the small jet is produced by a gluon. The right panel contains results from the Mark-J Collaboration.

23. Themes left uncovered

Amongst others: the QCD phase diagram, chiral dynamics and symmetry breaking, heavy-quark methods and the so-called “Early Universe Plasma”, with the quotes referring to the fact that the QCD plasma studied in $pp$ collisions has, locally, boundaries that the Universe did not have (which is a subject of concern and discussion) and does not have a thermally equilibrated bath of neutrinos (which is quite irrelevant).

The only remaining particle predicted by the Standard Model and not yet found is the axion. And this is definitely not because of a lack of attempts to produce and detect it in the lab, or to observe it as a plausible constituent of dark matter.

The most challenging QCD problem –confinement– is not yet solved, in spite of a one M$ prize awaiting whoever solves it. Almost all we have concerning confinement is the good old “stringy” lattice result for the potential $V(r)$ between two static quarks, $V(r) \to \sigma r + \text{const}/r$, as $r \to \infty$, with $\sigma$ the string’s tension.
24. One moral of the story

In the olden days experimentalists, particularly the ones working in the West Coast of the USA, were strongly motivated to disprove all theories and to mistrust almost all theorists; they were perhaps permeated by some arcane Californian faith that nature is intrinsically unfathomable. They definitely did not have in their data-analysis programs the current instruction stating: `iff result = standard; look “elsewhere”`. This made life most enjoyable and the case for the once-upon-a-time generally ignored Standard Model extremely strong. To gauge whether or not things have turned around, consider supersymmetry.

At various points in this personalized rendering of the history of QCD I have grumbled about experimentalists not citing the “phenomenological” papers that made their searches and even their findings possible. This continues to be very much the case. One example: for years on end, “QCD phenomenologists” have made an enormous collective effort to provide predictions beyond the tree level ones for a host of processes. More often than not, their work is referred to by experimentalists as the “Standard Model (SM) prediction”. But the SM does not make predictions. Specific people use the SM to make predictions. A hypothetical reader who has reached this far is presumably a QCD phenomenologist. I have no doubt that (s)he would agree with me on all this.
A recent example of disregard of crucial phenomenological inputs by experimentalists has to do with the discovery of the Higgs boson. A question to ask is: why was it announced as a candidate for such a particle? The first and foremost answer is that its production cross section agreed with the expected one. But the dominant production mechanism is a highly non-trivial gluon fusion via a heavy “triangular” quantum loop [138] Georgi, Glashow, Maria Machacek and Dimitri Nanopoulos [139] who first calculated the $pp \rightarrow H + ...$ cross section via gluon fusion, were not cited in either of the Higgs discovery talks.

A second point is that one of the two discovery channels was $H \rightarrow \gamma\gamma$. It is also a non-trivial one-loop quantum effect. The corresponding decay width was first computed by John Ellis, Mary K. Gaillard and Nanopolous [139]. Although in this particular case the experimentalists may have had an excuse not to quote these authors, the fact is that they did not.

Finally, there is a novel recently discovered way of not quoting the authors of a pivotal contribution. Close to the arbitrary $5\sigma$ “discovery level” several things obviously play a role: the detectors’ characteristics, pure statistical luck or lack of luck, and the analysis methods. So?

The second Higgs-discovery channel was $H \rightarrow \ell^+ \ell^- \ell'^+ \ell'^-$, with $\ell = e$ or $\mu$. There are seven relevant independent observables in this process, beautifully entangled in their 7D space, but differently so for the signal and the background. Accumulated

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\[1\text{The closing sentence in Ref. (139) is:} \ldots \text{we do not want to encourage big experimental searches for the Higgs boson, but we do feel that people performing experiments vulnerable to the Higgs boson should know how it may turn up.}\]
event by event, the likelihood ratio of signal to background is the optimal tool (now called “the matrix element method”) to enhance the significance of a result. For a Higgs boson (or other objects with various $J^{PC}$ values) lighter than two “on-shell” $Z$s the methodology and relevant results were procured by the authors of Ref. [41]. I am not quoting their names in text because that is what Joe Incandela did in his CMS Higgs discovery talk. But he showed the Physical Review reference without names, as if authorship went without saying, like in the case of Hamlet or La Gioconda. In his next transparency Joe showed that the significance contributed by the $H \to 4 \ell$ channel was crucial in their having (barely) reached $5\sigma$.

It is difficult not to feel that particle physics phenomenology is generally less appreciated than it should, particularly in comparison with theoretical work “beyond” this or that.

25. Windup

There has been an enormous progress in perturbative and non-perturbative QCD since quarks were invented, in late 1963. This progress often required phenomenally difficult theoretical or phenomenological developments, and the later played a key role in the understanding of experimental results, most recently at the LHC.

Theories with gauge degrees of freedom have played the central role in our understanding of nature for a very long time. Interestingly, almost all of the most challenging currently identified problems pertain to these theories: confinement, the “naturalness” of the Higgs boson mass and the question of renormalizability, in QCD, the Electroweak Theory and General Relativity.

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