Fifty years of Yang–Mills Theories: a phenomenological point of view

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ABSTRACT
On the occasion of the celebration of the first half-century of Yang–Mills theories, I am contributing a personal recollection of how the subject, in its early times, confronted physical reality, that is, its “phenomenology”. There is nothing original in this work, except, perhaps, my own points of view. But I hope that the older practitioners of the field will find here grounds for nostalgia, or good reasons to disagree with me. Younger addicts may learn that history does not resemble at all what is reflected in current textbooks: it was orders of magnitude more fascinating.

This account is dedicated to the memory of my thesis advisor: Angel Morales.

1. Introduction

In the book of which this note is a chapter we are celebrating Fifty Years of Yang–Mills Theories. Yang–Mills (YM) theories are non-Abelian field theories with an “isotopic gauge invariance” [1]. By now, even some schoolchildren are told that in a non-Abelian gauge theory the gauge quanta are “charged” sources, e.g. the gluons of quantum chromodynamics (QCD), which carry a “colour” charge [2]. In this sense, Einstein’s general relativity is the first non-Abelian gauge theory [3], since gravitons gravitate. We might as well be celebrating close to 90 years of Einstein’s theory of gravity. But there is at least one reason to concentrate on YM theories: they are “true theories”, in that they are understood at the quantum level, while gravity is not.

Gerard ’t Hooft, who is organizing and editing the commemoratory book on YM theories, has asked me to cover my views of their phenomenological aspects. You can see that I have accepted. Why?... that is unclear. To judge from the list of contributors, it is quite possible that I shall be the only one showing any real data. The responsibility to deal single-handedly with the nitty-gritty details of reality is considerable. I shall make it easier for myself by covering only a fraction of the subject: the early developments, which I witnessed from a very short distance.
The most noteworthy theoretical developments after the work of Chen Ning Yang and Robert Mills will be covered by others, and some of them have already obtained the proverbial first class tickets to the freezing winter of Stockholm. I am referring to the work of Sheldon Glashow, Steven Weinberg and Abdus Salam on the “standard” electroweak YM model [456], and to the work of Gerard ’t Hooft and Martinus Veltman on why YM theories—even with massive gauge quanta—are true theories [78]. My involvement with these early developments was small but not unimportant: I photocopied Shelly’s thesis in the Harvard library and discreetly sent it (upon request) to the relevant address. The birth of QCD was more of a collective enterprise, and the subject of whether it will, or will not, deserve someone’s trip to Sweden is one of the items of endless conversation in which idle particle physicists indulge.

Let me distinguish the YM electroweak gauge theory from the corresponding model. By theory I mean the full-fledged renormalizable construct, as tested—including quantum corrections—by the $e^+e^-$ colliders at CERN and SLAC. The no-mean feat of the experiments was precisely this set of tests, although it is not so easy to “sell” their importance to the general public (the Nobel Committee had to work harder than usual in 1999). By the electroweak model I mean that part of it that can be tested at tree level, notably the existence of weak neutral currents.

Historically, the phenomenological aspects of the electroweak theory were less interesting than those of the corresponding model a, in the sense that they required less of an experimental and theoretical effort to understand what the heck was going on. Also, the understanding of deep-inelastic electron and neutrino scattering as well as $e^+e^-$ annihilation in the “charmed” region were intimately intertwined with the proof of the reality of quarks and with QCD, whose asymptotic freedom makes it an inescapably subtle quantum theory for starters. In practice what all this meant is that particle physics, or at least its phenomenology, were infinitely more challenging and interesting in the 70’s than they were thereafter. These arguments are the excuse for the choice of topics I shall discuss at greatest length (Sections 2 and 3).

2. Neutral currents

The 1973 discovery of weak neutral currents in neutrino scattering by the Gargamelle bubble-chamber collaboration at CERN [9] made experimentalists, and the world at large, aware of YM theories, much as the 1971 work of ’t Hooft [7] immediately attracted attention from (field) theorists to the same subject. Both lines of work [789] were monumental in their difficulty, and run against deep prejudices. In

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aYoung theorists often take a cynical view of renormalizability and the distinction I made between model and theory, being satisfied with “effective” theories valid in a very limited energy range. This may be justified in the case of chiral Lagrangians, but it is harder to justify for YM theories. And the crucial role that renormalizability played in the development of QED, QCD and the electroweak theory is hard to sweep under the rug.
the case of neutral currents, the prejudices had various sources, among them:

- The very strong limits on their strangeness-changing variant, which in the 70’s were at the branching ratio level of $10^{-6} \ (10^{-9})$ for $K^\pm \ (K^0)$ decays [12].

- The perception that the measurement of neutrino-induced weak neutral currents—at least in the semileptonic channels with larger cross-sections than the purely leptonic ones—was nearly impossible in its practical difficulty [13].

- The existence of severe (and incorrect) upper limits on strangeness-conserving neutral-current processes, such as the one by Ben Lee, stating that [The results of W. Lee [14]] rule out the existence of the neutral current predicted by Weinberg’s model... [15], or the one by John Bell, J. Løvseth and Tini Veltman: Thus the ratio of neutral-current “elastic events” is less than 3% [16].

- The fact that neutrino experiments at the time were primarily designed to look for sequential heavy leptons and for the Lee–Yang process $\nu_\mu + N \rightarrow W^+ + \mu + N$—for light $W$’s, but not for neutral currents.

Naturally, the neutral-current processes favoured by theorists were the ones whose cross sections could be calculated with confidence in the standard model: $\nu_l, \bar{\nu}_l$ elastic scattering on electrons, whose standard cross-sections were worked out by ’t Hooft as early as 1971 [18] (he may also have been the first to emphasize a trivial but important fact: a measurement of the weak-mixing angle, $\theta_W$, would imply a prediction for the—then—enormous masses of the $W$ and $Z$). By January 1973, the Aachen group of the Gargamelle collaboration had found a “picture book” event, with a single recoiling electron [19]. But it was just one event and—while it immediately had the effect of putting Gargamellers even harder at work on neutral currents—various cautions and dangerous backgrounds kept the team from publishing this result until July, right before they published their work on semileptonic neutral currents [9].

In parallel to these developments, theorists were at work on what experimentalists wanted: predictions for semileptonic neutral-current cross sections, and in particular lower limits on them (the minimization is relative to $\theta_W$). Weinberg worked out limits on the elastic channels and on $\Delta$ production [20], while being amongst the first to recognize the need for charm and the GIM mechanism [21] to make the electroweak model compatible with the suppression of strangeness-changing neutral currents. Finally, Bram Pais and Sam Treiman [22] and Mani Paschos and Lincoln Wolfenstein [23] produced what experimentalists really wanted: results for inclusive channels, which were at the level of 20% for the minimal ratio of neutral to charged channels.

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bI have refreshed my memory on these issues by re-reading the talk by Bernard Aubert in [10], an interestingly uneven book; and their rendering by Peter Galison in [11], a thoroughly documented report, that reads like a good novel. I quote them freely in this section.
Neutral-current events at the level of 1/6 of the total!—nothing would seem to a theorist easier to detect. Yet it was far from easy to do it convincingly; the intricate story is well told by Galison [11] and Riordan [24], so I shall not try to summarize it here. The decisive talk by Paul Musset at CERN on 19 July 1973, in which he reported the Gargamelle results [9]:

\[ R_\nu \equiv \left( \frac{NC}{CC} \right)_\nu = 0.217 \pm 0.026 \]

\[ R_\bar{\nu} \equiv \left( \frac{NC}{CC} \right)_{\bar{\nu}} = 0.43 \pm 0.12, \]

was received with enthusiasm by some, complete scepticism by others. One of the sceptics is a colleague of mine at CERN, Nobel Laureate for the codiscovery of the second neutrino—to give you an extra hint. He always disbelieves new results, which is his way of being right some 99% of the time. This time was in his 1% category, and I believe that my good friend Paul Musset would have also become a Nobel Laureate—had he not died at an untimely age.

Inordinate amounts of spice and emotion were added to the neutral-current saga by the “alternating-current” results of the HPWF collaboration, which, after a few cycles, finally agreed with Gargamelle [25]. The happy ending was a talk by Bari Barish at the 1974 HEP conference in London, in which he reported the neutral-current results of the Caltech/FNAL collaboration, fully confirming their existence [26]. The difficulty of the early measurements of weak neutral currents can be summarized in a Nov. 1964 picture of Gilberto Bernardini, reproduced in Fig. (1). Notice the upper limit on the ratio \( NC/CC \) of “elastic” neutrino cross sections on nucleons... it is significantly below the currently established result.

I may have transmitted the impression, which I shall retransmit below, that experimentalists may sit for a long time on a gold mine before convincing themselves that that is the case. Sometimes theorists are even better at that:

As early as 1961, Gell-Mann and Glashow realized that YM theories were endangered by the fearsome spectre of strangeness-changing neutral currents [27]. Yet, in 1964, Hara [28] and Björken and Glashow [29], when suggesting charm and the concomitant weak current with the explicit mechanism for averting the danger, did not realize they had solved the problem first stated by Gell-Mann and Glashow. Only six years later, Glashow and/or Iliopoulos and/or Maiani [21] must have been idle enough to read the literature, and realize that the problem and its solution were stated in

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\(^6\)Whether others may have shared the prize I cannot tell, nor can I suggest any names at the risk of making \( n – 1 \) enemies, with \( n \) a large number, for Gargamelle was the first modern “large” and hard-to-manage collaboration [11].
papers that at least one of them may have been expected to have read.

3. QCD and charm in their early years

This section concerns the development of QCD from 1973 to 1978, spanning the period from the discovery of asymptotic freedom, covered by others in this book, to the discovery of the gluon\textsuperscript{d}.

3.1. A Deep Inelastic Dawning

In a talk reproduced in \cite{31}, 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 next door. At the time of the discovery of QCD’s asymptotic freedom \cite{32}, David Politzer had the office next to mine at Harvard, where 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 more formal reasons.

\textsuperscript{d}I make in this section abundant use of a previous rendering of the same story \cite{30}.
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 in deep inelastic scattering experiments. Thus, though the rationale for a rather low-energy “asymptotia” remained obscure for quite a while, the discovery of asymptotic freedom was received with a great sigh of relief by a then-persecuted minority of field-theory addicts.

Knives had been sharpened for long on inclusive reactions. Not surprisingly, the first concrete predictions of QCD concerned the deviations from an exact scaling behaviour. But the electron scattering and $e^+e^-$ annihilation data of the time covered momentum transfers, $Q^2$, of not more than a few GeV$^2$. Nobody (yet) dared to analyse 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 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 used (or, with the benefit of hindsight, slightly abused) the then-mysterious “Bloom–Gilman duality” relating the deep “scaling” data to the elastic and quasi-elastic peaks. I prefer the paper containing Fig. (2) 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”; to conclude “The results agree with experiment but are not a conclusive test of asymptotic freedom.”

Lustra later, looking back at the papers I just quoted, I notice that they were received by the publisher on consecutive days. This was not atypical of the Harvard/Princeton competition of those times, a competition that I then viewed as fierce and evil. Mollified by Chronos, I now view this tug of war as fierce and useful. Being a bit more inclined to data analysis than my Princeton competitors, I attempted to measure $\Lambda$, as in Fig. (2), while they simply chose the “right” value... somewhat surprisingly since their results—based on an analysis slightly different from mine—neither fitted the data nor subtracted from their confidence in the theory.

There were sporadic truces in the Harvard–Princeton wars. An example is a 1974 paper signed by an even mix of six authors from the two institutions. We derived the leading QCD predictions for structure functions $F(x, Q^2)$ in the “Regge” domain, $x \rightarrow 0$. In the consuetudinary notation and with $R$ an explicit function of $x$ and $Q^2$:

$$\frac{\partial \ln (RF)}{\partial \sigma} \rightarrow \text{"large" } \sigma \frac{12}{\sqrt{33} - 6 n_f/N_c},$$

$$\sigma^2 \equiv s \ln \frac{x}{x_0}, \quad s \equiv \ln \frac{\ln (Q^2/\Lambda^2)}{\ln (Q_0^2/\Lambda^2)}. \quad (1)$$

Unbeknownst to many, this paper enjoyed a second life as the Mother of All Physics.
Figure 2: The magnetic form factor of the proton normalized to a dipole fit, a first attempt to measure Λ that stumbled upon the right result. Thus distilled, the presentation obscures the fact that the theory and the actual cross-section data agree in a range of six orders of magnitude.

at HERA. The comparison\textsuperscript{11} of its predictions with experiment, shown in Fig. (3), was a belated but striking confirmation of QCD.

It was not easy to write the Harvard–Princeton paper\textsuperscript{10}. In those fax-less and email-less days we had to exchange drafts by post. Many references had two entries, a “Harvard” one and a “Princeton” one. The Orangemen would send a draft with all references in the P/H order, whence the Crimson would make some cosmetic changes in the manuscript and send it back with references reversed. The paper was published the Princeton way, which proves that we at Harvard were only joking.

Perhaps understandably, by our next paper\textsuperscript{12}, we were back to our peaceful parted ways. Elaborating on work\textsuperscript{13} by Giorgio Parisi (who had Mellin-transformed the QCD results on $x$-moments into direct information on the structure functions), we decoded their evolution at fixed $x$ for varying $Q^2$. The result, shown in Fig. (4), was to become heavily used... and systematically referenced to authors of much later papers. While yowling, I plead guilty to having learned much later that the simple underlying physics had been understood elsewhere: the renormalization-group\textsuperscript{14,15} picture of seeing partons within partons was drawn by Kogut and Susskind\textsuperscript{16}, the “physical gauge” diagrammatic image of a parton dissociating into others is due to the usual Russian suspects\textsuperscript{17}, and the vintage QED analogue is nothing other than the Weiszäcker–Williams approximation\textsuperscript{18}.

During the times I have just described, it was my impression that physics could
Figure 3: The data snuggle toward the slope predicted by QCD, for $n_f = 4, N_c = 3$.

Figure 4: Example of the evolution [in $Q^2$, or $s$ as in Eq. (1)] of a normalized non-singlet structure function $F(x, s)/F(x, 0)$ at fixed values of $x$. The predicted trend has been corroborated in detail by a multitude of experiments (and theorists).
not possibly be more hectic and interesting. I was wrong...

3.2. The November Revolution

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. Thirty 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 [24]. In a nutshell, the standard model arose from the ashes of the November Revolution, while its competitors died honourably on the battleground.

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-goodie-bean. In those days, both QCD and charm were already fully “established” at Harvard, where one could, without the scorn of one’s peers, violate the first rule of scientific fairy tales: not more than one fancy character per tale! 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 peaks and Coulomb-like 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.

Burt Richter was also caught in the 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 Tom, 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 (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 call summoning Burt back to SLAC delivered us from his scorn for theorists. Only in other multi-world histories of our Quantum Universe [52] do charmed theorists get to talk also with Sam Ting, prior to the Revolution. For an object of its mass, the $J/\psi$ is four orders of magnitude narrower than a conventional hadron, or just one order of magnitude wider than a weak intermediary (only because they are my good old friends do some people escape reference and derision at this point). Of the multitude of theoretical papers that immediately hit the press [53], only two attributed the narrow width to asymptotic freedom, one by Tom and David [49], who had intuited the whole thing before, the other by Sheldon Glashow and me [54]. I recall Shelly storming the Lyman/Jefferson lab corridors with the notion of the feeble three-gluon hadronic decay of the $J^P = 1^-$ orthocharmonium

\[\text{Sam did talk to a theorist at MIT, but Harvard’s charmed infatuation did not extend that far.}\]
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.

We did a lot in our extra week of asymptotic freedom) we related the hadronic width of the $J/\psi$ to that of $\phi \to 3\pi$, to explain why the resonance had to be so narrow. We correctly estimated 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. Re-reading this paper much later, I was surprised to see that we had already discussed mass splittings within multiplets of the same quark constituency as hyperfine, a fertile notion. In discussing paracharmionium ($J^P = 0^-$) 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. Not every week of my (scientific) life parallels this particular one. The commentary on this paper in the book by Riordan is one of those things that my grandfathers would have liked to read. My grandmothers may even have believed it!

3.3. A First Year of Lean Cows

At the time of the November Revolution, and for two or more years to follow, there were hundreds of theorists “out there”, determined to deride what is now called the Standard Model. On rare occasions, this was due to healthy scepticism or the invention of cute alternatives, e.g. [55]. More often than not the reason was intellectual inertia or militant ignorance. But history is what a society chooses to record, not what “really” happened. It behooves the wise to change their minds, says a Spanish proverb, and now we all sign onto the registry of wisdom.

I have relatively 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 at home, 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 out of breath and wits, the darned words would simply not come out. 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 [56] on Charmonium spectroscopy. Physical Review Letters was fighting its usual losing battle against progress (in nomenclature, $\sim$) and did not accept the title. Our original spectrum is reproduced in Fig. [5].

A few days after we submitted our opus we learned that a group at Cornell
had been working on very much the same subject (our usual competitors in Princeton did not participate in the more phenomenological skirmishes that were soon to take place). My recollection of how we learned about Cornell’s competition [30] is not shared by Kurt Gottfried, and I shall not reproduce it here. Our paper [56] was a bit less elaborate (they’d say) or a bit more uncommitted to a particular model (we’d say) than the Cornell paper but, I used to joke, the main difference between the two contiguous Physical Review Letters [56,57] is that the Cornell analogue of Fig. (5) is mirror-imaged. In fact, their work was based on explicit calculations with a confining linear-potential model, and resulted in very successful predictions for the photon decay rates.

Early in 1975, Howard Georgi was back from his excursion into $SO(10)$ and $SU(5)$. He, Shelly, and I wrote a paper [58] whose style reflects how high we rode. Here is how it began:

Once upon a time, there was a controversy in particle physics. There were some physicists$^1$ 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. Others$^2$, appalled by the teeming democracy of hadrons, insisted on the existence of a small number of fundamental constituents and a simple underlying force law. In terms of these more fundamental things, hadron spectroscopy should be qualitatively described and essentially understood just as are
atomic and nuclear physics.

We proceeded to do just that. Our second reference was to Gell-Mann and Zweig for the quark model \[59,60\]. Like most people to date, we did not know at the time that André Petermann may have been the first to consider spinor constituents of hadrons with “constituent masses” and fractional charges \[61\]. I cannot resist quoting him: *Ou bien, si l’on veut préserver la conservation de la charge, ce qui est hautement souhaitable, les particules doivent alors avoir des valeurs non-entières de la charge. Ce fait est déplaisant mais ne peut, après tout, être exclu sur des bases physiques.* To the non-relativistic version of the quark model, Howard, Shelly and I added chromodynamic quark interactions entirely analogous to their electrodynamic counterparts. To this day it is unclear why the ensuing predictions are so good. Our paradigmatic result was the explanation of the origin and magnitude of the $\Sigma$–$\Lambda$ 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 bag-model relativistic version \[62\] of the same work.

In \[58\] 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. Competing 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. Interestingly, only one person trusted a “QCD-improved” constituent quark model early enough to make predictions somewhat akin to ours: Andrei Sakharov \[63\].

3.4. 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. 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 \[64\], a Brookhaven bubble-chamber group \[65\] pictured a $\Delta S = -\Delta Q$ event, forbidden in a charmless world, and compatible with the chain:

$$
\nu_\mu \, p \rightarrow \Sigma^{++} \mu^-
\Sigma^{++} \rightarrow \Lambda^+_c \pi^+
\Lambda^+_c \rightarrow \Lambda \pi^+ \pi^+ \pi^-
$$

The mass and mass difference of the two charmed baryons caught here in one shot sat on top of the predictions \[58\]. This was a source of delight not only for us, but also for the experimentalists involved.
In the summer of ’75 —after a year of upper limits incompatible with the theoretical expectations— the first evidence finally arose for the $P$-wave states of charmonium \cite{66}. The DESY group 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 this story, concerning their competition with SLAC.

The discovery of a positronium-like $c\bar{c}$ spectrum converted crowds of infidels to the quarker faith. And the charmed quark was to continue playing a crucial role in the development and general acceptance of the standard lore.

3.5. Yet Another Year of Lank Cows

Measurements at SLAC of the $e^+e^-$ cross section into hadrons \cite{67} showed a doubling of the yield and structure aplenty as the $\sqrt{s} = 4/5$ GeV region is crossed. Much of the jump had to be due to the production of charmed pairs, which were not found. Howard Georgi and I innocently believed that a 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 \cite{68} 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 \cite{69} 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 Poggio, Quinn and Weinberg \cite{70}, 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 imprimatur of Steve et al., attracted considerable attention to this aspect of QCD and strengthened our conclusion: the measured total cross section, analysed on firm theoretical grounds, implied the existence of charm and of a new heavy lepton.

To come back to the role of charm in QCD, the determination of $\Lambda$ had been one of my fixations \cite{37} since the birth of this unique QCD parameter. In the paper with Howard \cite{68} we made an attempt to get a meaningful result, using fresh (but incomplete) data on deviations from scaling in deep inelastic scattering of muons on iron \cite{71}. We concluded *The $Q^2$-dependence for $\Lambda = .32$ GeV agrees qualitatively with the $\mu$-Fe data... If the standard model with one heavy lepton is correct, the actual
value of $\Lambda$ is probably between 1 and .32 GeV. The announced lepton was the $\tau$.

3.6. 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 more [72, 73]:

Shelly Glashow happened to chat with Gerson Goldhaber in an airplane. Something unusual took place. 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 [74]. 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”: $\Psi(3770)$ [75]. Alas, they failed.

The observation of charmed mesons ought to have been the happy ending, but there was a last-minute delay. The invariant-mass spectrum of recoiling stuff in $e^+e^- \rightarrow D^0(D^\pm)+...$ had a lot of intriguing structure, but no clear peak corresponding to $D^0\bar{D}^0$ [74] or $D^+D^-$ [76] 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.

But we had one last unspent cartridge [77]. 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 [72] $m(D^*) - m(D) \simeq m(\pi)$, 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. The $D^*$ decays had to be very peculiar: $D^{*0} \rightarrow D^+\pi^- \text{ is forbidden, } D^{*0} \rightarrow D^0\gamma \text{ competes with } D^{*0} \rightarrow D^0\pi^0$, etc. On the basis of these considerations (and with only one fit parameter) we constructed the recoil spectra shown in Fig. [6]. Case closed!

“Enemies”, “cartridge”... Why am I using such belligerent terms? Not only because of the vehement competition between divers schools of theorists. I have a keen memory of the East Coast experimentalists with whom I talked at the time (not a significant sample, in the statistical sense). They were strongly motivated to disprove all theories and theorists, perhaps permeated by some arcane Californian faith that nature is intrinsically unfathomable. They most certainly did not have an instruction on their data-analysis program stating IF (RESULT = STANDARD) THEN STOP. This made life most enjoyable and the case for the standard model veeeery strong.

Having worked over the last few years in astrophysics, I have discovered to my amaze-
Figure 6: Predicted and observed invariant-mass spectra, recoiling against neutral and charged $D$'s. The theoretical curves are a one-parameter fit.

ment that challenging the corresponding standard models is **not** a main motivation of theorists and observers, quite the contrary, see e.g. [78].

3.7. Back to the Deep Inelastic Domain

By the summer of '76 the skeleton of the standard model was almost complete: charm was established and the $\tau$-lepton [79] was making its unhasty way towards acceptance. A talk I gave that summer [80] —of which Fig. 7 was a transparency—could have been entitled *Status of the Standard Model*, even if it was not yet *standard*, or called that way. The masses of some of the missing bones ($b$ and $t$, and the various neutrinos), but not those of others ($W$ and $Z$) were still utterly unknown, and the Yang–Mills structure of the theory was essentially untested. Yet, a soon as the “sequential” pair ($\tau$, $\nu_\tau$) was established, “we all knew” that $b$ and $t$ had to exist, not only to cancel anomalies [81], but to fit into a decent theory of CP violation [82].

One reason why QCD was not better tested is that the only precise deep inelastic data pertained to modest momentum transfers. An analysis in terms of scaling, and QCD deviations thereof, would have to face tedious technicalities (target-mass corrections and higher-twist effects) and flagrant departures from scaling (prominent nucleon-excitation resonances). Though all of this squalor was well beneath the proverbial dignity of a self-respecting theorist, David, Howard and I decided to face the difficulties, in a paper unpronounceably entitled *Demythification of Electroweak Current Local Duality and Precocious Scaling* and in its accompanying letter [83].
Figure 7: The first 4 quark flavours in their 3 colours (those of the Spanish republican flag), c. 1974. Understandably, I have refrained from updating this figure to include top and bottom. I would have done it, if the names I proposed at the time for their flavours (tenderness and beauty) had caught.
I believe “Demyth” to be one of the nicest productions in which I have participated. I suspect it is the most influential, but I know (thanks to currently available electronic means) that it is not very much quoted. One reason why I think Demyth was influential is that one of its ingredients, the performance of target-mass corrections via $\xi$-scaling [84,85] provoked a violent reaction from numerous groups [86]. We believed theirs to be a misunderstanding, but to this day I am not sure whether it was our response [87], or something else, that defused this altercation [88].

Mysterious dualities are a recurring theme in physics. Bloom–Gilman duality is the observation that at low $Q^2$ a structure function shows prominent nucleon resonances, which “average” to the “scaling” function measured at some higher $Q^2_0$, and slide down its slope as $Q^2$ varies, all as in Fig. (8).

We argued in [83] that this duality is a consequence of QCD, inevitable if scaling is “precocious,” as it must be for small $\Lambda$ (a fraction of a GeV). In Fig. (8) the proton-pole positions are at the places labelled $\xi_p$. The area under their $Q^2$-dependent contributions ($\delta$ functions in $\xi$) approximately equal the areas under the corresponding smooth curves at large $\xi$. This justifies a posteriori the analysis of [37,38] and explains why those early attempts at QCD phenomenology resulted in a reasonable value for $\Lambda$. It is a satisfaction, particularly for theorists, to be the first to “measure” a fundamental constant of nature.

With the benefit of hindsight, I view as the most interesting result of [83] the conclusion that, in the comparison of experiment and QCD theory, excellent agreement is obtained using $\alpha_s(Q^2 = 4\text{ GeV}^2)/\pi = 0.17$. This corresponds to $\Lambda \sim 0.5\text{ GeV}$ and is, I believe, the first solid determination of this basic constant.
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. [89]. This test of QCD was not very severe. One reason is the neglect of higher twists [90,88]. Furthermore, it is not easy to measure the incident neutrino energy. Thus, in an unintended implementation of Bloom–Gilman duality, $F(x, Q^2)$ is significantly blurred in $x$ and $Q^2$. At the low $Q^2$ of a good fraction of the data, there must be bumps such as those of Fig. (8). Had the resolution of neutrino experiments been as good as that of their electron-scattering counterparts, the bumps would have been visible, and the data analysis would have had to be quite different.

With a pinch of poetic licence one could assert that, early on, many concluded that QCD was quite precise, but only because the data were not.

3.8. Seeing is Believing

Quarks have not been seen and may [91] never be. But their manifestation as quark jets was apparent since 1976 [92]. Kogut and Susskind [46] argued that the gluon, an important character that I have not yet discussed explicitly, could show up in the same way: the elementary process $e^+e^- \rightarrow \bar{q}qg$ may result in three-jet final states.

Further work on QCD jets was often based on “intuitive perturbation theory”, an appellation perhaps meant to reflect a fundamental lack of understanding. Decorum was regained by the work of Sterman and Weinberg [93], Georgi and Machacek [95] and Farhi [96], who exploited the fact that in QCD, as in QED, there are “infrared-safe” predictions [94], 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 [97] the pattern, binned in “thrust” [98], to be that of Fig. (9). 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 [98].

4. Grand Unification and proton decay

I remember an occasion, in 1974, when I was visiting SLAC. “Bj” Bjørken told me that he had recently read the article on $SU(5)$ by Georgi and Glashow [99]. He said that, as he was reading it, his heart was pounding faster and faster at the beauty of the unification of QCD and the electroweak theory, the convincing explanation of the commensurate integer and fractional charges of quarks and leptons, etc., etc. But, Bj said, his heart sunk when he read that protons were not forever. At the time I thought
Figure 9: Examples of predictions for three-jet distributions at different fixed thrusts, $T$. The left column contains the leading perturbative QCD predictions; in the right one the results are smoothed for "hadronization" effects. More often than not, the small jet is produced by a gluon.
his reaction was precisely the wrong one. Proton decay, I thought, was the best part of this Grand Unification: a candidate for the ultimate YM theory, including such a fantastic prediction! With the benefit of thirty years of hindsight, I am beginning to wonder whether Bj was right. I may have done this before, for Howard, Shelly and I once wrote a note [100] on an alternative trinification model, based on the gauge group $SU(3) \otimes SU(3) \otimes SU(3) \otimes Z_3$, with all the virtues of $SU(5)$ (or $SO(10)$ [101]), but no gauge-mediated proton decay.

Georgi, Hellen Quinn and Weinberg [102] were first to compute the renormalization of the gauge couplings of the $SU(3)$ and $SU(2) \otimes U(1)$ subgroups of a Grand Unified model, thereby refining the postdiction for the weak mixing angle and predicting the lifetime of the proton. In a sense, they also predicted, this time correctly—but not overtly— the mass of the proton: some three times $\Lambda$, the “infrared” momentum scale at which the QCD coupling becomes intractably large$^f$. John Ellis, Mary K. Gaillard and collaborators were the first to dare extend these renormalized strictures to relations between fermion masses [103]. The graph showing the unifying coupling constants is so famous that I shall not show it here, not even in its (still tenable) MSSM extension (the first M, I have always contended, describes the credibility of the Minimal Supersymmetric extension of the Standard Model). Instead of this graph, I show in Fig. (10) a 1974 picture of Howard and Shelly debating the advantages or disadvantages of $SO(10)$ versus $SU(5)$.

$^f$I have learned this way of looking at things from Savas Dimopoulos.
Grand unifications of YM theories and proton instability actually slightly preceded the work in [99]. Indeed, Jogesh Pati and Abdus Salam were the first to introduce the elegant idea of lepton number as a fourth colour [104]. I learned from Bram Pais, via Georgi [31], that their $SU(2) \otimes SU(2) \otimes SU(4)$ group even contained the full $SU(3) \otimes SU(2) \otimes U(1)$ gauge structure of the standard model! But they proceeded to break colour $SU(3)$ into integrally-charged Han–Nambu quarks, thereby distantiating themselves from the current standard lore.

The Harvard theorists were, this time, capable of immediately convincing experimentalists that proton decay was interesting to look for. Larry Sulak, in particular, was enthusiastic with the idea, and he was among those who developed the inverse-osmosis technique that proved to be crucial in making water—at a reasonable cost—sufficiently transparent to exploit a large water-Čerenkov detector. Larry deprived his colleagues at the University of Michigan of an elevator, to install in its shaft a sufficiently tall water container and test the technique. His colleagues may not have appreciated their extra stair-climbing efforts, but eventually this kind of detectors made great serendipitous discoveries: neutrinos from SN1987A, neutrino oscillations, ... Proton decay has not been observed to date, and who knows what future proton-decay detectors may uncover.

5. Gravity

Unbeknown to most, the Apollo 11 astronauts actually did something useful, other than testing Moon boots. In 1969, they placed the first passive laser reflector on the Moon. After so many years of sporadic lunar ranging measurements, some of the length parameters describing the lunar orbit are known with millimetre 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 $\sim 2 \times 10^{-13}$ [105].

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 these two bodies, differing accelerations would result [106]. In actual numbers, we know that the Earth’s bulk acceleration and that of its self-gravity are equal to a precision of $2 \times 10^{-3}$.

As I recalled in the Introduction, Einstein’s gravity is akin to a non-Abelian YM theory in the sense that gravitons gravitate. The diagrammatic translation of the previous paragraph is shown in Fig. [11]. 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 the equality (up to group-theory factors) of the colour charges of quarks and gluons, which is hard to measure “directly” [107]. There is a long way to go before
we have tested YM vector-boson theories to an astronomically satisfactory precision!

6. Afterword

The most profound mystery of nature is that it can be pictured and tamed in mathematical terms; the deepest quality of scientists is that they can imagine conceptual abstractions that turn out to be measurable physical realities. Yang–Mills theories are an example, and the visionary confidence of some of their developers always makes me recall the very same old story:

My fabulous math teacher in high school was totally embedded in math, in the sense that he fully believed in the actual reality of mathematical abstractions. He would go to the blackboard and draw an $X$ and a $Y$ axis. From the origin of coordinates his pinched fingers would slowly deploy a fictional $Z$ axis, pointing to the middle of his mesmerized audience. And for the rest of his lecture, as he paced up and down, he would never forget to hold an imaginary line with a steady hand, as he bent to cross under the $Z$ axis.

Apologies & Acknowledgements. Surely I must have missed some important references, and I am sorry for that. I thank Gerard ’t Hooft for giving me the opportunity to recall these good old times, and all my collaborators —named and
unnamed— for having put up with me for years.

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