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Review

When the Standard Model Was Ignored

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Abstract: It has become fashionable to celebrate anniversaries of, among other subjects, crucial steps in science. The Standard Model (SM) of particles and forces is not an exception. I shall emphasize the fact that "the community" was not always aware of the obvious: that the SM is a convincing contender for being part of "the truth"... and always was. In this writeup many figures reproduce good old times' transparencies, a touch of nostalgia.

Keywords: asymptotic freedom; November Revolution; charmonium spectroscopy

1. A Slow Start

The first "unified" gauge theory was Glashow's $\mathrm{SU}(2) \otimes \mathrm{U}(1)$ 1961 model of the weak and electromagnetic interactions. It became a theory with Weinberg's 1967 paper "A model of leptons", followed by Salam's 1968 conference proceeding on the same subject. Use the notation $\{\mathrm{Year}, \mathrm{Number\,of\,references}\}$ to recall the early success of these works: $\{67, 0\}$, $\{68, 0\}$, $\{69, 0\}$, $\{70, 1\}$, $\{71, 4\}$. The abrupt 1971 rise was due to the publication of GIM (It is unnecessary to give a GIM reference, "everybody" knows what the GIM mechanism is. The same for $\mathrm{SU}(2) \otimes \mathrm{U}(1)$. Citations in this paper will be sieved, but not systematic.) and its two correct guesses (that charmed quarks ought to exist and that $\mathrm{SU}(2) \otimes \mathrm{U}(1)$ might be renormalizable).

In a couple of extra years one can already see the start of an exponential trend: $\{72, 64\}$, $\{73, 162\}$. What had happened? As Sidney Coleman put it: *Gerard's kiss transmogrified Steve's frog into an enchanted prince*, that is 't Hooft published his proof that anomaly-free non-abelian gauge theories are renormalizable. Since Gerard did not attend the September 2024 Rome conference on *The Rise of Particle Physics* I refer to his recollection of the subject [1].

2. Time for Surprises

2.1. Discovery of Asymptotic Freedom

Renormalizable gauge theories have couplings whose strength increases at short distances. That had ceased to be true in the spring of 1973, when Gross and Wilczek (then at Princeton) and Politzer (then at Harvard) proved that non-abelian gauge theories, such as QCD, were asymptotically free [2,3]. Characteristically the papers reached PRL one week apart. But there were various precedents, the earliest one in 1970, by Khriplovich [4].

The first two phenomenological QCD papers, on the other hand, had no precedents. This time Princeton [5] was one day ahead of Harvard [6]. Both articles disregarded a (temporary [7]) lack of QCD-understanding of "Bloom-Gilman" duality and used data on the q^2-dependence of the magnetic form factor of the proton to study α_s, the strong fine-structure "constant". Only in [6] the theoretical analysis and the data did agree well enough for Λ^2 to be determined: it lies between 0.2 and 1.0 GeV^2, see Figure 1. Breaking with the tradition of Newton and Fermi, the constant Λ was thereafter called Λ_{QCD} and not Λ_{ADR}. In Figure 2 this leading twist and leading log result is compared with next and next-to-next order ones, differing, as expected, by about 30%.

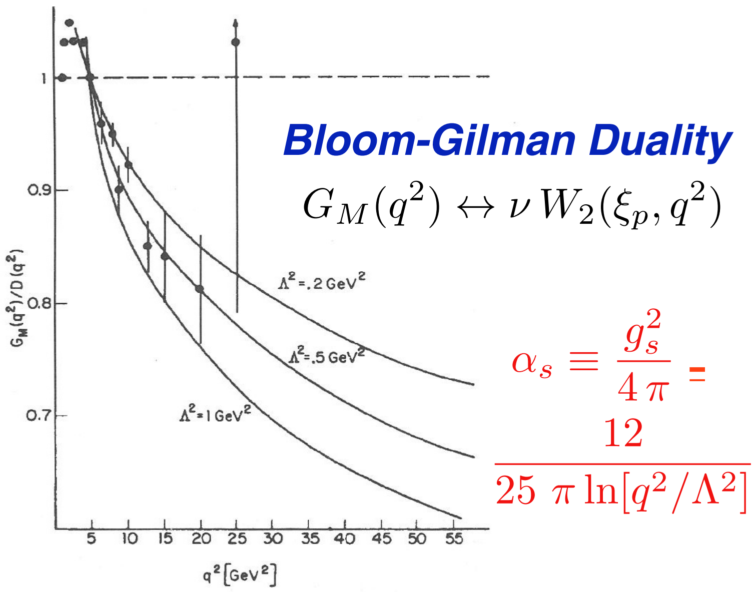

Figure 1. Data on the proton's magnetic form factor (normalized to a specific dipole approximation) and a QCD fit [6].

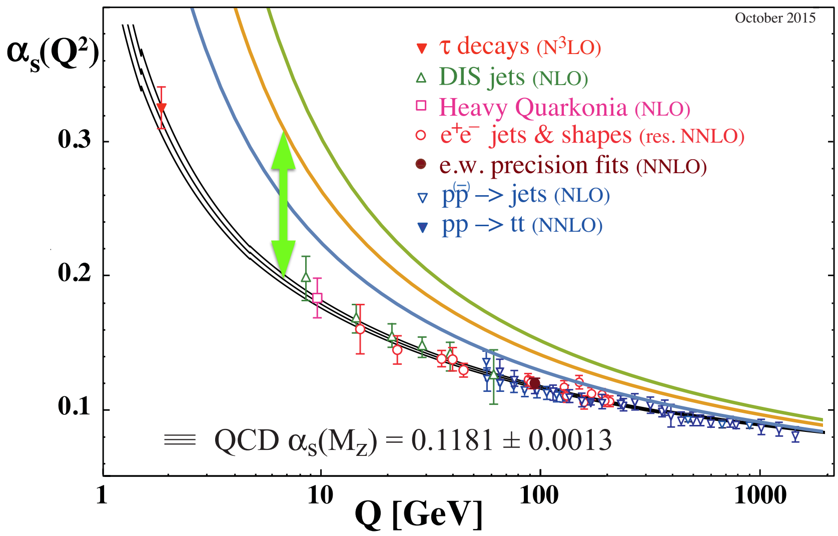

Figure 2. The colored lines are $\alpha_s(\mathrm{Q}^2)$ with uncertainty estimates [6]. The rest are later results.

2.2. The November Revolution

A revolution took place in 1974, see Figure 3. The discovery of the Ψ was announced one day after the one of the J (pronounced *Ting*, as in *Ting/Psi*). In the upper image from Augustin et al. a point is highlighted in the figure. It was this peculiarly high point, the chronicles of the time say, which allowed Richter and collaborators to catch the devil by the tail. The observed resonance has a long higher-energy tail due to initial-state radiation: $e^+ e^- \to \Psi + \gamma$ with the γ escaping detection. To hit gold (as in a medal) it sufficed to move down in collider energy from the observed tail-point.

The symbols "$\#\,\sigma's$?" in Figure 3 refer to the fact that nobody at the time insisted on quoting a number of standard deviations. Perhaps, as Val Telegdi used to put it, *If you need statistics to argue that you have made a discovery... perhaps you haven't.*

The J/Ψ was an unprecedented hadron, as dramatized in Figure 4. Well above the vertical scale of lifetimes one finds electrons, photons, neutrinos and protons, all of them—but one species—for very good reasons. From $\sim 10^{-10}$s to $\sim 10^{-5}$s one finds particles that decay weakly. Below that, a few electromagnetically decaying ones. And at the busy horizontal band the hadronic resonances known in 1974. The J/Ψ is in the middle of nowhere. The black line would be the lifetime of the $\mathrm{SU}(2) \otimes \mathrm{U}(1)$ Z, had it been that incredibly light. This misled a collection of my Italian friends [8] who proposed that interpretation, by far the least wrong of the wrong ones immediately published, not cited here out of respect for many eminent and far-out-of-the-box thinkers.

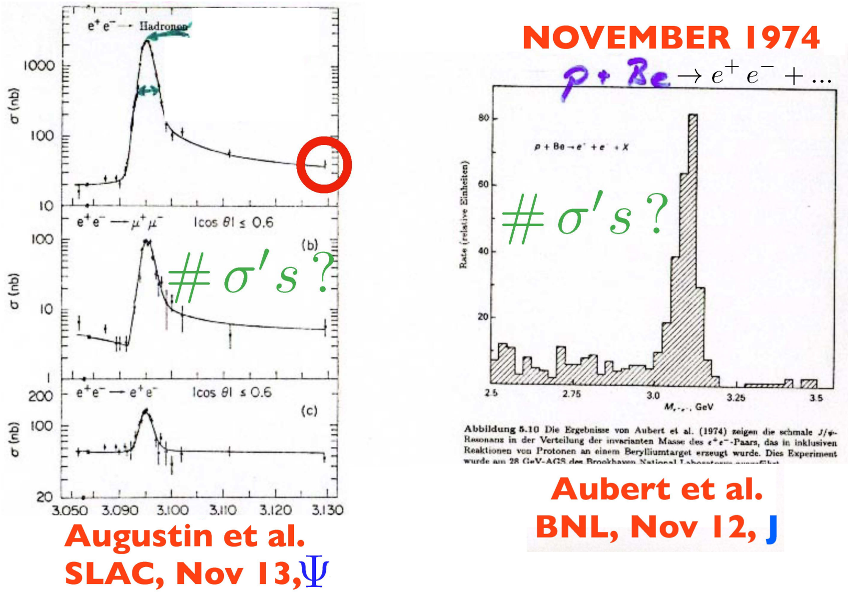

Figure 3. The data on the discovery of the J and the Ψ.

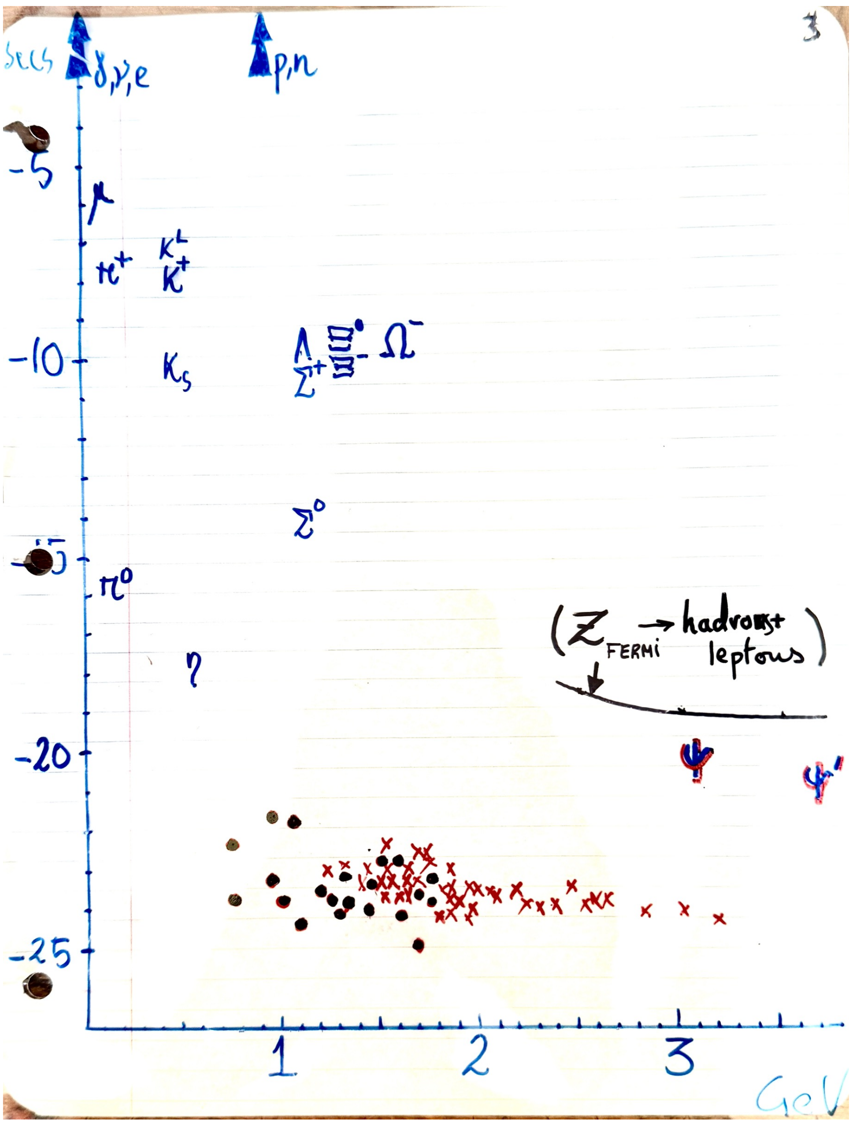

Figure 4. Masses and lifetimes of the particles known in 1974.

At Harvard the narrow width of the J/Ψ was not a surprise. Appelquist and Politzer, before the discovery, were already thinking about Charmonium, but they were hesitant because they could not believe their results, then based on a too Coulomb-like potential. But they knew [9], as Glashow and I also figured out a couple of offices away, that this composite vector boson decayed mainly into three gluons [10]. *Abusus not tollit usum,* Glashow and I compared the 3 gluon decay of the J/Ψ to the analogous one of the φ to pions, relating them by evolving α_s as in Figure 2. Lo and behold our predicted hadronic width of the J/Ψ is the currently measured one (We also correctly predicted the mass of the D^* and the existence of the Ψ', discovered before our paper was published.).

Two crucial ingredients in the understanding of charmonium are charmed quarks (As it is unknown, quarks were invented by André Petermann, see Who invented quarks?) and color. But, who invented quark color? This may constitute another surprise, west of the Iron Curtain. *Three identical quarks cannot form an antisymmetric S-state. In order to realize an antisymmetric orbital S-state, it is necessary for the quark to have an additional quantum number,* wrote Boris Struminsky (Greenberg and Han and Nambu are often credited in the West, but their colors are not the currently accepted ones.) in 1965 [11,12].

3. Fun Times

For the believers in the then non-standard Standard model the mid 70s were a great time to work. Competitors were very limited in number. The ideas to develop were occasionally very simple. One example: the charmonium Harvard crowd gathered for an entire night at my home—with my guest Éduard Brézin cooking delicious crêpes—to write a paper on predicting the realm of Charmonium Spectroscopy [13]. Our friends from Cornell wrote an article on the same subject [14]. It was more scholarly than ours, they had borrowed from Ken Wilson a program to implement the "Cornell potential".

The predictions are shown in Figure 5. After a couple of years finding limits below the predicted ones on the atom-like radiative decays, the experimentalists at DESY observed a line. The SLAC group found several transitions and announced their results a couple of weeks later, not quoting the DESY ones. This time a referee obliged them to give proper reference. Long live charmonium.

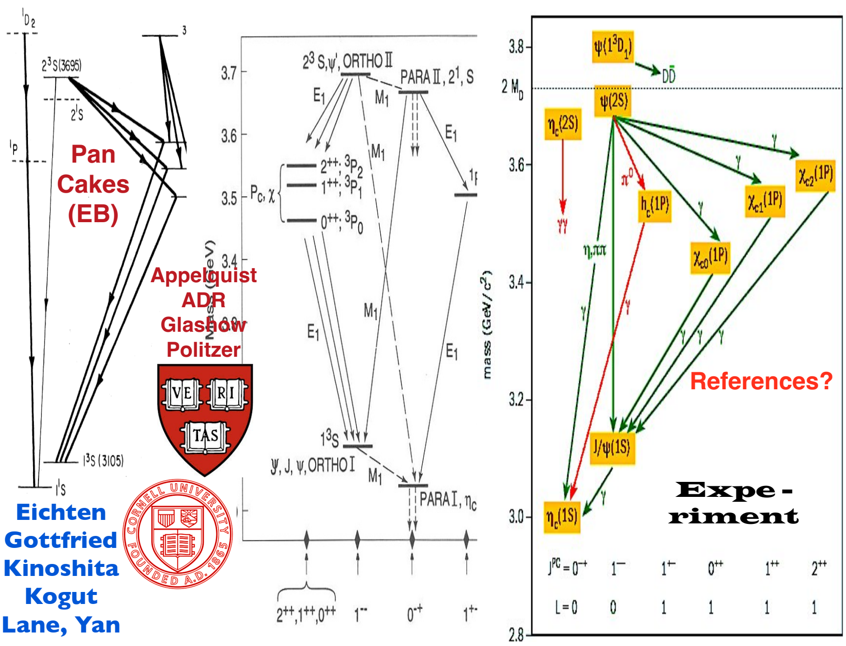

Figure 5. Charmonium spectroscopy. The data providers (right) did not refer to theory.

The SLAC data on the ratio R are shown in Figure 6. In a paper whose title *Finding fancy flavors counting colored quarks* was duly censored by PRL, Georgi and I made a "standard" analysis of the data. We found its best interpolation to $\sigma(e^+e^- \to \mathrm{quarks}+\mathrm{leptons})$ ("dual" to the resonances), including the charmed quark (and perhaps others) and a heavy lepton, all of them then hypothetical. We extended this analytical function to the spacelike domain, and compared it there with the perturbative-QCD predictions. We concluded 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* [15].

Times change. If a similarly novel analysis with a solid theoretical basis and statistical significance aplenty was made today on, say, LHC data, "the community" would conclude that a crucial discovery had been made. At the time only Martin Perl, inching his way towards discovering the τ, thanked us for the encouragement . Our work was thereafter totally forgotten (except at ITEP [16]).

In 1975 Glashow, Georgi and I wrote a paper [17] starting with the words *Once upon a time,* to refer to pre-standard model physics as surpassed. We introduced a QCD-improved constituent quark model with "hyperfine" mass splittings due to one-gluon exchange. That provided a successful description of all S-wave hadrons made of u, d and s quarks, explaining, for instance, the Σ^0/Λ mass difference, large though both particles have the same spin and uds constituency. With only one extra parameter (the charmed quark mass implied by the $c\bar{c}$ states) we predicted, correctly, the masses of all S-wave singly charmed mesons and baryons. Much later lattice gauge theories caught up with us, see Figure 7.

Still in 1975 Samios and collaborators [18, 19]took an impressive bubble chamber photo of the process $\nu_\mu + p \to \mu^- \, \Sigma_c^{++}$, $\Sigma_c^{++} \to \pi^+ \, \Lambda_c^+$, $\Lambda_c^+ \to \pi^+ \, \pi^+ \, \pi^- \, \Lambda$. The masses of Σ_c^{++} and Λ_c^+ were precisely the ones we predicted. For once, the experimentalist said so.

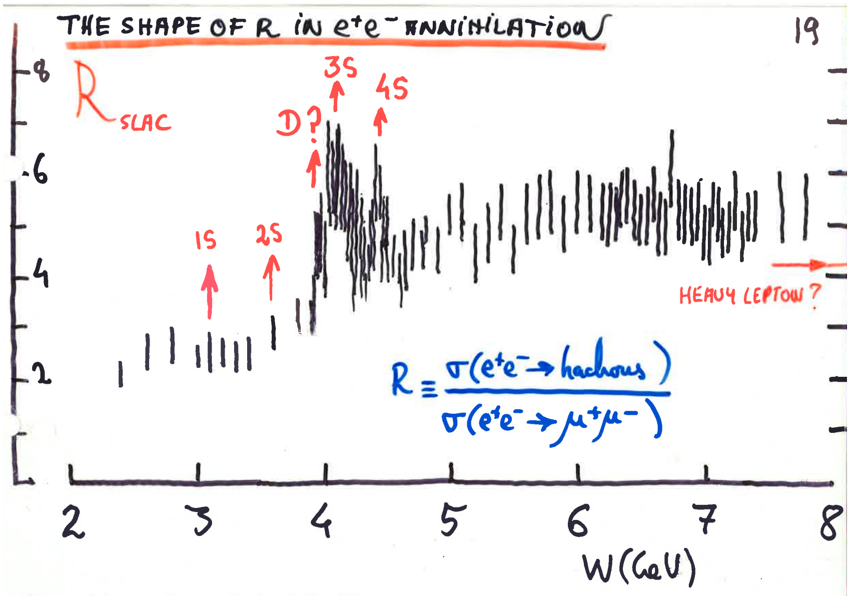

Figure 6. The well-know ratio R, measured at SLAC.

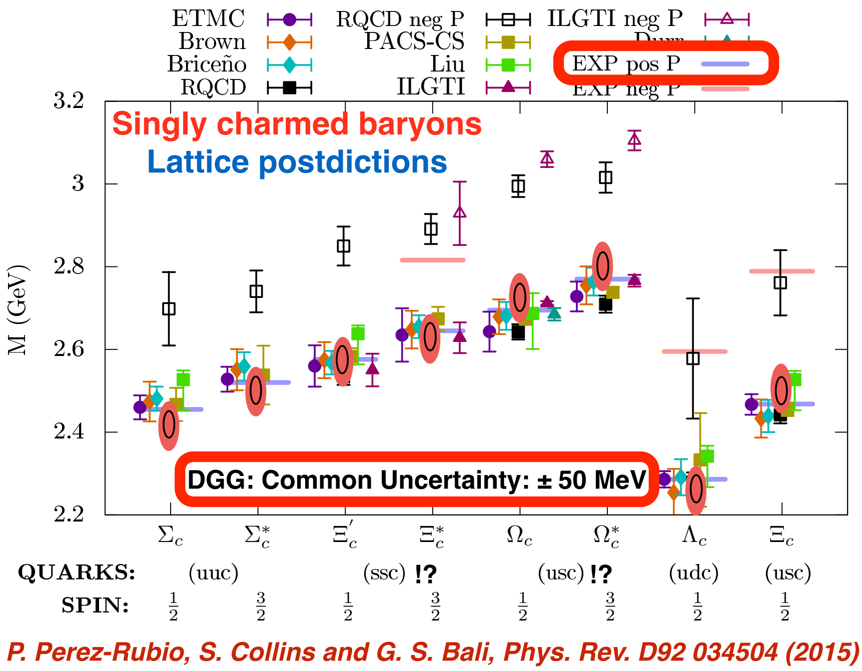

Figure 7. Masses of the singly charmed positive parity baryons compared to the data, the predictions in [17] and a collection lattice results.

4. Becoming Standard?

Given the many successes of the Standard Model one would have thought that by 1976 it would have been generally accepted. Not so. In July I gave a talk to an extremely well attended conference in Tbilisi [20]. I could not resist showing the twelve established quarks, as in Figure 8, and choosing for them the colors of the Spanish republican flag. Since Georgia was then part of the Soviet Union this triggered a standing ovation. Even a collection of quark pins for children became a must.

At Tbilisi intriguing new SLAC data on e^+e^- annihilation to states containing kaons were presented, see Figure 11.

The experimentalists suspected that their results had to do with charm (a good fraction of events contained kaons) but did not understand them. To do it, one had to believe our predictions for the masses and decays of charmed mesons, as in Figure 9. Notice that the decay $D^{0*} \to D^+\pi^-$ (or its charge conjugate) is forbidden, while in all other $D^* \to D\pi$ decays the pion has a very small kinetic energy. In Figure 10 we see how, at a collider energy just above the D^*D production threshold, a slow π may help fake a recoiling mass $M \approx m(D^*)$.

Figure 8. Three colors, four quarks and their pins in Cyrillic.

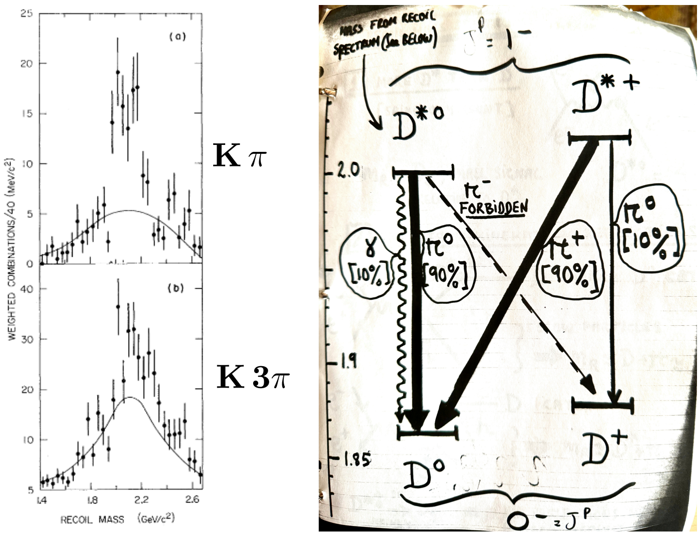

Figure 9. Invariant mass distributions of recoils against $K\pi$ and $K\pi\pi$ and the peculiarities of $D^* \to D$ decays.

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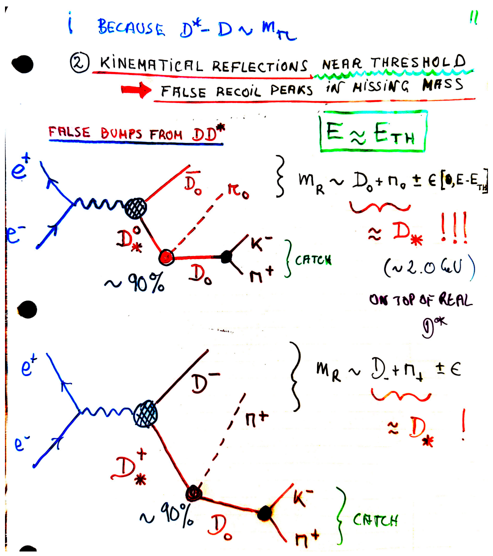

Figure 10. Origin of the "fakes" in the invariant mass of the ensembles of particles recoiling against $K^-\pi^+$.

Based on the above considerations we [21] could make a description (not meant to be a fit) of the observed recoiling mass spectra, with only one tuned parameter (b in $\exp[-(b\,E)]$ describing the suppression of D^*D production at a collider's energy not far above threshold. The results, shown in Figure 11, must have convinced the experimentalists, for they eventually made a correct analysis and published the discovery of charm.

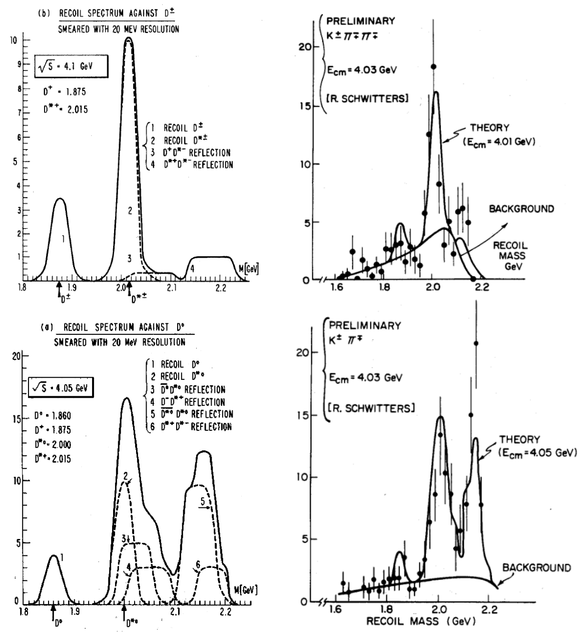

Figure 11. Recoil mass spectra in D^*D production [21].

5. The Higgs boson

Most scientists, often justifiably, feel under-cited. We all contribute to this. There are a lot of possible citations that I skipped. Citations are peculiar. One example which I shall not cite: perhaps the most cited article on the search for "the Higgs" is one that recommends not to waste time looking for it.

The discovery of the Higgs definitely established the most standard Standard Model. One aspect of it had to do with peculiar citations. In Figure 12, I reproduce, with extra commentary, a transparency shown by Joe Incandela in his Higgs discovery talk. It specifies that, in the search for $H \to Z\,Z^* \to 4$ leptons, an analysis was used accumulating the event-by-event likelihood ratio of it being signal or background. In tiny lettering and not spoken by Incandela there was an arXiv reference to [22] where the method was introduced for possible neutral objects of spin-parity $0^+, 0^-, 1^-, 1^+$ and 2^+. In the next transparency Incandela showed that this analysis contributed 3.2σ to the discovery of a 0^+ object which, combined with the analysis of $H \to \gamma\gamma$, resulted in a 5σ result. Quasi-standing ovation.

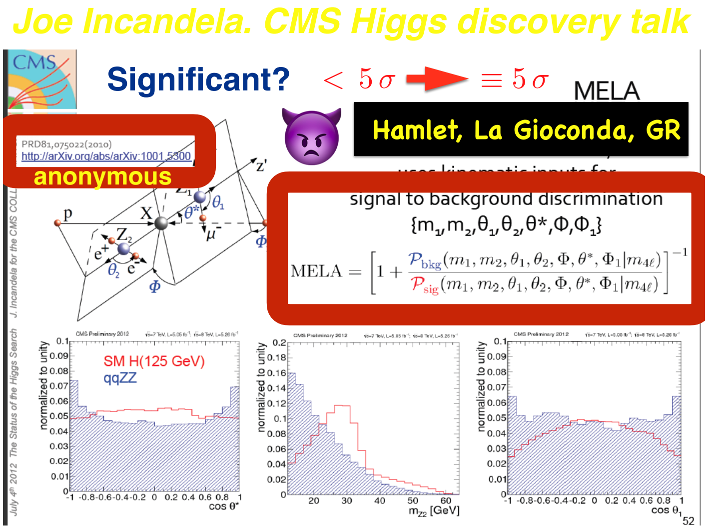

Figure 12. A transparency by Joseph Incandela .

Without the mentioned analysis CMS could not have reached the sacred 5σ at the time ATLAS did. ATLAS did not use this methodology, was it luckier? In Figure 12 I added "Hamlet, La Gioconda, General Relativity". Why? No doubt because "CMS" considered that, as in the cases just mentioned, who the authors of [22] were. . . was so well known that it would be offensive to believe that the audience did not know.

6. Conclusions

The interplay between theory, phenomenology and experiment in the development of the Standard Model was a blessing for those who participated. This being a compte rendu of a half-hour talk I have left a lot out. I wrote and cited more extensively on this subject in [23,24].

Conflicts of Interest

The author declares no conflict of interest.

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