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Opinion

The Standard Model Yesterday, Today and Tomorrow

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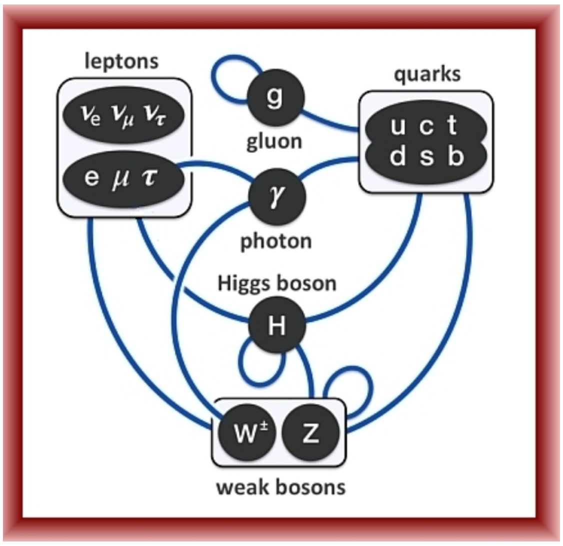
Abstract: The 1970s was the decade of the Standard Model! This decade which began with quantum field theory in disarray ended with a practical set of QFT tools for calculations for strong interactions at high and low energies and for electroweak interactions at all energies. After briefly remembering how we got there, I celebrate the remarkable achievements of particle physics in the 1970s and comment on where we go from here.

Keywords: Standard Mode; Massive Yang-Mills Fields; Renormalizable Lagrangians

I would like to thank the organizers for inviting me to this amazing Symposium. Who would not want to talk about the Standard Model in the Eternal City? And for me, being something of a homebody, it is a wonderful opportunity for me to see old friends from the heroic period when the Standard Model was built.

What is the standard model



johncarlosbaez.wordpress.com

What is the Standard Model? This is my favorite cartoon version, but of course the Standard Model is much more than that. This doesn't do justice to the neutrino sector. And indeed, we don't yet have a Standard Model of neutrinos. And the Standard Model is more than a clever picture or a list of fields and interactions. The development of the Standard Model went hand in hand with a change in the way we look at high-energy physics. This is a revolution that is well worth celebrating. Perhaps that is what the organizers had in mind with the title "The Rise of Particle Physics".

But I don't think the word "Rise" quite captures the unique feel of the decade of the '70s. To show you what I mean I want to go back another 20 years and remember some of the amazing particle physics in the 1950s and 1960s and try to understand how what we are celebrating is different. Here are a few of many highlights from the particle physics timeline.

I was too young in the 1950s to understand what was going on, but I had the great pleasure of working with Bram Pais twenty years later and heard a lot of stories. Just imagine the long list of issues that were under discussion in those years: the π°, the renormalisation group, the strong focusing, the bubble chamber, associated production, strangeness charge and isospin, Yang-Mills theory, the K-long K-short puzzle, the anti-proton and the anti-neutron, the detection of neutrinos and anti-neutrinos, parity violation, V-A, structure of the weak interactions, the prediction of two neutrino species, Regge poles. The list could continue with chiral symmetry breaking and the pion, macroscopic quantum interference, the Z and the weak mixing angle, the Goldstone bosons, the 8-fold way, the nucleon resonance, the evidence for two neutrinos, the Ω^-, flavour mixing, the quarks and the Higgs.

By the mid 1960s, I started to understand bits and pieces. SU(3) was one of the reasons I fell in love with our tiny world of elementary particles. I remember being excited by Jim Cronin's colloquium at Harvard soon after the discovery of CP violation. Not to mention the suggestion of charm, quark color, current algebra calculations. I was a beginning graduate student when the PRL containing Weinberg's "Model of Leptons" arrived in my mailbox [1]. Among the many issues under discussion in those years I have to mention also the solar neutrino problem, the dual resonance model, scaling and the parton model, chiral anomalies, the observation of partons, the operator product expansion. The last few years of the 1960s were certainly nibbling on the edges of the Standard Model.

So why were particle physicists depressed??

The reason, I think, is that these fantastic experimental discoveries and theoretical breakthroughs created as many puzzles and frustrations as they solved. Why no Flavor Changing Neutral Currents? Why approximate SU(3) and almost exact isospin? Since the quarks are partons, why don't we see them if they are almost free? How can the parton model work for electron scattering and fail for e^+e^- annihilation? Why don't we see fractional charges? Why do quark masses look so different in current algebra and constituent models? Why does not the η' look like a Goldstone boson? Why is CP violation so small? The list could go on and on!

Among the many frustrations was the ad-hoc character of current algebra. And even if it had been more systematic, it was still just an application of unexplained symmetry that did not address the fundamental problems of strong interaction. We just had no good way of dealing with the strong dynamics. The problem was certainly not lack of experimental results. By the standards of the day, there was a huge amount of data and it was neatly packaged by the particle data group into our particle data books. But it was not obvious what to do with it. We lacked the theoretical tools to fit the data into a useful calculational scheme.

And of course the biggest elephant in the room was that the renormalizability of Weinberg's model of leptons was just a speculation.

Steve was an honest man, and he did not claim too much. "*Is this model renormalizable? We usually do not expect non-Abelian gauge theories to be renormalizable if the vector-meson mass is not zero, but our Z and W mesons get their mass from the spontaneous breaking of the symmetry, not from a mass term put in at the beginning. Indeed, the model Lagrangian we start from is probably renormalizable, so the question is whether this renormalizability is lost in the reordering of the perturbation theory implied by our redefinition of the fields*" [1].

He very clearly identified the issues. Massless Yang-Mills looked renormalizable but there wasn't really a proof. And the Higgs mechanism added an additional layer of uncertainty. I am pretty sure that this is why the "Model of Leptons" paper did not make a big splash when it first appeared. I was still a baby at the time, but I remember looking at it when it arrived in my mail box and like most everyone else (including, I think, Steve himself), I ignored it because I didn't know what to make of it. It didn't look renormalizable to me. This is also why I was quite annoyed in 2017 when there were conferences about the 50th anniversary of the Standard Model. That was just nonsense.

This shows up spectacularly in a plot of the citations to Steve's paper by year, Figure 1. Almost nobody cared for a few years.

The end of the 1970s was a depressing time for quantum field theory. Julian Schwinger, who was one of the creators of QED and legendary for his deep understanding of all of physics, had given up on QFT. When I took his course at Harvard in 1966–1967, he taught his alternative "source theory". He didn't even mention Yang-Mills. Amazingly, virtually all these puzzles and frustrations that had accumulated over two decades were swept away in a few years in the early 1970s.

I think that this was not a "rise" but an "eruption". It was as if the previous two decades of particle physics had built up an enormous pressure for change and the crucial results and ideas exploded out in many different ways. Some old puzzles remained and some new ones were created. The eruption was dramatic and transformed particle physics: the result was the Standard Model. This is what I think we are celebrating today. It led to some Nobel Prizes, Figure 2.

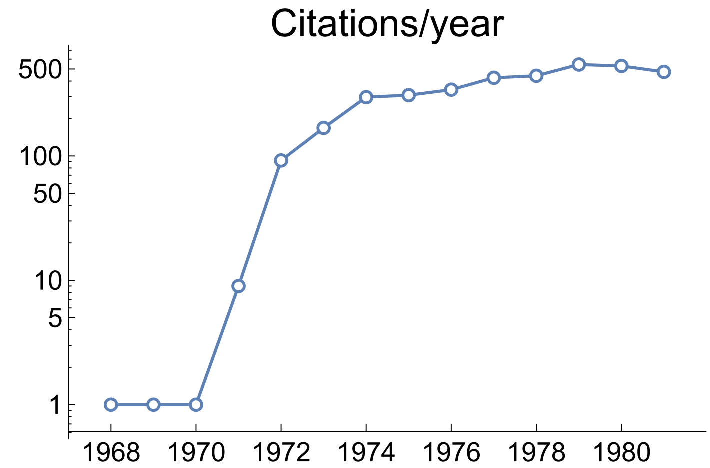


Figure 1. Plot of the citations/year of Steven Weinberg paper “Model of Leptons”.

Figure 2. Nobel Prize winners that contributed significantly to the Standard Model.

I now want to talk briefly about some of the components of this revolution. You have already heard about an important one from Sam. And you will hear more about some of the rest in subsequent talks. But I want to try to give a sense of how they fit together into the Standard Model.

The first piece in the puzzle was the Glashow-Iliopoulos-Maiani mechanism [2]. GIM identified the relation between the symmetries of the strong and the weak interactions that was responsible for the observed suppression of flavor changing neutral current effects. If the breaking of a chiral flavor symmetry is proportional to the mass matrix that defines what the flavors mean, then quark mixing for quarks of a given charge can be transformed away. While they only discuss four flavours, their mechanism was obviously extendable to any number of flavors. While this was brilliant, and critical for the success of the Standard Model, it is possibly the single most annoying piece of physics I know. What the GIM mechanism tells us is that we have absolutely no idea what flavor really is. Nothing distinguishes the different flavors of the same charge except their masses. We all know in our hearts that this can’t be true and our experimental friends have been searching for a violation ever since without definitive results.

In my view, the real birth of the Standard Model was ’t Hooft’s demonstration of renormalizability [3]. I love the concise abstract of this paper because it captures in three short sentences the enormity of what he did, Figure 3. He opened the door to a ”large” (in fact infinite) new set of QFT models. I frequently thank my lucky stars that this happened just as I was beginning my first postdoc because I had a beautiful shiny new theoretical playground to explore, including, of course, a version of Weinberg’s model of leptons. It was a fun time for model building because all the models were new—and because there were a relatively small number of model builders and a finite number of models. It was easy to understand everything that was going on. This has long since ceased to be even imaginable. ’t Hooft’s work grew out of the amazing general work on QFT done by him and Veltman [4]. It is also important to acknowledge the contributions of Lee and Zinn-Justin [5–8] and others in making this accessible to the community.

RENORMALIZABLE LAGRANGIANS FOR MASSIVE YANG-MILLS FIELDS

G.'t HOOFT

Our result is a large set of different models with massive, charged or neutral, spin one bosons, photons, and massive scalar particles. Due to the local symmetry our models are renormalizable, causal, and unitary. They all contain a small number of independent physical parameters.

Figure 3. The fundamental paper of Gerard 't Hooft.

The next piece to fall into place was dimensional transmutation. I am referring, of course, to the classic paper by Coleman and Erick Weinberg, "Radiative Corrections as the Origin of Spontaneous Symmetry Breaking" [9] This was an enormously influential paper on a not very interesting subject. This paper is a true classic. It did much more than to explain how renormalization converts a dimensionless parameter into a dimensional parameter and to show how to calculate the famous Coleman-Weinberg potential. It was a handbook on modern quantum field theory, a sort of "Well-Tempered Clavier" for QFT. It is worth reading every few years to see the work of the master Sidney Coleman at the height of his powers.

Motivated by experiments in quasi-elastic neutrino proton scattering that did not observe neutral currents, Glashow and I constructed a model based on the simple group SO(3) without a Z [10]. It is really ugly with heavy leptons and no explanation of universality, but we liked the fact that there was only one coupling and the fact that electric charge was quantized. We thought that this trivial algebraic quantization was different from Dirac's quantization in the presence of magnetic monopoles. In retrospect, it is clear that we should have thought harder about it. I think we did this at the end of 1971, but for some reason we didn't submit it until March 1972, by which time Steve Weinberg had done something that was also wrong, but was a little more interesting, Figure 4.

Mixing Angle in Renormalizable Theories

of Weak and Electromagnetic Interactions*

Steven Weinberg

$$\begin{pmatrix} e^- \\ \nu_e \\ \mu^+ \end{pmatrix}_L \quad \begin{pmatrix} \mu^- \\ \nu_\mu \\ e^+ \end{pmatrix}_L \quad \langle\phi\rangle = \begin{pmatrix} V & 0 & 0 \\ 0 & V & 0 \\ 0 & 0 & -2V \end{pmatrix}$$

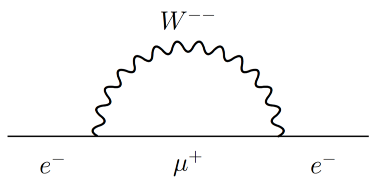


Figure 4. Steven Weinberg paper, 15 April 1972.

I call this Weinberg's second model of leptons [11]. His group was SU(3) cross SU(3) but it is easier to explain with a single SU(3) as shown. This is a real unified theory that has extra interactions with a very large VEV that break SU(3) down to SU(2) cross U(1) and leaves just the Standard Model of leptons at low energy. This super-strong symmetry breaking is a feature of all subsequent unified theories. The reason that Weinberg's version was more complicated was that he thought he might have a finite and calculable electron-muon mass ratio. At the time this didn't seem as ridiculous as it does now. But he got the renormalizations wrong and the ratio actually turns out to be infinite and thus a free parameter after renormalization. Glashow and I figured out how to fix the issue and the result was totally uninteresting because there were many parameters [12].

Weinberg's model wasn't renormalizable anyway because it had SU(3) gauge anomalies. 't Hooft had stated the importance of this in his original paper and Bouchiat, Iliopoulos and Meyer took it seriously and noticed that SU(2) and U(1) anomalies would cancel between leptons and quarks for three colours [13].

The next crucial piece of the model to appear was asymptotic freedom [14,15]. I have nothing to add about priority, 't Hooft, and all that. But I will simply say that this was amazing and occupied much of my time for many years after the discovery. It is clear, though, that all the authors assumed (just as I and everyone else did at that time) that in a realistic color SU(3) theory of the strong interactions, the gauge symmetry would somehow have to be broken dynamically to give mass to the gluons.

I will say more about the Pati-Salam idea of lepton number as as a 4th colour [16] in a few minutes, Figure 5. The original papers are very frustrating and difficult to read, mostly because Salam disliked the idea of fractionally charged quarks and he insisted on breaking the color symmetry, mixing with the SU(2)s to produce integrally charged quarks. Even back in 1972, they should have known that this was not consistent with what was observed in deep inelastic scattering, for example. But if you just don't break the gauge symmetry, the model shows how beautifully the quarks and the leptons fit together. If they hadn't done this crazy symmetry breaking, I think this would have been the first model with electric charge quantized in the right way with fractionally charged quarks.

$$\mathrm{SU}(2)_L \times \mathrm{SU}(2)_R \times \mathrm{SU}(4)$$

$$SU(2)_L \left\{ \overbrace{\begin{pmatrix} u_{rL} & u_{gL} & u_{bL} & \nu_L \\ d_{rL} & d_{gL} & d_{bL} & e_L^- \end{pmatrix}}^{SU(4)} \right.$$

$$SU(2)_R \left\{ \overbrace{\begin{pmatrix} u_{rR} & u_{gR} & u_{bR} & \nu_R \\ d_{rR} & d_{gR} & d_{bR} & e_R^- \end{pmatrix}}^{SU(4)} \right.$$

Figure 5. Pati-Salam: lepton number as a 4th colour.

The next really important piece was confinement, Figure 6. People had been looking for fractional charges for many decades when quarks were proposed, and while I have not tried to do an exhaustive search, it would astonish me if the idea of quark confinement had not been discussed extensively long before asymptotic freedom.

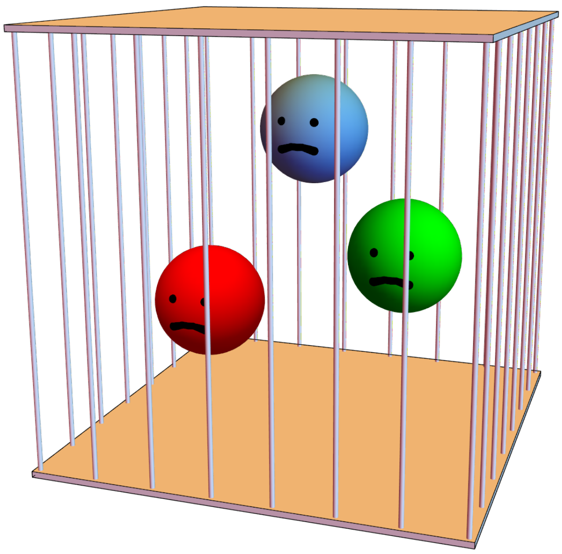


Figure 6. Quarks confinement.

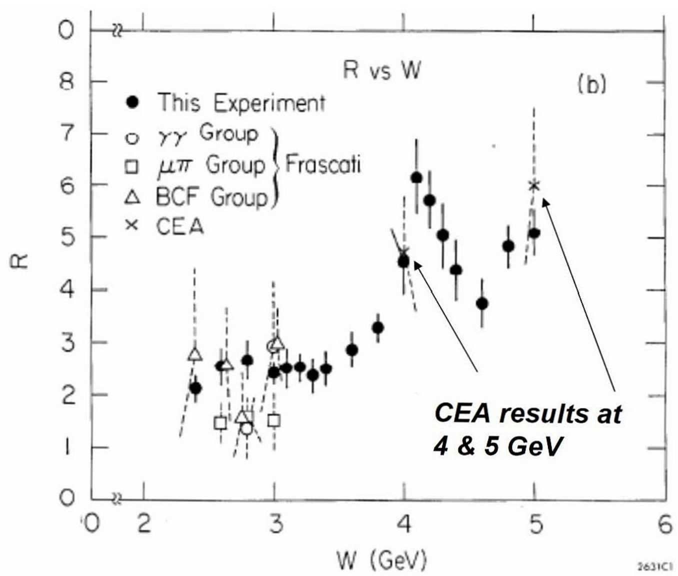
What was really new and revolutionary was the idea that leaving the color gauge symmetry unbroken might kill two birds with one stone by confining both quarks and massless gluons so that no massless particles appear as physical states, but fractionally charged quarks could still look like real particles inside hadrons, making sense of Feynman's parton picture. As far as I know, the first mention of this in the published literature is by Weinberg [17] but I think that several other people came to similar conclusions at about the same time.

Confinement allowed Glashow and me to think about unifying SU(3) with SU(2) cross U(1). It was obvious how to do this if you thought properly about Pati-Salam and I quickly constructed first the SO(10) theory [18] and then SU(5) [19]. One of my few regrets is that I did not discuss SO(10) in a footnote of the SU(5) paper, although I gave talks about it. This allowed Fritsch and Minkowski to find it later independently [20]. We didn't know quite what to do with the coupling constants but Glashow realized that we could suppress proton decay adequately with superstrong breaking at a huge scale. The gorgeous fit of the quarks and leptons and gauge bosons into these unified groups helped to make the Standard Model gauge group look more reasonable.

The large mass scale and large renormalizations in GUTs forced us to think more carefully about the renormalization group in the presence of broken symmetry. A few months after SU(5), Helen Quinn, Steve Weinberg and I [21] understood that the only practical way to deal with this was effective field theory - to calculate in the broken theory ignoring the heavy particles and to put the symmetry in as a boundary condition at the high scale. GUTs forced us into the first full effective field theory calculation in which the process of renormalization was taken apart into the two different steps of matching couplings at the boundaries between different effective theories and running in the effective theory between the boundaries.

1974 was an absolutely crazy time because there was so much going on all over the world trying to turn the prescient guess about unbroken asymptotically free color SU(3) into testable predictions. Deep-inelastic electron and neutrino scattering and e^+e^- annihilation were two of the primary pressure points where theory and experiment could interact. In deep inelastic scattering, it looked like the renormalization group improved parton model was working beautifully. This helped to confirm the SU(2) $\times$ U(1) model and pin down the weak mixing angle. But for many of us, the reverse logic was equally important. By this time, we were starting to believe in SU(2) $\times$ U(1), so the success of the parton model description definitely helped convince us that color and quarks were real and that we could calculate.

But the success of the parton model in deep inelastic scattering just heightened the puzzle of R - the ratio of hadrons to muons - in e^+e^-, Figure 7. Very soon after asymptotic freedom Appelquist and I [22] and independently Zee [23] had done the rather simple calculation of the color radiative corrections to the free-quark result for R in the short distance limit. This gives a modest enhancement - but is not even close to explaining the data. It was becoming clear that something more was going on—and it was becoming obvious to us that it was charm.



SLAC-PUB-1520, LBL-3621; January 1975

Figure 7. The puzzle of R, the ratio of hadrons to muons in e^+e^- collisions.

The week before the ψ was discovered at SPEAR, Burt Richter was at Harvard as a Loeb lecturer, giving talks on his theory that the reason for the apparently steadily rising R was that the electron was a hadron a small part of the time and we were seeing the constant cross-section of the hadronic component. We had a fancy lunch in the department library to celebrate his lectures, and there Appelquist and Politzer suggested that he look for narrow states. He did not appear to take this suggestion seriously.

Shortly after Richter's visit, the November Revolution changed the particle physics world dramatically [24,25]. We have heard about this from Sam, but let me just remind you that of the 8 theory papers that appeared in Physical

Review Letters in the first issue devoted to theory after the discovery, most were totally nutty, including several by brilliant physicists who should have known better. I will just briefly show the abstracts:

Are the New Particles Baryon-Antibaryon Nuclei? - Alfred S. Goldhaber and Maurice Goldhaber - Baryon-antibaryon bound states and resonances could account for the new particles, as well as narrow states near nucleon-antinucleon threshold, which were reported earlier.

These are terrific physicists but energy scales are all wrong.

Interpretation of a Narrow Resonance in e^+e^- Annihilation - Julian Schwinger - A previously published unified theory of electromagnetic and weak interactions proposed a mixing between two types of unit-spin mesons, one of which would have precisely the characteristics of the newly discovered neutral resonance at 3.1 GeV. With this interpretation, a substantial fraction of the small hadronic decay rate can be accounted for. It is also remarked that other long-lived particles should exist in order to complete the analogy with ρ^0, ω, and ϕ.

Ditto

Possible Explanation of the New Resonance in e^+e^- Annihilation—S. Borchardt, V. S. Mathur, and S. Okubo—We propose that the recently discovered resonance in e^+e^- annihilation is a member of the 15$\oplus$1 dimensional representation of the SU(4) group. This hypothesis is consistent with the various experimental features reported for the resonance. In addition, we make a prediction for the masses of the charmed vector mesons belonging to the same representation.

Mentions charm but completely misses the point. SU(4) is useless except for keeping track of the charm quantum number

Model with Three Charmed Quarks—R. Michael Barnett—The spectroscopy and weak couplings of a quark model with three charmed quarks are discussed in the context of recent results from Brookhaven National Laboratory, Stanford Linear Accelerator Center, and Fermi National Accelerator Laboratory.

I have no idea what he was thinking.

Possible Interactions of the J Particle—H. T. Nieh, Tai Tsun Wu, and Chen Ning Yang—We discuss some possible interaction schemes for the newly discovered particle J and their experimental implications, as well as the possible existence of two J^0s like the $K_S - K_L$ case. Of particular interest is the case where the J particle has strong interactions with the hadrons. In this case J can be produced by associated production in hadron-hadron collisions and also singly in relative abundance in ep and μp collisions.

Again great physicists, really confused.

Is Bound Charm Found? - A. De Rújula and S. L. Glashow - We argue that the newly discovered narrow resonance at 3.1 GeV is a $^3S_1$ bound state of charmed quarks and we show the consistency of this interpretation with known meson systematics. The crucial test of this notion is the existence of charmed hadrons near 2 GeV.

Exactly right.

Remarks on the New Resonances at 3.1 and 3.7 GeV - C. G. Callan, R. L. Kingsley, S. B. Treiman, F. Wilczek, and A. Zee - This is a collection of comments which may be useful in the search for an understanding of the recently discovered narrow resonances at 3.1 and 3.7 GeV.

They should have been brave and committed to charm!

Heavy Quarks and e^+e^- annihilation—Thomas Appelquist and H. David Politzer—The effects of new, heavy quarks are examined in a colored quark-gluon model. The e^+e^- total cross section scales for energies far above any quark mass. However, it is much greater than the scaling prediction in a domain about the nominal two-heavy-quark threshold, despite e^+e^- being a weak-coupling problem above 2 GeV. We expect spikes at the low end of this domain and a broad enhancement at the upper end.

Brilliant prediction—sadly submitted too late! If they had submitted this a few weeks earlier it might have been a Nobel Prize.

The confusions here by great physicists are an indication of just how revolutionary the idea of charmonium was at the time. It seems obvious now, but it certainly wasn't then.

The discovery of charmonium should have convinced everyone. But the month after the discovery I attended a conference at the University of Miami and I was astonished to find that most people we met at the conference were not convinced, and that many were convinced that the charm explanation was ruled out because charmed particles had not been seen. This situation changed surprisingly slowly over the next year and a half. I think the hang-up was that many physicists could not wrap their heads around what a huge difference the large mass of the c quark would make for the properties of charmed mesons and baryons. Consider, for example, this entry from the stable particle section of the 1974 particle data book, Figure 8.

Stable Particle Table *(cont'd)*

Particle	$I^G(J^P)C_n$	Mass (MeV) Mass^2 (GeV)^2	Mean life (sec) $c\tau$ (cm)	Partial decay mode: Mode	Fraction[a]	p or p_{max}[b] (MeV/c)
K^0	$\frac{1}{2}(0^-)$	497.70 ±0.13 S=1.1*	50% K_{Short}, 50% K_{Long}			
K^0_S	$\frac{1}{2}(0^-)$	m^2=0.248	0.886×10^{-10} ±.007 S=2.4* $c\tau$=2.66	$\pi^+\pi^-$	(68.77±0.26)% S=1.1*	206
				$\pi^0\pi^0$	(31.23±0.26)% S=1.1*	209
				$\mu^+\mu^-$	(< 0.3) 10^{-6}	225
				e^+e^-	(<35) 10^{-5}	249
				$\pi^+\pi^-\gamma$	c(2.0 ±0.4) 10^{-3}	206
				$\gamma\gamma$	(< 0.4) 10^{-3}	249
K^0_L	$\frac{1}{2}(0^-)$		5.179×10^{-8} ±0.040 $c\tau$=1553	$\pi^0\pi^0\pi^0$	(21.3 ±0.6)% S=1.1*	139
				$\pi^+\pi^-\pi^0$	(11.9 ±0.4)% S=2.2*	133
				$\pi\mu\nu$	(27.5 ±0.5)% S=1.1*	216
				$\pi e\nu$	(39.0 ±0.6)% S=1.1*	229
				$\pi e\nu\gamma$	c(1.3 ±0.8)%	229
	$m_{K_L}-m_{K_S}$ = $0.5403\times10^{10}\,\hbar\,\mathrm{sec}^{-1}$ ±0.0035			$\pi^+\pi^-$	(0.177±0.018)% S=4.9*	206
				$\pi^0\pi^0$	(0.093±0.019)% S=1.5*	209
				$\pi^+\pi^-\gamma$	c(< 0.4) 10^{-3}	206
				$\pi^0\gamma\gamma$	(< 2.4) 10^{-4}	231
				$\gamma\gamma$	(4.9± 0.4) 10^{-4}	249
				$e\mu$	(< 1.6) 10^{-9}	238
				$\mu^+\mu^-$	i(< 1.6) 10^{-8}	225
				e^+e^-	(< 1.6) 10^{-9}	249
				$e^+e^-\gamma$	(< 2.8) 10^{-5}	249

Figure 8. Stable Particle Table in 1974 Particle Data Group.

The corresponding table for D^0 today runs to over 10 pages. The particle physics world had really changed!

Meanwhile, from 1974 to 1978, the development of the details of the Standard Model continued at a rapid pace. Wilson constructed lattice gauge theories. Kobayashi and Maskawa extended GIM to six quarks and predicted CP violation. SLAC found P-wave charmonium states, silencing many of the skeptics. 't Hooft and Polyakov showed that unified theories have monopole solitons, connecting algebraic charge quantization in unified theories with Dirac's. Witten constructed an effective field theory of low-energy effects of charm. Charmed particle masses were calculated at Cornell and Harvard. Perl discovered the tau lepton. Cornell and CERN groups did systematic charmonium calculations. Goldhaber discovered charmed particles just where they were predicted. 't Hooft showed how instantons solve the chiral U(1) problem. Witten constructed an effective theory of weak interactions. Politzer and Altarelli-Parisi developed the QCD parton model. The Upsilon was discovered at FNAL. Peccei and Quinn discovered their symmetry in a 2-Higgs model, while Gilman and Wise estimated epsilon prime over epsilon in the KM model. In 1978, Prescott and Taylor found parity violation in deep inelastic electron-deuteron scattering, vanquishing the last challenge to the Standard Model from atomic parity violation experiments. Weinberg and Wilczek established the properties of axions in the Peccei-Quinn model. Experimenters at DESY found evidence for gluons in hadronic jets. Dimopoulos and Susskind and Weinberg showed that Technicolor could break the electroweak symmetry dynamically. Weinberg capped a remarkable decade with his Phenomenological Lagrangians paper that established the Effective Chiral Theory.

Thus the 1970s was the decade of the Standard Model. This decade which began with quantum field theory in disarray ended with a practical set of QFT tools for calculations for strong interactions at high and low energies and for electroweak interactions at all energies. After the Prescott-Taylor experiment, the gauge symmetry of the Standard Model was firmly established and the focus shifted to the physics of the symmetry breaking and to radiative corrections. In the 1990s, the t quark was discovered at Fermilab and LEP pinned down the properties of the Z and this left very little wiggle room for symmetry breaking by any mechanism other than the vacuum expectation value of a scalar Higgs particle. Nevertheless, until the Higgs was discovered in 2012, I was hoping against hope that we had missed something and that something very different would appear. Even though it appeared where it had to appear based on previous data and radiative corrections, its existence still amazes me. It remains unique - the one example of its kind. Since it stubbornly refuses to disappear, I very much hope that we can find something about it that suggests where to look next.

The current status of the Standard Model can be summed up in this wonderful cartoon by Randall Munroe about unexpected experimental results, Figure 9.

Our hero suggests betting that any interesting result is wrong! Of course I am not being totally serious here. I hope that the g-2 anomaly, for example, holds up. That would be a problem for the Standard Model. And problems is what we need! We have to flesh out the neutrino sector. Maybe that will generate problems for us. But at the moment we have lots of puzzles but no problems. We have issues like dark matter that may be a problem for particle physics, but may be something else entirely. We have no problems like the inconsistencies in particle physics itself that led to the Standard Model. I had hoped to give some kind of an overview of the approaches to our puzzles. But it turns out that this is Impossible.

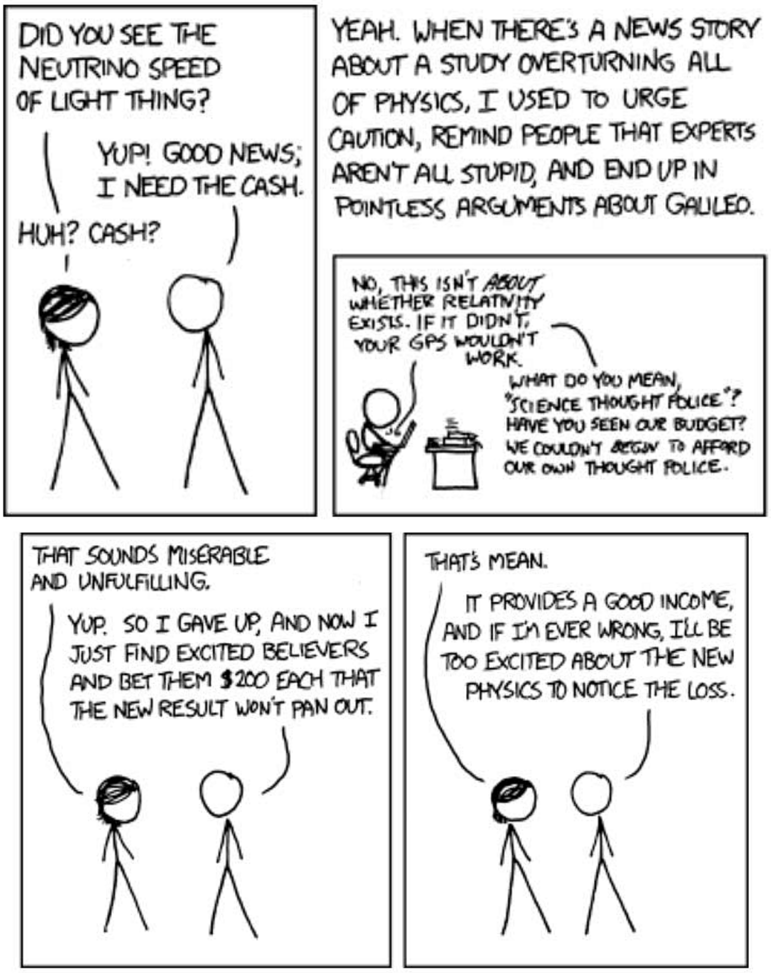


Figure 9. Betting that any interesting new physics result is wrong!

I used Inspire to count the number of papers that mention the Standard Model as a function of time, Figure 10. While there is a kink in the exponential growth after the 1980s, we are still in a period of doubling every 10 years. There is just too much to even think about, let alone summarize. Because I am at Harvard I will just mention one currently popular approach to our puzzles.

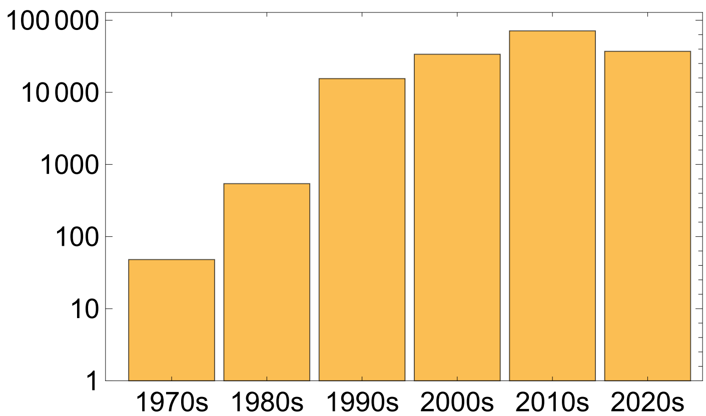


Figure 10. Number of papers mentioning the Standard Model as a function of time.

The Swampland conjectures [26], as I understand them which is not very well, are a set of constraints on effective field theories motivated by string theory and quantum gravity.

The swamplanders draw amusing pictures. Only if a theory satisfies their conjectures, we are asked to believe, will it poke out of the mess of the swamp onto the high land of the string landscape.

They claim to be able to draw testable conclusions for example about neutrino masses. I think the idea is that a theory needs enough light degrees of freedom or it will be pulled into the swamp. This does not seem too restrictive to me. I assume that if neutrinos are Majorana, or too heavy, they will just tell us that there are additional light particles that we haven't seen. The swampland conjectures reminded me of the time back in the early '90s when my late friend Nathan Isgur used to talk about the "brown muck of low energy hadron physics". Though it has very little to do with my talk, I can't resist showing a drawing done by the multitalented Michael Peskin for a Heavy Quark meeting, Figure 11.

Figure 11. H. Georgi escapes from hadron dynamics (Original drawing by Michael Peskin).

Now that is a "swamp"! Let me close by briefly discussing one puzzle that I think has been solved by my grand-student Matt Schwartz and his students [27]. They find that the universe will last for at least 10^{65} years. So at least we will have plenty of time to solve the other puzzles of the Standard Model. Personally, I plan to spend my time in the swamp.

Conflicts of Interest

The author declares no conflict of interest.

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