Fifty years of research at CERN, from past to future: Theory

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Abstract
A *personal* account is given of how work at CERN-TH has evolved during the past 50 years, together with some *personal* thoughts on how it may develop in the near future. No serious attempt is made towards achieving completeness, objectivity, or historical precision. For a more systematic — and somewhat complementary — study of CERN-TH (at age 40), the account by J. Iliopoulos is highly recommended.

1 The past

The past 50 years were ones of remarkable achievements in our understanding of nature at its deepest level and, in particular, a truly revolutionary epoch in theoretical physics. It was, in many respects, a two-fold revolution:

– in our understanding of the physical world, and
– in our research tools and work style.

I shall discuss these two aspects in turn.

1.1 Revolutions in our understanding

In terms of novelty, this half-century was second only to its predecessor, the one that shook the world of physics at the beginning of the last century through two profound conceptual revolutions: Quantum Mechanics and Special Relativity.

These revolutions came, almost simultaneously (as exemplified by two of Einstein's famous *annus mirabilis* papers), as two *conceptually unrelated* developments. For the high-energy theorist at CERN — and elsewhere — much of what has happened since had to do with finding a consistent framework where these two new paradigms could happily coexist . . . and that can possibly explain the data. A combination of ingenious experiments (see Daniel Treille's talk), technical developments (accelerators, computing, see talks given by Kurt Hübner and David Williams), and theoretically sound ideas, made all this an incredibly successful story.

I shall try to retrace the developments of our understanding of elementary particles and fundamental forces as seen through the eyes of a theorist at CERN[1]. The world of high-energy physics, and particularly of theory, has no geographical or political barriers. This makes it impossible to isolate CERN-TH activities from what happened world-wide. For this reason I shall occasionally mention developments and episodes that took place elsewhere.

I was lucky enough to be active — and at the beginning of my career — by the mid-1960s, the start of a 'golden decade' during which our theoretical ideas truly underwent a 'phase transition'.

I shall insert this crucial decade inside a broader picture and also mention other remarkable theoretical (if not yet experimental) revolutions that took place in the more recent past. I shall spend comparatively little time on that recent history, since on the one hand it is better known to most of you and,

[1]Actually, I joined CERN only in 1976. But, even during the previous decade, I visited TH at regular intervals in or around the summer (basically every year from 1967 to 1974). This allowed me to take the 'temperature' of CERN-TH at regular intervals and to feel its evolution from year to year.

on the other, it has not survived (yet!) the test of time. I shall divide this part of my account into four periods:

– 1900–1954: B.C. (meaning 'Before CERN');
– 1954–1974: Genesis of the Standard Model;
– 1974–1984: Consolidation of the Standard Model and first steps beyond it;
– 1984–2004: Leaps forward.

I shall then come to other aspects of the way CERN-TH has evolved (tools, style) and conclude with some thoughts — and worries — about where we may be heading.

In Fig.1 I have tried to represent how our understanding of the four known basic interactions (electromagnetic, weak, strong, gravitational) has evolved with time. A characteristic feature of this sketch is that the horizontal dividing lines have been progressively removed. As early as the 19th century, Maxwell erased the separation between electricity and magnetism. In spite of appearances, electric and magnetic phenomena are different manifestations of one and the same physical principle as seen in different reference frames. This 'theory merging' process continued in the 20th century, partly because of the theorists' quest for simplicity, but even more so under the push of experimental discoveries. Forces whose macroscopic manifestation is completely different (strong and weak, for instance) turned out to be described, at the microscopic level, by very similar theoretical concepts, those at the basis of gauge field theories.

At the beginning of the 20th century only the two long-range forces, electromagnetism and gravity, were known. Later on, the weak and strong forces, both characterized by their short range, were discovered. In spite of the above range-wise distinction, theoretical progress has proceeded independently, during many decades, for gravity and for the other three forces. For this reason, I shall start my journey by 'getting rid' of the bottom part of Fig. 1, only to come back to it later.

1.1.1 Gravitation and cosmology (1915–1974)

Ten years after introducing special relativity, Einstein, starting from the Galilean universality of free fall, proposed his theory of General Relativity, in which gravity is seen as a manifestation of space–time curvature. At the beginning his proposal caused much scepticism. But just a few years later some striking tests (deflection of light, precession of Mercury's perihelion) convinced the physics community.

Not much later, the first cosmological applications of General Relativity were made by Alexandre Friedmann (1922); these had their confirmation with the discovery, in 1929, of the red shift and Hubble's law and with their interpretation in terms of an expanding Universe. This eventually led (1935) to the standard (Friedmann–Lemaître–Robertson–Walker) hot big bang model, itself confirmed by the discovery of the cosmic microwave background and applied successfully to the production of light elements (big bang nucleosynthesis).

During all those years, in spite of its mathematical beauty and experimental successes, General Relativity attracted very little interest, if any, inside the high-energy physics community and at CERN-TH: after all, everybody knew that gravity is irrelevant for elementary-particle physics! Furthermore, not much effort was made to go beyond the classical theory: for the macroscopic world it was aiming at, classical General Relativity looked entirely sufficient and satisfactory.

1.1.2 B.C.: Before CERN! (1900–1954)

These were very important years for the advancement of theoretical and experimental physics. On the one hand, quantum mechanics was formulated in precise mathematical terms by Heisenberg, Schroedinger, Dirac and others (even if its interpretation remained puzzling[2]), and on the other, the weak and strong

[2]The most significant breakthrough in this area was the work done by John Bell at CERN (1964–1966), the celebrated Bell inequalities, which, after the experiments were carried out, would have probably given CERN-TH its first Nobel prize.

interactions were discovered and their theory started to be developed.

It is interesting that the theories of the latter two interactions, in spite of their vastly different phenomenology, have followed parallel histories from the beginning till they found their final place within the Standard Model. Indeed, Fermi's theory of the weak interactions dates back to 1934, while Yukawa's model of the strong interactions, with his postulate of the existence of a meson, later called the pion, came just one year later.

Right after World War II (Shelter Island, June 1947), Quantum Field Theory (QFT) had its first realization with QED, the first example of a quantum-relativistic theory with precise predictions. Its striking successes in explaining the Lamb shift and $(g-2)_e$ swiftly followed and consecrated QED as *the* quantum theory of electromagnetism.

1.1.3 1954–1974: the golden decades

These were the years during which the Standard Model of particle physics was conceived and developed. But, before theorists gave their full confidence to QFT once again, they had a moment of crisis, starting within QED itself.

Two problems indeed mitigated the enthusiasm generated by QED's smashing successes:

- The apparent divergence of perturbation theory. But, even more,
- the renormalization group evolution of the fine-structure constant and the associated so-called zero-charge problem. L. Landau has been quoted as saying, in 1955: "The Lagrangian is dead. It should be buried, with all due honours of course."

These problems, together with those encountered in providing a full quantum version of either the Fermi or Yukawa theory, caused a certain uneasiness with QFT within the HEP community: S-matrix theory gained in popularity, particularly for strong interactions under the flag of G. Chew's bootstrap program. Within that S-matrix climate, new ideas on how to handle the strong interaction sprang out.

However, before discussing those, let us blow up the central part of Fig. 1 (as shown in Fig. 2) in order to summarize the highly non-trivial 'road map' that, through many false starts and partially successful attempts, eventually led to the Standard Model as we know it today.

Again, developments in the weak and strong interactions went hand in hand. In the weak interactions the discoveries of strange particles (1953–1955) and of parity non-conservation (1956) paved the way to the first improvement of Fermi's theory, based on the current–current (JJ) interaction. The two currents were soon identified (1957) as being just a precise combination of vector and axial-vector currents, leading to maximal parity violation (only one helicity carrying the weak interaction). The vector and axial-vector currents were understood to be conserved (CVC, 1958) and partially conserved (PCAC, 1961), respectively, and to be 'rotated', in what we now call flavour space, by the Cabibbo angle (a CERN-TH paper, 1963).

Interestingly, the hadronic pieces of these same currents were also playing an important role in strong interactions. They were at the basis of the isospin and (flavour) SU(3) symmetries that successfully described the hadronic spectra (see Gell-Mann–Ne'eman's 'eightfold way', 1961). The hypothesis of spontaneous breaking of chiral symmetry led, through PCAC, to a successful theory of the masses and interactions of the pions (and of other light pseudoscalars) as pseudo-Nambu–Goldstone bosons. These hadronic currents satisfied the so-called current algebra, and theorists got busy trying to 'saturate' current algebra relations through families of hadronic states (see the program of S. Fubini and collaborators). The growing number of (almost daily!) discovered resonances got neatly classified through T. Regge's theory (1959), which, thanks to the work of G. Chew and S. Mandelstam, became a theory of high-energy scattering. The description of multiparticle production at high energy through the multiperipheral model (Amati–Fubini–Stanghellini, early 1960s) was another important achievement in that same direction.

In the next decade (1964–1974) both areas blossomed. In the weak interactions we may recall the discovery of CP violation (1964) and the seminal papers of Glashow, Salam and Weinberg which did not receive a lot of attention at the time. The important Bell–Jackiw anomaly paper came out of CERN-TH in 1969, while the GIM mechanism (1970) was successfully confirmed by the J/Ψ discovery in 1974. Meanwhile, in 1971, renormalizabilty of the GSW theory had been proved by Gerard 't Hooft, and this quickly led to the establishment of the present electroweak theory.

Let me briefly recall how I lived that crucial moment, as a young post-doc at MIT, through a visit of 't Hooft and a lunch seminar by Steven Weinberg. Weinberg had just joined the MIT faculty in the new Center for Theoretical Physics led by V. Weisskopf. Before 't Hooft's paper on the renormalizability of spontaneously broken gauge theories, Weinberg would rarely talk about his 1967 model. He was working mainly on chiral Lagrangians and even got interested in the dual-resonance model (see a paper written with M. Ademollo and myself). When 't Hooft's article appeared, Steve immediately grasped its importance, invited Gerard to MIT for a seminar (and tough private discussions), and then he himself gave one of the traditional lunch (journal club) seminars. After filling the blackboard with the electroweak Lagrangian, he added something like: "This is not a particularly pretty Lagrangian, neither is it clear whether it has anything to do with the real world, but it seems to define a consistent quantum field theory of the electroweak interactions from which definite predictions can be drawn". It marked the beginning of a new era in particle physics, that of electroweak unification.

Parallel striking developments were going on in the strong-interaction domain. Current algebra and flavour symmetries were nicely described, mathematically, in terms of quark fields. Quarks could also explain some puzzle with Fermi statistics, at the price of attributing to them, besides the usual 'flavour' label, one called 'colour'. Gradually, these mathematical entities acquired respectability as truly important degrees of freedom in hadronic physics, and colour was transformed from a book-keeping device into a dynamical concept, the source of a field that is responsible for the unusual behaviour of quarks. Indeed, processes like $e^+ e^- \to$ hadrons (at Cambridge's electron accelerator), of deep-inelastic lepton–hadron scattering (at SLAC), and of hard hadron–hadron scattering (at CERN's ISR), were all showing that quarks behaved as almost free particles, and yet nobody, in spite of many searches, had ever seen a free quark!

Before I go more into that story let me mention that important developments also occurred in the S-matrix approach to strong interactions, such as those that gave, out of the CA programme and Regge theory, superconvergence (1966), finite-energy sum rules, duality (1967), and, eventually, dual-resonance models and string theory.

I can add a couple of personal stories on that (strong-interaction) side. In the summer of 1967, I was attending the Erice summer school: there were plenty of interesting talks but two sentences by M. Gell-Mann particularly stuck in my mind:

– "Nature only reads books in free field theory";
– Thanks to the newly discovered Dolen–Horn–Schmit duality, the possibility of a 'cheap bootstrap' program (as opposed to Chew's expensive one!) had emerged.

From the autumn of 1967 to the summer of 1968 a Harvard (M. Ademollo)–Weizmann Institute (H. Rubinstein, M. Virasoro and myself) collaboration applied the 'cheap bootstrap' idea to the reaction $\pi\pi \to \pi\omega$, with very encouraging results. In the summer of 1968, while at CERN on the way to my first post-doc at MIT, I completed a paper I had begun just before leaving Israel. After discussing with several people at CERN-TH[3] I submitted it to Nuovo Cimento. By the time of the Vienna Conference (August–September) it had already received much attention and was soon named the dual-resonance model: no one knew it at the time, but string theory was born!

[3] I wish to stress here how important CERN-TH was, and still is, as a reference point, particularly for European theorists: a place where they can find experts in all branches of high-energy theory, check their ideas, learn new things, and get inspiration. This, of course, besides its role in supporting and guiding CERN's experimental programme.

However, this development had a hard time being accepted by the establishment: it was neither quantum field theory nor a pure S-matrix bootstrap à la Chew. A sentence by Fubini (around 1970) expressed well the feelings of the day: "A piece of 21st century physics that fell accidentally in this century". A couple of years later, the string reinterpretation of the dual-resonance model did not make its magic properties less mysterious, but at least made the whole game more 'respectable'. Even then, the community's attitude towards it was very polarized: love it or hate it!

A third, crucial episode came for me at a seminar at CERN-TH by Sid Coleman in 1973. I remember its title very well:

The price of asymptotic freedom

and its abstract:

Non-Abelian gauge theories.

One finally did have an answer to Gell-Mann's puzzle: Nature is reading books in *asymptotically* free quantum field theory! At the same time, non-Abelian gauge theories could explain the absence of free quarks by the highly non-trivial behaviour of the strong force at large distances and could solve Landau's zero-charge problem. Another pillar had been added to our understanding of elementary particles and their non-gravitational interactions, and the Standard Model was now complete!

What about string theory? Well, it had to concede defeat. Yet, for many of us, it was difficult to give up such an elegant construction. I still felt that there was something deep and unique in the way string theory was reorganizing perturbation theory diagrams, something very unlike what any normal quantum field theory does. I kept working on a 'planar bootstrap' and a topological expansion. Soon after, however, QCD naturally provided its own way of achieving all that. This came from 't Hooft's 1974 CERN paper, where he showed that, in a suitable large-N limit, QCD diagrams get reorganized precisely as to mimic a string theory! That was enough for me (and, I guess, for most string theorists of the time). Those interested in strong interactions (sadly?) turned a page and switched to working on QCD. In just a few years, quantum field theory had made a spectacular comeback and it was the turn of S-matrix theory to fall in to disgrace.

Meanwhile, in a 1974 paper that went practically unnoticed, J. Sherk and J. Schwarz proposed reinterpreting string theory as a theory of quantum gravity ...

1.1.4 1974–1984: Standard Model confirmations and attempts to go beyond

The following decade can be characterized as a period of consolidation and experimental verification of the theoretical ideas that had just blossomed.

On the experimental side (see Daniel Treille's talk for details) let me recall, besides the already mentioned J/Ψ discovery, two of CERN's most glorious achievements, the discovery of neutral currents and that of the W and Z bosons.

On the theoretical side, Cabibbo's theory was extended to the CKM matrix, naturally incorporating a CP-violating parameter in the Standard Model. In 1974 Ken Wilson proposed lattice gauge theory, and not much later the first numerical proofs of confinement in QCD came about. Perturbative QCD was greatly developed justifying a QCD-corrected version of Feynman's parton model; quark and gluon jets, as well as their shapes, were predicted and found. Instantons were discovered, offering an elegant solution to the $U(1)$ problem, while resurrecting the one of a strong CP violation (for which a solution within the Standard Model is still lacking). Various expansions of the $1/N$ kind were proposed, which bridged different approaches to hard and soft strong-interaction physics.

This decade also witnessed the first bold attempts to go beyond the Standard Model with the first Grand Unified Theories (GUTs), the prediction of a long but finite proton lifetime, and the unsuccessful search for its decay in non-accelerator experiments. Supersymmetry (SUSY) was invented and developed[4]. It was then generalized into a new theory of gravity, called supergravity, with the hope, soon dashed, that some suitable version of it (e.g., $D = 11$ supergravity) could be consistently quantized. Theory was quickly moving to energy scales (GUT scale, gravity's Planck scale) never considered before to be of relevance to particle physics.

1.1.5 1984–2004: the leap forward

Another personal story. In the autumn of 1984, Tom Banks arrived from the US for a short visit to CERN. As soon as we met he asked: "Have you heard about SO(32)?" I probably answered: "What do you mean?" A few days later he gave a talk about a recent paper by M. Green and J. Schwarz which provided the first example of an anomaly-free superstring theory with chiral fermions, i.e., of a theory that could in principle contain both the Standard Model of particle physics *and* quantum gravity. It marked the beginning of an exciting decade in theoretical physics that we may characterize as 'The (S)-matrix reloaded'.

The paper by Green and Schwarz became known as the first string revolution; string theory came back into the spotlight as nothing less than a 'Theory of Everything' (TOE). Some beautiful early mathematical developments led to over-optimistic claims that not only would one soon arrive at an experimentally viable TOE, but also that the choice would be uniquely determined by theoretical consistency. As time went by, those hopes started to fade away but, in any case, the conceptual barrier between gravitational and gauge interactions was gone for ever.

Also the attitude towards QFT changed. In the sixties and seventies QFT had been regarded as a complete framework. In the eighties theorists started to view it as an *effective* low-energy theory that cannot be extrapolated above a certain energy scale without an 'ultraviolet completion', string theory being just one example of such a completion. In this optic, Landau's zero-charge problem disappears, provided the ultraviolet cut-off is kept finite. I can recall at this point a brief encounter I had at CERN with A. Sakharov in or around 1987. He had become very interested in string theory (see his Memoirs) and came to ask me about several issues that interested (or bothered?) him. He had a particularly burning question: Is the induced-gravity idea (Sakharov, 1968) borne out in string theory? I gave him a straight answer: No! In string theory, gravity is already present at tree level, loops just renormalize Newton's constant by a finite amount (in Sakharov's induced gravity, instead, the Newton constant is entirely determined by radiative corrections). Years later, after his death, I came back to the issue and concluded that the induced-gravity idea is also possible in string theory and, actually, could be a very interesting option . . .

I shall go quickly to the last decade (1994–2004) during which the so-called second string revolution took place. It brought many new theoretical tools and ideas through the concept of (mem)branes and of new dualities interconnecting different string theories in various regimes. The number of consistent string theories went down to a single one, called M-theory, but, unfortunately, the number of solutions went up by many orders of magnitude, making the dream of uniqueness move further and further away.

These developments, however, reconciled much of the theory community — until then sharply divided into enthusiasts and violent critics — with string theory. The new 'stringy' techniques offered powerful tools for studying gauge theories in various regimes, as wonderfully exemplified by the Seiberg–Witten 1994 solution of $\mathcal{N} = 2$ super-Yang–Mills theory. Not only: these developments paved the way to the possibility that the extra dimensions of space envisaged by string theory may be 'large' or that our own Universe is just a membrane immersed in this higher-dimensional space (brane-world scenarios). In both cases, new physical phenomena may be expected to show up at accessible energies (such as those to be reached at the Large Hadron Collider) and/or at short distances (e.g., in submillimetric deviations

[4]First discovered (in the West) in dual resonance models, it became a bona fide quantum field theory after the work of J. Wess and B. Zumino (CERN, 1974).

from Newton's law).

There have also been quite spectacular achievements in explaining, through string theory, the microscopic origin of Black Hole entropy and in establishing a holographic bridge between gravity and gauge theories. Hope is growing that, one day, we shall finally 'close the circle' by understanding what kind of strings describes the long-distance behaviour of QCD, and how theorists in the sixties and seventies were led to string theory by following an amazing 'red herring'.

Finally, this decade brought the fields of particle physics and of astro-cosmo-particle physics closer than ever before. Fruitful interactions between particle theory and cosmology had actually started much earlier, in particular with theories of baryogenesis based on realizing, within the Standard Model or its extensions, Sakharov's famous three criteria, as well as with the theory of inflation, developed by particle theorists in the eighties. At the same time, experiments in astrophysics and cosmology made a quantum jump in precision and led to striking discoveries, some of which were totally unexpected (dark matter, dark energy, ultra-high-energy cosmic rays).

Non-accelerator experiments gave another striking piece of news: neutrino masses and oscillations discovered in both solar- and atmospheric-neutrino experiments. Although a slight extension of the Standard Model can accommodate these phenomena, and theorists had largely anticipated this possibility, they were caught by surprise as far as the neutrino-mixing pattern appears to be realized. Thus, astrophysical and cosmological data appear to offer the much-sought-after crises that theorists need to make progress (quoting S. Weinberg's "physics thrives in crises").

1.2 Revolutions in our work's tools and style

The tools theorists use have greatly evolved during the last 50 years. In particular:

- *Mathematical tools*
  The level of mathematical sophistication needed in order to take part in (or just to follow) some theoretical developments has reached unprecedented levels. This is particularly true of string theory, an area of research that not only exploits the most advanced mathematics but that has also contributed to its development (see E. Witten's Fields Medal). This is reflected in the mathematical background of the average young theorist today (no comment on his/her average physics background!).
- *Computing tools*
  I still remember the computing facilities I had at my disposal when I did my Master's thesis in Florence in 1965: punching cards to compute a one-dimensional integral on a big IBM machine took a substantial chunk of my time. Nowadays I can do that integral in a fraction of a second on my laptop! Lattice calculations, for instance, could not have been conceived of at that time. They still rely today on the exponential growth of computing power.

As with our tools, our style of research has changed in many respects:

- *Specialization*
  In the early days of my career I could basically follow all that was happening in high-energy theory: there were fewer areas and they were closer to each other, the number of new papers per month was manageable. Today, every field is very specialized and trying to follow what is going on (or even looking at what is new on the archives) requires a big time-consuming effort. Language barriers across different areas of research are often another obstacle and people do not seem to try to make the effort to be understood beyond their inner circle. Perhaps as a result, I find discussions at TH seminars less frequent and interesting than they used to be 30 years ago.
- *Communication, search tools*
  Again a huge revolution has taken place. I remember when, during the Weizmann–Harvard collaboration, we were sending long letters full of calculations over the Atlantic to get an answer, at

best, a couple of weeks later (very exceptionally, we would dare to make a short phone call). Since then, we have learned to take advantage of:

- fax,
- e-mail,
- electronic archives,
- the Web, and its search engines.

Unfortunately all these goodies come with some side-effects:

- *Mathematics*
  Warning to theorists: “Excessive use of mathematics can be dangerous for your health”, i.e., there can be a danger of becoming so addicted to mathematics that one forgets about the original physics goals.
- *Computers*
  There can be a tendency to lean too much on numerical computations, without trying each time to understand what exactly one has put in and what exactly the output means. There is a too widespread attitude to use the computer as an all-encompassing black box.
- *Electronic archives*
  These are very useful indeed. However, they appear to encourage quick writing and even quicker reading of papers. The flood of papers is such that most theorists use an ‘on-line trigger’ (like our experimental friends) to select what to read (or at least to glance at).

2 The future

2.1 New theoretical ideas

The past 30 years have not been stingy in terms of new theoretical ideas. Quite the contrary: theorists have proved their great imagination again and again. The problem, if any, was elsewhere: the lack of experimental checks on whether any of those ideas had much to do with real life! I would say that, in general, the relative status of Theory and Experiments determines to a large extent the way we make progress in physics. We can indeed consider, roughly, three cases:

- *Experiment (much) ahead of Theory*
  Examples are the whole of particle physics of the 1950s and 1960s, with the exception of QED. More recently, the nature and origin of the CKM matrix, astro- and cosmo-particle physics.
  I would characterize this situation as being tough (for theorists), but healthy and challenging.
- *Experiment and Theory at a similar level*
  Examples are QED, HEP in the 1970s and 1980s, the Standard Model facing LEP, HERA, and Tevratron data.
  I would call this situation a perfect fit, although it may become too perfect, and a bit boring, when experiments keep finding no disagreement with theory. Finally:
- *Theory much ahead of Experiment*
  Also here there is no lack of examples: the beginning of General Relativity, much of high-energy theory in the last two decades, with supersymmetry, superstrings and the like.
  I would characterize this case as being easy but dangerous, always for the theorists, of course.

In other words: very precise experimental data mean little without a theory matching that precision, while theoretical research, without the guide of experiments, tends to make random walks. Possibly, while waiting for great news from the LHC, theory should appeal more to astro- and cosmo-particle data where the situation looks much like that of particle physics in the 1950s and 1960s: lots of good data and very little understanding (see dark matter, dark energy, cosmic microwave background, ultra-high-energy cosmic rays, gamma-ray bursts . . .).

2.2 New tools, new styles

Computing power is going to grow, allowing us to tackle problems we have not been able to deal with in the past (e.g., in lattice gauge theories and numerical relativity). Means of communication will also improve offering easier and easier access to information.

But we have to watch the side-effects ... particularly on the youngest generation!

What could be at stake?

– *Free choice of research*
  The job market constrains more and more the choices young people have on the kind of research they want to carry out.
– *Embarking on risky projects*
  The system penalizes projects that go out of the mainstream. Even if one achieves some nice results there, chances are that the community will realize it too late ...
– *Enlarging and deepening one's knowledge*
  The pressure to produce is so high that little time is left for broadening one's culture beyond a very limited range of topics.
– *Interactions between different areas of theory and with experiments*
  As people concentrate more and more on a narrow area, interactions between the various groups of theorists, and with experimentalists, become harder and harder to maintain.

Dangers exist but, if we are aware of them, they can be overcome. Another golden era, I am pretty sure, is just around the corner ...

Bibliography

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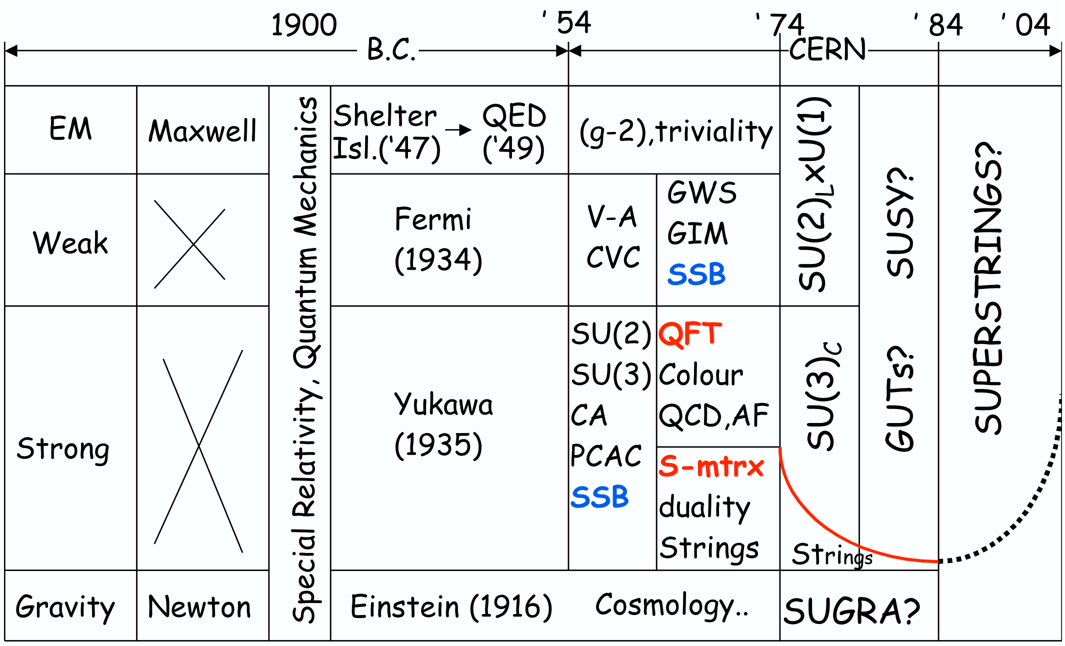
1900
B.C.
' 54
' 74
CERN
' 84
' 04
EM
Maxwell
Special Relativity, Quantum Mechanics
Shelter Isl.('47)
QED ('49)
(g-2),triviality
$SU(2)_L \times U(1)$
SUSY?
SUPERSTRINGS?
Weak
Fermi (1934)
V-A CVC
GWS GIM SSB
Strong
Yukawa (1935)
SU(2) SU(3) CA PCAC SSB
QFT Colour QCD,AF
S-mtrx duality Strings
$SU(3)_c$
GUTs?
Strings
Gravity
Newton
Einstein (1916)
Cosmology..
SUGRA?

Fig. 1

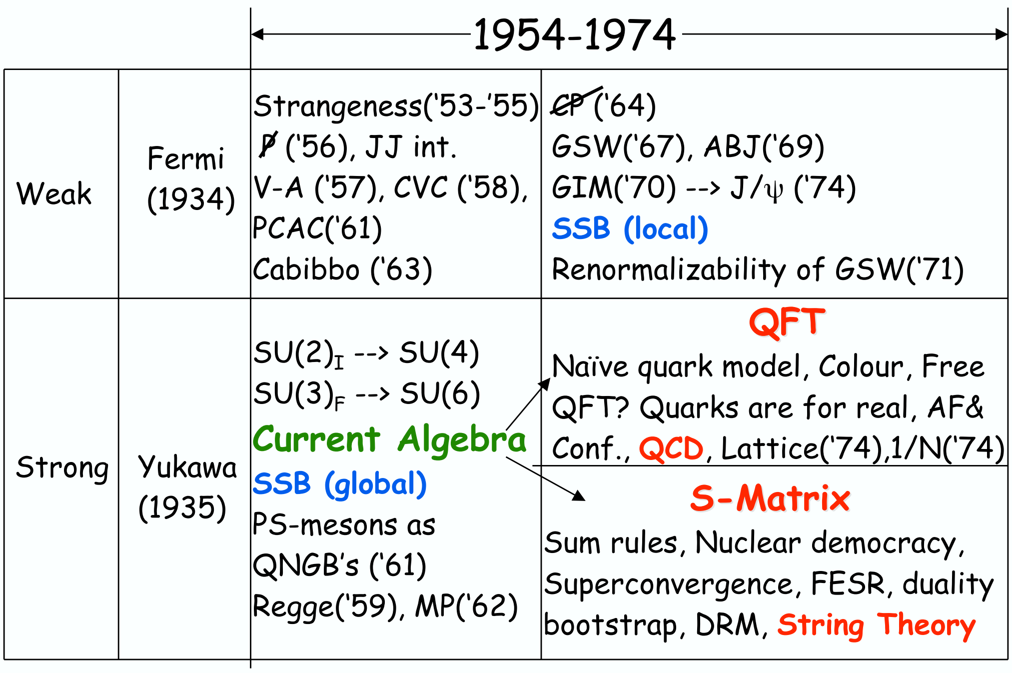
1954-1974
Weak
Fermi (1934)
Strangeness('53-'55)
$\cancel{P}$ ('56), JJ int.
V-A ('57), CVC ('58),
PCAC('61)
Cabibbo ('63)
$\cancel{CP}$ ('64)
GSW('67), ABJ('69)
GIM('70) --> J/ψ ('74)
SSB (local)
Renormalizability of GSW('71)
Strong
Yukawa (1935)
$SU(2)_I$ --> SU(4)
$SU(3)_F$ --> SU(6)
Current Algebra
SSB (global)
PS-mesons as
QNGB's ('61)
Regge('59), MP('62)
QFT
Naïve quark model, Colour, Free
QFT? Quarks are for real, AF&
Conf., QCD, Lattice('74),1/N('74)
S-Matrix
Sum rules, Nuclear democracy,
Superconvergence, FESR, duality
bootstrap, DRM, String Theory

Fig. 2