PROCEEDINGS OF SCIENCE

J/Ψ, fifty years later

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The year 2024 marks the fiftieth anniversary of the J/Ψ discovery[1]. The organisers of the DIS2024 Workshop asked me to give a review talk on the significance of this discovery. This article is an edited version of my talk. I want to argue that it was not just the discovery of a new resonance; we knew already a large number of them. It was not even only the first indirect evidence for the existence of a new quark flavor. It was all that, but it was also much more. It was the final proof which convinced the large majority of our community that we were witnessing a radical change of paradigm in our understanding of microscopic physics. From phenomenological models and specific theories, each one applied to a restricted set of experimental data, we had to think in terms of a fundamental theory of universal validity. For most of us this transition was a revelation, for some others it was a painful experience. It is this story that I will attempt to narrate in this note, although I do not consider myself to be an unbiased observer.

31st International Workshop on Deep Inelastic Scattering (DIS2024)
8–12 April 2024
Grenoble, France

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1. Elementary Particles: The origins

Our field is called “The Physics of Elementary Particles”. Although the discontinuous structure of matter[1] has been the subject of great scientific debates for many years, by the beginning of last century the question was settled[2]. However, the nature of the elementary constituents kept on changing following the increasing resolution power of our microscopes. We thus discovered the chain *molecules* → *atoms* → *nuclei* + *electrons* → *protons* + *neutrons* + *electrons* → *quarks* + *electrons* → ?? We may believe intuitively that there exists such a thing as “an innermost layer", but there is no proof for such belief, and, even less, for the claim that we have already reached these hypothetical “truly elementary” particles. Obviously, this question cannot be addressed independently of that concerning the nature of the second Democritean concept, that of the vacuum, a question related to the microscopic structure of space and time for which we know practically nothing. It follows that, for the moment, the only “definition” we can give is that an elementary particle is an object for which we have not yet been able to detect any internal structure. Although this definition is logically correct, it describes only a small part of the objects we study in our research which include in addition all hadrons and hadronic resonances. So, the practical definition is a cyclic one, namely that an elementary particle is an object which appears as an entry in the “Table of Elementary Particles”.

If I had to assign a year to the birth of modern particle physics I would choose 1950, the discovery of the neutral pion by H.J. Steinberger, W.K.H. Panofsky and J.S. Steller at the Berkeley electron synchrotron[3]. It is the first “elementary” particle discovered with an accelerator (the charged pion was discovered in 1947 in cosmic rays[2]). The existence of π^0 was predicted in 1938 by N. Kemmer[4] who wrote the first isospin invariant theory for the nuclear forces[3]. So, π^0 is the first particle whose existence was predicted by an argument based on an internal symmetry and also the first particle to be discovered in an accelerator. Since that time accelerators became the main engines of discovery in particle physics[5]. The first consequence was the separation of the two communities, particle and cosmic ray physicists[4]. With the use of accelerators the field expanded

[1]The atomic hypothesis is attributed to Democritus of Abdera (c.460 – c.370 BC) for whom the basic constituents of matter are the “atoms" and the “vacuum", i.e. empty space between the atoms.“Νόμω γάρ χροιή, νόμω γλυκύ, νόμω πικρόν, ετεή δ′ άτομα καί κενόν’, which in free translation says that “Laws determine the tone, the sweetness or the bitterness, but everything consists of atoms and empty space".

[2]The first cyclotron was built at Berkeley by E. O. Laurence and his student M. S. Livingstone in 1930. It was a toy accelerator with a diameter of only 4 inches, but in the following years Laurence built in his laboratory a series of larger and larger accelerators reaching a diameter of 184 inches. They were capable of producing pions abundantly but the experimentalists had not yet developed detection techniques suitable to the new environment. It is not enough to acquire a new toy, you must also learn how to play with it. It is also true that Laurence was more interested in building accelerators than in making experiments and did not allocate sufficient resources to detectors.

[3]Kemmer has not received the appropriate recognition for his groundbreaking work. His equations for the pion-nucleon interaction can be found in every textbook, but his name is rarely mentioned.

[4]The last conference which was common to both was held in France, at Bagnères-de-Bigorre, in July 1953. It was organised by the French L. Leprince-Ringuet and the cosmic ray physicists complained claiming that he allocated too much time to the still scarce results coming from accelerators. C.F. Powell, the discoverer of the charged pion and a leading figure in cosmic ray research, said in his closing lecture: “Gentlemen, we have been invaded . . . The accelerators are here”. Note however, that in recent years the two communities often joined forces again with the emergence of a new discipline, the “Astroparticles”.

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very rapidly and the nature of experimental work changed[5]. Dedicated research centers were built – Brookhaven in the USA, CERN in Europe – which were not associated with a particular University. The drive to build larger and more complex detectors, and the rate with which data were collected, led to the creation of large international collaborations. We entered the era of "Big Science". A measure of this exponential expansion is given by the number of elementary particles. In 1940 we knew the proton and the neutron, the electron and the neutrino (not yet detected), the photon and the particle which was believed to be the Yukawa meson (it was in fact the muon). In 1960 the Table of Elementary Particles had several dozen entries and that of today several hundred, although we know that very few among them are "elementary".

If experimental particle physics followed a monotonically rising trajectory, that of its theoretical counterpart was more circuitous. Modern theoretical physics has a precise date of birth: June 2 1947, the date of the Shelter Island Conference, the first postwar scientific meeting sponsored by the National Academy of Sciences on the Foundations of Quantum Mechanics. It was held at Long Island's Shelter Island at Ram's Head Inn and gathered 24 participants around J.R. Oppenheimer. With the aura of the Manhattan Project, he was considered as the founding father of the American School of theoretical physics. Regarding the Conference, Oppenheimer later declared it was the most successful scientific meeting he had ever attended. Indeed, in retrospect Shelter Island is a landmark of theoretical physics. The most important contributions which were presented in that conference were not theoretical breakthroughs but two experimental results: the non-zero values of the Lamb shift and of the electron anomalous magnetic moment. Both these results were important because, for the first time, they showed, beyond any possible doubt, that the predictions based on the Dirac equation for the electron are only approximately correct. This in turn motivated a serious study of quantum field theory. As S. Weinberg has put it[6] *The great thing accomplished by the discovery of the Lamb shift was not so much that it forced us to change our physical theories, as that it forced us to take them seriously.* Indeed, the formalism of quantum field theory existed already for both bosons and fermions. But the enormous prestige of the Dirac theory on the one hand, and the absence of a clear physical motivation on the other, discouraged theorists to face seriously the problem of the divergences of perturbation theory. The fact that the theoretical ideas existed already in a subconscious form is witnessed by the fact that it took practically no time to develop them. The first estimation of the Lamb shift in a non-relativistic approximation was done by H.A. Bethe in the train which brought him back from New York to Ithaca. In the following months R.P. Feynman and J.S. Schwinger, independently, using apparently different formulations, set up the program for the renormalised perturbation expansion of Quantum Electrodynamics and Schwinger gave the first calculation of $g-2$. As it turned out, similar results were obtained independently in Japan by Sin-Itiro Tomonaga, who obtained also the first complete calculation of the Lamb shift. The equivalence of all these approaches was formally shown by F. Dyson in 1948. Rare are the examples in physics in which so much progress was accomplished in such a short time.

The Shelter Island conference was meant to be the first in a series on the same subject. Indeed a follow up conference was organised in Pocono (Pennsylvania) in 1948 and a third one in Oldstone (New York) in 1949. After that the programme of the Foundations of Quantum Mechanics was

[5]We should also acknowledge the fact that this rapid expansion was to a certain extent due to the success of the Manhattan project during the war.

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declared complete. Today we know that this was a rather optimistic view, but at the time it seemed to be justified. The results obtained by applying to quantum electrodynamics the newly established principles of renormalised perturbation theory were in spectacular agreement with experiment.

The first hint that everything was not perfect came from a seemingly mathematical problem. With the exception of some very simple toy models, we have no exact solutions in quantum field theories. Every result is obtained in the form of a formal power series in a parameter, known as the coupling constant, whose value reflects the strength of the interaction. For QED it is the fine structure constant $\alpha \approx 1/137$. The theory of renormalisation guarantees that every term in this expansion is calculable but, in practice, the actual calculations are so complicated that, even today, only very few terms have been computed. However, since $\alpha << 1$, the higher order terms are believed to give negligible contributions. Nevertheless, from the mathematical point of view, there remains the obvious question concerning the convergence properties of the expansion. Dyson addressed this question for QED in 1949. In the prevailing euphoria, he was convinced he would prove that the series was convergent[6], however pretty soon he found a very simple argument showing that this was not true; the perturbation series diverges[7]. This was certainly a drawback for a mathematician like Dyson, but I do not think that the high energy physics community worried too much about it. Since the low order results agree so well with experiment, we could leave the study of the mathematical properties for later.

The natural next step was to apply this approach to the other two interactions, to wit the strong and the weak ones. The former were represented by the isospin invariant pion-nucleon interaction and it was shown that the renormalisation program applied to it. However, the results were useless because the effective pion-nucleon coupling constant turned out to be very large, on the order of 10, making the power series expansion meaningless. There remained the weak interactions. Naturally, they are much weaker, but now a new problem appeared: the renormalisation program is a quite complex process and applies only to a small number of quantum field theories. The Fermi theory, which describes very well the low energy weak interactions, is not one of them.

This double failure soon tarnished the glory of quantum field theory. The disappointment was such that the subject was not even taught in many universities. By the late fifties the theoretical high energy physics landscape was fragmented in many disconnected domains, having no common trends and often ignoring each other. For strong interaction processes the main approach was based on the assumed analytic properties of the S-matrix elements, but we had also several simple models, none with any solid theoretical basis, each one applied to a particular corner of phase space. For weak interactions the Fermi theory proved to be a very good phenomenological model, but it had no logical justification and no obvious connection with anything else. The only real progress in our understanding of Nature's fundamental laws came from the application of symmetry principles, a method that proved to be extremely powerful. Quantum field theory was noticeable mainly by its absence. A totally marginal subject confined to very few precision calculations in quantum electrodynamics. Many physicists had only vague and often erroneous ideas about it and, to a certain extent, this misunderstanding has survived even today.

[6]It seems that W. Pauli had warned him against such expectation.

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2. The secret road to the New Theory

The title of this section could have been: *Quantum Field Theory strikes back*, but it would have been misleading. The new theory did not emerge out of the blue. It was built on results coming from all lines of research and each one contributed its fair share to it. The construction of the Standard Model, which became gradually the Standard Theory, is, probably, the most remarkable achievement of modern theoretical physics. It has not been triggered by an unexpected experimental discovery; it had instead a theoretical, rather an aesthetic motivation. For many years it consisted of a long series of isolated and mostly confidential contributions. In this secret road there was no well defined direction. Several milestones did not seem to point to a single path and they went often unnoticed when they were first proposed. Many important ideas had to be rediscovered again and again. The pioneers were not always aware of each other's work and it is only now that we can see a coherent picture[7].

3. The phase transition

There exist several books and articles[8] presenting the development of the Standard Model, although a complete History has not yet been written. I will not attempt to write it here and I will concentrate instead on the influence of the J/Ψ discovery which was essential in changing the mood of the physics community towards this new theory. It was a phase transition: from mistrust, to acceptance, yielding finally to enthusiasm. An important milestone, very close to the critical point, was the 17th International Conference on High-energy Physics (Rochester Conference), held in London in July 1974.

The Standard Model was completely written by 1973. It is a *quantum field theory* which is *renormalisable* and invariant under *gauge transformations* of the group $SU(3) \times SU(2) \times U(1)$ *spontaneously broken* to $SU(3) \times U(1)_{\mathrm{em}}$. All these four ingredients – quantum field theory, renormalisability, gauge invariance, spontaneous symmetry breaking – were quite obscure for the physicists of the early seventies. There was a wide spread mistrust for quantum field theory in general and the theory of renormalisation, in particular. When the model was first proposed, it was considered as a wild theoretical speculation, far remote from reality. It was introducing too many new and strange ingredients for very few experimentally verifiable predictions. Let me go through a short list:

• *The 12 vector bosons.* Out of them only the photon was a known physical particle. $W^{\pm}$ and Z^0 were predicted to be out of reach of any existing, or planned, accelerator, and the 8 gluons were declared unobservable because of the assumed property of confinement.

• *One, or more, scalar bosons*, but with unknown mass.

• *Neutral currents.* The Gargamelle collaboration had confirmed their existence in 1973 but not everybody was convinced (I have won several bottles of wine by betting for neutral currents against

[7]In our University courses we often get the impression that science progresses towards a clear and well defined goal. This is almost unavoidable because we learn only those attempts that have succeeded. However, scientific research resembles more a montecarlo algorithm. Many directions in the phase space of ideas are explored and we pursue only the most promising ones. Occasionally we find later that they lead to a dead end and we have to come back and start a new direction. Text books never present this real story, thus creating the illusion of a unique line of thought.

some among my more sceptical colleagues.) There were several reasons for that. Gargamelle was the first large (55 physicists!) international collaboration. It was also the first to use a sophisticated (for the time) montecarlo program to simulate the entire detector in order to control the neutron background. Finally, the mere existence of neutral current interactions was not considered to be a sufficient proof for the entire model and, even less, to make us change our ways of thinking in theoretical high energy physics.

• *An entirely new hadronic spectroscopy resulting from the assumed existence of a fourth quark.* This was the prediction which people found very hard to accept. It was based on two arguments: First, the need to suppress strangeness changing neutral currents. In the three quark model the hadronic part of the charged weak current is given by $\bar{u}\gamma_\mu(1+\gamma_5)(d\cos\theta + s\sin\theta)$ with u, d and s the fields of the three quarks and θ the Cabibbo angle. The commutator of this current with its adjoint, which should be part of the neutral current, contains strangeness violating terms proportional to $\bar{d}\gamma_\mu(1+\gamma_5)s$+hc, in other words the three quark model predicts decays such as $K^0 \rightarrow \mu^+\mu^-$ at first order in the weak interactions, in contradiction with experiment. This was the reason why the 1967 Weinberg-Salam model[9] was originally proposed only for leptons. The introduction of a fourth (charm) quark with charge 2/3 solves this problem, since now the neutral current is diagonal in flavor space[10]. However, this is not enough because the same decay can be produced also at one loop order with a rate which is still too large. In order to bring the rate bellow the experimental limits, one should assume an upper bound for the mass of the charm quark which translates into a bound of a few GeV for the masses of the charmed hadrons[10]. The second argument for the existence of the charm quark was based on the need to cancel the Adler-Bell-Jackiw anomalies of the axial current[11]. It was shown that the renormalisability of the theory requires the axial current to satisfy a canonical – i.e. non anomalous – divergence equation, which implies the condition that the sum of the electric charges of all the fermions in every family vanishes[12]. This condition is not satisfied in the three quark model and requires the introduction of charm[8]. To summarise: gauge theories claimed that some obscure higher order effects – triangle diagrams for the anomalies, or square diagrams for flavor changing neutral currents – would dictate the structure of the world. Admittedly, it takes a solid faith in quantum field theory to accept such a claim.

• *Asymptotic freedom and scaling violations in deep inelastic scattering.* The first SLAC results on inclusive deep inelastic electron-nucleon scattering exhibited the property of scale invariance. It was a very interesting discovery because it could be easily understood by invoking a very simple, and wrong, argument, namely the assumption that the nucleon is made out of constituents which interact with the incident virtual photon as free, point-like particles. The argument is obviously fallacious because free particles cannot build a nucleon. Nevertheless it was the model, called the *parton model*, which was used with a certain success. When QCD was discovered some people misinterpreted the term *asymptotic freedom* by dropping “asymptotic” and keeping only “freedom”, thus concluding that QCD “proved” the parton model. As a result, they were reluctant to admit the presence of scaling violations[9].

• *The constant value of R*, the ratio of the hadronic to $\mu^+\mu^-$ production cross sections in

[8]This condition applies to any family. For example, the subsequent discovery of the τ lepton implied the prediction for the existence of two new quarks, in other words families should be complete.

[9]In my report on gauge theories at the London Conference[13] I made a joke invoking an analogy with politics “. . . whenever someone talks about freedom it invariably turns out that he really means something else . . . ”

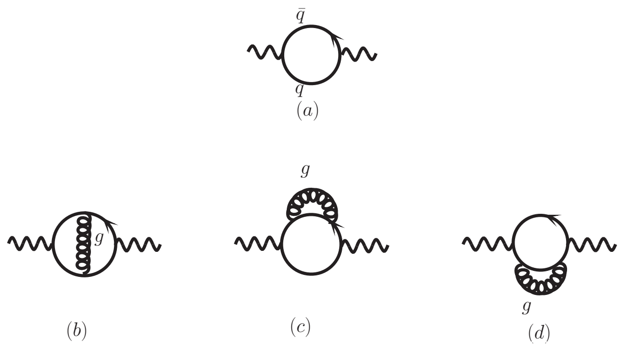

Figure 1: The diagrams contributing to R: (a) at zero order and (b), (c) and (d) at first order in QCD.

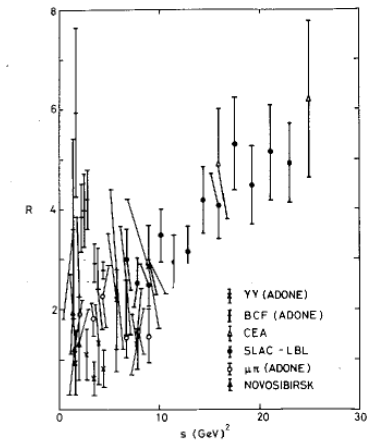

Figure 2: A compilation of all early measurements of the ratio R, as presented in the 1974 London Conference by Burton Richter.

electron-positron collisions. This prediction was not only considered as highly speculative, but it seemed also to be in violent disagreement with experiment. In QCD R is easily computed by the diagrams of Figure 1 with the result:

$$R(Q^2) = \sum_i e_i^2 \left(1 + \frac{\alpha_s(Q^2)}{\pi} + O(\alpha_s^2)\right) \;\; ; \;\; \alpha_s(Q^2) = \frac{1}{4\pi b_0 \ln\left(Q^2/\Lambda^2\right)} \tag{1}$$

with $\Lambda \sim O(200 \text{ MeV})$. It follows that R is predicted to approach asymptotically the value $\sum_i e_i^2$ from above, where e_i is the electric charge of the ith quark which can be produced with energy Q^2. In a three quark model $\sum_i e_i^2$=2. Figure 2 shows a compilation of all measurements of R which were available in July 1974. R appeared to be monotonically rising. In my report at the London Conference[13] I said: *"... the hadron production cross section, which absolutely refuses to fall, creates a serious problem. The best explanation may be that we are observing the opening of the charmed thresholds, in which case everything fits together very nicely."* The charm quark, which has charge 2/3, would bring the prediction[10] for R at 10/3. Therefore I concluded with[13] *"I have*

[10] An accident complicated the early calculations. It happens that the threshold for a $\tau^+\tau^-$ pair production is at the

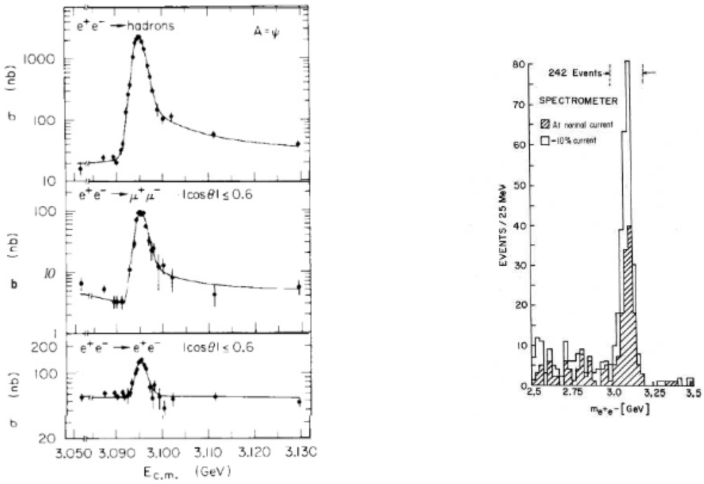

Figure 3: The discovery of J/Ψ in November 1974 independently by two groups, SPEAR (left) and AGS (right).

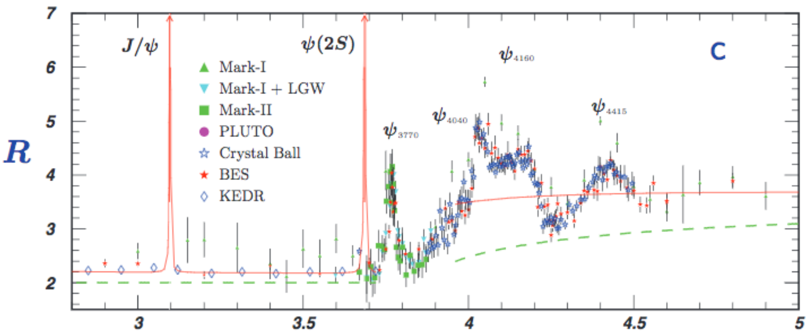

Figure 4: The value of R for energies between 3 and 5 GeV as it is given today by the Particle Data Group.

won already several bottles of wine by betting for the neutral currents and I am ready to bet now a whole case that if the weak interaction sessions of this Conference were dominated by the discovery of the neutral currents, the entire next Conference will be dominated by the discovery of the charmed particles."

4. The turning point

In November 1974, SPEAR decided to go back and sweep the region above 3 GeV in fine steps of 1 MeV. To their great surprise they obtained a totally different picture, the one shown in Figure 3. It clearly exhibits a very narrow resonance around 3.1 GeV. Enlarged pictures are shown in Figures 4 and 5, the latter including the b-quark resonances.

same region and confused the issue because, before the τ discovery, these events were counted as hadrons, thus adding a spurious extra unit to R.

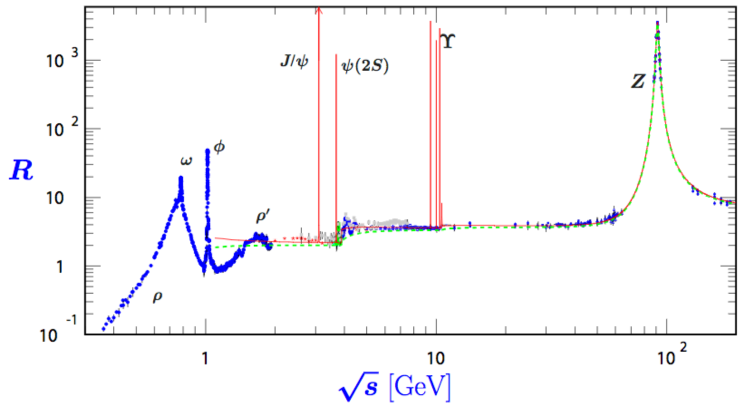

Figure 5: The ratio R from low energies, up to and above the Z mass. The green curve is the parton model prediction and the red one includes QCD corrections. Remarkable agreement.

The discovery was made on Sunday November 10 but, at that time, news did not travel with the speed of internet. B. Jean-Marie flew from Stanford to Paris with the results. I still remember the morning in which A. Lagarrigue called me at home to invite me at an improvised meeting. Gerard 't Hooft was visiting Ecole Normale, so I took him along. The news was indeed impressive. A 3 GeV resonance, decaying to hadrons, presumably through strong interactions, with a width of less than 100 keV, would have left any physicist of that time bewildered[11]. I told Lagarrigue I could not understand it. It was 't Hooft who first gave me the explanation. In the framework of QCD it is in fact very simple.

In the three quark model we have three non-strange, neutral, 1^- vector bosons: ρ^0, ω and ϕ. The last one is a pure $\bar{s}s$ state with a mass of 1020 MeV and a total width of only 4.2 MeV. For comparison, I remind you that ρ, with a mass of only 770 MeV, has a width of 142 MeV. Even more astonishing is the fact that ϕ decays mainly in a $\bar{K}K$ pair, although the available phase space is tiny; its pionic partial width is only 650 keV. In the old days we had a rule to describe this fact, the OZI-rule (Okubo-Zweig-Iizuka) which states that in a decay of a quark–anti-quark system, the modes which require the annihilation of the initial $\bar{q}-q$ pair are strongly suppressed[12]. Let us apply this rule: ρ lies well abole the 2π threshold, but we know that pions are particularly light because they are the pseudo-Goldstone bosons of chiral symmetry. Chiral symmetry is a poor approximation for strange particles and, as a result, kaons are substantially heavier. The decay $\phi \to \bar{K}K$ is barely possible. It is reasonable to assume that a hypothetical $\bar{c}c$ 1^- vector boson would lie below the threshold for charm–anti-charm mesons and it can only decay into pions. The corresponding decay amplitude has to go through three gluons, so the width is proportional to α_s^3. In the asymptotically free QCD, between 1 and 3 GeV α_s has dropped by a factor of two, which means that the width of J/Ψ should be 8 times smaller than the pionic width of ϕ. Surprisingly, this oversimplified argument gives the right result[13].

[11]In fact, many theorists proposed exotic models to explain the narrow width.

[12]H. Rubinstein was calling it "the rearrangement" model, one of those phenomenological rules of the time with no solid theoretical basis.

[13]As I just said, I first heard this argument by 't Hooft the very day we learned about the discovery of J/Ψ. There

Mesons with naked charm were naturally discovered among the decay products of the broad resonances we see in Figure 4 above 4 GeV. It was in 1976. In the meantime a rich charmonium spectroscopy was discovered in full agreement with the theoretical predictions. Although nobody paid the bet I offered in the 1974 Conference, the entire 1976 one was indeed dominated by charmed particles and gauge theories. The phase transition was complete.

5. What next?

So far the Standard Model has won all the battles. In fact, it works better than we could reasonably expect. It is based on renormalised perturbation theory and a measure of its success can be read off Figure 5. It shows the value of R from low energies up to the Z^0 pole. The striking feature of the data is that R has the value predicted by QCD almost everywhere outside small regions where we know that strong interactions are important (low energies and close to the two thresholds for charm and b production.) We do not understand why perturbation theory is so good, but let us take it as an experimental fact. In a talk I gave at the 2011 meeting of the European Physical Society in Grenoble I tried to argue that the validity of the perturbation theory outside the region of strong interactions makes it possible to predict the existence of new physics at the TeV scale. A simplified version of the argument was based on the expected search for the BEH scalar boson. A heavy boson, with mass above 200 GeV, was incompatible with the LEP data, unless there was new physics to invalidate the perturbative extrapolation. On the other hand, a light boson would receive high corrections to its mass, unless it is protected by some new physics. My conclusion was that for LHC, which was about to start operating, new physics was around the corner! Today we know that LHC has found no corner. But I secretly believe the argument is correct, only the corner is a bit further down. Although I will not see it, I am confident some of you will find it.

These notes have touched so many subjects that a complete list of references is impossible. The selection is minimal and arbitrary and I apologise for the numerous omissions.

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exists an article by T. Appelquist and D. Politzer[14] which contains this argument and has submission date Nov. 19 1974. I presume that they had thought of it before the experimental discovery.

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