

WHEN WAS ASYMPTOTIC FREEDOM DISCOVERED?
or
THE REHABILITATION OF QUANTUM FIELD THEORY*

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Abstract

We glance back at the short period of the great discoveries between 1970 and 1974 that led to the reestablishment of Quantum Field Theory and the discovery of the Standard Model of Elementary Particles, in particular Quantum Chromodynamics, and ask ourselves where we stand now.

1. An apology

At meetings such as this one, one should be looking forward, not back. But what happened in the crucial period 1970–1974 has been decisive for our field of research, and to set the record straight is not only of importance for us personally, but also allows us to learn very important lessons for the future. Quantum Chromodynamics as a theory for the strong interactions, and in particular asymptotic freedom, were not single discoveries made at one single instant. These insights have a complicated history, and a large number of physicists contributed here, everyone in his or her own way.

* Talk delivered at the QCD Euroconference 98 on Quantum Chromodynamics, Montpellier, July 1998.

2. Prehistory (1947-1970)

In the early '60s, most physicists did not believe that strong interactions were described by a conventional quantum field theory. Much effort went into the attempts to axiomise the doctrine of relativistic quantum particles strongly interacting among themselves. All we had, for sure, was that there should exist an S -matrix, and it obeys all sorts of properties: unitarity, causality, crossing symmetry.^{1, 2}

Perturbatively renormalizable quantum field theories did exist, and there were two prototypes: *Quantum Electrodynamics* (QED), which had been very successful in explaining features such as the Lamb shift and the anomalous magnetic moment of the electron, and $\lambda\phi^4$ theory, describing a self-interacting scalar field, but which had no obvious application anywhere in elementary particle physics. In spite of their remarkable successes, these theories were considered to be suspect, for various reasons³. On hindsight, one could say that this was based mostly on misunderstandings concerning the renormalization group.

In the (former) Soviet Union, Quantum Field Theory was rejected on the basis of renormalization group arguments. In particular, Landau and Pomeranchuk claimed that inadmissible poles would appear in the ultraviolet region. This argument appeared to be based on the Källén-Lehmann representation⁴ of the propagator,

$$D(k^2) = \int_0^\infty \frac{\varrho(m^2)dm^2}{k^2 + m^2 - i\varepsilon} ; \quad \varrho(m^2) \geq 0 , \quad (2.1)$$

which seemed to imply that the function now called β -function, had to be positive.

In the West, the Renormalization group had been introduced by A. Peterman and E. Stueckelberg⁵ in 1953, and it was used by M. Gell-Mann and F. Low⁶ in 1954 to argue that a renormalizable theory could have a *fixed point* in the ultraviolet. This would be much better than a pole; indeed, the investigation had been inspired by the (false) expectation that this might allow one to actually calculate constants of Nature such as the finestructure constant. But, here, renormalizable field theories were also considered to be ugly; they gave the impression that the difficulties, the infinities, were being “swept under the rug”.³

Thus, it was widely thought that the real world is not at all described by a renormalizable quantum field theory (RQFT), and the search for alternative approaches began.

Experimental evidence showed that strong interactions have a well-defined symmetry structure. There is a practically exact $U(2)$ symmetry (isospin and baryon number conservation), a softly broken $SU(3)$ (Gell-Mann’s “Eightfold way”^{7, 8}), and a more delicate chiral $SU(2) \times SU(2)$ that is both spontaneously broken (so that the pions became massless Goldstone bosons) and explicitly broken (the pions still have a non-vanishing mass). It was suspected that the Noether currents associated with these symmetries were playing an essential role in the dynamics. It was attempted to write down rigorous algebras for these currents, such that their representations would generate the sought-for S -matrix⁹. They were combined with unitarity, causality and crossing symmetry as these were suggested by quantum field theory, but one did not want to use quantum field theory itself.

A promising new idea was “duality” (in terms of the Mandelstam variables s , t and u for elastic scattering events)¹⁰. A great discovery by G. Veneziano¹¹ was that one could write down simple mathematical expressions for amplitudes that obeyed this duality requirement. Since this duality is somewhat different from what one has in standard quantum field theory, it was clear why there was much enthusiasm for it. After Z. Koba and H.-B. Nielsen found how to generalise Veneziano’s expressions to n -particle amplitudes,¹² the physical interpretation was also discovered, by Nielsen and D. Fairlie¹³, T. Goto, L. Susskind and Y. Nambu:¹⁴ the amplitudes are the scattering amplitudes for strings, colliding, merging and splitting one against the other.

3. New models

Yet interest in RQFT did not die completely. A monumental beacon was the paper by C.N. Yang and R. Mills¹⁵ in 1954, in which they showed that a fundamental principle could be used to construct impressive field theories: *local Non-Abelian gauge symmetry*. Being a direct generalization of QED, this theory appeared to be renormalizable. The Yang-Mills Lagrangian was

$$\mathcal{L}_{\text{YM}} = -\frac{1}{4}G_{\mu\nu}^a G_{\mu\nu}^a - \bar{\psi}(\gamma_\mu D_\mu + m)\psi, \quad (3.1)$$

The paper suggested to use this theory to turn isospin symmetry into a local symmetry, but this clearly could not work, because it would imply the existence of massless, charged mesons with spin one, a manifest absurdity.

Nevertheless, it was also clear to many researchers that this was a very deep idea, and, somehow, it should be tried to incorporate it in new theories for the fundamental interactions. R. Feynman¹⁶ was inspired by it in his first attempts to quantise the gravitational force. S.L. Glashow¹⁷, in 1961, used the Yang-Mills Lagrangian as a starting point for an electroweak theory with $SU(2) \times U(1)$ symmetry, but he had been forced to alter the theory somewhat in order to accommodate for the mass of the intermediate vector boson. S. Weinberg¹⁸ and A. Salam¹⁹ invoked the Higgs mechanism to produce a mass term, but they hit upon two problems: the renormalization was not understood, and the hadronic sector did not seem to fit well. M. Veltman²⁰, during the years 1963–1970, used Glashow’s approach and tried to enforce renormalization schemes on it. He had convinced himself that there had to be a Yang-Mills structure in the weak interactions, but abhorred formal theories including the Higgs-Kibble mechanism.

In the USSR, L. Faddeev and V. Popov²¹, E. Fradkin, and I.V. Tyutin²² continued investigations on pure Yang-Mills and gravity theories. They generally avoided renormalization issues, but the renormalization program, for theories simpler than the Yang-Mills system, and strictly in perturbative terms, was also continued, by N.N. Bogolyubov, O.S. Parasiuk, K. Hepp, H. Lehmann, K. Symanzik and W. Zimmermann, among others.

Then, in the ’60s, new interest in renormalization arose with the advent of the σ -model. This was a remarkable new renormalizable model describing protons, neutrons, pions, and one newly proposed particle, the σ . It had been invented by Gell-Mann

and M. Lévy²³ in order to have a realization of the observed spontaneously broken chiral $SU(2) \times SU(2)$ symmetry. Its formal structure was investigated by Symanzik²⁴, B.W. Lee²⁵ and J.-L. Gervais²⁶. but, as its coupling constant, the pion-nucleon interaction constant g , is quite large, one had to attempt to replace the usual perturbative expansion by something more convergent. It was attempted to use Padé approximation techniques to achieve more meaningful expressions²⁷.

4. Renormalization

In 1970, we suspected that explicit mass terms are not allowed in Yang-Mills theories, but we still experienced much resistance against the idea of a Higgs mechanism. Not only did it seem to be ‘ugly’, in comparison with the ‘clean’ Yang-Mills concept, the rejection of the Higgs mechanism was also rationalised by expressing the fear that this would induce far too large contributions to the gravitational cosmological constant²⁸. Thus, since my then thesis advisor rejected the Higgs mechanism, and I rejected the introduction of explicit mass terms, a compromise was agreed upon, and I began my research by investigating the renormalizability of pure, massless Yang-Mills theories. It was established²⁹, that renormalizability requires the validity of certain generalized Ward identities[†], which will guarantee unitarity, causality and renormalizability to be maintained at all orders in perturbation theory.

Once this was realised, it became evident how to do the ‘massive’ case³²: one *must* have a Higgs mechanism if a mass term is desired. Gauge-invariance then still guarantees unitarity and causality. Thus, the Higgs mechanism is far from formal, it is a necessary ingredient for the electroweak interaction theory.

The new renormalizable electroweak theory enjoyed instant successes. It could quickly be extended to encompass the hadrons, because now the *exact rules* were understood. The ingredient needed for this had already been proposed in 1970: the GIM mechanism³³. This mechanism did require the existence of a new quark flavor, charm, and furthermore it predicted the existence of neutral components in the weak interactions. These ‘neutral current events’ were confirmed in 1973. Finally, there was what appeared to be a technical detail: to ensure the cancellation of dangerous anomalous contributions that would otherwise have destroyed renormalizability, it was predicted that the number of quark flavors and lepton species had to be the same. This was confirmed when the discovery of a new family of leptons, τ and ν_τ , was followed by that of two new quark flavors: bottom b and top t .

5. Confusions about Scaling

So, what happened to the earlier objections against renormalizable field theories? It now seems that these were primarily based upon confusions concerning the scaling behavior of

[†] These identities are now called Slavnov³⁰-Taylor³¹ identities, after the authors who rephrased these identities to have them also hold off-mass shell. These off-shell versions are straightforward generalizations, and not directly needed for the renormalization program.

known as the Callan-Symanzik equations. They explicitly constructed their equations only for QED and $\lambda\phi^4$ theory, but they expected them to have universal validity. In particular, the β functions were found to be positively valued, and this too was thought to be a universal feature, valid for *all* quantum field theories, Abelian as well as non-Abelian ones. For this reason, it was generally felt that *no quantum field theory will ever explain Bjorken scaling*⁴⁰. I could not understand why people were saying this because I knew about the beautiful scaling behaviour of non-Abelian gauge theories. Suspecting that this feature should be known by now by the experts on the subject of scaling, I did not speak up louder. When I mentioned my suspicions to Veltman, that a pure $SU(3)$ gauge theory could be all what is needed to describe the forces between quarks, he warned me that no-one would take such an idea seriously as long as it could not be explained why quarks cannot be isolated one from another.

By 1972, I had calculated the scaling behavior, and I wrote it in the form

$$\frac{\mu d}{d\mu}g^2 = \frac{g^4}{8\pi^2} \left(-\frac{11}{3} C_1 + \frac{2}{3} C_2 + \frac{1}{6} C_3 \right), \quad (5.3)$$

in a theory in which the gauge group has a Casimir operator C_1 and where fermions have a Casimir operator C_2 . Scalars contribute with the Casimir operator C_3 . In the $SU(2)$ case this would mean that with less than 11 fermions in the elementary representation (and no scalars) the β function would be negative, and for $SU(3)$ the critical number would be $33/2$.

In June, 1972, a small meeting was organised by C.P. Korthals Altes in Marseille. Symanzik explained that he worked on $\lambda\phi^4$ theory with negative λ , because of its interesting scaling properties⁴¹. It was unfortunate, but, in Symanzik's opinion, perhaps not insuperable, that the only models with such properties that he could think of were theories with a negative squared coupling constant, $g^2 < 0$, or $\lambda < 0$. He explained that perhaps this problem could be cured by non-perturbative effects, which at that time were very badly understood. I announced at that meeting my finding that the coefficient determining the running of the coupling strength, that he called $\beta(g^2)$, for non-Abelian gauge theories is negative, and I wrote down Eq. (5.3) on the blackboard. Symanzik was surprised and skeptical. "If this is true, it will be very important, and you should publish this result quickly, and if you won't, somebody else will," he said. I did not follow his advice. A long calculation on quantum gravity with Veltman had to be finished first.

When Veltman and I had worked out many examples of gauge models⁴², we knew how to compute the renormalization counter terms:

$$\Delta\mathcal{L}_{\text{Dim.Regul.}} = \frac{1}{8\pi^2(4-n)} \cdot \frac{1}{4} G_{\mu\nu}^a G_{\mu\nu}^a \left(-\frac{11}{3} C_1 + \frac{2}{3} C_2 + \frac{1}{6} C_3 \right). \quad (5.4)$$

Clearly, this had to be related to the scaling behavior found in Eq. (5.3). The basis for this relation was quickly found⁴³. Furthermore, it was found how to use background field methods for a quick derivation of Eq. (5.4), and hence also (5.3). With these methods, we could limit ourselves to diagrams of the kind used in Eq. (5.1), and ignore the ones of (5.2), by making use of slightly altered Feynman rules⁴⁴.

As for what happened in the States, and how the sign of the β function for non-Abelian gauge theories was discovered, I leave to D. Gross to account.

When I heard about the publications by H.D. Politzer⁴⁵, D. Gross and F. Wilczek⁴⁶, I was surprised, not about the result, which I had known all along, but about the stir they caused. And, finally, people began to talk about a pure $SU(3)$ theory for quarks. Well, I thought, they still have nothing; as Veltman had taught me, what really matters is the quark confinement problem. No real advances had been made yet. I set out to try to understand confinement from first principles. QCD had only come half-way.

In 1973, Gross and Coleman showed furthermore that asymptotic freedom would *never* occur in theories that only contain fermions and scalars.⁴⁷ First, this result could be generalized by writing down the complete algebra for all one-loop beta functions[‡].

In all renormalizable theories, one can write the basic Lagrangian as

$$\begin{aligned} \mathcal{L}^{\text{inv}} = & -\frac{1}{4} G_{\mu\nu} G_{\mu\nu} - \frac{1}{2} (D\phi)^2 - V(\phi) \\ & - \bar{\psi} \gamma D\psi - \bar{\psi} (S(\phi) + i\gamma_5 P(\phi)) \psi, \end{aligned} \quad (5.5)$$

where $V(\phi)$ is at most a quartic polynomial in the scalar field(s) ϕ_i , and S and P are linear expressions in ϕ_i , which may be anything as long as the total Lagrangian is kept gauge invariant. In (5.5), the fields ϕ_i are written as real fields, ψ_i and $\bar{\psi}_i$ are complex.

The one-loop counter Lagrangian is then⁴⁴:

$$\Delta\mathcal{L} = \frac{1}{8\pi^2(4-n)} \left\{ -\frac{1}{4} G_{\mu\nu} G_{\mu\nu} \left(\frac{11}{3} C_1 - \frac{2}{3} C_2 - \frac{1}{6} C_3 \right) - \Delta V - \bar{\psi} (\Delta S + i\gamma_5 \Delta P) \psi \right\}, \quad (5.6)$$

with, writing $S + iP \equiv W$,

$$\begin{aligned} \Delta V = & \frac{1}{4} (\partial_i \partial_j V)^2 + \frac{3}{2} \partial_i V (T^2 \phi)_i + \frac{3}{4} (\phi T^a T^b \phi)^2 \\ & + \phi_i V_j \text{Tr}(S_i S_j + P_i P_j) - \text{Tr}(S^2 + P^2)^2 + \text{Tr}[S, P]^2; \\ \Delta W = & \frac{1}{4} W_i W_i^* W + \frac{1}{4} W W_i^* W_i + W_i W^* W_i \\ & + \frac{3}{2} (U_R^2 W) + \frac{3}{2} W (U_L)^2 + W_i \phi_j \text{Tr}(S_i S_j + P_i P_j), \end{aligned} \quad (5.7)$$

where T^a , U_R^a and U_L^a are the generators of gauge transformations on Φ_i , $\psi_{i,R}$ and $\psi_{i,L}$, which determine the coupling strengths of these fields to the gauge field. $V_i = \partial_i V$ stands for differentiation of $V(\phi)$ with respect to the fields ϕ_i .

Carefully studying the signs of these expressions, one finds in general:

$$\beta(g) = (-R_1 + R_2 N_f + R_3 N_s) g^3; \quad (5.8)$$

$$\beta(Y) = R_4 Y^3 - R_5 g^2 Y; \quad (5.9)$$

$$\beta(\lambda) = R_6 \lambda^2 - R_7 g^2 \lambda + R_8 g^4 + R_9 Y^2 \lambda - R_{10} Y^4, \quad (5.10)$$

[‡] Gross and Coleman had included two-loop diagrams for the scalar field renormalization, but these are not needed for the result.

where g stands short for all gauge field coupling constants, Y for all Yukawa couplings, and λ for all quartic scalar field self-couplings. R_{1-10} are all positive constants. The relative strengths of these constants depends on further details such as the nature of the gauge group representations and the multiplicities of these fields, and can vary quite a bit. In general, one can find several choices of gauge field theories and coupling constants such that all constants run to zero at infinite energies.

6. Color $SU(3)$ – the successes of QCD.

The early quark theory suffered from an apparent contradiction with the spin-statistics theorem, since the baryonic states appeared to be described by totally symmetric quark wave functions, instead of the expected antisymmetric ones. In 1964, Greenberg⁴⁸ suggested a solution in terms of a new kind of statistics called ‘parastatistics’. The first ideas of an internal ‘color’ gauge group causing the confining forces between quarks go back to Han and Nambu⁴⁹ in 1965, who produced models in which electric charge was linked to color in such a way that physical charges would be integral, in what would nowadays be called a Higgs theory. The idea is not as far away from the truth as one might think. If there existed scalar quarks, these could automatically be cast in a form that would make QCD look like a Higgs theory, without major departures from its present appearance. At the time that Gross and Wilczek wrote their papers⁵⁰, there were also proposals by H. Fritzsch, Gell-Mann and H. Leutwyler⁵¹, who investigated pure $SU(3)$ color theories for quarks, and they recognised a serious problem (the $U(1)$ problem, to be discussed in Sect. 7). There were also similar suggestions by H. Lipkin⁵².

None of these papers had a satisfactory answer to the question why quarks are confined. But then several key developments took place. The *dual resonance models*^{10, 11} gave a first clue: mesons appear to behave as little stretches of strings, with quarks at their end points. This most beautifully squared with the observed linear relation between the mass-squared and the angular momentum of the heavy mesonic resonances, the so-called Regge trajectories[§]. It suggested that the colored forces between quarks form *vortex lines* that connect color charges. How can one understand why these are formed? This is a long-distance problem, and precisely at these long distances (of a fermi and more), the coupling strength grows so large that perturbative arguments will be meaningless.

Independently, Nielsen and P. Olesen⁵⁵ in Copenhagen, and B. Zumino⁵⁶ at CERN, discovered a new and extremely important feature in an *Abelian* Higgs theory: this model would allow for the existence of a magnetic vortex line that would have several features in common with the dual string: it is unbreakable, its mass-energy content is entirely due to the tension force, and it exhibits an internal Lorentz invariance (for boosts in the direction of the string). If quarks would be magnetic monopoles with respect to this spontaneously broken $U(1)$ force, you would get exactly the model you want. In fact, this model is nearly identical to much older models for superconducting material in which the Meissner effect is active. The vortices are just the well-known Abrikosov vortices formed by magnetic fields

[§] The work of J. Chew¹, S. Frautschi⁵³ and V. Gribov⁵⁴ should be mentioned here.

that succeed in penetrating into a superconductor.

But quarks are not monopoles. This theory was not QCD, and so it would not explain Bjorken scaling and the symmetric $SU(3)$ representations of the hadrons. What happens if one replaces the Abelian $U(1)$ theory by a non-Abelian theory with Higgs mechanism? Does that also allow for Abrikosov vortices? I asked myself this question, and ran into a puzzle: there would be vortices, but they would differ from the Abelian ones in that now they can break. The resolution of the puzzle was revolutionary: the vortices break because they can create pairs of magnetic monopoles and antimonopoles. It was not known yet that magnetic monopoles can exist in fairly ordinary non-Abelian gauge models!⁵⁷ Independently, but a couple of months later, this discovery was made in Moscow: A. Polyakov had constructed the same gauge field solution⁵⁸, and it was remarked by L. Okún, that the object carries a single magnetic charge.

However, it looked as if the monopole was a side-track. By itself it did not bring us closer to understanding quark confinement. Yet it would later turn out to be a crucial ingredient.

First, something else happened. K. Wilson was studying the renormalization group for non-Abelian gauge theories restricted on a space-time lattice. His interest was primarily in the domain of computer simulations. But he made a crucial observation⁵⁹: if, for practical, technical reasons, you restrict (Euclidean) space and time to form a lattice, you can perform a new perturbative expansion: the $1/g^2$ expansion. This large coupling constant expansion becomes accurate at large distance scales. Surprisingly, one can read off right away that the only states that one can then talk about are gauge-invariant (that is, colorless) states, formed by quarks that had to be connected one to another by line segments of the lattice, exactly as in the dual string theory! In other words, quark confinement is evident *and absolute* in the large coupling constant expansion!

In my recollection, this was the first indication that quark confinement is absolute, quarks can never escape from each other. It looks as if, for a theory on a lattice, when the coupling constant is varied from small to large, a *phase transition* occurs. QCD is said to be in the *confinement phase*. Yet, in the continuum theory, the interactions are generated at very tiny distance scales, where the coupling constant is weak, and then, by cumulative effects, they generate larger couplings at large distances. Does this justify a large coupling constant expansion for continuum theories? Not really. We were coming closer, but the exact nature of the confinement mechanism had not yet been explained convincingly.

Because of Dirac's relation⁶⁰ between the fundamental electric charge quantum e and the magnetic charge quantum g_m ,

$$g_m e = 2\pi n \hbar c,$$

where n is an integer, the interchange electric–magnetic also corresponds to a small coupling–large coupling interchange. Therefore, to understand confinement, it should be a good idea to interchange the electrically charged particles with the magnetic monopoles in the theory. If, subsequently, a Higgs mechanism is assumed to take place among the magnetic monopoles, we obtain the dual analogue of the older model of Nielsen, Olesen

and Zumino. The magnetic vortex in that model would now manifest itself as an electric vortex line, and the role of the monopoles in that model would now be played by the quarks. They will be held together absolutely and permanently by these vortices. Bingo, this is permanent quark confinement.^{61, 62}

The next step would be to reformulate this mechanism in more precise terms. The description of phase transitions using dual transformations turned out to be related to what is done in statistical systems.⁶³ But also, one had to rephrase carefully the gauge constraints of the non-Abelian theory. It had been noted by Gribov⁶⁴, that the usual procedure for fixing the gauge degree of freedom in an Abelian gauge theory leads to a complication when applied to the non-Abelian case: the Lorenz condition does *not* fix the gauge freedom completely; there is an ambiguity left. This ambiguity is harmless as long as one sticks to a perturbative expansion. There, we know exactly what kind of non-local effects are caused by gauge fixing: it is the Faddeev-Popov ghosts.

But in attempts to go beyond perturbation expansion, Gribov's ambiguity is important. It is then more difficult to distinguish physical states from ghosts. Ghosts arise as soon as the gauge fixing procedure requires the solution of differential equations; their kernels are the ghost propagators. If one wants to avoid ghosts altogether, one must use a *local, non-propagating* gauge fixing procedure. Indeed, it is possible exactly to give a gauge constraint that removes *the non-Abelian parts* of the local gauge invariance, without any ambiguity⁶⁵. After such a gauge fixing procedure, one is left with an apparently Abelian effective theory, *with* in addition to the electrically charged particles, *magnetic monopoles*. They emerge as singularities due to the gauge fixing. Since the Abelian gauge group that survives is the Cartan subgroup of the original gauge group (the largest Abelian subgroup), we call this procedure the 'maximal Abelian gauge fixing procedure', or 'the Abelian projection'.

In the maximal Abelian gauge, we find 'color-electrically' charged particles, 'color-magnetically' charged particles, each with finite masses depending on the dynamical features of the system, and massless $U(1)$ 'color-photons' (gluons), that couple to the 'color-electric' and 'color-magnetic' particles in a totally symmetric fashion.

All one is now left with to do, is to invoke the Higgs mechanism in this color regime. If the Higgs mechanism applies to the color-electrically charged particles, then these will be free, with short-range interactions only, since they exchange gluon-photons that now have become massive. The color-magnetic particles will be held together eternally by color-magnetic vortex lines. But the effective model in the maximal Abelian gauge equally well allows us to assume the Higgs mechanism to work on the color-magnetically charged particles, in which case the color-electrically charged particles are held together by color-electric vortices, being the dual strings. Whether the color-electric, or the color-magnetic Higgs mechanism takes place, or perhaps neither, depends on the relative strength of the color-electric and the color-magnetic charges. Due to asymptotic freedom, one generally expects the color-electric charges to become large when it is attempted to separate them by large distances; the color-magnetic charges will become small, with a tendency to be screened completely. It is natural to assume that the latter undergo Bose condensation, just as in super conductors, and then one has permanent quark confinement.

A daring experiment was first carried out by M. Creutz *et al*, on a computer⁶⁶: they took a lattice in four-dimensional Euclidean space and defined the QCD field variables on this lattice. Because of the complexity of the problem, and the limited capacity of these early computers, the lattices had to be very modest in size. Nevertheless, their computer simulations showed clear evidence of a transition towards a confining phase. Nowadays, by using much larger and faster computers, as well as much improved analytical procedures, this transition can be studied in much more detail. One has also been able to impose maximal Abelian gauge conditions on the leading color field configurations that were obtained, and quite generally, the picture of Bose-condensing magnetic monopoles has been confirmed⁶⁷. Today, it is fair to say that quark confinement is no longer seen as a deep puzzle in quantum field theory. We therefore understand why protons did not break into quarks in the ISR experiments at CERN⁶⁸. If a color vortex ever breaks, it is because of quark-antiquark pair formation, so that hadrons are always kept color neutral. A vortex breaking without quark pair formation would require large-scale structures (*i.e.*, low energy excitations) of a kind that directly contradict the lattice simulations. Therefore, we do not expect that experimentalists will ever detect free quarks.

Experimental support for QCD then came in numerous ways. Notably, the quark and gluon substructures became clearly recognisable as ‘jets’, and the slow variations of the running coupling constants were confirmed.

7. The $U(1)$ problem

A serious problem, however, was cause for concern. This was the so-called $U(1)$ or η - η' problem. The *explicit* breakdown of chiral $U(1)$ symmetries had been noted as an ‘anomaly’ in quantum field theories⁶⁹: the π^0 decay into two photons, for instance, would have been forbidden because of a chiral conservation law, but the process nevertheless takes place. It also is found to take place if one performs a simple-minded calculation, either using an intermediate proton-antiproton state (Fig. 1a), or quark-anti-quark states (Fig. 1b).



Fig. 1. The $U(1)$ anomaly, a) in terms of virtual protons, b) in terms of virtual quarks.

But what happened to this symmetry in QCD was not understood. The problem was recognised already by the earliest authors, Fritzsche, Gell-Mann and Leutwyler⁵¹. The QCD Lagrangian correctly reflects all known symmetries of the strong interactions, and the hadron spectrum is reasonably well explained by extending these symmetries to an approximate $U(6)$ symmetry spanned by the d , u and s quark each of which may have their spins up or down.

But the QCD Lagrangian also shows a chiral $U(2) \times U(2)$ symmetry, and consequently, it suggests the presence of an additional axial $U(1)$ current that is approximately conserved. This should imply that a pseudoscalar particle (the quantum numbers are those of the η particle) should exist that is about as light as the three pions. Yet, the η is considerably heavier: 549 MeV, to be compared with the 135 MeV of a pion. For similar reasons, one should expect a ninth pseudoscalar boson that is as light as the kaons. The only candidate for this would be one of the heavy mesons, originally called the X^0 meson. When it was discovered that this meson can decay into two photons⁷⁰, and therefore had to be a pseudoscalar, it was renamed as η' . However, this η' was known to be as heavy as 958 MeV, and therefore it appeared to be impossible to accommodate for the large chiral $U(1)$ symmetry breaking by modifying the Lagrangian. Also, the decay ratios – especially the radiative decays – of η and η' appeared to be anomalous, in the sense that they did not obey theorems from chiral algebra.⁷¹

Clearly, there is an anomaly akin to the Adler-Bell-Jackiw anomaly. The divergence of the axial vector current is corrected by quantum effects:

$$\partial_\mu J_\mu^A = \frac{g^2}{16\pi^2} \varepsilon_{\mu\nu\alpha\beta} \text{Tr} G_{\mu\nu} G_{\alpha\beta}, \quad (7.1)$$

where J_μ^A is the axial vector current, $G_{\mu\nu}$ the Yang Mills gluon field, and g the strong coupling constant. So, the axial current is not conserved. Then, what is the problem?

The problem was that, in turn, the r.h.s. of this anomaly equation can also be written as a divergence:

$$\varepsilon_{\mu\nu\alpha\beta} \text{Tr} G_{\mu\nu} G_{\alpha\beta} = \partial_\mu K_\mu, \quad (7.2)$$

where K_μ is the Chern-Simons current. K_μ is not gauge-invariant, but it appeared that the latter equation would be sufficient to render the η particle as light as the pions. Why is the η so heavy?

There were other, related problems with the η and η' particles: their mixing. Whereas the direct experimental determination of the ω - ϕ mixing⁷² allowed to conclude that in the octet of vector mesons, ω and ϕ mix in accordance to their quark contents:

$$\omega = \frac{1}{\sqrt{2}}(u\bar{u} + d\bar{d}), \quad \phi = s\bar{s}, \quad (7.3)$$

the η particle is strongly mixed with the strange quarks, and η' is nearly an $SU(3)$ singlet:

$$\eta \approx \frac{1}{\sqrt{3}}(u\bar{u} + d\bar{d} - s\bar{s}), \quad \eta' \approx \frac{1}{\sqrt{6}}(u\bar{u} + d\bar{d} + 2s\bar{s}). \quad (7.4)$$

Whence this strong mixing?

Several authors came with possible cures. J. Kogut and L. Susskind⁷³ suggested that the resolution came from the quark confinement mechanism, and proposed a subtle procedure involving double poles in the gluon propagator. Weinberg⁷⁴ also suggested that, somehow, the would-be Goldstone boson should be considered as a ghost, cancelling

other ghosts with opposite metrics. My own attitude⁶¹ was that, since K_μ is not gauge-invariant, it does not obey the boundary conditions required to allow one to do partial integrations, so that it was illegal to deduce the presence of a light pseudoscalar.

Just as it was the case for the confinement problem, the resolution to this $U(1)$ problem was to be found in the very special topological structure of the non-Abelian forces. In 1975, a topologically non-trivial field configuration in four-dimensional Euclidean space was described by four Russians, A.A. Belavin, A.M. Polyakov, A.S. Schwarz and Yu.S. Tyupkin⁷⁵. It was a localized configuration that featured a fixed value for the integral

$$\int d^4x \varepsilon_{\mu\nu\alpha\beta} \text{Tr} G_{\mu\nu} G_{\alpha\beta} = \frac{32\pi^2}{g^2}. \quad (7.5)$$

The importance of this finding is that, since the solution is localized, it obeys all physically reasonable boundary conditions, and yet, the integral does not vanish. Therefore, the Chern-Simons current does not vanish at infinity. Certainly, this thing had to play a role in the violation of the $U(1)$ symmetry.

This field configuration, localized in space as well as in time, was to be called “instanton” later⁷⁶. Instantons are sinks and sources for the chiral current. This should mean that chirally charged fermions are created and destroyed by instantons. How does this mechanism work?

It was discovered that near an instanton, the Dirac equation shows special solutions, which are fermionic modes with vanishing action⁷⁶. This means that the contribution of fermions to the vacuum-to-vacuum amplitude turns this amplitude to zero. Only amplitudes in which the instanton creates or destroys chiral fermions are unequal to zero. The physical interpretation of this was elaborated further by Russian investigators, by R. Jackiw, C. Nohl and C. Rebbi⁷⁷, and by C. Callan, R. Dashen and D. Gross⁷⁸. Instantons are tunnelling events. Gauge field configurations tunnel into other configurations connected to the previous ones by topologically non-trivial gauge transformations. During this tunnelling process, one of the energy levels produced by the Dirac equation switches the sign of its energy. Thus, chiral fermions can pop up from the Dirac sea, or disappear into it. In a properly renormalized theory, the number of states in the Dirac sea is precisely defined, and adding or subtracting one state could imply the creation or destruction of an antiparticle. This is why the original Adler-Bell-Jackiw anomaly was first found to be the result of carefully renormalizing the theory.

With these findings, effective field theories could be written down in such a way that the contributions from instantons could be taken into account as extra terms in the Lagrangian. These terms aptly produce the required mass terms for the η and the η' , although it should be admitted that quantitative agreement is difficult to come by; the calculations are exceptionally complex and involve the cancellation of many large and small numerical coefficients against each other. But it is generally agreed upon (with a few exceptions⁷⁹), that the η and η' particles behave as they should⁸⁰ in QCD.

It was after the resolution of the confinement problem and the $U(1)$ problem, that QCD could be accepted as a viable theory. Strong experimental support was the observation of jet events, which clearly reflect the short distance structure, and the observation of

the running coupling constant.

8. New developments still needed

QCD is in a very good state theoretically as well as experimentally. Yet, there is still very much to be desired, first of all, a more reliable on-shell calculational technique. A very promising result was that the theory simplifies if the number of colors, N_c , whose physical value is three, is sent to infinity instead. One can subsequently consider the corrections in powers of $1/N_c$. The advantage of such an expansion would be that, already at $N_c = \infty$ the spectrum of physical states is suspected to be very similar to the real spectrum; only the mesons in this limit are non-interacting. So, we suspect in the $N_c \rightarrow \infty$ limit an effective meson theory.

Unfortunately, this limiting theory has not yet allowed a rigorous mathematical description. We suspect it to be very similar to string theories, but the desired string theory has not been found. very promising recent developments suggest that progress here is possible.⁸¹ Also, the author recently reinvestigated some aspects of the “planar” diagram expansion required by this theory. We suspect quark confinement to be a fundamental property of planar diagrams, related to divergences in the planar diagram expansion.⁸²

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