How Quantum Chromodynamics took us by surprise

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50 years QCD

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Abstract

Discoveries are rarely made by single individuals without parallel developments that could well have been instrumental. Quantum Chromodynamics is no exception. Around 1970 there were several observations, made by experimental groups, by theoreticians, and by phenomenologists that indicated what the most likely scenario was to understand the strong force. Nevertheless, the transparent nature of this force and the underlying simplicity came as a surprise. Also surprising was the strong relationship with all other forces among the subatomic particles

Bjorken scaling, mass shell, AF, inhoud:

Particle physics before 1970.

Rosenfeld table of elementary particles and resonant states: electron, proton, photon, muon, pion, neutrinos, a few particles with strangeness: K , Σ, Ξ, Λ, , and a bunch of resonances: Δ, N^*, etc. A recent addition had been Ω^-.

New subject: ‘Regge trajectories’: plot angular momentum L (or spin) against mass-square. You got those mysterious straight lines, and later, – I was puzzled why it took so long before it was realised that a natural explanation of these straight lines was that rotating particles may form line-like structures – strings. And why had I not thought about it?

These tables still exist, but they now look as voluminous as lists of all chemical molecules ...

The weak force could be mimicked by the Fermi interaction, which could be explained in terms of exchange of high mass bosons. Originally, these bosons could have spin 0,1 or 2; usually spin 1 was preferred, but not well understood. In any case, the weak interaction seemed to be easier to figure out than the strong force. It was, as history would soon show.

For me, an exciting time began. At the 1970 Gargèse school for theoretical physics, the first school for graduate students I went to, I had learned about renormalization from Benjamin Lee and Kurt Symanzik. We would soon become close friends. In those days, vector particles were considered notoriously difficult, but my advisor, Martinus (Tini) Veltman had made great progress. He understood what the conditions were for vector particles to be stable and obey unitarity correctly. But this lead to equations so messy that sensible people only looked at spin 0 or $\frac{1}{2}$.

But I returned from that school knowing exactly what calculations I wanted to do. And it figured.

1971 Kurt Symanzik was the first to invite me to give a talk outside my country, DESY Hamburg. I did not understand what he was trying to calculate.

That year I made my first trip to the USA, and it was a last-minute decision to visit David Gross in Princeton. David expressed his determination to disprove quantum field theories since they have an impossible scaling behavior. I did not understand what he was doing, but I urged him to look at the vector theories.

In May 1972 I met Symanzik again, when we both stepped out of the same airplane at Marseille. He explained again what he was calculating and now I understood.

He wanted to understand Bjorken scaling, and for that, he needed what later would be called "asymptotic freedom".

I did not understand what Bjorken scaling had to do with it, but I did know that what he wanted to achieve would require vector particles. I had done that calculation several times. You needed propagator corrections, 3-point functions and 4 point functions to do the calculation. Complicated? Not *that* complicated, in particular if you used some tricks.

Symanzik had looked only at spin 0 and spin $\frac{1}{2}$. Vector particles were too messy. We had an interesting discussion in the taxi from the airport.

Symanzik gave his talk at that Marseille meeting in Aix. Now I knew by head the outcome of a calculation I had done:

After his talk, Symanzik looked at me, adding that he had not studied the spin one sector. I stood up and wrote my result on the black board:

$$\beta(g^2) = \frac{1}{16\pi^2}\big(- \tfrac{11}{3} C_1 + \tfrac{1}{6} C_2 N_s + \tfrac{2}{3} C_3 N_f \big) g^4 + \mathcal{O}(g^6)\big) \ .$$

$$SU(2): \ \ C_1 = 2\,, \ \ C_2 = C_3 = 1 \ ;$$
$$SU(3): \ \ C_1 = 3\,, \ \ C_2 = C_3 = 1 \ .$$

Symanzik gave me a sensible advice. If it was true what I had just said, I should publish it quickly. "Otherwise someone else will do it," he warned.

I should have followed that advice, but I had two problems. And I should have ignored them both: the trick I had that would explain on the spot how this works, would be needed to be added in the publication, I thought.

Now I know better.

It would suffice just to spell out the result, explaining what it means in my eyes. Secondly, I had much more work to do. My advisor Veltman had ensured me that only few people had understood my explanation of the renormlizability of the Higgs model, and with him, I should write down just a simple example showing how this worked. I should have ignored *that* advice.

Later in 1972 I met Giorgio Parisi. He had come to CERN to talk with me about strong interactions. Had I known that he was sent by Symanzik, who wanted to know whether I was still sure of the sign of my calculation, I would probably have raised the point of scaling. But Parisi was thinking of very different mechanisms, which did not have my primary interest.

After the announcement from Princeton of the discovery of asymptotic freedom of gauge theories by D. Gross, F. Wilzcek and H.D. Politzer, Parisi realised that we should have discussed more, and we would have made the discovery together. This is what I thought too. I now know that this missed chance was due to my lack of experience. I am the only one to blame.

Actually I had one more reason not to rush to produce a publication. I thought that the calculation was just a basic one, that many other people could do, and probably it had been done. But, seeing how other investigators tries to understand "quark confinement", I should have realised that the connection with scaling was basically not understood in the existing literature.

Innocently, I did write what I knew in my second paper, July 1971 (page 1):

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"A much more complicated problem is formed by the infrared divergencies of the system. Weinberg has pointed out that, contrary to the quantum electrodynamical case, this problem cannot merely be solved by some closer contemplation of the measuring process. The disaster is such that the perturbation expansion breaks down in the infrared region, so we have no rigorous field theory to describe what happens [there]."

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Iosif Khriplovich in 1969 computed the "Green's functions in theories with non-abelian gauge group" (in a heroic attempt using radiation gauge in a Hamilton formalism)

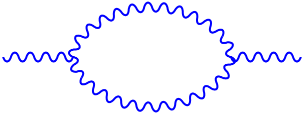
So yes, calculations had been done but the theoretical situation had not been cleared precisely.

The first time I did the calculation was late 1970, or early 1971. I had been struggling with the question of anomalies, when renormalising gauge theories. How does one prove that all anomalies cancel out? I had proven this by using an early version of dimensional renormalization, which works only for one-loop diagrams: introduce a fifth dimension.

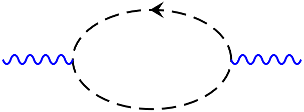
But maybe, if the amplitudes become small when you scale towards higher energies, the contributions from higher order diagrams to the anomalies would dwindle.

It is actually hard to do this right. I did do the scaling calculation up to one loop, but, as stated, he calculation was complicated.

When I repeated the calculation later, I found how delicately the results depend on the gauge fixing, but the sign was obvious. Only when I was at CERN, 1972 - 1974, I met J. Honerkamp. who was studying the background field method, introduced by Bryce DeWitt. That method makes the calculation easy; only diagrams with two external lines were needed; you can do this in a few pages.



$+3$ (electr.) -12 (magn.)



-2 (ghost)

Background method: Write the fields in the theory as a sum:

$$A_\mu(x,t) = A^0_\mu(x,t) + \delta A_\mu(x,t)$$

Gauge invariance, $A_\mu \to g\,\Lambda \wedge A_\mu - \partial_\mu \Lambda$ now holds in two ways:

$$1) \qquad A^0_\mu \to A^0_\mu\ , \qquad \delta A_\mu \to \delta A_\mu \to g\,\Lambda \wedge A_\mu - \partial_\mu\Lambda$$

$$\text{or}\quad 2) \qquad A^0_\mu \to A^0_\mu + g\,\Lambda \wedge A^0_\mu - \partial_\mu\Lambda\ , \qquad \delta A_\mu \to \delta A_\mu + g\,\Lambda \wedge \delta A_\mu\ .$$

Added up, these two "symmetries" are the same. But for gauge fixing they make a difference.
If we choose $D_\mu \delta A_\mu \ = \ \partial_\mu \delta A_\mu + g\,\Lambda \wedge \delta A_\mu \ = \ 0$ then the gauge 1) is fixed, while invariance 2) still holds.

Also the Faddeev Popov ghost is then still gauge-invariant.

This makes the renormalization counter terms gauge-invariant!
In the pure gauge sector, this leaves only one coefficient to be calculated: a simple self-energy diagram.

Qualitatively, what this calculation does to understand quark confinement is clear: the forces at large distances will augment without bound, until you keep only gauge invariant bits of matter apart. But this could not be considered as a proof of quark confinement, even though this link was immediately made by all involved.

Actually, it would just about make as much sense as the converse statement. In view of this property of asymptotic freedom, it would be impossible to understand how isolated colour charges would behave. Stating that these cannot exist would be totally reasonable.

Discovery of the jet theory (George Sterman, 1977) was very welcome, enabled us to identify elementary particles even if surrounded by gluons.

A beautiful, coherent and unique theory soon emerged. It gave employment to a large crowd of physicists, theoretical as well as experimental, and it led to many new discoveries and insights.

With the discovery of QCD as a dynamical and unique theory for the strong force, a unique picture of all forces of Nature emerged, like different members of one family:

- electromagnetic,
- electro-weak,
- strong,

and - gravitational,

They all are based on local symmetries, all to be subject to (almost) the same mathematical principles of quantum field theory.

Theoretical physics does not end here. I am not putting my money on (super)string theory, even though it is built from powerful mathematical principles. We still have work to do on the subject of pure quantum mechanics, just in order to understand better Nature's internal logic.

This was explained in the lectures I gave earlier this week in the School of "Quantum Connections".

See http://arxiv.org/abs/2306.09885

The discovery of ‘asymptotic freedom’ could now be claimed by D. Gross, F. Wilczek and H.D. Politzer.

I am happy with Frank’s comment at the end of his Nobel lecture:

> “Id like to thank Murray Gell-Mann and Gerard ’t Hooft for not quite inventing everything, and so leaving us something to do.”

I thank Frank for this remark. Their reward is richly deserved.

And thank you for listening.