Physics in the sixties

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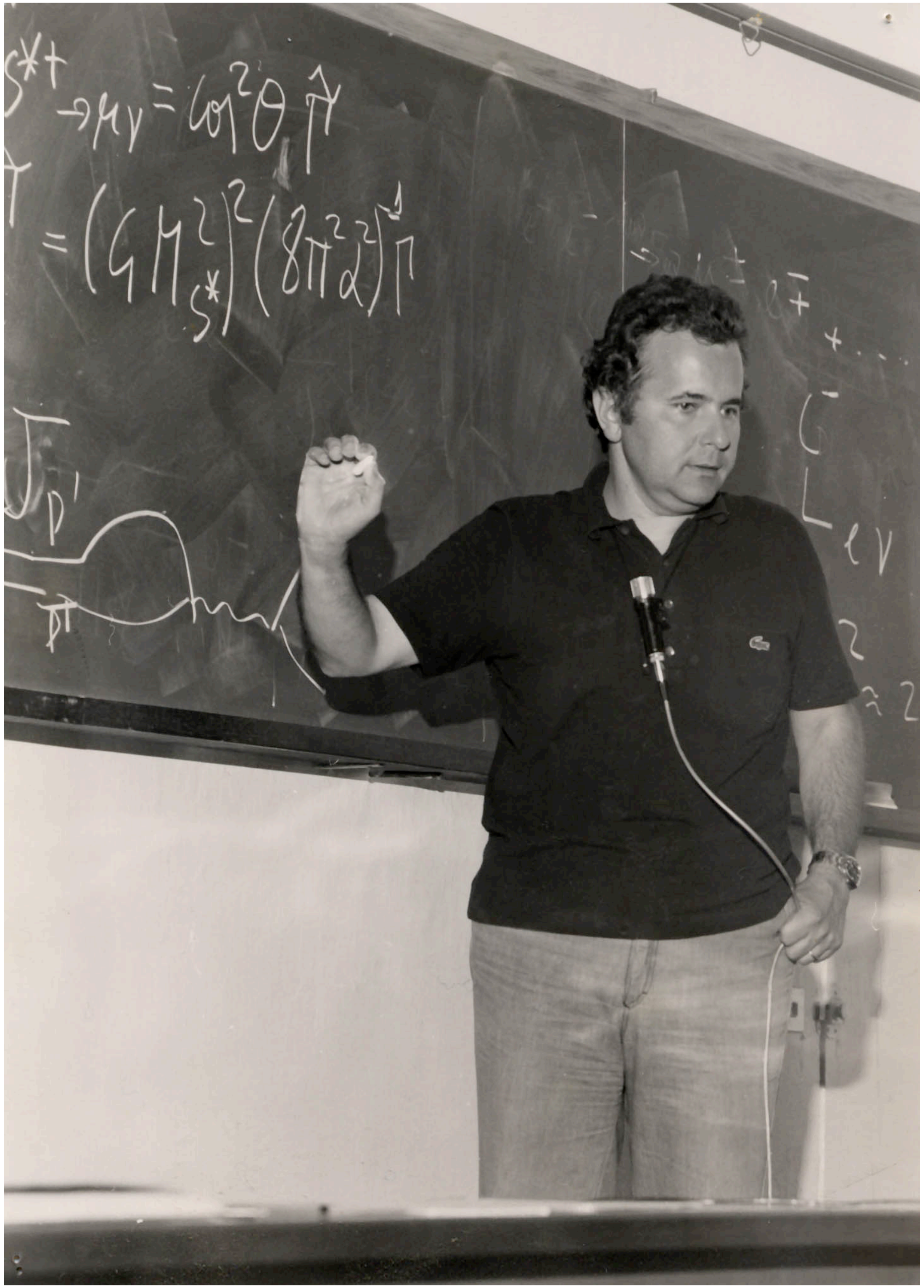
Dedicated to
Nicola Cabibbo, 1935-2010

Rome, 12 Nov. 2010



Scottish summerschool

Edinburgh, Newbattle Abby, summer 1960

Dispersion relations.

Dispersion relations were the hot topic of the time. Speakers:

Chew, Frazer, Fubini, Jackson, Jauch, Moravsik.
Polkinghorne, Thirring.

Chew was the leader of the day, and the title of his lecture shows the ambitions of that period:

"Dispersion relations and Unitarity as the basis for a dynamical theory of strong interactions"

Field theory and Lagrangians were things of the past. This was the modern view.

Students

I was a student at that school. Here are some other names:

**Alles (Bologna), Bollini (London), Cabibbo, da Costa (Bari)
Donnachie (Glashow), Fairlie (Cambridge), Glashow (CERN)
Hearn (Cambridge), Hepp (Zurich), Kabir (Calcutta)
Pagiola (Padua), Penzlin (Heidelberg), Robinson (Oxford)
Sexl (Vienna), Spearman (Cambridge)**

I remember being impressed with Nicola, because he had already publications to his name (with Gatto).

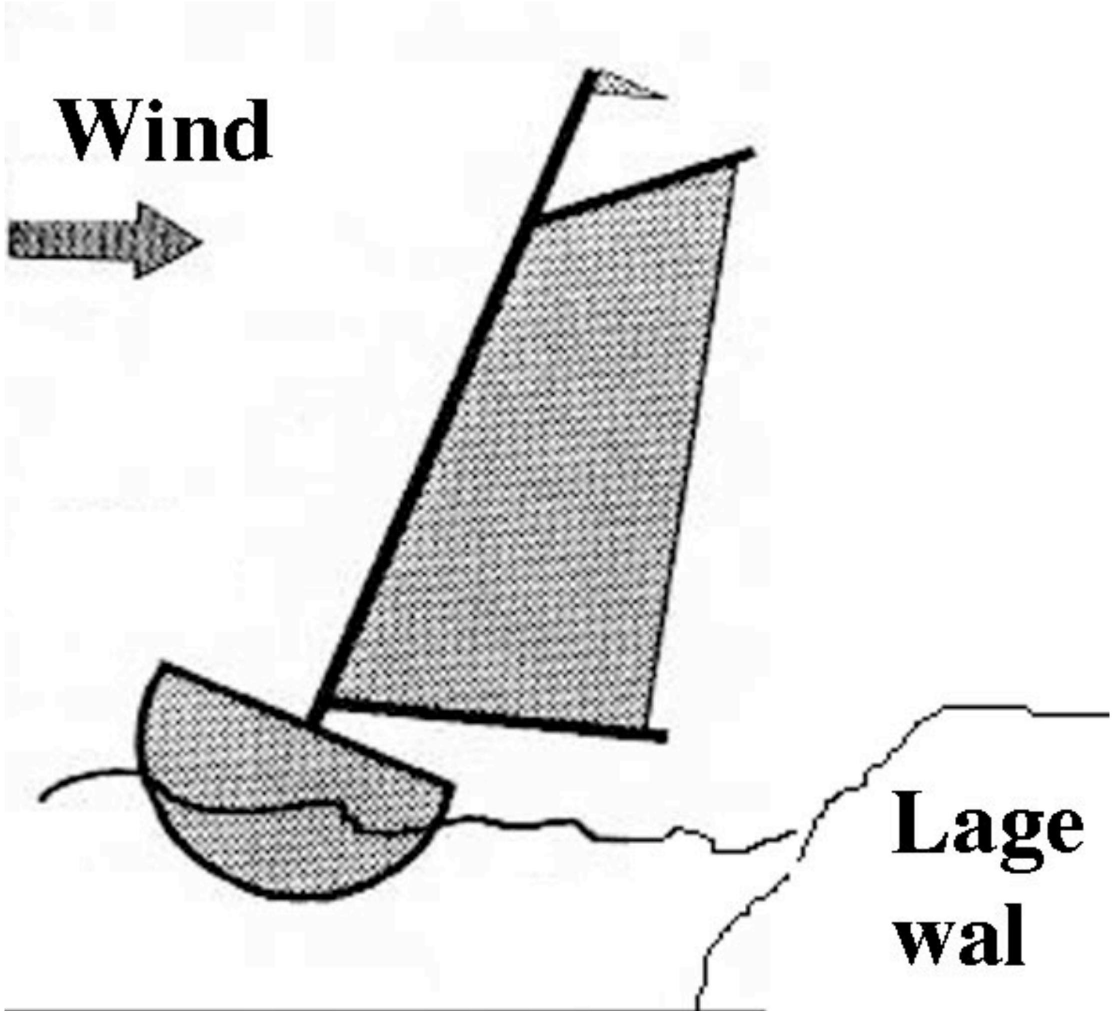
The school was very good to me. I learned a lot and some of the students that I met there, like Nicola, remained lifelong friends.

In particular, I invited Derek Robinson and Nicola to come to the Netherlands for a sailing trip on Dutch lakes.

Lage wal

On this sailing trip Nicola learned an important lessen:

Stay away from the "lage wal"



Physics

In those days people tried to prove dispersion relations for scattering amplitudes. These relations displayed the singularity structure of those amplitudes when continued in the complex plane as a function of energy etc.

For some the starting point were the ideas of axiomatic field theory. I may mention here the famous work of Lehmann, Symanzik and Zimmermann in which an axiomatic framework was proposed (the LSZ scheme). Other names were Wightman and Kallen. There were things such as Wightman functions, edge of the wedge theorem etc.

Other people tried to exhibit analytical properties on the basis of Feynman diagrams (Polkinghorne, Eden).

Lines of research

The line of research of dispersion relations continued into Regge poles, the Veneziano model, strings.

Another line of research was the calculation of radiative corrections within the framework of renormalizable theory, in particular the calculation of the magnetic moment of the muon.

Impressive because of the agreement with experiment.

A third line was the phenomenology of weak and strong interactions.

Among others SU3 was the upcoming symmetry of the strong interactions.

The knowledge of weak interactions was in terms of certain rules…

Strong interactions

Experiment had produced an enormous amount of data. Many symmetries were tried; there was Global Symmetry (Gell-Mann), the Sakata model, SU3 (Gell-Mann, Ne-eman)….

SU3, if correct, was in any case quite strongly broken. In particular strange particles (Λ, Σ, Ξ, K) were much heavier than the non-strange particles (N, P, π). Assuming SU3 was correct the question was: how was the symmetry broken?

One important part of the SU3 symmetry were the particle multiplets. In 1961 Pevsner et al. discovered the η, and for some this was seen as strong support for the SU3 symmetry.

(The Ω^- , completing the baryon decuplet, was discovered in 1964)

In 1963 the SU3 symmetry for strong interactions was by no means universally accepted. I remember scepticism expressed by Yang in a private conversation.

Weak interactions

There were three types of weak processes:

Purely leptonic (muon decay), semi-leptonic (beta decay) and non-leptonic (K decay into 2 pions) processes.

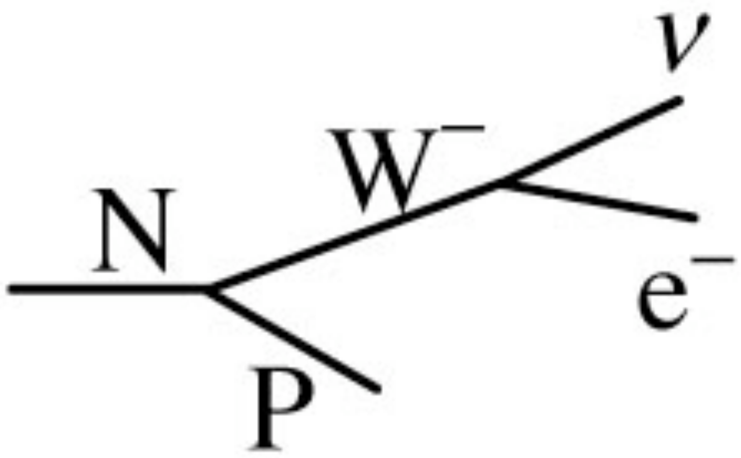
For the purely leptonic and semi leptonic processes there were the V-A, the CVC and the PCAC hypotheses.

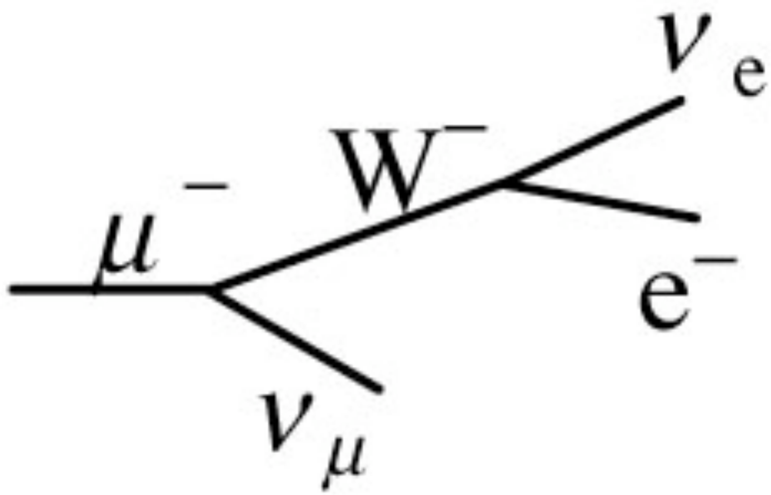
The V-A hypothesis applied to the semi-leptonic and leptonic interactions. The agreement with experiment was not very good.

In particular the vector coupling constant of muon decay was not precisely equal to that of beta decay (~6 % diff).

Most of us believed in the idea of an intermediate vector boson. Some thought that this was responsible for the difference between muon and neutron decay vector coupling constants. This is conceivable if the W is sufficiently light (< 500 MeV).

Neutron and muon decay





Propagator W: 1 / -s + M^2

For N decay: s ~ (1 MeV)^2

For μ decay: s ~ (60 MeV)^2

Thus μ decay is enhanced by an amount depending on the magnitude of M.

Selection rules

And then there were the selection rules, for strangeness and isospin (S = Strangeness, I=isospin):

The ΔI =1 rule (π → π + e + ν) for S conserving processes

The ΔI = 1/2 and ΔS = ΔQ rules for semi-leptonic S changing processes (K0 →†π + μ + ν; Milla Baldo-Ceolin, Andre Lagarrigue)

The ΔI = 1/2 rule for nonleptonic processes.

(This rule explained among others the low rate of decay of the K+ into 2 pions as compared to that of the K0: factor ~ 130, suggestive of breaking by e.m. interactions)

No one knew were these rules came from, and in particular the last rule seemed to be incompatible with the W idea (there was a paper by Lee and Yang speaking of the schizon, a W behaving ambigeously with respect to isospin).

Cabibbo theory (Apr. 1963)

Very shortly put: Nicola formulated semi-leptonic decays in an SU3 context.

Thus vector and axial currents were assigned SU3 properties (put in SU3 octets, which is the simplest possible). In addition the CVC hypothesis was interpreted as meaning that the vector current coupled like the vector current of e.m. int.

Furthermore, an assumption had to be made on the relative strength of the vector current in S non-conserving and S conserving processses. Nicola introduced an angle θ, the strengths being proportional to sin θ and cos θ respectively. We now call this the Cabibbo angle.

Such an angle was previously mentioned in a paper by Gell-Mann and Levy, but in that paper no further development of that idea or inclusion of SU3 was mentioned.

Selection rules

In addition, Nicola's paper explained most of the selection rules mentioned. Not the isospin rule for non-leptonic decays.

It should be noted that at that time the rule $\Delta S = \Delta Q$ was strongly in doubt as there was an experiment disagreeing with that. Also with respect to some of the other rules doubt existed.

Nicola went ahead anyway.

In time, experiments have come to agree with Nicola's theory.

There is one rule not explained by his theory: the $\Delta I = 1/2$ rule for non-leptonic decays.

However, after the discovery of the Ω^-, it was soon seen that the decays modes do not follow that rule.

Here an impressive table from Nicola's paper:

Predictions

Table I. Predictions for the leptonic decays of hyperons.

Decay	Branching ratio Feynman-Gell-Mann	Branching ratio Present work	Type of interaction
$\Lambda \rightarrow p + e^- + \bar{\nu}$	1.4 %	0.75×10^{-3}	$V - 0.72\,A$
$\Sigma^- \rightarrow n + e^- + \bar{\nu}$	5.1 %	1.9×10^{-3}	$V + 0.65\,A$
$\Xi^- \rightarrow \Lambda + e^- + \bar{\nu}$	1.4 %	0.35×10^{-3}	$V + 0.02\,A$
$\Xi^- \rightarrow \Sigma^0 + e^- + \bar{\nu}$	0.14 %	0.07×10^{-3}	$V - 1.25\,A$
$\Xi^0 \rightarrow \Sigma^+ + e^- + \bar{\nu}$	0.28 %	0.26×10^{-3}	$V - 1.25\,A$

Importance

Of course, Nicola's theory at once brought order in the multitude of known decays. But the importance of this paper went much further.

Perhaps the most important fact is that Nicola's paper did put SU3 for hadrons on a firm footing.

Furthermore, in 1964, at a school in Varenna Nicola told me: my scheme integrates perfectly with the quark model.

He was right. Although much noise has been made about the angle, in particular whether credit for that angle should have gone to Gell-Mann and Levy (calling it the Gell-Mann-Levy angle rather then the Cabibbo angle), it really seems to me that this is missing the point.

Actually Gell-Mann has, probably unwittingly, said so….

Brookhaven conference

In sept. 1963 both Nicola and I went to the Brookhaven conference.

I reported on the CERN neutrino experiment in which no vector boson had been seen (heavier than 1500 MeV).

This confirmed Nicola's idea that the difference between the vector coupling constant in muon decay and that in beta decay was due to a factor cos θ rather then a light W.

If I remember correctly Nicola did not talk, but Gell-Mann gave a talk on that subject.

If you look that up in the proceedings there is a surprise…

INTERNATIONAL CONFERENCE ON FUNDAMENTAL ASPECTS OF WEAK INTERACTIONS

HELD AT

BROOKHAVEN NATIONAL LABORATORY

September 9-11, 1963

Gell-Mann's talk:

"Conserved Vector Current"

by

Murray Gell-Mann

Experts have assured me that this is from Schubert's unfinished….

Unfinished…

Indeed, unfinished (by Gell-Mann). Nicola's paper is the closing part for the SU3 theory of strong interactions.

That, in my opinion, is the true consequence of Nicola's paper.

No one could doubt any more that symmetry. The subsequent introduction of quarks by Gell-Mann and Zweig was a natural development.

When I was asked for a nomination by the Nobel committee I therefore nominated Gell-Mann and Cabibbo.

However, in 1969 the Nobel committee made its first mistake (in my opinion) on this subject. The prize was awarded to Gell-Mann alone.

Let us consider how Nicola's theory fits into the quark model.

Charm

A compact description of the Cabibbo theory for quarks.

$$\begin{pmatrix} \overline{u} \\ \overline{d} \\ \overline{s} \end{pmatrix} \begin{pmatrix} 0 & cos & sin \\ 0 & 0 & 0 \\ 0 & 0 & 0 \end{pmatrix} \begin{pmatrix} u \\ d \\ s \end{pmatrix}$$

where *cos* and *sin* stand for *cos* θ and *sin* θ.

Anyone looking at this thinks: how strange, half a rotation matrix.

So let us complete it, introducing a new particle (charm quark):

$$\begin{pmatrix} \overline{u} \\ \overline{c} \\ \overline{d} \\ \overline{s} \end{pmatrix} \begin{pmatrix} 0 & 0 & cos & sin \\ 0 & 0 & -sin & cos \\ 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 \end{pmatrix} \begin{pmatrix} u \\ c \\ d \\ s \end{pmatrix}$$

Charm fits also naturally in Cabibbo's theory.