SYMMETRY AND CONSERVATION LAWS IN PARTICLE PHYSICS IN THE FIFTIES

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Août 1985

IHES/P/85/48

The "gruppenpest" that so many physicists fought so much before the war had died out in the forties. Although some famous physicists made pionnier and fundamental contributions in the theory of infinite dimensional representations of groups, the community of physicists ignored them (see note a). However, in the late forties, with the discovery of new particles: π-mesons, V-particles, there was a need to know more about conservation laws. For instance, that a spin 1 boson cannot decay into two photons was known from a short brillant paper by Landau in Russian [La 48] and by "Wigner unpublished" quoted by Steinberger [St 49] . Using field theory concepts particle physicists were eager to establish the list of selection rules for particles decays e.g. Yang [Ya 50] and Peaslee [Pe 50a] gave their proof of spin $1 \not\rightarrow 2\gamma$. In a long review I wrote in 1951 [Mi 52] , I carefully gave references on all that was known. A complete list of selection rules for angular momentum and parity conservation appeared only later [Mi 53a] ,[Wi 55] .

Then came the $\tau-\theta$ puzzle. Its history has been excellently given by R. Dalitz in this conference: obviously this was a typical case of parity doublet; Lee and Yang described it in an elegant paper[1] [Le 55] , six months before they proposed parity violation as the explanation [Le 56a]. So much has been written on the history on P violation (and some contributions to this conference are devoted to it) that here I wish only to mention some "prehistoric" relevant publications. Curie [Cu 94] in a fundamental and classic paper (in which he formulated his famous symmetry principles) explains parity conservation and the difference between axial and polar vectors in classical physics. Wigner [Wi 27] showed the importance of parity in quantum mechanics. For different particles, one may be able to define only relative parity. As explained by Racah ,[Ra 37] , because they made a different choice of what is really "relative parity" between n,p,e,ν from Fermi [Fe 34] , Konopinski and Uhlenbeck [Ko 35] found an opposite sign for some terms proportional to the neutrino mass in the β decay "allowed" spectrum.

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1) It is interesting to note that two weeks after they had sent their famous paper [Le 56a] to Physical Review they sent another one, very detailed, on their parity doublet model [Le 56b] .

Schoenberg [Sc 41] did consider the possibility of violating parity by introducing both scalar and pseudo-scalar coupling constants (that I did in my thesis [Mi 53b]; let me also mention Ferreti [Fe 41] spin zero meson theory in which the 3 members of an isospin triplet have different parity! (I used this idea in an unpublished manuscript on $\tau - \theta$ puzzle). Also, parity was sometimes violated implicitly: e.g. by Touscheck [To 48] in his theory of double β-decay or Enatsu [En 50] who explained the Vand A weak couplings (that was before some wrong experiments excluded them!) through a unique charged pair of spin one intermediate bosons! Nearly all this references were discussed in my review [Mi 52] .

In a parity violating decay, the asymmetry in the angular distribution of the decay products becomes a powerful tool to measure the spin of the initial particle. This was immediately used by Lee and Yang [Le 57] in the beautiful paper which showed that Λ^0 has spin ½. However the systematic use of angular momentum conservation and (for strong and electromagnetic interaction) of parity conservation was mainly done outside particle physics. The cornerstone is Racah's "IV The theory of complex spectra" [Ra 49] . The first three papers in 1942, 1943, barely mentioned groups theory (probably as a result of the intolerance war against it). Paper IV explains successfully hundreds of spectrum lines from few radial integrals; not only it introduces the seniority quantum number , but it uses many results on the theory of irreducible unitary representations of the classical simple Lie groups (and also G_2). All this was completely unknown to particle physicists: they learned it more than ten years when they badly needed it. However Racah's work was immediately extended by Jahn [Ja 50a] to nuclei just before the birth of the nuclear shell model (Goeppert-Mayer, Jensen). So this use of group theory among nuclear physicists culminated in the fifties.

In the same period, the general theory of angular correlation was developed mainly by and for nuclear physicists (see the historical introduction in the relevant chapter of Handbuk der Physik [De 57]). hence it is not astonishing that the simple constraint on particle polarization due to parity conservation in a two body reaction or decay, so useful in particle physics, was found by A. Bohr [Bo 58] . At last, the last year of the fifties came Jacob and Wick [Ja 59] . So it is only in the next decade and later that particle physicists have become interested by these problems (see note b).

Parity violation is accompanied by C violation (C= charge conjugation). The story of the other discrete symmetries C and T has not been as thoroughly written as that of P. (I have written on C before 1955, [Mi 85]). In 1950 some of us were asking if the neutrino was a Dirac or a Majorana particle. Meanwhile some famous and basic papers (and many more less famous, such as some of mine) were violating C and T. It is only in the beginning of the fifties that β-decay phenomenologists began to understand a twenty year old Wigner paper [Wi 32] , and included the effects of T conservation in their papers. Field theorists went deeper and unearthed the CPT theorem [Lu 54] , [Pa 55] , [Jo 57] . More generally, symmetry principles were systematically exploited by axiomatic field theory, from the middle of the fifties, mainly under the influence of A. Wightman (see his report).

In planning this twenty minute lecture (which I did not give!) I knew that for lack of time I will be more sketchy about internal (i.e. non geometrical) quantum numbers. Anyway the history of isospin has been well made by several authors and the most recent contribution [Ke 82] contains all earlier references. But I became really disturbed when I could not trace back the basic law of conservation of nucleons to the work of Dirac. Professional science historians should study the question. The appendix of this paper gives my personal recollections on this subject. It seems to me that the basic paper is from Wigner again, but as late as 1952 [Wi 52] . He asks rightly whether this conservation law is due to a gauge invariance of the first or second kind (old Pauli terminology in his paper of quantum mechanics), and he deduces non trivial physical consequences. Happily for the audience of this lecture, Wigner asks himself about the origin of this conservation law, in a long foot note that I reproduce in the appendix. Then came Pais associated production (I hope he will speak about it) and Gell-Mann strangeness: strange particles violate the rule known at that time $(-1)^{2(j+t)}=1$ where j is the spin and t the "isotopic" spin (as we used to say). With vividness and freshness Gell-Mann wrote this part of the history for the Paris Conference [Ge 82] .

At that period emerged the frame that we kept for the next twenty years: there is a hierarchy in increasing strength and internal symmetry of four fundamental interactions: gravitational, weak, electromagnetic and strong, the latter being already broken at the level of nuclear forces. Many efforts were made to guess the internal symmetry group of the strong interaction. As you know the success came only at the beginning of the sixties. The story of the success has been given by its authors Gell-Mann, Ne'eman and the less publicized Nauenberg and Speiser (1st Meeting on History of Scientific Ideas, San Felice de Guixols 1983, to appear). As rather usual, the story of the previous failures has not been made. It is one of the rare case where this history might be not very instructive. There was also the question of lepton conservation: lepton charge attributions changed several times. At the end of the period, the Brookhaven experiment taught us a deep truth: ν_e and ν_μ are different!

Seen from the present time, two important questions for the end of the fifties should have been:

1) Can local gauge invariance of electromagnetic theory be extended to the other fundamental interactions?
2) Why some internal or discrete symmetries are only approximate; are they spontaneously broken? Of course it is ridiculously easy to ask questions twenty five years too late and one could not expect that they correspond to the fashion of the period: fashion was playing and still plays a too great influence on research in physics.
   Alas Sakurai is no longer with us to tell about the paper he wrote in a prophetic style about vector dominance; but he could not explain why the Yang-Mills field was so heavy; there were other prophets.
   What strikes me now and, I say it frankly, I did not really comprehend it then, is that two of the giants were studying this very question . Up to his death Pauli worked on gauge invariance of the strong interactions. F. Gursey, whose work was then remarked by Pauli

should be invited to tell this story.

Before the war Landau [La 38] had understood second order phase transitions and had given a model for the spontaneous breaking of symmetry which might accompany them. In the fifties this became an active topic of research, but mainly in USSR; it lead to spontaneous gauge breaking in superconductivity. Thanks to Goldstone and mostly Nambu, the high energy physics community absorbed it in its culture, but only in the sixties. It was too early for having the possibility to reach success, but Heisenberg was working on unified theory (the corpus of data had been considerably enriched when Gell-Mann and Zweig independently made the bold invention of the quarks, seven years later) and he knew that the internal symmetry of his equation had to be spontaneously broken.

I know how dangerous are oversimplifications; since the seventies, symmetry plays a central role in our philosophy of unification of physics. Such an idea was alien to the physics literature in the late forties. But there was an irresistible ascent of the role of symmetries in the fifties and the attitude had completely changed: most of us then shared the enthusiasm that Heisenberg had at that time (although his theory was not fashionable).

In his autobiography "Der Teil und das Ganze" he recalls his discussion with Pauli in 1958; with the Genesis and may be Faust in the background, he proclamed:

"Am Anfang war die Symmetrie"

Note a

The first mathematical paper which gave complete series of unitary irreducible representations of a non compact non semi simple Lie group has been Wigner's paper on the Poincaré group. This paper, one of the most quoted of the century according to some citation index, was refused by several journals before to be accepted by Annals of Mathematics.

Majorana had implicitly considered unitary representations of the Lorentz group earlier [Ma 32] and Dirac later [Di 45] ; this problem has been solved completely in 1947, independently by Bargmann [Ba 47] and Gelfand and Naimark [Ge 47] . In his paper Wigner used unpublished von Neumann's results which appeared several years later. In fact he solved an even much more difficult problem, that of projective representations, since they are those exactly required by physics. This fundamental paper [Wi 39] was used by a few physicists in the fifties and was practically ignored by the community.

It is intriguing that Wigner, who had taught us that time reversal, T, should be represented by an anti-unitary operator [Wi 32] , studied unitary time reversal in this famous paper. He published the physical T version only in 1962 [Wi 62] , but taught it many years before. Some of these irreducible co-representations of the full Poincaré group correspond to parity doublets of particles. A Wightman and I described them in a fat set of unpublished but widely distributed notes in Princeton around 54-55. (This answers a question asked at the conference about a quotation in 1955 concerning these unpublished notes).

b) When the ρ and hundred of resonnances were found, after 1960, their observation through their decay products yielded some information on their polarization. Angular momentum and parity conservations impose some boundary to the domain of polarization parameters, just as the boundary of the Dalitz plot gives the constraints due to energy-momentum conservation. For usual experimental observations of spin 1 and 3/2 resonnances (e.g. ρ and Δ) the boundary of the polarization domain has been given by two of my former students [Mi 66] , [Do 67] . Together, and some times with other physicists, we studied systematically these polarization domains e.g. [Do 72a] , [Mi 74]. We even extended it to sets of reactions related by internal symmetry e.g. isospin [Do 72b] . These constraints due to an angular momentum and parity conservation were often used by European physicists (namely French); they have never been used in USA outside A. Krisch group. I am still astounded by such a lack of intellectual curiosity.

Appendix

After enjoying the first two days of this meeting it appeared to me that the oral lecture should be rather different from the "scholarly" written text prepared for this conference "On history of particles physics in the fifties". The oral lecture should be more informal and also give some personal reminiscences. It was also a good occasion to raise questions. I had also the feeling that this corresponded to the wish of the organizers. This appendix presents this oral lecture:

I first wish to thank the organizers of this interesting conference for the invitation to write a "scholarly contribution" on the history of particle physics in the fifties . This invitation also included my coming here to meet many of the active particle physicists in this period. This is a great pleasure. The invitation also give me the privilege to speak to this audience for twenty minutes. I am very honored and very happy to do it now. Because I have much fewer opportunities to speak to science historians, I will mainly address them.

For two days we heard the history of a long battle made by Man to unravel some secrets of Nature. But we heard only the accounts made by generals and commanders in chief. I hope the historians are also interested by the witness account of a low ranking officier. We form a large group, happy to do physics. We have to work, we have to sweat, but at least our life does not know a great danger: that to loose the friendship of the colleagues with whom we have made our discoveries; indeed those are not worth to make such dreadful deeds. Of course I had the happiness to make some discoveries in physics, but I had more occasions to make a permanent friendship with colleagues and this brought me even deeper joys.

We already heard a lot here on particle physics at end of the forties. However I have a strong feeling that this does not give a balanced account of how it was really. There were many more conferences than what we were told. Many of them left no proceedings. Some of these forgotten conferences were very important for the communication among physicists; remember that there were then no preprints and no long distance phone calls. It seems to me very necessary for the historians to have a compilation on all scientific conferences in this domain between 1946 and 1950, to have a list of their participants and to have an idea on what they said. My witness report will mention the first scientific conferences I attended.

By a student exchange, in the spring 1947 I was working in Blackett's laboratory in Manchester. There were two "V particles (D. Rochester told us their story yesterday). I do remember so well the seminar given there by Powell and Occhialini. This new particle, was it the Yukawa meson? Franck had another explanation of these $\pi - \mu$ tracks[1]. I wished I would understand better, so I wished to learn theory. Before going back to France, I went to see Prof. Léon Rosenfeld who had just arrived in Manchester. He agreed to receive me in his group if I could find some French financial support. I was back in Manchester in July 1948, working on some particle decays: I soon discovered that I was not the only one doing it in the world! To save money, I went hitchhiking to my first scientific conference; it was in Bristol from September 20 to 24, 1948. I remember very vividly Powell's formal night lecture, celebrating "nuclear emulsions" and the pagentry going with it. We have already heard, mainly from Perkins about the important experimental contributions which were presented.

Foot note 1. Franck [Fr 47] pointed out that the $\mu^- p^+$ bound state, neutral and very small, could induce a fusion with another hydrogen nucleus; the spectator μ^- would then receive some kinetic energy. This possibility has been observed ten years later [Al 57] .

The proceedings of this conference have been published in book form "Cosmic Radiation" Butterworths Sci. Publ. London 1949. Under the title:

"COLSTON PAPERS based on a Symposium promoted by the COLSTON RESEARCH SOCIETY and the UNIVERSITY OF BRISTOL in September 1948, now published as a Special Supplement to Research a Journal of Science and its Applications." There were only four theoretical papers: I quote from the table of contents:

PRESENT STATE OF MESON THEORY

The week before there had been a Conference in Birmingham. I think there was no proceedings (Sept.14-18). There were American physicists eg. Oppenheimmer and Pais. Indeed Møller quotes their contributions in his report[2] (and so he was probably there himself).

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2) I cannot refrain to give the quotation to Oppenheimmer "An obvious way out of this difficulty would be to say that the nucleons are not correctly described by the Dirac equation in the region of pair production and that antinucleons do not really exist".

There is not a word about the so <u>famous</u> and <u>important</u> Pocono conference which had occured few months before in the United States.

Could physicists now believe this extraordinary lack of propagation of news? Historians have to check these facts if they want to understand how particle physics research was at this period.

The only thing I can say about this situation hard to imagine to day: it was a great luck to me ! Otherwise I would have not written my first scientific paper which made my name to be used as a label for a parameter characterizing the possible electron energy spectra in μ-decay Indeed C. Møller did not mention the entensive work of Tiomno and Wheeler (presented at the Pocono conference) on this subject. But he presented that of Horowitz, Kofoed-Hansen and Lindhart [Ho 48] on the decay $\mu^{+} \to \mu^{0} + \varepsilon^{+} + \nu$ analogous to β-decay. However, since the μ^{0}-mass had probably dwindled to zero, the electron energy spectrum should be computed again. C. Møller gave two examples; in one of them a completly new feature: the spectrum was not going to zero at maximal energy.

I was very excited about "particle spectrum". Klein [Kl 48] had already pointed out that μ was an excited electron and Møller had explained in his talk that ε's were excited η's . However, back in Manchester, I first did the computation suggested by Møller on the electron spectrum from μ-decay. Why in the letter sent to Nature fews months later I did not quote him ? To day, I do not know. Probably because there was no text to quote(3) (no preprints in those times, at least in Europe).

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3) My revered teacher, Léon Rosenfeld, who had to correct a lot my first writing in English, did not remark either this omission, although Møller was his personal friend. He had made with him what was then "the best" meson theory [Mo 40] : a mixture of vector and pseudo-scalar meson geared to suppress the r^{-3} singularity of the static potential (we would explained it to day as the effect of its supermultiplet SU(4) symmetry). This theory explained the deuteron tensor forces: the pseudo scalar life time what that of the cosmic ray meson while that of the vector meson was much shorter.

At last I repair this omission to day. Some time after I had sent my first letter to Nature we received in Manchester the first 1949 issue of Reviews Modern Physics containing Tiommo and Wheeler papers [Ti 48] . There is now doubts on their priority; and no American physicist quoted my paper for five years although J.A. Wheeler, very generously, propagandized for it. I say that for two reasons. First I want to emphasize the generosity of the physicists of the generation previous to mine. To me, most of them were a model that I tried to pass to my students. Second you get the impression in any text book that the parameter was immediatly used: that was only true in Europe e.g [Le 50] [Pe 50b] and of French physicists.

The second physics conference I attended was at Edimburgh. On November 14, 1949 I was sitting besides my teacher, Léon Rosenfeld while I was hearing the lecture of N. Feather explaning that his attempts to observe the spontaneous emission of antiprotons from nuclei had failed. "Bien sur, c'est impossible!" said I to Rosenfeld . "Ah! Pourquoi ?" he answered. As far as I know, there has been no proceedings of this meeting; but there is a general photograph [3] .From it one can say that very likely Born, Darwin, Peierls, Møller, Fröhlich, Fierz, Proca, Pryce, Fuchs, Dyson, Matthews, Hamilton, Touschek, Abragam, Bloch as well as Blackett, Powell, Wilson (J.G.), Rochester, Butler, Pontecorvo [4] , Perkins etc., heard Feather's lecture. I cannot say why no one raised an objection [5] ; but I can say that later on I tried to convince Rosenfeld. When I succeeded, he advized me to write a letter to Nature. This was sent on March 7, 1950, with a copy for Feather.

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3) Niels Bohr is not on it, but my wife and I do remember that he and his wife Margrethe were at this meeting because we saw them for the first time.

4) It is less likely for Bruno Pontecorvo with whom I talked a lot, because I remember that he played several tennis games during the meeting.

5) I was much too shy myself to do it.

I was at the Paris international conference in 1950 and I remembered well the lectures by Källen and by Feynmann that I saw for the first time. Of course I was not invited at the Unesco sponsored conference held at the Tata Institute of Fundamental Research, Bombay on 14-22 December 1950. But I find very astonishing that no physicist from United States was invited: the list of invited participants is in the Proceedings. Also p.117 to 123, the contribution of N. Feather "Recent attempts to detect negative protons and dineutrons", the conclusion of the first half is "Thus all attempts to detect the p^- by experiments with low energy particles have failed. Michel has recently pointed out that just because the nuclei are stable, and do not disappear spontaneously by the n, n $\rightarrow p^+$, p^- reaction, the spontaneous emission of negative proton from a nucleus is impossible".

At the end of the lecture, Professor Rosenfeld gave a two page summary of my letter to Nature [Mi 50] . In it I spoke of "mesic charge". But this letter is complicated because it also considers the case of Majorana particles of spin ½ (in his paper [Ma 37] , Majorana thought wronghly that neutrons could be of that sort).

Feather was not the only one looking for antiprotons from nuclei. I also quoted [Sun 49] , a letter to Phys. Rev.: "On the negative Proton" and I can add now [Br 47] ,[Sc 50] .

If I had been asked few months ago about conservation law for nucleons and its consequence for antiproton production, I would have answered that this was well understood from Dirac 1931 paper. Thanks to this conference I had to read, or recollect, much conflicting evidence. Wigner's short paper [Wi 52] "On the law of conservation of heavy particles" is clear and basic; note that it does not speak of baryonic charge but of "neutronic charge" in one paragraph and "mesonic charge" in the next one. I reproduce here the first foot note, in which Wigner rises the question:

"[1] It is difficult to trace the first statement of this principle. It is clearly contained in the writer's article in Proc. Am. Philos. Soc. 93, 521 (1949), but may have been recognized about that time also by others, cf .T. Okayama, Phys. Rev., 75, 308 (1949). C.N. Yang informs me that the purpose of introducing an imaginary character to the reflection properties of certain fermions in the paper of J. Tiommo and C.N. Yang (Phys. Rev., 79, 495 (1950)) was to explain this principle. Cf. also L.I. Schiff, Phys. Rev.85, 374 (1952) and, in particular, P. Jordan, Z.f. Naturf., 7a, 78 (1952)".

Remark: [Ja 50] is similar to Yang and Tiommo.

When I wrote my letter to Nature I did not know the references [Wi 49], [Ok 49] . Only foot note 9 of the former reference deals with the subject I am discussing: "It is conceivable, for instance, that a conservation law for the number of heavy particles (protons and neutrons) is responsible for the stability of the protons".

Ref. [Ok 49] quoted by Wigner is a letter to Phys. Rev. "On the Mesic charge". For its author" it is unlikely that a negative proton exists", but he does point out a new conservation law for nucleons if they obey Dirac equation; and his remarks on charge conyugation in Dirac theory were new and right!

There were also interesting papers on conservation of heavy particles in Japan in 1951 [Na 51] , [On 51] .

After my talk, Prof. S. Oneda has given to me the relevant reference [Be 40].

I was very much interested to read in [Me 85] that in 1947, "when the proposal to build a 10 GeV accelerator" (in Berkeley) "was submitted to AEC, to reduce the cost the energy was first lowered to 5 GeV and immediately raised to 6 GeV, so that it was at least above the threshold of nucleon pair production". This corresponds much better to what I believe to have happened in history of particle physics, even if it is conflicting with what I heard, read and lived thirty five years ago! I was only a low rank officer....

P.S. Just before this manuscript was to be typewritten, I received a letter from Professor R. Dalitz. I am very grateful to him for giving me the answer to several questions I asked him. There seems to have been no proceedings of the Birmingham conference on "Fundamental Physical

Theory" (July 23 to 26, 1947) but there is a report by Pryce, in Nature on 11 Oct. 1947 p. 627-28. Similar situation for the Nuclear Physics Harwell Conference (Sept.18-19, 1947), Nature report on 11 Oct. 1947 p. 492-494. Dalitz was at the 1948 Birmingham conference (Sept. 14-18) and has from it a 40 page "Informal Report" due to Oliphant, Peierls and Moon. Oppenheimmer and Bethe attended this conference. The former gave a report on the work done in US on QED and the radiative corrections. But he did not speak on μ-decay or μ-capture. This is coherent with what I heard from Rosenfeld who was and spoke at this conference.

Acknowledgements

I am grateful to the Department of Physics of Politecnico of Turin and to the Institute for Scientific Interchange, Villa Gualino, Torino, for their hospitality. This appendix was written during my stay in Turin.

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