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Alberto Sirlin: Källén and Radiative Corrections

Towards the end of his remarkable career, Gunnar Källén became very interested in phenomenology and, in particular, in the radiative corrections to beta-decay, a subject in which I have worked for a long time.[1] I had the pleasure of meeting him at the 1968 winter school in Schladming, Austria. In that occasion I gave a talk describing some important developments that took place in 1966–67 and he gave a summary talk, where he referred to my presentation as well as other contributions. We had very nice and cordial conversations. I was shocked and greatly saddened by the news of his untimely death. What a loss for physics and for all of those that were close to him!

In order to explain the reason for his interest in the radiative corrections to beta-decay, and the very different approaches that he and I followed at the time and in later years, a bit of interesting history is useful. When Feynman and Gell-Mann proposed in 1958 the "conserved vector current" (CVC) hypothesis, as well as the V – A theory (also proposed by Marshak and Sudarshan), they proceeded to compare the coupling constants of muon-decay and O^{14} beta-decay (a superallowed $0^+ -> 0^+$ Fermi transition, where only the vector current contributes to zeroth order in α). They found a difference of about 2%. The smallness of the difference gave strong support to CVC because, without this hypothesis, one would expect a large renormalization of the vector coupling in beta-decay due to the strong interactions. On the other hand, the 2% shift suggested the possibility of a QED effect. Motivated by this observation, Toichiro Kinoshita and I on one side, and Sam Berman, a student of Feynman, on the other, proceeded to calculate the $O(\alpha)$ corrections to muon and beta decays in the V – A theory. The results presented a very serious problem: while the corrections to muon decay were finite, those for beta decay were logarithmically divergent! At first, Feynman (as well as Kinoshita and I) thought that the reason for the UV divergence was that we had not taken into account the strong interactions. A possible explanation was that the strong interactions could give rise to form-factors that would cut the high frequency contributions to the radiative corrections. If so, it was natural to think that

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the cutoff was of O(1 GeV). However, in 1966–67 a very important development took place: using current algebra and the associated Ward identities, Bjorken, and Abers, Dicus, Norton, and Quinn, reached the conclusion that the strong interactions could not tame the UV divergence of the corrections to beta-decay! Two different solutions were then proposed:

i) Cabibbo, Maiani, and Preparata, and Johnson, Low, and Suura, proposed to change the space-space part of the current algebra in such a way that the UV divergences from the vector and axial vector currents canceled each other
ii) I proposed, instead, to keep intact the current algebra and appeal to the W boson scenario. The argument was that in this scenario the corrections were divergent for both muon and beta decays, but the dominant divergences canceled in the ratio, so they could be absorbed in a universal renormalization of G_μ. There remained some subleading UV divergences, but they were extremely small numerically even for very large values of the cutoff. As pointed out by Källén, one big problem in this scenario was that the W-boson had not been discovered and consequently its mass was unknown at the time. In my Schladming lecture I explained these 1966–67 results, as well as a method I had recently developed that allows to calculate the radiative corrections to the electron or positron spectrum in beta decay in the presence of the strong interactions, provided very small terms of $O[(\alpha/\pi)(E/M_N)]$ are neglected (E is the electron or positron energy and M_N the nucleon mass).

When I began to work in the Standard Model (SM) framework around 1972, I felt that it was very important to re-examine the issue of the radiative corrections to beta decay. I argued with myself: if the theory is renormalizable and I calculate something physical, I should get a finite answer! My first step around 1974 was to consider a simplified version of the SM with integer charged quarks, neglecting again the strong interactions. In this simplified model the calculation quickly reduced to three classes of contributions: 1) one contribution was the same as in the local V – A theory with the cutoff set equal to M_W 2) a box diagram involving W and Z that changed the cutoff from M_W to M_Z and 3) diagrams that canceled in the ratio of beta and muon decay rates. The answer was clear: in the SM the cutoff in the beta decay calculation is M_Z rather than O(1GeV)! The next step was to do the calculation in the real SM, taking also into account the effect of the strong interactions. This led me to generalize to the SM the current algebra techniques I had learned in the framework of the local V – A theory. In fact, the great advantage of the current algebra approach is that it allows to control to a large extent the effect of the strong interactions and can also deal without difficulty with fractionally charged quarks. For example, one finds that to $O(\alpha)$ the complete contribution of the vector current to the Fermi amplitude in beta decay (both divergent and finite parts) is independent of the strong interactions provided one neglects again very small terms of $O[(\alpha/\pi)(E/M_N)]$. The QED corrections involving the axial vector current are controlled with less precision, but considerable progress in their analysis was attained in a letter I wrote with William Marciano in 2005. My main results in the seventies were published in a long paper "Current algebra formulation of radiative corrections in gauge theories and the universality of the weak interactions", Revs. Mod. Phys. 50, 573 (1978). The corrections to the beta decay rate are dominated by large logarithms: $3(\alpha/2\pi)\, ln(M_Z/2\,E_m) + (\alpha/2\pi)\, ln(M_Z/M_N)$, where E_m is the end-point energy of the electron or positron. In the case of O^{14}, for example, $E_m = 2.3$ MeV and the above corrections amount to 4%, a very large effect! Over the years, I introduced several refinements in these calculations, mainly in collaboration with William Marciano. I also showed in a 1982 paper that the short distance part of these corrections affects essentially all the semileptonic decays mediated by the W boson, so they are now used in several processes such as π, K and tau decays and, very recently, muon capture! On their side, nuclear physicists such as Hardy and Towner refined some nuclear corrections that enter in the analysis and expanded considerably the number of superallowed beta decays under consideration. These developments have led to a very precise test of the unitarity of the CKM matrix involving the elements of the first row. A recent update gave:

$$| V_{ud} |^2 + | V_{us} |^2 + | V_{ub} |^2 = 0.9999(6),$$

which, in my opinion, is quite an impressive test of the SM at the quantum-loop level. I find rather remarkable the fact that phenomenologically one needs very large corrections and the SM provides them in a natural manner. Putting this in a more dramatic way: if the 4% electroweak corrections were ignored, the r.h.s. would be about 1.04 and CKM unitarity would be violated by about 0.04/0.0006, roughly 60 standard deviations! It is also important to note that the V_{ud} values extracted from a vast number of beta decay processes agree very well with each other!

Returning to Källén, he recognized and emphasized the importance of obtaining finite radiative corrections in beta decays, since these are fundamental physical processes and play a crucial role in the determination of the Cabibbo angle or, equivalently, the CKM element V_{ud}. His approach, however, was very different: the effect of the strong interactions was described by the introduction of phenomenological form factors. In my view, his work in this area was interesting, as in almost everything he did, but it was superseded by the new developments, namely the emergence of the SM that essentially guaranteed the finiteness of the corrections in the presence or absence of strong interaction effects, and the powerful current algebra techniques that allow to control such effects to a considerable extent. Also, as illustrated by the discussion above, phenomenologically one needs a large cut-off, of $O(M_Z)$, to get agreement with unitarity, while phenomenological form factors would naturally lead to cutoffs of O(1GeV). I wonder what his reaction would be if he were alive today, and were able to examine the recent developments such as the precise unitarity test I discussed above.

In conclusion, I would like to say that it is very meaningful for me to look back over four decades to meet again this extraordinary physicist, who has contributed so much to our discipline!

YES to Plenty of Equations and NO to "Epsilontics" – a Preview

> "If this kind of mathematics ever becomes a fashion in physics I am going to abandon the subject."
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> Källén to Rudolf Haag (1958)

The next three chapters deal with Källén attitude toward mathematics and his only purely mathematical (not for publication) work. Actually, he "loved" mathematics more than he ever admitted. This is evident, from his correspondence, by his great joy when he had found an alternative derivation of the Bergman-Weil integral and how much he was looking forward to talking to mathematicians.

Källén was, however, much annoyed by what he called "epsilontics" in mathematics – the rigor imposed by mathematicians' beloved epsilons and deltas. In his opinion, this had an insignificant role to play in physics. If something went wrong in physical calculations, it was *most probably* due to forgetting minus signs or factors of two, etc., rather than exchanging the order in doing a sum and an integral.

After having been disappointed, because he did not achieve what he has expected in the domain of the n-point functions, he became hostile to the "axiomatic" approach in physics.