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The Schladming Schools 1966–68

In the following two Schladming Schools (1966 and 1967) Källén talked about his work on radiative correction to beta decay. In those days the prevalent model, for this process, was the point-like four-fermion interaction and the radiative corrections were divergent. It was popular to take the cutoff energy to be *very large* and by that one did *not* mean the Planck mass but the nucleon mass, roughly about one GeV! Källén's original idea was that perhaps nature provides a cutoff in these processes because the nucleons are not point-like. Therefore, one should introduce form factors, which might help remove divergences.

Actually, with his work on radiative corrections, Källén was trying to switch to research in particle physics. This field interested him very much, after having learned the subject by lecturing and writing a book about it [1]. And he was indeed a master of doing complicated calculations. Radiative corrections, with all the integrals to be done and symmetry arguments to be employed, did present enough challenge to attract him. For more information on Källén's work in this field see the article by Alberto Sirlin in Part 4.

By the time of his last Schladming School Källén had made a transition into a new area of particle physics: current algebra. This new field was giving wonderful new results and what is more they could be compared with experiments! The equal-time commutators of currents were the main players and in some cases the postulated commutators led to inconsistencies which in turn required modifications by addition of extra terms. Generally, these terms were referred to as the Schwinger terms, a terminology that Källén detested as the existence of such terms had been noted by Goto and Imamura four years before Schwinger. In Schwinger's defence it should be said that he, in a one page article [Phys. Rev. Lett. 3 (1959) 296], gave a very simple and elegant example of how such terms arise due to singularities.

This was very typical of Källén. For him, the credit was to be given to the discoverers and not to famous people who did it later, at times much more elegantly and perhaps understood better what was going on. Schwinger terms were for Källén gradient terms, Mandelstam kinematic variables had been invented by Møller, Källén – Lehmann representation was due to Kamefuchi

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and Umezawa – Lehmann's role being that he, several years later, gave a pedagogical summary, etc.

I (CJ) was present at the 1966 Schladming School. I noted that Källén was very kind to students but would hardly speak to Francis Low, a distinguished theorist from MIT. Low, also, was surrounded by a cloud of students but avoided Källén. Incomprehensible as it was to us students, we didn't mind at all. We could talk to both of them. At that School there were several contributions suggesting the relevance to physics of groups with many generators and complicated classification schemes [such as SL(6,C)]. It was obvious that Källén didn't believe any of it. Fortunately, later, all those monstrous constructions disappeared from physics scene.

Källén Recalls a Casimir Anecdote

At his last Schladming meeting, Källén must have been in a very good mood. There had been a talk on the decay modes $\eta \rightarrow$ neutrals, i.e. a neutral particle decaying into neutral particles. This talk inspired Källén to tell the following story [2]:

> "I'll close this evening with a little anecdote: When I was a young student there was a meeting in the late 1940's in Copenhagen, and at the end of this meeting there was a joking summary made by Casimir[1] – as you know this was in the days when everybody was very excited about the existence of two different kinds of mesons (π and μ), new counting techniques etc. – and in this summary Casimir was making fun of all the techniques, of course, and his biggest joke was the following: He showed an absolutely blank slide, and then said: 'Here you see a really exciting thing: one neutral particle decaying into two other neutrals'. And, of course, everybody was laughing very heartily in those days. I believe, if people had been able to look 20 years ahead and know that the experimentalists 20 years afterwards would have the impertinence not only to discuss the decay: one neutral into two neutrals, but actually to discuss the branching ratios between the three different neutral modes in the decay of one neutral particle, they would have been really impressed."

[1] Hendrik B. G. Casimir (1909–2000) was a well known Dutch physicist. He had been an assistant of Pauli 1932–1933. Weisskopf once told the following story about him and Pauli. Pauli was driving 'like mad'. Casimir sitting next to him had expressed his dissatisfaction with his driving. This had prompted Pauli to say: if you criticize my driving, I'll criticize your physics. I (CJ) had the honor of meeting Casimir and attending a talk by him at the 1983 meeting of the Norwegian Physical Society. In his talk he said that one of the biggest puzzles in physics was: why the ratio of the masses of the proton and the electron is about 1836. I was very surprised.

By the time of 1969 Schladming School, Källén had passed away. Paul Urban honored his memory by presenting a detailed account of his scientific achievements and his close relationship with the School [3].

References

1. G. Källén, "Elementary Particle Physics", Addison-Wesley (1964)
2. G. Källén, *in* Proc. 1968 Schladming School, Acta Phys. Austriaca Suppl. V (1968) p. 503
3. P. Urban, "In memoriam Professor Gunnar Källén", Proc. 1969 Schladming School, Acta Phys. Austriaca Suppl. VI (1969) p. VII

At 1961 Solvay Conference – a Preview

> "The real problem is: *why is nobody solving anything*?"
>
> R. P. Feynman
> (Solvay Conf. 1961)

At the 1961 Solvay Conference on "The Quantum Theory of Fields" (9–14 October 1961, Brussels) Niels Bohr gave the opening talk, with the title "The Solvay Meetings and the Development of Quantum Physics". First of all he noted that:

> "The careful recording of the reports and of the subsequent discussions at each of these [Solvay] meetings will in the future be a most valuable source of information for students of the history of science wishing to gain an impression of the grappling with the new problems raised in the beginning of our century. . . ."

Then, in his long and detailed talk, he recalled several such problems. Källén was present at this meeting, and presented a talk, which was followed by a lively discussion. The following chapters in this Part are devoted to his talk and the response of his distinguished audience.

Källén was also very active at the final discussion session of the Conference, where the main theme could be summarized by: "*The Battle of Field Theory and S-Matrix Theory*". He was the prime critic of the S-Matrix approach, advocated at this meeting by Geoffrey F. Chew and Stanley Mandelstam.

A few years later, Källén participated and gave a talk at the 1967 Solvay Conference. His talk is listed as paper [1967c] on his list of publications in Part 5 of this book.

Figure 57.1 Källén, Abdus Salam and Rudolf Peierls listening to Oppenheimer at the 1961 Solvay Conference (Courtesy of Kristina Källén)