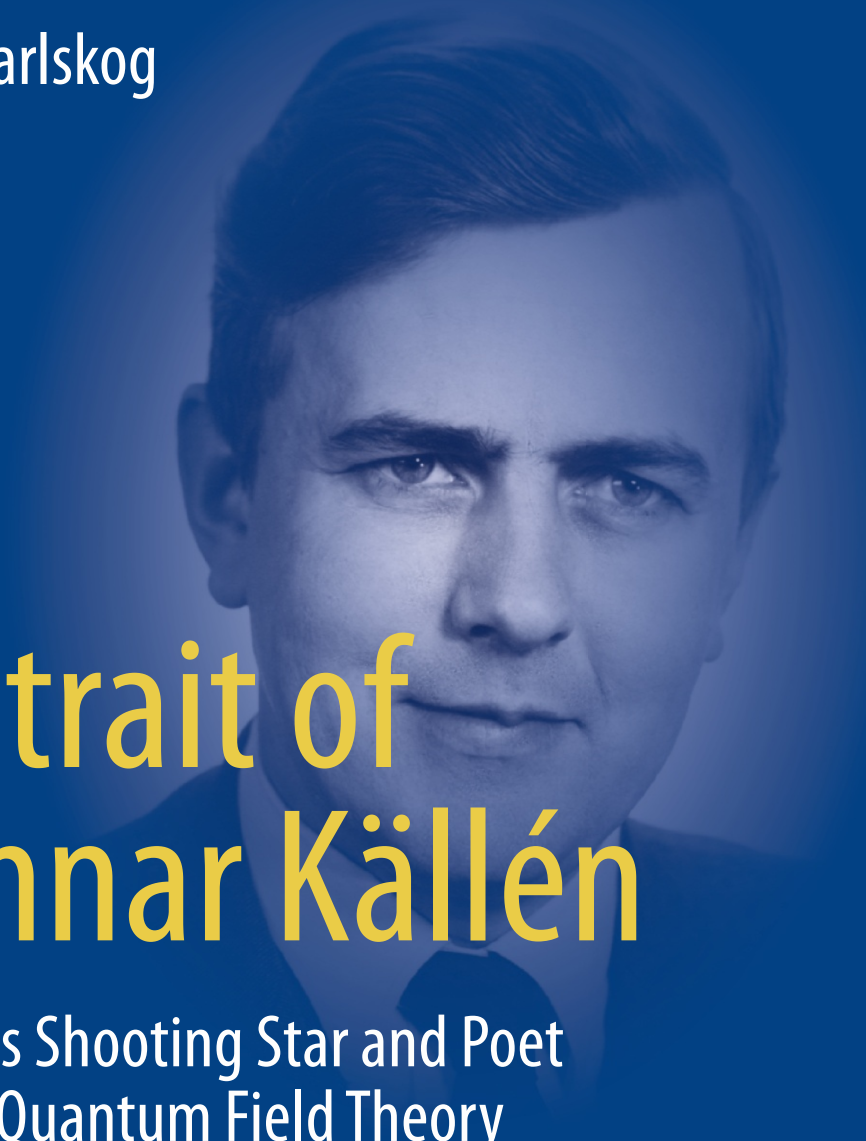
Cecilia Jarlskog
Editor



Portrait of Gunnar Källén

A Physics Shooting Star and Poet of Early Quantum Field Theory

Springer

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(Editor)

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The Last Picture of Gunnar Källén,
taken on the "order" from his wife, as he put it

Wolfgang Pauli referred to him as "my discovery", Robert Oppenheimer described him as "one of the most gifted theorists" and Niels Bohr found him enormously stimulating. Who was the man in question, Gunnar Källén[1]? This book attempts to give at least a partial answer to this question.

In addition to being a remarkable scientist, Källén had a very interesting personality, well worth knowing. His appearance on the physics sky was like a shooting star[2]. His scientific interventions caused excitement among young and old. He was not intimidated by anyone and a master of responding in kind, even to Pauli.

Alas he died much too soon, at the age of 42, in a plane crash near Hannover, Germany.

He deserves to be remembered.

[1] Källén's, for English speakers difficult last name, can approximately be pronounced as Shellen, with accented last syllable where the "e" is pronounced as in bet or peck.
[2] This remark is due to the distinguished Danish physicist Christian Møller (see Chap. 65).

Preface

In theoretical physics, history can be largely unfair. In times when there are urgent problems to be solved, it quickly creates a number of "great scientists" and immortalizes them, crowned with glory, in its books. This was certainly the case during the first decades of the 20th century. There were experimental puzzles crying to be solved and in a relatively short time the remarkable fields of special relativity and quantum mechanics were born. Many were those who went to history as great scientists, by contributing to different aspects of this revolution. Without hints from nature, it is very difficult for a theoretical physicist to "show the whole world" how great he/she is, even if he/she were the smartest and most creative scientist of his/her time. Creating a new wave, without experimental hints, is a tremendous challenge. The best (and only?) example is the creation of general relativity, where Albert Einstein "single-footedly" climbed up all the way to the top of the ladder of fame by single-handedly proposing his theory of general relativity. But he could *afford* it, as he was already at the top due to his other contributions which had been prompted by experimental observations. One may wonder what would have happened if Einstein had *only* postulated his theory of general relativity and nothing else. Had the scientific community noticed it and cared enough about it to send expeditions to Brazil and Africa to check his "speculations"?

Källén sometimes expressed his regret for having been born "too late", as he put it. He had come to Lund University in 1948 as a full-fledged 22 year old electrical engineer who wished to re-orient himself toward theoretical physics. In no time at all he had acquired an incredible amount of knowledge, as it is testified by his very first paper published already in 1949. Indeed, Källén was born in 1926, while the founding fathers of quantum electrodynamics, with whom he compared himself, had been around years before he appeared on the scene. They had almost done it all! Years later, his four years older collaborator, from Princeton University, Arthur S. Wightman said about him:

> "At that time I was trying to puzzle out the grammar of the language of quantum field theory, and here was KÄLLÉN writing poetry in the language."

Here below is a more extended version of Wightman's description[3] of Källén:

> "Gunnar Källén's death in an airplane accident, October 13 at Hannover, Germany, is a great personal loss to his many friends all over the world, a loss to research and education in Scandinavia, in which he played an important role, and a loss to theoretical physics where his contributions are well known. Gunnar Källén was a proud continuer of the tradition in quantum field theory established by Wolfgang Pauli[4]. His papers on quantum electrodynamics in the period 1950–1954 carried the non-perturbative approach to quantum electrodynamics forward to a point beyond which very little essential progress has been made up to the present day. I still remember the impact of the Helvetica Physica paper of 1953. At the time I was trying to puzzle out the grammar of the language of quantum field theory, and here was KÄLLÉN already writing poetry in the language!
>
> In 1960's Källén spent an increasing fraction of his time on the phenomenology of elementary particles. A by-product of this was his book "*Elementary Particle Physics*". A typical remark about the book was: "That is the book on elementary particles the experimentalists really find helpful". Those of us who knew him expected that.
>
> Källén's unflinching adherence to what he thought was essential and true in theoretical physics was combined with personal cheerfulness and friendliness."

As a second year graduate student from Lund University, Källén was sent to Zürich to attend Pauli's lectures in the 1949 summer session at ETH (Swiss Federal Institute of Technology, a prestigious institute for higher education and research in Zürich). This event staked out Källén's research path for years to come. It was also the beginning of a most fruitful interaction between Källén and Pauli, for almost a decade until Pauli's death in 1958. They were mutually attracted to each other. Perhaps Pauli saw in Källén an image of himself as a young man. Källén found Pauli's approach to physics, his strong opinions, his wit and sharp tongue quite similar to his own. They enjoyed each other's company.

[3] Wightman's entire article is published in Communications in Mathematical Physics, Volume 11, Number 3 (1968–1969) pages 181–182. See also Chap. 64 and 78.
[4] Wolfgang Pauli will be introduced in Chap. 3.

Källén's scientific work can largely be grouped into four main categories, namely:

(1) **Quantum electrodynamics and renormalization**, without using perturbation theory: It was in this area that the young Källén demonstrated his legendary ability to grasp difficult issues quickly as well as his mathematical power and originality. His work placed him in Julian Schwinger's "Hall of Fame of Quantum Electrodynamics". In the literature, his name appears in the Källén-Yang-Feldman formalism, Källén-Lehmann representation and Källén-Sabry potentials. He would have been pleased to see that his "potentials" are used even now, after more than half a century, within a *broad* spectrum of applications whenever precision is required, such as in quantum chromodynamics, Z-decays, atomic transitions, and exotic atoms.
(2) **The Lee Model**: The joint Källén-Pauli 1955 paper on the mathematical structure of T. D. Lee's model of a renormalizable field theory added richly to the understanding of a certain class of field theories. The Nobel Laureate **T. D. Lee**, (together with R. Friedberg) has written a special article for this book to honor Källén's memory. Why Källén was at all interested in the Lee Model is described in a chapter in this book by another Nobel Laureate, **Steven Weinberg**, who also gives his views on the future of quantum field theory. Weinberg considers himself as one of Källén's "disciples".
(3) **The *n*-point functions**: Källén devoted several years to the study of these functions, which are the vacuum expectation value of the product of *n* scalar fields. He was hoping that this novel approach would help him resolve the issue of whether quantum field theory (specially quantum electrodynamics) is a consistent theory. This was a topic close to his heart, as he expressed it himself. The challenging mathematical beauty and complexity of the *n*-point functions appealed to him. Källén loved to solve difficult problems. Pauli disapproved of Källén's involvement in this field of research and warned him that he was wasting his time. Källén disagreed but a few years later he got very disappointed that this line of research did not lead to progress in *physics* that he had anticipated.
(4) **Radiative corrections in weak interactions**. After having written his book "Elementary Particle Physics", published in 1964, Källén started doing research in this field and for a few years worked on radiative corrections to neutron beta decay. In this volume, **Alberto Sirlin**, a pioneer and expert in this field, gives a status report and discusses Källén's work.

In addition to his scientific heritage, Källén left behind a substantial number of "**disciples**", many of whom became university professors.

The purpose of this book is to present Källén, who was one of the shining stars on the physics sky of 1950's and 1960's. The reader is invited to get to know his unusual personality and become acquainted with some aspects of the history of our science in those days, as related by him and those who corresponded with him. In addition, a selection of his most important and not easily accessible papers is included, for the specialists to enjoy. If the reader is interested in any of his other papers he/she is invited to contact me (CJ) as I have a complete collection.

Acknowledgements

This book consists of five Parts. The first four contain material of both biographical and scientific nature. In them, there are chapters written to be comprehensible to anyone who wishes to know more about Gunnar Källén and his time. The fifth Part is intended for specialists, as it includes a selection of Källén's articles as well as commentaries that give more information about his work.

As everyone who has ever produced a book perhaps knows, but I (CJ) didn't realize is that producing a book such as this one takes a huge amount of time. I would not have started this enterprise if I had not been reminded, on several occasions, by Professor André Martin at CERN that "someone" should do "something" so that Källén is not forgotten. Thereby, he gave me such a bad conscience that I felt that I had no choice but to plunge into "doing something" and do it as quickly as possible. Indeed, the number of people who personally knew Källén is on constant decline and even some of his students have already passed away.

My most profound thanks go to Professor Steven Weinberg who came to Lund to give the 2009 Källén Memorial Lecture and for his article included in this volume. I am also indebted to Professor T. D. Lee who (together with R. Friedberg) has written an article for this book, to honor the memory of Gunnar Källén. Special thank go to Professor Alberto Sirlin for his assessment of status of radiative corrections in weak interactions and Källén's contributions to this field.

This volume has been further enriched by contributions or comments from Herbert Abels, Sture Allén, James D. Bjorken, Anders Bojs, Karl-Erik Eriksson, Ludwig D. Faddeev, Herbert M. Fried, Stephen Gasiorowicz, Nico van Kampen, Benny Lautrup, Poul Olesen, Afaf Sabry, Raymond Stora, Bengt E. Y. Svensson, Martinus J. G. Veltman and Antonino Zichichi. I wish to express my deepest gratitude to them all.

Special thanks go to Källén's four children and his brother Bengt Källén. Källén's children, Erland, Kristina, Elisabeth and Arne, for their support of this project. Erland – himself a private pilot – upon being asked immediately agreed to write an article for this volume, on the fatal plane accident that took

his father's life. Professor Bengt Källén has contributed with a precious article about his brother.

I have found almost all the material for producing this book in Sweden – primarily in Lund. I would like to take this opportunity to thank the members of the Manuscripts & Archives Division of the Lund University Library for their hospitality during the countless hours that I spent at their reading room. I am also indebted to the staff of the Center for History of Science at the Royal Swedish Academy of Sciences (Stockholm) as well as that of the Royal Physiographic Society (Lund) for their kindness and for letting me go through their precious archives. I have also benefited from visits to the Central Library at CERN.

Finally, sincere thanks go Linda Jarlskog for her gratuitous artistic work - photographing portraits at the home of Arne Källén, as well as scanning and retouching pictures supplied by Kristina Källén, presented in this volume.

Last but not least I wish to thank Jens Vigen, the Head of the CERN Library, whose enthusiasm and good advice have always impressed me. I am also indebted to Christian Caron, the Executive Editor at Springer. I can't imagine a more positive and helpful Editor than he.

Lund, January 2013 Cecilia Jarlskog (abbreviated by CJ in this book)

To the Readers

For the benefit of the readers, this book starts by giving a bird's-eye view of how the material in this book has been organized.

Part 1: Youth, Career, Personality, Legacy, Family and Fatal Accident is primarily of biographic nature and gives an overview of Gunnar Källén's life from childhood to his untimely death at the age of 42. Here, the reader gets acquainted with circumstances which made him into the scientist that he became, among them how he was sent to Wolfgang Pauli's "Court" in Zürich and the impact of this sojourn. I describe his academic career and how a personal professorship was created for him at Lund University – an utmost rare event in those days that had to be approved by the Swedish Parliament. Afterward we follow Källén to his last conference in Vienna, to which he flew piloting a small plane. His tragic death a few months later, while flying to a meeting at CERN, is described by his son Erland.

Part 1 also scrutinizes Källén's personality, his likes and dislikes, and issues such as the origin of his grudge against Julian Schwinger. I describe Källén's genuine interest in the education of young people and his kind fatherly attitude toward them. The reader is also invited to meet a person of utmost importance in Källén's life – his wife Gunnel. The two made a lovely couple and were manifestly devoted to one another as well as to their four children. They used to sign their names together in the form **Gunn**$^{\mathbf{el}}_{\mathbf{ar}}$.

Part 2: Correspondence with Pauli, Heisenberg and Dirac; Källén in Action dives deeper into a discussion of Källén's unusual personality and his views about science. The material in this Part is largely based on his correspondence (found in Lund) with Wolfgang Pauli, Paul Dirac and Werner Heisenberg. Some of his collaborators are presented in this Part, together with his opinion about them and theirs about him. The reader will find Källén in action, giving talks at conferences and schools. Källén was an exceptionally interesting person on such occasions. His sharp intellect and speed with which he did complicated mathematical computations were legendary, as were also his sharp tongue and lack of respect for authority. Moreover, he "enjoyed a fight", as he put it himself, and the students even more. To see the giants of theoretical physics fight over scientific issues meant that the topics discussed must have been captivating and of utmost importance. This by itself was a source of inspiration. Who was right or wrong was a secondary issue to the students who usually didn't understand what was going on.

Part 3: Promotion of Science in His Honor gives a very brief introduction to the Royal Physiographic Society in Lund and the Royal Swedish Academy of Sciences in Stockholm as well as the Ettore Majorana Center at Erice, Sicily. This Part describes what these promoters of science have done to honor Källén's memory. The next item presented is the written version of a "Källén Lecture" by Steven Weinberg, held in Lund under the auspices of the Royal Physiographic Society. The title of the lecture was "Living with Infinities".

In the second half of 1950's, Källén, as well as several other prominent scientists, spent much effort to understand the renormalization aspects of a soluble model proposed in 1954 by T. D. Lee. In return, now more than half a century later, T. D. Lee honors Källén's memory by a special article for this book, written together with R. Friedberg. The article, "A Soluble Model of 'Higgs boson' as a Composite", constitutes the last chapter of Part 3.

Part 4: On His Scientific Work sets the scene for the presentation of a selection of Källén's scientific work. The bulk of the material presented in this Part originates from Källén's correspondence at the Lund University Library. The reader finds Källén proudly informing Wolfgang Pauli of his achievements. His studies of non-perturbative renormalization in quantum electrodynamics lead him to conclude that this theory is inherently sick. This achievement places him in the "Hall of Fame of Quantum Electrodynamics". As is often the case, such a remarkable work prompts controversies. We examine how he deals with them. Afterwards, we follow him through his Lee Model period into his more mathematical work on the *n*-point functions, as well as his subsequent transition into particle physics phenomenology. In a special chapter in this Part, Alberto Sirlin gives his views on Källén's work in this domain.

In spite of the fact that Källén often expressed misgivings about the role of stringent mathematics (what he called "epsilontics") in physics, he loved *applied* mathematics and was very good at it. Wolfgang Pauli considered him to be superior to himself, as we shall see later! In Källén's correspondence there is a manuscript with the title "A Connection Between the Bergman-Weil Integral and the Cauchy Integral". It is marked *not for publication*. There can be no doubt that Källén enjoyed this work of his very much. He sent the manuscript to several people, among them some mathematicians, asking for their comments. This unpublished article is presented in the last chapter of this Part. After all, according to the current rules, reproduction in a book *does not* count as publication.

The **Part 5: Papers and Commentaries** includes a selection of Källén's papers. Of course, these can only be understood by specialists. Nowadays, in the academic circles, the specialists have in general easy access to articles published in many journals – a few clicks is all that it takes! Therefore, primarily articles that are *not* easily available, at least not yet, are included in this Part, for example, his papers published in the journal of the Royal Danish Academy of Sciences, "Matematisk-Fysiske Meddelelser (Det Kongelige Danske Videnskabernes Selskab)". In addition, commentaries are given on several of Källén's other articles that due to page limit could not be included. The purpose is to briefly inform our interested readers about their contents.

In producing this book, much effort has gone into making it as easy-to-read as possible. Therefore, there are some repetitions in the presented material – this for the benefit of those readers who do not read this book from cover to cover. We apologize to those who do and hope that they may find consolation in the words of the Danish philosopher Søren Kierkegaard:

"Repetition is the reality and the seriousness of life".

Contents

Part 3
Promotion of Science in His Honor

Part 4
On Källén's Scientific Work

Part 5

Publications, Selected Papers and Commentaries

Invited Contributions

This book contains invited contributions by the distinguished authors listed below, all of whom knew Gunnar Källén personally:

- **K-E. Eriksson** (Chalmers University of Technology, Gothenburg, Sweden): Some Reminiscences of Gunnar Källén's Role in Sweden
- **B. Källén** (Lund University, Lund, Sweden): The Childhood and Youth of Gunnar Källén
- **E. Källén** (Stockholm University, Stockholm, Sweden and European Centre for Medium Range Weather Forecasts in Reading, England): My Father's Passion for Flying and His Mortal Accident
- **T. D. Lee**, together with R. Friedberg (Columbia University, New York, USA): A Soluble Model of "Higgs boson" as a Composite
- **A. Sabry** (Egypt): How it Was to Work With Prof. G. Källén
- **A. Sirlin** (New York University, New York, USA): Källén and Radiative Corrections
- **R. Stora** (CERN, Geneva, Switzerland): Källén's Constant *M*
- **B. E. Y. Svensson** (Lund University, Lund, Sweden): Some Reminiscences of Gunnar Källén as Supervisor
- **S. Weinberg** (university of Texas, Austin, Texas, USA): Living With Infinities

Major Sources of Information and References

The *most important* source of information in this book has been the "Gunnar Källén Collection" at the Manuscripts & Archives section of the Lund University Library. It consists of 18 "boxes" that were deposited there after Källén had passed away. They contain his correspondence as well as other documents, such as scientific articles, reviews and referee reports, from late 1958 to October 1968. Only his correspondence with Wolfgang Pauli extends further back in time, to 1949. Here, this archive is referred to as the "**Källén Collection**".

Källén became a professor in Lund on 1 August 1958. He and his wife designed their new home and the family moved to Lund in the spring of 1959. In Lund, from September 1959 until his death, Källén had a very competent secretary, Ms Margareta Bergsten, who would archive his papers and correspondence. The fate of his earlier correspondence is not known. The disruption caused by his sudden death, followed shortly after by that of his wife, could have been the reason for this unfortunate situation. The material in the Källén Collection is in several languages, in addition to English, primarily in German and Swedish but also to some extent in French and Danish. Unless specified otherwise, the reader can take for granted that any letter in this book, from or to Källén, stems from this Collection.

Another frequently used source of information in this book is the monumental collection of Wolfgang Pauli's correspondence (Wolfgang Pauli, Wissenschaftlicher Briefwechsel ...) in several volumes, edited by Karl von Meyenn. It constitutes a true goldmine for those interested in the history of physics, especially during the first half of the 20th century. In addition to Pauli's correspondence it also contains some related letters. This "**Pauli Collection**" is an invaluable source of information about Källén, such as Pauli's opinion about him that he transmitted to others. The letters in these volumes are conveniently labeled in the form [number], where for example [1234] denotes letter number 1234 in the Collection. The Pauli Collection is primarily in German. However, letters in English have been reproduced in the original version but, for example, letters in Danish have been translated into German, actually sometimes by Källén. Whenever the reader encounters a reference of the form "[number] in the Pauli Collection" the source is the above volumes edited by von Meyenn.

The originals of Pauli's letters to Källén are in Lund, together with copies of Källén's letters to Pauli.

Throughout this book, I (CJ) have been responsible for all the translations (from German, Swedish, Danish and French) into English.

For more detailed information about the above Collections, the reader may consult Appendix I-A, at the end of Part 1.

Biographies

The names of a large number of physicists appear on the pages of this book, primarily those whom Källén encountered on his scientific path. They are presented only briefly to the readers because nowadays a great deal of biographical material is available on the internet, though not all can be trusted and sometimes one finds glaring errors in them. Therefore, we specifically indicate the Nobel Laureates simply because their autobiographies are easily accessible on the internet and are generally trustworthy. Some other sources of information about people are Wikipedia, CERN Courier, Array of Contemporary American Physicists as well as various encyclopedias.

Reprints and Quotations

This book contains reprints of a number of Källén's articles and quotes from proceedings of conferences and schools. We wish to thank the people listed below who, on behalf of their organizations, have given us the permission to include them in this book. The relevant journal or conference is indicated inside the square brackets. Sincere thanks go to:

- Karl-Fredrik Berggren, President of the Swedish Physical Society [Kosmos];
- Kirsten Hastrup, President of the Royal Danish Academy of Sciences [Mat. Fys. Skr. Dan. Vid. Selsk.];
- Marc Henneaux, President of the International Solvay Institutes [Solvay Conferences 1961 and 1967];
- Levente Istvan Koltai, Head of Rights & Permissions [Springer-Verlag, Acta Physica Austriaca];
- Christophe P. Rossel and Ulrich Straumann, President and Vice President of the Swiss Physical Society [Helvetica Physica Acta];
- Jens Vigen, Head of CERN Library [1956 CERN Conference];
- Roger Wäppling, Chief Editor at the Royal Swedish Academy of Sciences [Arkiv för Fysik and Physica Scripta].

Part 1

Youth, Career, Personality, Legacy, Family and Fatal Accident

Preface

> "I wish to thank you very much that you have sent to me Dr.[1] Källén, who turned out to have great skill and talent. He is working both very quickly and [is] very reliable."
>
> Pauli to Gustafson (1949)

> For every error Källén makes I am allowed to make two.
>
> Harry Lehmann's "Grand Theorem" translated from German (1955)

This Part is mainly about the life and personality of Gunnar Källén. He is introduced by his brother as well as by himself, and his unusual personality is exhibited through his correspondence.

First we follow him on his academic path and introduce those who helped him to get started. Then, we dive deeper into a description of his personality. As an example, we look into the issue of his seemingly hostile attitude toward Julian Schwinger.

We describe his status on the international scene and how he was perceived by his contemporaries. The legacy he left includes not only his scientific achievements but also his "disciples" some of whom, perhaps without realizing it, were much influenced by his strong character and his demand of honesty and rigor.

In the last chapters of this Part, Källén's wife Gunnel is introduced. Finally, Källén's oldest son describes his father's passion for flying and his tragic fatal accident.

[1] Källén was a second year graduate student and not yet a Dr.

1

Bengt Källén: The Childhood and Youth of Gunnar Källén

In the middle of the 19th century, most people in Sweden worked on farms.[1] When industrialization began, new demands arose, among other things a need for a more systematic education of the population. In 1842, a general school ("folkskolan") was introduced which developed into a seven year compulsory school and some bright farmer children continued their education. One of these was Gunnar's paternal grandfather, Anders Persson (the son of Per) who came from a farmer family who for generations had cultivated land on an island east of the Swedish mainland, Öland, at a place called Källa (Swedish for a well). When Anders started his higher education, he changed his family name to Källén based on his place of birth. He ended up as a high school teacher. He married a girl who came from a different background with members from the clergy, military and law sections of the society. Anders died young, leaving his wife with two young sons. Despite these odds, both sons managed to get higher educations and both became teachers.

Anders Olof Gunnar Källén was born on the 13th of February, 1926 in Kristianstad, a small city in the South of Sweden, as the second child to the younger of Anders' two sons, Yngve, who was a teacher in mathematics and physics. His wife Karin was also a teacher, in Swedish and history. According to the customs at that time, Karin stopped working as a teacher when she had children. There was an elder child in the family, Margit, born in 1924. As an infant she had severe encephalitis which resulted in a moderate permanent brain damage. In 1929 the family was further extended by the birth of Bengt, the author of these lines. In the autumn of 1929, the family moved to Gothenburg where Yngve took up a job at one of the so-called "gymnasiums", a Swedish school form which corresponds to upper secondary school in the United Kingdom and to senior high school in USA.

Our father had to interrupt his academical training and only got the lowest exam for a teacher even though his capability certainly could have allowed a further academic career. This was for economical reasons; even before he got his own family he had to help to support his mother. I think that his dream was

[1] Note added by me (CJ): Bengt Källén is the brother of Gunnar Källén. He is professor emeritus in embryology at Lund University. This article was written by him in 2008.

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Figure 1.1 Gunnar to the right with his elder sister and younger brother. Photo taken in August 1930 when Gunnar was 4 1/2 years old

that his eldest son should fulfill what he had not been able to do. A common quotation which was heard at home was "Ille faciet" (he shall do it) which has been ascribed to the Swedish king Charles IX about his son Gustavus II Adolphus, the king who played an important role in the 30 year European war.

It was a typical middle-class family, no real shortage of money but no excesses allowed. This was in the 1930s, the time of the great depression continuing into the Second World War. We lived in a three-room flat and Gunnar and I shared one of the rooms until I left to go to medical school in Lund in 1947. The head of the family was without doubt father – the family structure was very patriarchal. We reacted differently to this – Gunnar accepted the principle and may have carried it into his own married life; I rebelled and took the side of our mother. No bodily punishment existed – it was enough that our father knitted his brows to subdue an obstinate child. Our father had a tendency to get caught up in injustices in life but never compromised. He was confident that he was right and if, for instance, his headmaster had another opinion, the worse for him. And he usually got his way. By his example, we were trained to stand up for our opinions and express them clearly, irrespective of possible resulting inconveniences.

Gothenburg is a major harbour in Sweden and it was therefore often fish for dinner. This was not a great favourite of the children – this was a time when fishes were served with all bones intact, not as rectangular blocks of fish meat.

In order to get the children to eat their fish, our mother used to tell them that you get intelligent by eating fish. Gunnar, aged approximately five, looked at her with unsmiling eyes wide open and said: "Did not Mother get fish when Mother was little?" A repost which became part of family sayings.

At this time, the Swedish school system basically consisted of various choices, taken according to ability (and perhaps economy). There was a four year preparatory school as part of the compulsory seven year school system and you entered school at the age of seven. After these four years, it was possible to leave the main stream education and begin in a school system called "realskola" which you could follow for another five years and then leave with an exam called "realexamen". After that one could continue to the "gymnasium" (see above) for three years but it was also possible to enter "gymnasium" after four years in "realskola" and then you had four years in "gymnasium". This period ended in an examination ("student examination", a graduation) which made it possible to continue with university studies.

We were both sent to school at the age of six, one year before compulsory school age. This meant that we had to attend private schools for the first four years and then had to pass an exam to be allowed to begin in the "realskola". We attended the same school where our father was a "gymnasium" teacher. Gunnar came home rather upset from that exam because there was only one question which he had not been able to answer: what can the hairs of a badger be used for? He thought that his parents had failed their parental task by not telling him that one could use them for making a shaving brush.

The family was rather intellectual and especially our father demanded maximum school performances from his sons. Margit, for reasons explained above, could not manage ordinary school but followed classes with the exception of some subjects, including mathematics. A school mark for Margit above minimum level caused more pleasure than when the sons came home with top marks – it was just expected. Gunnar was looked upon as the intelligent one and had the highest expectations on him – our father regarded me as second rate. One of my proudest moments was when he should help me catch up with some mathematics after I had spent a couple of weeks in hospital. After the first "lesson" he looked at me and said: "You might not be as stupid as I have thought." On the other hand, every effort we made in the way of sports was looked upon with suspicion as a waste of time. We both stayed away rather well from sport activities with the exception of rare and very informal soccer games in the back-yard.

Obviously we had hobbies; to some extent we shared them. One such interest was to build and fly model airplanes, driven by rubber strings. We built (mainly Gunnar of course) a large such plane called "Gladan" ("The Kite") which we flew together. Gunnar was, in contrast to our father, very handy

Figure 1.2 Gunnar with "Gladan". Aged about 15

and always put things together in a perfect way. Gunnar had another hobby; he built electric equipments with switches, home-made ampere and voltage meters and signal lamps etc. in the form of a switch-board. Its handling sometimes resulted in short cuts which did not matter much because instead of fuses we had a kind of relays which broke the electricity in case of a short cut. When I look back I must admire my parents who not only had to stand the experiments made by Gunnar but also the biological ones made by me, including bacteria cultures on top of the gas-driven refrigerator and boiling skulls in the kitchen in order to skelet them and having fish tanks scattered over the house. Within reason we were allowed to follow our inclinations to explore the world.

The handy Gunnar was the family repair man. A memorable experience was, however, when we were visiting our maternal grandmother in Stockholm. She had a grandfather clock which did not want to chime the hours, so Gunnar started to repair it. After some work it was ready, and everyone collected around the clock to hear it chime – and it did, 1, 2, 3 up to 12 – and then continued to chime without ending. Gunnar had to get back into the clockwork and I think he managed to get it right but his proud expression up to number 12 suddenly changing to bewilderedness was a pleasure to watch for his nasty younger brother.

Family games were played. Three main games existed: chess, backgammon, and bridge. Gunnar concentrated on chess and looked down on backgammon because there is a definite component of hazard in it. He had no great feelings

Figure 1.3 Gunnar after graduation from the "gymnasium" outside the school building and with his teacher from the first four school years

for bridge but as four people were needed he had to attend and was also good at it. But he refused to deal the cards so somebody else had to do it for him.

Gunnar and I communicated verbally and by mail in a semi-secret way. We learned a language which distorted Swedish to a non-understandable gibberish. We both spoke it fluently and had some fun for instance when traveling by train. I took a correspondence course in shorthand and Gunnar read the books after me so we could send letters with messages which nobody else in the family could read. We also both learned the Morse alphabet and had some fun with that on a telegraph line which I suppose Gunnar built.

There was always reading matters available, if not at home we could get what we wanted from the city library and most spared moments were spent with books. I have been trying to recall to what extent Gunnar read fiction or if he only read books of facts but I cannot remember. I am, however, pretty sure that he at least did not waste time on poetry which I enjoyed much in my teen-age period.

The years of the Second World War partly coincided with our school education – Gunnar finished "gymnasium" in 1944. Even though Sweden was spared actual war activities there was much scare of a German invasion. There was, however, not much talk about politics at the dinner table. Only when our father got heated up over various reforms of the school system, political issues could turn up. A series of ministers of education were verbally scourged but, by and large, we were lucky to avoid most new experiments in education.

Our parents belonged to a generation when German was the leading language of culture and especially father considered English a barbarous language, suitable only for businessmen. Like many of the intellectuals of his generation, he had got some training in Germany and initially had an admiration for everything German which, however, brutally was killed when the truth about the Nazis surfaced. Mother, on the other hand, had from the very beginning an aversion towards everything which Hitler represented. It can be mentioned that the family held two daily newspapers, one was "Göteborgs Handels- och Sjöfartstidning", the foremost Swedish bulwark against Nazism, led by the famous Torgny Segerstedt who held a proud position at the top of the Nazi death list in case of an invasion of Sweden. Both Gunnar and I had German as the first foreign language at school and English only as the second one. I remember how our English teacher at school was worried when he heard that we should go to England and USA for postgraduate studies.

Both our parents were thus school teacher – and they did not want that future for their sons. We both wanted to try an academic career but our parents insisted that we should get an education which could save us from being school teachers in case the academic careers were unsuccessful. For Gunnar this meant to take an exam in engineering at Chalmers School of Engineering in Gothenburg – he chose electricity as the main field and finished the usual four years training in three and a half years. Only after that he came to Lund and started his brilliant career in the academy. For me it meant to select medical school instead of zoology which perhaps was closer to my heart. It turned out that both of us could stay in academic life but the alternative jobs were an insurance which we should have been thankful for.

What about girls? We went to a school only for boys and our few friends were all boys. Neither Gunnar, nor I had any girl acquaintances and did not participate in school dances or similar events. I met my first love the summer I had finished school, a few years later we married and have stayed married for 57 years by now. Gunnar met his future wife Gunnel Bojs at a course in pedagogic. According to Gunnel, she had to make a series of manoeuvres to get his attention but finally it worked and they got married a few months after us and stayed happily married until death did them apart.

How much meant genes and early environment for Gunnar's development? It is always difficult to say. Without doubt there was in the family a genetic trend of mathematical ability. Our grandfather was a teacher in Latin and Greek but according to our father also an interested mathematician. Members of later generations have also shown mathematical aptitude. But genes are not everything. I think that the rather special family environment meant a lot. For us it was quite natural that school performance was our foremost goal and there was no excuse not to get the highest possible school marks. We were

Figure 1.4 Gunnar and Gunnel, newly wed in 1951, in the aisle of the Castle Church in Kalmar, Gunnel's city of birth

expected to stay in the lead in competition and both carried that with us into academic life.

A Note on Källén's Father, Yngve Källén Bengt Källén has described the enormous influence of his father on the attitudes of the two brothers towards higher education.

Actually, several young people at the time who had Yngve Källén as their teacher at the high school [Vasa Läroverk, Göteborg (Gothenburg)] have given similar testimonies.

Indeed Yngve Källén's influence extended far beyond his two sons and his students. Professor Sture Allén[2] who attended the same high school recalls that (private communication to CJ, translated from Swedish):

> … I studied physics, chemistry and so-called special mathematics but didn't have him as my teacher. Nevertheless, I heard quite a lot about his demanding methods, and my teachers worked hard to achieve comparable results.

Källén admired his father. He published a paper with him on the theory of relativity and insisted on putting his father's name before his own (paper [1956a] on the list of his publications, in Part 5 of this book).

[2] Sture Allén is since 1980 one of the 18 members of the "Swedish Academy" – a body that has the selection of Nobel Laureates in Literature among its duties.

2
As a Young University Student

A person who had known Källén as a student was the Swedish theoretical physicist Lamek Hulthén[1]. His name is associated with Hulthén potentials, Hulthén-Kohn variational principle and Bethe-Hulthén ansatz.

Reminiscing in 1980, at the opening talk of a symposium [1] he said the following about the 19 year old Källén [2]:

> "… In the forties I taught as 'docent' at the University of Lund but in the spring term 1945 I took a temporary appointment as professor of mechanics at the Chalmers Institute of Technology in Gothenburg. After a lecture on particle dynamics one of the first year students came up to me and said he would like some extra reading. I asked what he had in mind and he answered "What about relativity?". So I gave him Einstein's "Vier Vorlesungen über Relativitätstheori[2]". After a surprisingly short time he came back for an examination, with a brilliant result. The student's name was Gunnar Källén.
>
> Well, I went back to Lund and Källén carried on at Chalmers with his characteristic energy and purposefulness, passing his degree in electrical engineering in 1948. Then he went straight to Lund and it didn't take him two years to become a full-fledged theoretical physicist. In the title of his first paper, published in the Swedish journal "Arkiv för Fysik", "The second approximation of the asymptotic phase for the Yukawa potential, treated with Laplace transformations" one may trace the electrical engineer, trained in exploiting Laplace transformation.
>
> When I left Lund for Stockholm in 1949, Gunnar Källén was on his way to Zürich to work with Pauli, who found a kindred spirit in him and praised him accordingly, surprising those who knew Pauli and his rough way of dealing with students. So began Källén's work on quantum electrodynamics and field theory that took an abrupt end by his untimely death in October 1968, at the age of 42 … ".

[1] Lamek Hulthèn (1909–1995) was a member of the Nobel committee for Physics 1966–79, and its chairman 1975–1979. The reader will meet him again in Chap. 3.
[2] In English: Four lectures on theory of relativity.

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References

1. International Symposium "Perspectives in Modern Field Theories", Stockholm (September 23–26, 1980), dedicated to the memory of Gunnar Källén; proceedings edited by B. Nagel and H. Snellman, Physica Scripta, Vol. 24, No 5 (1981)
2. L. Hulthén, Opening Address at the above Symposium

3

Two Men of Utmost Importance: Pauli and Gustafson

In this chapter we wish to introduce two men who played a crucial role in shaping up Källén's academic career. They were:

Wolfgang Pauli (1900–1958) who hardly needs any introduction. There is a great deal of information about him on the internet, the reason only partially being that he is a Nobel Laureate – he was in addition a very colorful character. Though this book is *not* about him, our readers will find him all over in it. He was the prime scientific supporter of the young Gunnar Källén. Moreover, for this book the Källén-Pauli correspondence has been an invaluable source of information about the young Källén. The reason is that, in spite of trying hard, we have not been able to locate Källén's early correspondence.

A year after becoming a professor in Lund, Källén got a personal secretary, Ms Margareta Bergsten[1] who would systematically file his correspondence. Fortunately, there are also a few letters from 1958 in the files but none from earlier date than that. The disruption caused by his sudden death, followed shortly after by that of his wife, could have been the reason for this unfortunate situation. The only exception is his correspondence with Pauli, which goes back all the way to 1949, when Källén was a second year graduate student, and thus gives us a glimpse of him as a young researcher. Here below, we briefly present Pauli, concentrating on aspects having to do with Källén.

Pauli received the 1945 Nobel Prize in Physics for "the discovery of the Exclusion Principle, also called the Pauli Principle" and here is a short excerpt from his Nobel biography that can be found on the internet:

> "Pauli was outstanding among the brilliant mid-twentieth century school of physicists. He was recognized as one of the leaders when, barely out of his teens and still a student, he published a masterly exposition of the theory of relativity. ..."

This concerned general relativity which is a notoriously difficult subject to master. Pauli wrote a research article on the subject when he was still a teenager! Pauli's correspondence shows that this child prodigy was very sure of himself.

[1] Källén often praised the competence and efficiency of his secretary Margareta Eriksdotter Bergsten (1922–2008). She was appointed in September 1959 and served him until his death.

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He had strong opinions and a sharp tongue. Many physicists were afraid of his criticism. His Nobel biography continues to tells us that:

> "Pauli was a Foreign Member of the Royal Society of London and a member of the Swiss Physical Society, the American Physical Society and the American Association for the Advancement of Science. He was awarded the Lorentz Medal in 1930.
>
> Wolfgang Pauli married Franciska [should read Franziska] Bertram on April 4th, 1934."

What the biography doesn't tell, but is important for us, is that Pauli was also a foreign member of the Royal Swedish Academy of Sciences. He was elected as the "successor" of Albert Einstein when the latter died in 1955. Einstein, had in turn been the successor of Hendrik A. Lorentz, who passed away in 1928. An unbeatable line of heritage: Lorentz – Einstein – Pauli! The point is that Pauli was much appreciated in Sweden. He was also elected, on 2 April 1952, as a foreign member of the Royal Physiographic Society in Lund which is in charge of the "Gunnel and Gunnar Källén Memorial Fund", for promotion of science. We shall discuss this matter in Chap. 60.

In 1954 Pauli received an honorary doctorate from University of Lund, but due to teaching duties he could not take part in the ceremony. He did, however, come to Lund at the end of June to give a talk at the "Rydberg[2] Centennial Conference". He was particularly looking forward to experiencing a total solar eclipse on 30th June. Unfortunately, instead, he experienced bad weather and no sun at all!

In a nutshell, Pauli loved to visit Scandinavia and especially to visit his friend Niels Bohr[3] in Copenhagen, long before Källén appeared on the scene. Afterwards, visiting also Källén added to his enjoyment.

Torsten Gustafson (1904–1987), usually called TG, was the only professor of theoretical physics at the university[4] when Källén came to Lund as a PhD student in 1948. His field of research was very broad, extending from the study of air-foils and ocean currents, to quantum electrodynamics and nuclear physics. The contacts between Pauli and Gustafson started in 1945, when Pauli had just seen a paper by Gustafson "On the elimination of divergences in quantum field theory". Gustafson was using a method introduced by the

[2] Johannes (called Janne) Rydberg (1854–1919) was a professor at Lund University. His formula and constant are well-known in physics.

[3] Niels Bohr (1885–1962) was one of the greatest scientists of the last century. He received the 1922 Nobel Prize in Physics.

[4] Professorships were rare in those days. At Lund University, until 1948, there were only two physics professors, the second one being Bengt Edlén (1906–1993), a distinguished experimentalist who worked in the area of atomic spectroscopy.

mathematician Marcel Riesz[5]. Pauli did not know the Riesz scheme and had not been able to figure it out by reading the paper. He complained about this in a letter to Lamek Hulthén[6], in Lund, with whom he used to correspond (letter [809] in the Pauli Collection):

> "…I was therefore interested in Gustafson's papers, but I found them difficult to read because they describe in great length what every physicist knows which, on the other hand, are jumping very briefly over methods which no physicist knows. I am thinking particularly on the method of M. Riesz …"

Gustafson was eventually dissuaded, due to subsequent comments by Pauli, to follow his line of research. But this state of affairs had also a positive outcome, i.e., the two men established a good collegial relationship, which turned out to be very good for Sweden, and of utmost importance to Källén (see below).

Gustafson's ambition was to build up a strong institute in Lund. He was a true visionary and had a great deal of social competence, which is sometimes absent among theorists. He knew almost all top theorists and had good relations with them. Moreover, he was politically well-connected. One of his close friends was a man, Tage Erlander, who had been the first chairman of the newly created Mathematical Society in Lund but later advanced to the impressive rank of the longest lasting Swedish Prime Minister. He was in office non-stop during 23 years (1946–1969). Another of his friends was Niels Bohr, who trusted and appreciated Gustafson's friendship a great deal.

Before coming to Lund for the Rydberg Conference, Pauli informed Gustafson that he had had the pleasure of sitting next to Erlander at the Nobel dinner[7] and found him a very "sympathetic person". (Indeed he was.) At Rydberg Conference, in Lund 1954, Pauli was to meet Bohr, Erlander and Gustafson. He wrote[8] to Gustafson (letter [1817] in the Pauli Collection):

> "… I am looking forward to see you soon and I am also very glad to see Bohr on this occasion, though I am in no way glad, and he knows it, to have an

[5] Marcel Riesz (1886–1969) was a much appreciated mathematician in Lund, known not only for his mathematics but also for his colorful character. Many stories were told about him.

[6] Lamek Hulthén was introduced in Chap. 2. He was much respected by Pauli, and during the second world war Pauli would write to him not only concerning physics but also asking him for information about what was happening to physicists in the occupied countries. Hulthén once told CJ the following anecdote about Pauli. His wife while dancing with Pauli in Leiden had asked him: Professor Pauli, does your wife care about physics?, to which Pauli had immediately replied: No, but much more importantly, she cares about me!

[7] Pauli received the 1945 Nobel Prize. On the "Nobel day" of that year he was at Princeton, and was honored by speeches given by Einstein and others. Instead, he came to Sweden in 1946, to give his compulsory Nobel lecture and to take part in Nobel celebrations. There he met the newly appointed Prime-Minister, Erlander.

[8] Pauli and Gustafson corresponded in English.

occasion to hear him talk politics. A subject which interests me is whether or *not* it is wishable that scientists shall make politics. My answer is no, Bohr's answer is yes, and we could have a hot debate on this principle question, without talking temporary politics, which is entirely destructive, and about which nobody can make any predictions. With the prime-minister and you as abitrator [arbitrator] between Bohr and me, hoping to get your answer soon . . . "

In building up the institute, the education of young scientists was a primary concern. Gustafson would arrange funding and send some of the most gifted ones abroad. Thus several young Swedes, among them Gunnar Källén, got the opportunity to attend Pauli's lectures at ETH in Zürich. Källén considered this to have been the most crucial event in his scientific career.

Källén realized that he had been lucky to have encountered a person like Gustafson . In his doctoral thesis he wrote, in 1950:

"In this work I have had the invaluable advantage of daily opportunities of discussion with Professor TORSTEN GUSTAFSON, head of the Institute, as well as with the other members of the department. I wish here to express my sincere gratitude to Professor GUSTAFSON for the never-failing interest he has taken in my work and in my studies, for the visits to foreign universities he has arranged for me and for the good advice he has always given me."

Gustafson was indeed an unfailing supporter of Källén. It was he who did all the work for getting a personal professorship for Källén (see Chap. 7). After Källén's death and that of his wife a few months later, Gustafson, who had been a member of the Royal Physiographic Society in Lund since 1940, proposed that the Society be in charge of a Memorial Fund honoring the couple (see Chap. 60). It was truly a wise decision. It was also Gustafson who proposed to the Royal Swedish Academy of Sciences to invite and fund a guest professor from Russia to come to Lund, as we shall discuss later in Chap. 62.

Finally, also Bohr on several occasions expressed his appreciation of Källén. For example, in a letter dated 16 March 1953, he wrote to Robert Oppenheimer at the Institute for Advanced Study, Princeton, proposing that Källén be invited to the Institute:

"... As you may also know, he was last year working with Pauli who has expressed his high appreciation of Dr. Källén's abilities and who shares warmly in the expectations we all have for his future activities. I think he must be considered one of the most outstanding among the younger European physicists who, with his scientific enthusiasm and gifts, combines very attractive human personality."

Already on 20 March 1953 Oppenheimer offered the 27 year old Källén membership in the Institute for the academic year 1953–1954. At times, one is amazed by how efficient the postal delivery could be in those days in Europe as well as across the Atlantic Ocean.

4

The Young Källén at "Pauli's Court"

This chapter describes more in detail how Källén happened to end up at "Pauli's Court" so that the great man, as he put it himself, could "discover" him.

As mentioned in the previous chapter, the only professor of theoretical physics in Lund, Torsten Gustafson (1904–1987), had established a good contact with Wolfgang Pauli in Zürich. After all, both were often in Copenhagen, visiting their common friend Niels Bohr. Gustafson would send scientific articles to Pauli [1], to get his opinion, and would invite Pauli to visit Lund. When in Copenhagen, a small detour to Lund, situated nearby, would not cost Pauli much time or effort.

On a few occasions Gustafson, after having obtained Pauli's consent, would provide financial support for a student to go to Zürich to attend his lectures. Several young scientists from Lund had already benefited from this arrangement before this great opportunity was offered to Källén.

Gustafson and Pauli corresponded in English, therefore here below their letters are reproduced as they were written. On 17 February 1949, Gustafson wrote to Pauli:

> "Dear Professor Pauli,
>
> You have been so very friendly to the young people from Lund, that I am encouraged to ask you if it would be possible for a young man, Gunnar Källén, who is very interested of theoretical physics, to follow your lectures in the summer term, May to July. He is rather young, but he has been studying quantum mechanics and relativity theory with great interest and it would be of the greatest value for him to be able to hear you. I beg you to tell me if it would be difficult to receive him this term. …"

where the dots stand for other matters (not related to Källén) that Gustafson communicated to Pauli.

In this, for Källén and Lund most important historical letter, Gustafson also informed Pauli that:

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> "We are just now waiting for Dr. Bradt[1]. . . . It would be of the utmost value for us, I must say that your predictions of his ability have been really fulfilled. Also the University people are interested, for it is the first time that the Swedish authorities have promised to create a new professorship for a foreign scientist."

This was to be a historical event – a foreigner becoming a professor in Lund! However, this plan supported by Pauli and Paul Scherrer in Zürich, did not materialize due to untimely death of Helmut Bradt (1917–1950). It is not sure that Bradt would have come to Lund, had he lived, as he had other attractive offers as well.

Returning to the issue of Källén, Pauli answered quickly, as he often did. In a letter dated 22 February 1949, he wrote:

> "Dear Colleague Gustafson!
>
> Thanking for your letter I am glad to inform you, that Mr. Källén will be very welcome in this summer term, which is lasting from end of April till middle of July. . . "

And so Källén, who was a second year doctoral student went to "Pauli's Court", to attend his lectures during the summer term 1949. Already on 15 July 1949, in a letter to Gustafson, Pauli expressed his great appreciation of the 23 year old student:

> "Dear Prof. Gustafson!
>
> I wish to thank you very much that you have sent me Dr. Källén, who turned out to have great skill and talent. He is working both very quickly and [is] very reliable.
>
> About a paper on application of Schwingers formalism to higher approximations of the vacuum-polarisation which he is just going to finish he will write to you soon himself. We shall publish it in the Helvetica Physica Acta.
>
> I would be very glad if we could have him here again the summer term 1950 (which starts end of April). (During the winter I shall go to Princeton.) . . . "

Later on Pauli referred to Källén as "my discovery". The two men established an extensive correspondence (in German). Almost all of these letters have been preserved in the Källén Collection which has been the most important source of information in this book. These letters show that Pauli used Källén, to a large extent, as his sounding board and always looked forward to seeing him. They

[1] Bradt was an experimentalist who had studied in Zürich, under the famous Swiss atomic and nuclear physicist Paul Scherrer (1890–1969).

also exhibit that the two men admired each other and were "friends" – provided one would be allowed to use such a word. Both Pauli and Källén abided by formal conventions[2] not so uncommon in those days. The age difference (26 years) was too big for them to address each other in the familiar (German "du") form or to use first names.

When Pauli passed away, Källén received a letter from his widow, Franziska (called Franca) Pauli, asking him to return Pauli's letters to her. Källén answered that the letters were so precious to him that he couldn't envisage returning them but instead sent copies of them. Hence, the originals of Pauli's letters to Källén remained in Lund.

We shall have much more to say about Källén-Pauli correspondence later on.

References

1. Torsten Gustafson Collection, Manuscripts & Archives, Lund University Library

[2] This was especially the case with Pauli, who seldom used his first name in his correspondence. He would sign his letters "W. Pauli". He addressed and was addressed with first name only in his correspondence with a handful of scientists (see also Chap. 35). Not even his former assistant and later much trusted "friend" Victor Weisskopf (born 1908) was ever promoted into his first name category.

5
Introducing Himself

On 11 May 1954, at the age of 28, Källén writes (translated from German):

> I, Anders Olof Gunnar Källén, was born on 13 February 1926 in Kristianstad, Sweden, as the son of the high school teacher Anders Olof Yngve Källén and his wife Karin Sofia Magdalena Källén, born Redin. ...
>
> After further 3 1/2 years of study at the Technical "Hochschule[1]" in Gothenburg, I graduated as an engineer in 1948 and started my studies at the University of Lund. With the exception of one year interruption for military service[2], I have been working here at the Institute for Mechanics and Mathematical Physics and during this period I have passed the following examinations ...
>
> In 1951 I received a PhD degree. During this period I have undertaken the following journeys abroad for scientific studies: to Zürich (ETH[3]) 1949 and 1951 (3–4 months each) and to Princeton, U.S.A. (Institute for Advanced Study) 1953–1954 (about 7 months). During the years 1952–1953, I have also participated in European Collaboration in the area of nuclear physics (CERN) in Copenhagen. Currently I am employed as lecturer at the University of Lund ...

Källén continues by specifying courses that he has been giving in Lund as well as in Copenhagen and ends his letter by assuring ETH that his

> knowledge of the German language is adequate also for giving lectures in German in the area of quantum theory.

The above application, found in the Källén Collection, may look a bit strange due to its emphasis on courses, teaching and the German language. The reason is as follows: Källén had received a letter, dated 1 May 1954, from Pauli who informed him that there is an opening for an "extraordinary" professorship in theoretical physics at ETH (translated from German):

[1] The German Hochschule (Högskola in Swedish) corresponds to University or College.
[2] The military service was compulsory.
[3] ETH stands for "Eidgenössische Technische Hochschule" (the Swiss Federal School of Technology) a prestigious institute for higher education and research.

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> It would be good if you would apply (even though someone who has not applied can also be considered).

Subsequently, Källén had sent a draft of his application to Pauli who had responded (translated from German):

> I find the draft, that I hereby send back to you, very good. I would only recommend that – in your CV where your employment as a private docent is stated – you add something about your teaching experiences, and especially briefly indicate the subject of the courses that you have given in Lund. It would also be good, if you in addition mention that your knowledge of the German language is adequate for giving lectures in German in the area of quantum electrodynamics.

Apparently, teaching was considered to be very important at ETH. In Sweden, in those days, there were no such formal requirements for a professorship. One was not even required to have a doctoral degree. Returning to Zürich, at the time Pauli was the only professor of theoretical physics at ETH. Establishing an "extraordinary" professorship was an unusual act. The driving force for this action was the creation of CERN on the outskirts of Geneva. By strengthening the theory group at ETH, this prestigious institution intended to stay in touch with the latest developments in the field of particle physics and to be among the leaders in theoretical physics.

Källén did not get the professorship, which went to the eight years older Swiss theorist Res Jost[4] (1918–1990).

A brief Källén chronology is presented in Appendix I-B, at the end of this Part.

[4] For a brief biography of this eminent physicist see, for example, CERN Courier, Dec. 1990. Our readers will encounter Jost several times in this book.

6

As a Staff Member of the Newborn CERN

In the previous chapter, Källén in his 1954 application for the professorship in Zürich told us that he had participated in European collaboration in the area of nuclear physics (CERN) in Copenhagen. He was a fellow of CERN *before* the organization was officially created and at the same time a lecturer in Lund.

CERN was officially created on the 29 September 1954, after an intense period of preparations, involving many steps in several countries. The glorious history of its creation is well worth reading [1] as it shows the dedication and commitment of a large number of distinguished international scientists, not only in Europe but also in America[1]. Moreover, there was ample support by prominent politicians for the idea of creating a European center for, *not applied* but **basic** science. The site was chosen to be on the green fields of Meyrin, a satellite village to the city of Geneva in Switzerland, a decision which was approved by the citizens of Geneva through a referendum.

The CERN "Group of Theoretical Studies", was created through a resolution passed by the CERN Interim Council in Amsterdam in May 1952. The Group was quickly formed, and could start its work, as it was believed that it did not need any accelerators. It was based in Copenhagen[2] which was a great place to be, due to already existing infrastructure and presence of such distinguished local physicists as Niels Bohr and Christian Møller[3]. Indeed, by that time, Copenhagen had been a world center for theoretical physics dur-

[1] Among them J. Robert Oppenheimer (1904–1967) and Isidor Rabi (1898–1988). Oppenheimer, as the scientific director of the Manhattan Project, is too well-known to need an introduction. For us, what is important is that he was a visionary and realized the importance of international collaboration. As the director of the Institute for Advanced Study, he created an inviting atmosphere for visitors such as Källén. Pauli had a standing invitation to his Institute. Rabi received the 1944 Nobel Prize. Hence, his biography is easily available on the internet.

[2] The Dutch physicist H. A. Kramers (1894–1952) who had been the first "assistant" of Bohr, played a crucial role in this connection. Unfortunately, he did not live to enjoy the fruits of his work. See also Chap. 67 on Kramers.

[3] Christian Møller (1904–1980) was a distinguished Danish physicist who worked in several areas of theoretical physics. In particle physics, his name is associated with Moller scattering. Källén used to point out that Møller had introduced Lorentz invariant kinematical variables, in computing the cross section for this scattering. See his paper: Ann. Phys. **14** (1932) 531. These quantities were later called Mandelstam variables.

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ing several decades. Thus, in 1952, Källén became a fellow of the yet unborn CERN.

After the official creation of CERN in 1954, we can learn more about Källén's situation by consulting the reports of the organization. The Annual Report 1955 [2] informs us that:

> "The Theoretical Study Division is located in the Theoretical Physics Institute, University of Copenhagen.
>
> The work of the Division has proceeded according to the programme fixed during the interim period and includes:
>
> **a) scientific research on fundamental problems of nuclear physics, including theoretical problems related to the focusing of ion beams in high energy accelerators;**
>
> **b) training of young theoretical physicists;**
>
> **c) development of active co-operation with the laboratories of Liverpool and Uppsala, whose machines and equipment have been placed at the disposal of CERN.**"

Here we have boldfaced the above items in order to emphasize what the "Founding Fathers" believed the theorists should be doing! But, of course (except for b) that was not what the theorists actually did!

Further down the report we are informed that CERN Theoretical Study Division has two full time senior staff members: Dr. G. Källén and Dr. B. R. Mottelson[4] and that they "are taking part in the leading of the scientific work". Note that these "leaders" were both below the age of 30! This was a general feature of the young CERN – even its accelerators were built by "youngsters".

As CERN staff member, Källén was very active in giving talks and lectures. For example, we find his name on the list "Lectures and Colloquia", in the period 1 October 1954–31 December 1955 [2], giving the following contributions:

- (November 3, 1954): Scalar and longitudinal photons and the gauge invariance of quantum electrodynamics;
- (November 15, 1954): A field theoretical model suggested by T. D. Lee;
- (April 29, 1955): Some impressions from the Conference in Moscow[5];
- (October 2, 1955): Introduction to "Ghost Problems";

[4] Mottelson received the 1975 Nobel Prize in Physics jointly with Aage Bohr.
[5] This event is discussed in Chap. 52.

- (Starting October 19, 1955): Weekly lectures on quantum electrodynamics;
- (November 14, 1955): On some problems in connection with unstable particles and "ghosts".

Källén was a very active member of CERN until 1 October 1957, when CERN's theory division moved to Switzerland, but he chose to remain in Copenhagen. As a "compensation" for the drastic change, and in order to strengthen theoretical physics in the Nordic countries, Niels Bohr got a new institute in Copenhagen, NORDITA. The acronym, originally, stood for Nordic Institute for Theoretical Atomic Physics, though the Institute, from the very beginning, also dealt with theoretical studies in subatomic physics. Källén became a staff member of the newly created NORDITA. However in the following year he was appointed as professor in Lund.

In spite of leaving CERN, Källén took a keen interest in the development of CERN. For example, he belonged to a small pool of scientists from Sweden who took turns in participating in important meetings at CERN, such as those of the Restricted ECFA, a committee which had as its main duty to make proposals for future accelerators.

Early in 1960's it was decided that CERN should build a new "*very high energy accelerator*" – the so-called 300 GeV project, "*involving a ten-fold scaling up of the largest existing accelerator*" and "*near the limit of what is technically feasible*". In this endeavor, the question of site was left open and the member states were asked to bid for it. Källén took this seriously. In a letter (found in Källén Collection) dated 14 May 1965 and addressed to his experimental colleague Gösta Ekspong[6] in Stockholm, he writes (translated from Swedish):

> I have been looking into the soil types, height differences and so on in the Malmö-Lund region, together with experts in geology from [Lund] University. After having made measurements for a long time, back and forth with compasses, unfortunately we came to the conclusion that there is no competitive option for the site of the 300 GeV accelerator here in the surroundings … I give my full support to the "Uppsala-plain".

Finally, no bid from outside was considered to be good enough and the new machine ended up on the CERN site which, however, had to be extended into a neighboring region in France.

[6] Källén was a member of the site-search committee in Sweden. Ekspong (born 1922) chaired this committee. Ekspong has played a vital role in the development of experimental particle physics in Sweden and has chaired several important committees, such as the CERN Scientific Policy Committee and the Nobel Committee for Physics.

References

1. A. Hermann et al., "History of CERN", Vol. 1: "Launching the European Organization for Nuclear Research", North-Holland (1987). In addition there are two more volumes covering other historical aspects of this Organization.
2. CERN Annual Report 1955

7
Professorship at Lund University

Before 1960's professorships were very rare in Sweden. Generally, at each department/division there was only a single chair and the chair holder was automatically the head of the department. Thus professors came in succession, one at a time per department. Källén's exceptional scientific merits quickly convinced several scientists that Sweden can't afford to lose him, just because there happened to be no vacancies for him to occupy. But how was one to achieve this goal? Ivar Waller[1] and Torsten Gustafson, introduced earlier in Chap. 3, (as well as a few more people, according to Gustafson) came up with a bright idea, i.e., to try to create a personal professorship[2] for Källén. Written documents show [1] that such a proposal had to pass several hurdles and at the very end it had to be approved by the Swedish Parliament for budgetary reasons.

It all went quickly. On 31 January 1957, Gustafson wrote to the Council of the University of Lund, the top executive body at the university, and urged that they include in their planned budget, to be presented to the Swedish parliament, the required funding for establishment of a professorship in theoretical physics at Lund University. Gustafson was directly in contact with the person in charge at the relevant Ministry. The arguments he presented to the authorities in favor of his proposal were: Källén's exceptional scientific brilliance, that he had already supervised PhD students and had several offers from abroad (among them from Princeton and Maryland). Moreover, Källén had declared that he would value a professorship at Lund University higher than any other offer in the world. This was a golden opportunity for Sweden not to be missed, etc. In fact, in this initiative, the support of Ivar Waller was important to demonstrate the *national* character and thus credibility of the proposal. However, all the necessary work was done by Gustafson alone.

In the meanwhile, early in January 1957, Gustafson had written to Niels Bohr, Robert Oppenheimer, and Wolfgang Pauli, explaining to them Waller's

[1] Ivar Waller (1898–1991) was, due to his breath and depth of knowledge supplemented by his devotion to science, one of the most influential theoretical physicists of the 20th century in Sweden. Several of his "disciples" became professors. The reader will meet him several times in this book.
[2] Such a professorship was, in principle, person-specific and not inherited by a successor.

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and his initiative and asked them for recommendation letters on behalf of Källén. He pointed out that

> "Personal professorships are very rare here in Sweden and therefore it is very important that the Swedish authorities can study the opinion of leading scientists."

The errand climbed up all the way to the office of the "Chancellor for the [Swedish] Kingdom's Universities" which supported the proposition and provided funding, from the academic year 1958/1959.

Gustafson's social competence and his direct contact with top politicians as well as scientists was instrumental in the quick success of his plan.

In a book, such as this, it is very interesting to know what the referees thought of Källén. Here below, we reproduce their opinions, which all arrived within a month.

Recommendation Letters From Oppenheimer, Bohr-Møller and Pauli

In a letter from Princeton, dated 25 January 1957, **Robert Oppenheimer**, the Director of the Institute for Advanced Study, wrote [1]:

> "Dear Professor Gustafson:
>
> It is with great pleasure that I learn of your plans for the institute in Lund, and of the possibility that you may offer a professorship to Dr. Gunnar Källén. It is hard for me to imagine a more auspicious beginning. Dr. Källén is known to us here, as indeed throughout the world, as one of the most gifted young theorists of the generation. His keen physical insight is matched by great mathematical power; and his sense of rigor happily complemented by a deep sense of irony and humor.
>
> His contributions to physics will be well known to you. They seem to me singularly impressive, and have formed the foundation on which much else has been built. More than anyone else, he is responsible for defining the limits of what is known today about the meaning of quantum electrodynamics in particular, and quantum field theories in general. Dr. Källén is an admirable expositor, and combines the ability and love of solitary work with a talent for collaboration. I feel confident that under his leadership a great international

school of theoretical physics will develop in Lund; and I hope that you will be successful in taking the step of appointing him to a professorship.

With cordial good wishes,

Robert Oppenheimer”

The second letter, written in Danish and dated 28 January 1957, came from CERN Theoretical Study Division in Copenhagen where Källén was a staff member. It was signed jointly by **Niels Bohr** and **C. Møller** [1]. Translated into English, it reads:

… Dr. Gunnar Källén came to Copenhagen as CERN fellow in the academic year 1952/53, after which he has spent one year at the Institute for Advanced Study in Princeton, U.S.A. Already during the first stay in Copenhagen he presented a number of highly appreciated lectures on quantum electrodynamics and in addition took part in all the discussions concerning problems in the area of field theory. It was therefore natural, that one wished to attach dr. Källén to the study division's leading staff in Copenhagen, when the structure of CERN's activity became definitive[3] in October 1954.

Dr. Källén's achievement, during his attachment to CERN, both as lecturer and supervisor of the young members of the study division, has been of invaluable importance for the working of the group. His deep insight in quantum field theoretical problems and his strongly developed critical attitude have been infinitely stimulating both for the younger as well as the older colleagues.

Dr. Källén has both before and during his attachment to CERN carried out a large number of outstanding, partly fundamental pieces of work on problems within the quantum electrodynamics. These have been of decisive importance for the latest development in this branch of physics and have given him a leading status among the researchers in this field, whereby he has also become an internationally recognized physicist, who has close contacts and scientific collaboration with colleagues in the whole world.

Finally, the letter [1] from **Pauli**, dated 5 February 1957, is in English. It reads:

“Dear Gustafson,

It is a great pleasure for me to recommend to the Swedish authorities very warmly your plan of establishing a personal professorship in theoretical physics for Dr. G. Källén. He is certainly the most able candidate for such a professorship among the younger Swedes.

[3] Bohr and Møller are referring to the official birth of CERN.

> Of course I followed his work in quantized field theories in many details since his first stay in Zürich (1949), when he was often considered as my "discovery". I was indeed extremely glad to have him a second time in Zürich 1952, when he published an important work (No. 7 of the list), which was later continued by himself and also by Lehmann and others. My personal contact with him in the exchange of views on scientific questions is still continuing. In the course of it, we published a common paper (No. 12). His Handbook-article (No. 16) gives an excellent review of the present stage of the theory of field quantization, which proves that he is a first class expert of this difficult subject.
>
> By judging Källén's work in this field one has to take into account that the socalled renormalization method in field theories is a system of rules and approximations, which are not linked (and probably will never be linked) together into a common whole of a consistent theory. Källén is well aware of these difficulties which make it at present unpredictable in which direction the theory will develop in future. It is certain, however, that this kind of formalism will be indispensable in the theory of the interactions of the socalled "elementary" particles for a long while to come. Experts like Källén will therefore be of the greatest value in the next steps of the developments of theoretical physics.
>
> Sincerely yours,
>
> W. Pauli"

The Swedish authorities must have been mighty impressed. Already on 1 August 1958, at the age of 32, Källén began his final career, as a personal professor of theoretical physics in Lund.

References

1. Torsten Gustafson Collection, Manuscripts & Archives, Lund University Library

Personality, Sharp Tounge and Fighting Spirit; Källén and Julian Schwinger – a Preview[4]

> If you wish to get an answer from letters to Copenhagen, I give you the practical advice not to write to Møller or Glaser but to write to Källén. To wit, he gladly writes long letters (especially when he believes he can prove you wrong, which is almost always the case).
>
> Pauli to Heisenberg (1957) (translated from German)

The previous chapters of this Part dealt with Gunnar Källén's childhood and youth, his university studies as well as his professional career ending up with the professorship in Lund in 1958. In the next few chapters we would like to briefly describe some aspects of his personality.

In one of the chapters here below, we quote from Källén's popular science article on the 1965 Nobel Prize in Physics to Feynman, Schwinger and Tomonaga and discuss his relationship with Schwinger.

Källén loved physics. Attacking difficult problems was his trade mark. He would gladly leave "easy" problems to others.

Further insight into his personality is obtained by consulting his correspondence with Wolfgang Pauli, Werner Heisenberg and Paul Dirac (see Part 2).

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[4] Julian Schwinger (1918–1994), Nobel Laureate 1965, was one of the giants of the 20th century physics. He supervised a large number of students, among them four who also became Nobel Laureates, three in Physics (R. J. Glauber, S. L. Glashow, and B. R. Mottelson) and one in chemistry (W. Kohn). For more information about him see the webpage of the "Julian Schwinger Foundation".

8

Kind to Students Tough on Celebrities, Scientific Honesty and Sharp Tongue

In 1960's, Källén was the leading figure at the Department of Theoretical Physics in Lund. Whenever available, he would not miss the departmental tea gatherings at 3 o'clock in the afternoon. That is how the students got to know him better. He was generally friendly and happy – there was much laughter at such occasions. He had strong opinions, on physics as well as on physicists, and would express them directly and bluntly. Usually, nobody challenged his opinion, even though he most probably would have appreciated it, simply because there was hardly anyone who was competent enough to do so.

This friendly and kind-to-students man would undergo, what we physicists usually call a "phase transition" when he encountered senior theorists. The greater the theorist, the harsher he would be on him [there were hardly any eminent female theoretical physicists in those days]. This behavior was evident when great theorists visited him and gave talks at the Department. His sharp tongue and lack of respect for authority turned many seminars into memorable occasions, for students. To see the giants of theoretical physics fight over issues such as putting a charge behind the moon or existence/non-existence of a certain physical quantity in a given theory meant that the topics discussed must have been captivating and of utmost importance. Who was right or wrong was a secondary issue to the students, who in general didn't understand what was going on. What was gratifying to them was that Källén's harsh attacks upwards on celebrities were amply compensated by his kindness and helpfulness downwards to students.

Källén's correspondence shows that he was not submissive to anyone, not even to Pauli whom he truly admired. As his brother put it (see Chap. 1):

> "By his example, we were trained to stand up for our opinions and express them clearly, irrespective of possible resulting inconveniences."

where the role model, i.e., the example, was their father. In fact Källén was also tough on himself. He would admit his own errors without any reservations.

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Källén demanded total honesty in scientific presentations and cared about the question of priorities. An author making unjustified claims would be hit by his bitter criticism, expressed bluntly, regardless of how famous he was. Here are a few examples from his correspondence.

In a letter to his friend and collaborator Arthur Wightman dated 28 November 1960 he writes:

> "As you know, I am rather particular about 'priority problem'. This goes both ways and I do not want to get credit for something which I have not done."

He would object, for example, whenever anyone said "Källén-Lehmann representation" as then two Japanese scientists (H. Umezawa and S. Kamefuchi) were left out. Källén had found this representation independently in 1952 and had made much use of it in his calculations. Lehmann, had come along later (1954) and according to Källén had done absolutely nothing original in this respect – he had only given a pedagogical account!

Another sore label was "Mandelstam variables" which were very popular in those days. Källén would point out that such kinematic variables were actually introduced long before Mandelstam by Christian Møller (see the footnote in Chap. 7).

Källén's sharp tongue and outspoken style is manifest in his correspondence. For example, in a referee report that he sent in 1962 to the editor of a journal he wrote:

> "You ask for my opinion concerning the originality and scientific value of the paper. The manuscript can make a certain claim to originality as it contains a large number of misunderstandings of modern physics in general and field quantization in particular which are entirely new – at least to me. Scientific value of the paper: NONE."

In a letter to Freeman Dyson, dated 13 December 1958, he writes:

> "Dear Dyson,
>
> Today, we have received your card from California announcing your marriage. Both my wife and I wish to send you and your wife our congratulations and best wishes. We hope to get the pleasure of meeting your wife somewhere soon.
>
> Let me take this opportunity to say that I have read with great interest your review of the Handbuch paper in the Physics Today. Of all the reviews I have seen so far, yours is not the one I dislike most – but it comes in a good second! ..."

Dyson had branded Källén's article as old-fashioned for not dealing with the "New Look" in the field (see Chap. 19).

In a footnote to a letter to his friend and collaborator Arthur Wightman, he writes in 1964 (see Chap. 51):

> "P.S. This letter is a very much tuned down version of my original draft which was so acid that even I hesitated to mail it off!"

He writes, on 20 Oct 1965, to a theorists who has invented a method to get rid of ghosts[1] that he has committed several mathematical errors. After a few pages of calculations and explanations he draws the following conclusion:

> "In summary, I really do not believe that the heuristic mathematics which you outline in your manuscript has any future. I apologize for this somewhat blunt statement, but I really can find no other formulation which would be politer but still say what I want to say.
>
> Sincerely yours"

He rendered perhaps his sharpest comment ever to a friend who was giving a talk, telling that he was making a certain assumption.

> Källén: Why do you make this assumption?
>
> Speaker: Because everybody makes it.
>
> Källén: Which only shows that there are more fools around than just you.

Indeed, for Källén consensus was no proof at all! However, Källén was not always negative. He appreciated a good and solid piece of work and said so. He wrote nice recommendation letters for people who in his opinion had done such work. More than anything else in science, he detested speculations that were presented as facts.

The distinguished Austrian theorist Walter Thirring, who had several times been hit by Källén's bitter criticism, in his autobiography[2] describes two "phases" in Källén's personality (translated from German):

> … However, this [i.e., Källén's] aggressive behavior was limited only to the scientific domain. Since he stayed somewhat longer in Princeton than fore-

[1] These are states with negative probability that appear in some theories and, unless compensated, spoil the theory.

[2] Walter Thirring, "Lust am Forschen", (Seifert Verlag, 2008) p. 128. This book is in German. Its title means: Appetite for Research.

> seen, we invited him to stay with us, and he turned out to be a very pleasant and helpful guest. …

Finally, on one occasion, after a seminar by a young student, a very irritated Källén stood up and said: “This is exactly how a talk should not be given!”, putting the emphasize on the word “not”. A few days later, at the beginning of the next seminar, he openly apologized to the young man. His improper behavior had been caused by lack of sleep, he said. One of his children had been sick and had kept him awake all night. In his own words – he was “only human”.

9

A Trustworthy and Popular "Helpdesk"

Källén's scientific honesty and his exceptional faculty for fast analysis made him an ideal advisor for many scientists. They knew that they could trust him and would send their manuscripts to him, for his comments, *before* sending them to journals. As was noted before, Pauli recognized this speciality of Källén and wrote about it, for example, to Heisenberg: If you wish to get an answer from letters to Copenhagen, I give you the practical advice not to write to Møller or Glaser but to write to Källén. To wit, he gladly writes long letters (especially when he believes he can prove you wrong, which is almost always the case).

Young people would ask him all sorts of questions, such as how does a maser work, where do angle – angular momentum uncertainty relations come from, etc. More senior theorists would consult him primarily about the intricacies of quantum electrodynamics and quantum field theory.

Källén's correspondence shows that he enjoyed his role as a trusted unpaid consultant. After all he loved solving problems. Even doing an integral by using especial tricks gave him visible joy. After such performances, sometimes his face would be shinning and he would write or say "voilà". On one occasion Hartmut Pilkuhn[1] and I (CJ) went to Källén with an integral that we didn't know how to calculate (it involved four-body phase space factor multiplied with some function). This was on a Friday afternoon. Källén looked truly happy, as if solving this problem was going to give him much pleasure during the week-end. Indeed, on the following Monday, he presented the solution, having used a "magic" trick.

His answers to the questions he got were sometimes quite long, containing several pages of calculations. He could be very blunt. If he thought a paper was not worth publishing he would say so and if the author nonetheless published the work and thus "made a fool of himself" that was his problem. Källén didn't care. In those days the current pressure to publish did not exist and many great scientists, at least in Europe, had few publications. The community was much smaller and it was generally known who the good and reliable scientists were.

[1] Hartmut Pilkuhn (1936–2006) was at the time a "docent" (senior researcher with a rather limited teaching load) in Lund and later became a professor in Karlsruhe.

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Many of the great scientists of the past, had they lived today, would have not qualified for a postdoc position, due to their meager output!

Some authors withdrew their work from journals because of Källén's comments. As one author once wrote to him:

> "You have spared me the embarrassment of making absurd statements in public, and I sincerely appreciate it".

In the literature, one finds acknowledgement to Källén from various corners of the academic society. For example, Trilochan Pradhan from India, who corresponded with him ends up writing in his published article (Nucl. Phys. 43 (1963) 11):

> "The author would like to express his gratefulness to Prof. G. Källén of Lund University, Sweden, with whom he had correspondence on the subject and from whom he received a number of important comments and suggestions on several aspects of the work, particularly on the S-matrix and the fermion Green function, which have been incorporated in this paper."

As mentioned above, Källén took young people and their physics problems very seriously. One finds several scenarios in his correspondence, such as:

Källén lectures at a school or is at a meeting. There, a discussion starts on some related topic. Either he makes a suggestion, as to what could be interesting to investigate, or he just asks some questions. After some time he receives a letter that reminds him of the event and presents what the young man (there were hardly any young women in the field in those days) has done about it. Källén checks the calculations, etc., and generally writes a long letter, sometimes more than 10 pages long, full of explanations and formulae. His answer is so friendly that the young man is encouraged to send new letters, and thus the correspondence continues.

A second general pattern is that a young man believes that he has found a mistake in some book or article and writes to Källén. Again Källén takes the matter seriously and a new series of exchanges starts.

On some occasions, Källén is asked to act as "associate promoter" of a thesis of someone whom he has never met but has corresponded with. If not as a promoter, he is sometimes asked to be the judge of the quality of the work done by such young people. For example, on 26 October 1966, Asher Peres, from Haifa, Israel, writes to him:

> "I apologize for bothering you once more about the doctoral work of my student, Mr. Moshe Glück, who insisted on working in axiomatic field theory, although no one in this country can guide him or assess the value of his work."

As usual, Källén accepts the task. All this is fine, and is how it should be in an ideal academic world, but what is amazing is that Källén takes time to answer even some foolish letters. In such cases, he first points out a few glaring errors. Examples of his conclusions are:

> "I am sorry to say that I am really not very much impressed by your work."
>
> "I should sincerely advise you to abandon this line of research and do something else instead."
>
> "I am sorry, but I sincerely believe that future correspondence between us about this question will be rather useless."

Källén spent a lot of time acting as "helpdesk", never asking for any favor in return. He commented, corrected, and made suggestions on articles he had received and then simply pointed out that implementing the changes was solely the responsibility of the author. He would have considered it far below his dignity to write a paper just for the purpose of pointing out errors in other people's work.

Why wasn't Källén more "selfish"? Why didn't he shrug off from requests for help by all these people so that he would have more time for his own work? I don't know the answer to this question. If I were to speculate, the answer could perhaps be because both his parents were teachers. His teaching instincts were indeed very strong as manifested by his initiation of several new academic courses in Lund and his efforts to improve the general level of education in Sweden. He didn't have to do all that work and yet did it! He genuinely cared about the education of young people.

Going one step further, why did he take time to also answer manifestly pointless letters? Why didn't he just throw them into the waste-paper basket? The answer could be (perhaps) that Källén had a very strong sense of duty. He felt that, as professor, he was expected to answer.[2]

[2] When I was professor in Bergen, Norway (1976–1985), one was required by law, not only to answer all letters, but in addition to answer them in the version of the Norwegian language in which they were written ("bokmål" and "nynorsk"). In Sweden, the professors seem to be, happily, unaware of whether a corresponding law exists or not.

10
Making Errors

Pauli described Källén's remarkable speed in doing computations, his reliability and capacity to quickly spot errors in articles by others. Here are some of Pauli's comments.[1]

To Gustafson:

> I wish to thank you very much that you have sent to me Dr.[2] Källén, who turned out to have great skill and talent. He is working both very quickly and [is] very reliable.

To Heisenberg:

> If you wish to get an answer from letters to Copenhagen, I give you the practical advice not to write to Møller or Glaser but to write to Källén. To wit, he gladly writes long letters (especially when he believes he can prove you wrong, which is almost always the case).

Concerning making error, the distinguished Austrian theoretical physicist Walter Thirring in his autobiographical book[3] writes (translated from German):

> His [Källén's] scientific brilliance was beyond any doubt. In computations he never made any errors. In another area, however, he did once commit an error, namely as airplane pilot, an error that one does not commit a second time.

Källén's correspondence shows that he considered work by Thirring, which was usually sent to him by Pauli to scrutinize, as "sloppy", because sometimes he would find errors in them. Pauli, had to admit that Källén had "won the bet", as he put it. The error was exactly where he had spotted it. But that

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[1] For the benefit of those of the readers who do *not* read this book from cover to cover, some of these comments are repeated in this book to make the chapters somewhat more self-contained. I apologize to those who read the whole book and would like to remind them again of the proverb by the Danish philosopher Søren Kierkegaard: "*Repetition is the reality and the seriousness of life*".

[2] Källén was a second year graduate student and not yet a Dr.

[3] Walter Thirring, "Lust am Forschen" (Seifert Verlag, 2008) p. 126. See also the footnote in Chap. 8.

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was OK. After all it was a matter of circulating articles before publication and errors could be corrected. Thirring would never give up and indeed ended up making lasting contributions to mathematical physics.

We have also quoted[4] "Lehmann's Grand Theorem" (1955) which says: For every error Källén makes I am allowed to make two.

Because of Källén's reputation, there was excitement, almost morose delectation, in the "community" whenever anybody spotted an error in a preprint by Källén. The reader may think that these great scientists were childish. Perhaps they were, due to their enormous dedication to their work and competition for winning the "gold medal at the 'Olympics' of theoretical physics".

Didn't Källén ever make any mistakes? In an issue of the journal "Communications in Mathematical Physics" that was dedicated to Res Jost and Arthur S. Wightman, Jost writes the following about Gunnar Källén making an error which helped Jost in his career[5]:

> "In 1955 I left Princeton for Zürich. My next contact with Arthur [Wightman] was indirect. It was in fall 1956. On my way back from the U.S. I stopped over in Göttingen, where I met with R. Haag, H. Lehmann, K. Symanzik, and W. Zimmermann. In a discussion I discovered a mistake in a preprint by Gunnar Källén on the three-point function in field theory. With great restraint (so it seemed to me) I communicated my observation to Gunnar – and obtained a most indignant reply[6], in which he defended his error. It turned out that Arthur was visiting in Copenhagen, and Gunnar thought he had his support. It matters little how the misunderstanding finally was resolved. It was only important that my ties with Arthur became strengthened. On March 29th 1957 he and Anna-Greta [Mrs Wightman] visited us in Bern. While the ladies toured the city, Arthur and I worked in front of a blackboard in my old Gymnasium: he explained to me his field theory. That was my initiation. In the early morning of the next day my second son Beat was born."

It should be added that Källén was not good at proofreading. One finds quite a few not errors but misprints in his articles. The results are, of course, correct. He had too many demands on his time and was impatient. Sometimes, but not often, he gave the impression of a stereotypical absent-minded professor. He had been the prime cross-examiner at the (open to the public) dissertation of Hartmut Pilkuhn in Stockholm. Pilkuhn used to tell that, during the act

[4] This is quoted in a letter from Källén to Pauli, dated 16 November, 1955, where Källén complains about this attitude of his colleagues at CERN in Copenhagen. This letter is missing in the Pauli Collection.
[5] R. Jost, Comm. Math. Phys. 132 (1990) 4.
[6] Unfortunately, with the exception of correspondence with Pauli, the content of the Källén Collection dates from after late 1958 and, therefore, the letter that Jost mentions is not in it.

and in front of a large audience, Källén, dressed in formal tailcoat, had pointed at a numerical result 3/4 in the thesis and had exclaimed that it must be wrong because when he had done the calculation he had obtained a different result, namely 6/8! Of course, this pleased the audience, which was delighted that the great man could make such a trivial error. According to Pilkuhn, however, it did not seem as if Källén was just play-acting. If he did, he must have been a good actor.

11

Language, Literature, Music and Sports

Many theoretical physicists don't worry about issues having to do with **language**. Even some who say they do care, in practice do not. After all, an equation often expresses more than thousands of words. Källén, on the contrary, was very sensitive to language issues and tried to be a "purist", and not to mix languages.

In Sweden, one sometimes talks about "Swenglish" (svengelska, in Swedish), that is Swedish language with an admixture of English words. The "English words" may actually be of Latin, Greek, old Norse, or other origins. The scientists in Sweden had (and still have) a tendency to quickly resort to English words when discussing physics. Of course, it is not easy to quickly find appropriate words for concepts such as "gauge transformation", "strangeness-changing non-leptonic weak interactions", etc. Källén would invent new words. For example, he would use gradient instead of gauge, etc. In quantum electrodynamics, an area of his research, gradient is fine but, unfortunately, he did not live long enough to tell us which Swedish words to use when talking about concepts such as Non-Abelian gauge theories, where the gauge transformation is no longer just a gradient transformation.

Källén used to point out that Swedish language is closer to German than to English. Therefore, if required, one should resort to Germanic construction rather than English. For example, for the English word "renormalization" one should use in Swedish "renormering" and not "renormalisation" because in German it is "Renormierung". The prefix semi (for example, in "semi-simple Lie groups") should be translated as in German, where semi-simple is "Halbeinfach" which would be like saying half-simple in English. Therefore, he would argue, that in Swedish one should replace semi by half (halv in Swedish). Sometimes we wondered if he was against semi-finals in tennis or soccer championships as well, and would have wanted us to say half-finals instead but we never dared to ask. The essential point is that he really tried to make the students conscious of these issues.

Källén also believed that Scandinavians should communicate with each other in their own languages, even though communicating in English was

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in general easier. Once he expressed his dissatisfaction to Benny Lautrup[1], in Copenhagen, for having sent him a letter in English rather than in Danish. [In the Källén Collection one finds their rather extensive correspondence, Lautrup writing in Danish and Källén in Swedish.] Lautrup defended himself on 9 December 1964 by answering Källén (translated from Danish):

> Of course we should communicate in our respective mother tongues. The reason I used English, for the first time, was that I wanted to be sure to avoid pitfalls, which exist between Swedish and Danish. Niels Bohr is quoted to have said that a person expresses himself most clearly in a language that is not his mother tongue, because only then he uses a word in its simplest signification.

Niels Bohr was, of course, talking about communicating science and not literature[2].

It is interesting to note a comment by Pauli concerning Bohr and languages. In a letter that he sent in 1954 from Princeton, to a biologist (reproduced as letter [1739] in the Pauli Collection) he wrote:

> "I am enclosing herewith a translation of a part of Bohr's letter into English made by the Swedish physicist, Dr. G. Källén. That Bohr's sentences are very difficult to read is true in *every* language. ..."

Källén would sometimes try to tell Pauli about the origin of certain words. This seems to have delighted Pauli, as he would repeat them in his letters to others. As an example Källén had taught Pauli that the English word "window", which is "vindue" in Danish[3] comes from the old Swedish word "vindöga" (literally translated: eye of wind) and would explain to him how this word was born. Källén was, of course, not a linguist but was genuinely interested in the topic.

Actually, most of Källén's letters to his students were written in English. The reason was that he wrote them when he was abroad, and the typewriters available to him did not have the three letters å, ä and ö, which are frequently used in Swedish.

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[1] Benny Lautrup (1939–) has worked primarily at Niels Bohr Institute in Copenhagen. He has done fundamental work in field theory. The reader will meet him again in Chap. 26.

[2] Bohr appreciated good literature and did not insist that it be written in the most straight-forward way. According to Gustafson, one of Bohr's standard presents was the book "Babettes gæstebud" [English title: Babette's feast, written by the Danish author Karen Blixen (1885–1962) whose years of birth and death happen to coincide with those of Bohr's]. This was long before this work was made known to a broader public through an Oscar in 1987, i.e., about 25 years after Bohr had passed away.

[3] Pauli had been exposed to Danish during his frequent visits in Copenhagen and liked to use some Danish words or sentences in his letters.

At times the Källéns would invite all the members of the Department to their home. On one such occasion we noticed that the Källéns had a large number of books on their bookshelfs which were not about science! We couldn't imagine that Källén had time to read **literature**. When asked whether he had read any of them, he promptly answered that he had read them all. He had made it a habit of his to buy and read such books during his numerous trips.

What about **music**? In 1967 Källén was invited to deliver a series of lectures at a McGill summer school in Montreal, Canada. The organizers were keen to offer him some cultural activities during his stay. On 28 March 1967, he wrote to one of the organizers, Harry Lam[4]

> "Dear Harry:
>
> Thank you very much for your letter of March 15th and the kind invitation to the festivities in Montreal. It is really very nice of you to try to arrange tickets already now. I very much appreciate this and want to thank you very much for it.
>
> First of all I must, however, confess that I am completely tone deaf. To me, a symphony by a great composer and a street car turning round a corner sound pretty much the same. Therefore operas, violin solos etc. are rather wasted on me. However, I very much like the theatre, both modern and classical, and also good films. I don't really know what 'theatre of Japan' actually implies but unless it is some opera, I think I would be very much interested in seeing some performance of that company. ..."

Källén also expressed his interest in going to movies. Moreover, he didn't like to go to meetings of the learned societies that he belonged to, the only exception being those of the Royal Swedish Academy of Sciences.[5] However, his correspondence shows that he enjoyed to participate, together with his wife, in festivities organized by such societies. He liked to laugh and have a good time.

Concerning **sports**, in a letter dated March 1, 1962, Källén wrote:

> "P.S. If you think my signature looks a little peculiar that is because I have broken two fingers on my right hand. I was down at a conference in Austria the other day and they had the idea that one should try to do some skiing in between. The outcome of my feeble attempt in this way proved the theory

[4] Harry Lam (C. S. Lam), currently Emeritus Rutherford Professor at McGill University, had spent the 1966 summer at Stony Brook and participated in the research led by Källén, as quoted in the paper denoted by [1966b] in this book.

[5] On such occasions, he sometimes piloted a small plane from Malmö in the south of Sweden to Stockholm.

> I have always been advocating, viz. that unnecessary bodily exercise is very dangerous."

Indeed for several weeks after his participation in the 1962 Austrian Winter School in Schladming, Källén would have someone sit next to him, to write his equations for him.

12
Sense of Humor and on Committees

Källén appreciated humor and had a sense for it. Here are a few examples from his correspondence. To Louis Michel[1], whom he considered to be the greatest French theoretical physicist, he writes, on 20 Nov. 1959:

> "Dear Louis,
>
> … We are sorry to hear about the chicken-pox your children have had. Fortunately, it is not a mortal illness and I hope they are recovering alright. Ours had it about a year ago and they are still alive."

Christian Møller, asks Källén to give a talk at NORDITA, in Copenhagen, on 13 February 1961. Källén responds that he would like to come either a week before or after because (translated from Swedish):

> It should presumably be possible for me to come on 13 February, in case of emergency, but absolutely suitable it is not. To wit, we have a birthday in the family exactly on that day, and I would very much like to be at home.

What Källén does not say is that the day in question is actually his own 35th birthday. On another occasion, Antoine Visconti (addressed as Toni/Tony by his friends) from Marseille informs him that for his visit there, the French authorities require his CV. He answers Toni on 13 February 1960:

> "… I enclose a photostatic copy of a curriculum vitae which was written about a year ago for internal use here in Lund. It is written in Swedish, but that will only be a good exercise for you to translate it! Roughly speaking it says when and where I was born, the name of my parents, various academic degrees I have and tells about my appointments at CERN and NORDITA in Copenhagen and in Lund. There is also a list of published papers, which

[1] Louis Michel (1923–1999) should be well-known for several contributions to physics, such as the Michel parameter in weak decays and G-parity. Michel had once complained to Val Telegdi that his contributions were hardly known in the community. Telegdi had replied (in French, of course) that Michel had himself to blame for that, and had added: Louis that is because you are the only physicist who would say the two elements of the smallest non-trivial Abelian group instead of simply saying ± 1.

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should be slightly modified now. The paper listed 'under tryckning' (in print) has now appeared. ..."

He goes on to list a number of his more recent papers and concludes by stating:

"These are all my sins, as far as I remember for the moment."

It would have been interesting to know how Visconti solved the problem, which he must have done, as Källén was a frequent visitor at his Institute. As is well known, in those days the French authorities loved to collect many documents.

On Committees

Källén expresses a clear opinion on committees in a letter, dated 1968, and addressed to Carl-Erik Fröberg[2] who happened to be on a short visit in USA. Källén wrote (translated from Swedish):

Brother,[3]

It is with the greatest pleasure that I wish to inform you of the sad news that you have been elected to be the dean of the mathematics-physics section [of Lund University], for three years starting 1 July 68. I entrust to you the onerous tasks of moving papers from one pile to another and serving on various pointless (nonsensical) meetings.

In spite of his negative attitude to committees, Källén did serve on a few, among them (as auxiliary member) of the Nobel Committee for Physics and as a member of the Nordic Accelerator Committee.[4]

The late Secretary of the Nobel Committee for Physics and Chemistry, Arne Magnéli (1914–1996), used to recount the dramatic changes that took place whenever Källén was present at a meeting of the Physics Committee. He astonished everyone by his breath of knowledge and strong opinions. When

[2] Carl-Erik Fröberg (1918–2007) was a theoretical physicist in Lund who also had been sent to "Pauli's Court" in Zürich, in 1946. Pauli was satisfied with his work. Fröberg was the initiator of computer sciences at Lund University as well as in charge of its first electronic computer, SMIL.
[3] A less formal way of addressing a man, though not so often used any more.
[4] The Nordic Accelerator Committee was set up in the spring of 1963, with two members from each of the countries Denmark, Finland, Norway and Sweden, as well as a number of Nordic scientists working at CERN. The aim was to look into the possibility of constructing a particle accelerator on the Nordic territory. In January 1964, the Committee presented its results proposing a proton accelerator of 10 GeV and with an intensity of at least 10^{12} protons per second. It was to be designed so that it could also accelerate deuterons and thus give neutron beams. At the end, these plans did not materialize.

I asked Magnéli what Källén's most important contribution had been he answered immediately (translated from Swedish):

> He got Waller to shut up!

Ivar Waller was one of Sweden's most prominent theoretical physicists and much respected for his knowledge. According to Magnéli, however, Källén knew more than Waller on *every* topic that was discussed. This could have been due to the fact that Källén was educated as an engineer; he was very skilled in applied mathematics and was a world expert on the most difficult topic of that time – quantum field theory – which interested Waller a great deal.

13

On Scientific Publications and Journals

In the academic circles, "times have changed" dramatically since Källén's days. To better or to worse? We leave the answer to this question to our readers. In this chapter we would like to present some of Källén's views on a number of academic issues. As mentioned before, the great majority of distinguished scientists of the last century did not publish much. Consider for example an early-starter, the great Wolfgang Pauli (1900–1958). A glance at publications, as defined and listed by Web of Science, shows that his first paper carried the title "The theory of the gravitation and electricity by Hermann Weyl" and was published in Physikalische Zeitschrift, Volume 20 (1919) 457–467 when he was still a teenager! When he died in December 1958 he had "only" about 40 publications! Some of these were not even research papers but articles honoring other scientists such as Niels Bohr, Arnold Sommerfeld and Janne Rydberg. Nevertheless, several of Pauli's publications are true jewels and worth reading even now. Paul Dirac (1902–1983) lived much longer and, by the same measure, produced less than 100 papers, not all research papers but again quite a few of historical nature. From Källén's generation, we could mention John S. Bell (1928–1990) who published about 70 papers, among them many talks and lectures. Again these papers made an impact, not by their number but by their originality and quality. Pauli, Dirac and Bell are only three examples among a large number of such great scientists.

Indeed, as mentioned above, the scientific "culture", if one may use such a word, has changed dramatically. Nowadays young people are under constant pressure to publish. Källén used to say "you should not refer to a paper that you have not read and understood". Perhaps that was going a bit too far. We are sure that he would have not objected referring to a paper that one had read but *not* understood, provided the author explained what the issue was that he/she did not understand and why. The current "culture" forces young people to often refer to papers that they have never seen! Cutting and pasting from lists made by others is easy and giving many references improves the chances of oneself being cited, with the result that a lot of "trash" is produced. After all, the young need to survive. Knowing that they will be judged by their h-index, they learn to play the game. Indeed Källén, like most of the great physicists of

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his time, cared about quality and not quantity. They would have been shocked by some of the stupid current quality-markers, such as the h-index.

The attitude of Gunnar Källén to publishing and journals, as described below, reflects the culture of learning of his time. On January 9, 1965, Källén writes to Jauch[1]

> "Dear Jauch:
>
> Recently I got a letter from the North Holland Publishing Company signed by a Mr. W. H. Wimmers, managing editor, concerning 'an international journal on theoretical physics of which the editorial policy was mainly determined in Europe'. According to that letter you are 'very enthusiastic about the project' and had accepted the editorship of this journal. The North Holland Publishing Company now seem to want me as some kind of co-editor. ...
>
> My own personal feeling and, I believe, also the feeling of very many other physicists (please don't ask me how many because I have no exact figures) is that there are already too many journals which publish scientific papers in physics, both experimental and theoretical ones. However, the most amount of garbage is certainly published in theoretical papers, and if one should wish for something here, it should be a diminishing in the number of papers published and not in increase. Therefore, any new journal on physics, theoretical or experimental, should have a very good 'raison d'être' in my opinion. ..."

Upon further insistence by Jauch, Källén writes a new letter to him, on 18 January 1965. This letter gives further information about how he reasons:

> "Dear Jauch:
>
> Thank you very much for your long letter of January 15th. I believe I understand a little better now how you look at the new journal and what your reasons for supporting it are. However, I am still not quite convinced myself.
>
> You write that you want to 'encourage a better mathematical underpinning of theoretical physics' and a little later you say that you feel 'that the present situation is such that more sophisticated mathematics can be very useful for theoretical physics' as well as a few more statements along the same general line. My own situation in this respect is rather peculiar. Ten years ago, I should have supported you wholeheartedly. Since then we have, however, seen a few

[1] Josef M. Jauch (1914–1974) was one of the distinguished assistants of Pauli and later a professor at the University of Geneva. One summer in 1960's when I was a student I shared an office with him at CERN. Unfortunately, except for exchanging greetings upon arrival and departure, Jauch and I never exchanged a single word. His book with F. Rohrlich, "The Theory of Photons and Electrons" was a standard reference for students of field theory, in those days.

very discouraging examples of what so rigorous mathematics can lead to. As you probably understand, I am thinking of the Wightman school in particular and also of people like Haag, Jost, Kastler etc. I must say that I am extremely unhappy about these developments. A particularly tragic example is the recent book by Streater and Wightman. This book, as far as I can see at least, avoids any contact with the world in favor of rigorous but, in my opinion, uninteresting mathematics. I am afraid that the starting of a new journal in mathematical physics will encourage these people even more (not that they need any encouragement!). Besides, the Haag journal[2] is probably going to do its best (or, rather, its worst) to promote this kind of so called physics. I do not want to take too active part in developments along such lines. (I have accepted to be on the advisory board of the Haag journal, but that really means very little.)

On the other hand, it is certainly true that there is very much sloppy mathematics presented in theoretical physics, e.g. by the British school. The trouble is, of course, that one has to strike a compromise between these two extremes. Probably everyone thinks that he has found just the right compromise – and I am afraid that I can make no exception for myself in this respect. Just because this, to a very large extent, is a question of personal opinion and judgement, I don't know if it is wise to try to force one's own personal views on other people. Also, the effect of trying to do this with the aid of a new journal would lead to a very restrictive policy (and you say on the first page of your letter that you are against too much restriction) and runs the risk of degenerating into a farce. It is perhaps also a little conceited to think that one's own personal judgement is necessarily the right one.

In short, I am still very dubious against any active participation as co-editor of your journal, but, on the other hand, certainly do not want to take the responsibility of having you dropping the project completely. Therefore, my suggestion would rather be first of all to wait for some time (of the order of magnitude of at least a year) and see how the Haag journal develops. Referring to what I have said above, you see that I am not really hoping that it is going to be a very good journal, but, at least one should give it a fair chance and see what happens. …

Perhaps it may interest you to hear that I am going to give an anti-mathematical talk in Zürich in a few days [Monday January 25th]. My intentions are to try to irritate Jost and his school as much as possible. Probably, the attempt will not succeed as these people have a very large inertia and are not easily influenced by external perturbations!"

[2] Here Källén is referring to the journal "Communications in Mathematical Physics". Haag's purpose was to create a European counterpart to the Journal of Mathematical Physics, in the USA. Since 1965, Communications in Mathematical Physics has published about 300 volumes.

Actually, the above "emotional" letter by Källén reflects his disappointment with his own research in the domain of mathematical physics. Indeed, he is criticizing his own collaborator, Wightman! Källén had hoped to find "new physics" in his studies of the n-point functions, partially done in collaboration with Wightman and others, but that had not happened. Therefore, he thinks one should abandon that path. However, many theorists were satisfied with new mathematical "discoveries" and therefore did not share his opinion. They were happy to go on and do what they loved to do regardless of relevance to physics. One could, of course, never exclude possible remote-future applications.

Finally, on this issue, Källén writes, on 3 February 1965, to W. H. Wimmers, North-Holland Publishing Company:

> "When I saw Professor Jauch in Geneva a few days ago, I learned that you and he have decided to wait a little with the realization of the plans about the new physics journal. As you can infer from my correspondence with Professor Jauch, I think this is a very wise decision and I heartily approve of it."

Jauch was not the only person who urged Källén to contribute into starting up a new journal. There were several others, such as Gerald (called Gerry) E. Brown, who wrote to him, from Copenhagen, on 1 December 1961:

> "Dear Gunnar,
>
> Many physicists have felt that it is getting increasingly difficult to keep up with physics in general, and even with one's specialities. … North Holland Publishing Company is planning to add such a letter journal, *Physics Letters*, to their range of journals … "

Brown argues that the new letter journal "Physics Letters" intends to publish quickly and thus help physicists to keep up with what is going on while the regular journals are slow in publishing a submitted article. He invites Källén to be a member of the Advisory Editorial Board. Källén answers Brown's letter on 5 December 1961:

> " … Frankly speaking, it is not quite clear to me how great the need of a new physics journal really is. In one way of looking there are too many journals being published already and it is rather difficult to keep up with the existing ones. However, looking at the problem from this angle one could say that as there are already so many, one journal more or less can't hurt very much. Consequently, I shall be happy to oblige and you can put me on your list as a member of the advisory Editorial Board. I hope the responsibilities involved are not going to be too heavy."

In conclusion, Källén's attitude towards scientific journals is rather complex. He is against axiomatic-mathematical ones but does not object so much against a phenomenologically oriented journal such as Physics Letters. He fears that the former journals encourage the physicists to get lost in a non-physical landscape and do what he used to call "epsilontics". Moreover, in his opinion, although journals are needed there should not be too many of them, so that people would have a chance to at least glance through them and read some articles. He wants to be on the editorial boards but finds that it is not always worth it. The journals move in directions not to his liking and publish papers that are not good enough for "his" journals. Sometimes he resigns. By the time of his death he was on the Board of several journals, among them the Journal of Mathematical Physics and Acta Physica Austriaca, the latter due to his personal interest in the "Schladming Schools" (see Chap. 55).

Källén as Referee

Källén's correspondence shows that he took his referee work seriously and dealt with it very quickly. If needed, he would give kind and fatherly advice on corrections and improvements. However, he could be very tough and unforgiving in dealing with people on the boards of journals, such as chief editors. This happened when he suspected that the editor was demonstrating his power rather than acting on purely scientific grounds. As an example, we quote from a letter that he wrote to an editor, in 1964:

> "Referring to our telephone conversation yesterday I hereby return the paper[3] to you, essentially unchanged. Both the author and I feel very strongly that to add explanations about the problem that bothered you will be more confusing than helpful to the reader.
>
> There are really several things I would like to point out to you in this connection. However, that would probably be a waste of time. Therefore, I will just mention that if ridiculous discussions of this kind are going to occur too frequently in the future in connection with papers which I have cleared from here, I will resign as a member of the advisory editorial board of the Physics Letters."

The Editor wrote back a few days later:

> "You have unfortunately quite misunderstood my letter of 1. December. I certainly was not rejecting the paper, I was only trying to be helpful by sug-

[3] Here we leave out the name of the young author.

gesting an improvement in language on a point where the manuscript led me into some confusion. I am sorry that this has angered you.

Nevertheless, I again repeat that it would be nice to see you soon. Could you find time to come here to give some talks in February?"

Indeed Källén was easily offended by general or vague criticism. He considered them as unjustified demonstration of power. But he welcomed concrete criticisms, such as pointing out errors in equations. In Chap. 74 we will find him scornful of the editor of Physical Review Letters.

14

On the 1965 Nobel Prize in Physics to Feynman, Schwinger and Tomonaga

Already as a young man Gunnar Källén established himself as the leading expert in Sweden in the area of quantum electrodynamics. With an astonishing speed, he became a world expert in this most advanced and difficult domain of theoretical physics of that time. His distinguished four years older collaborator, Arthur Wightman, expressed it with the words:

> "At that time I was trying to puzzle out the grammar of the language of quantum field theory, and here was Källén already writing poetry in the language".

Steven Weinberg, while visiting Stockholm in connection with an "Oskar Klein Lecture"[1] said to me (CJ) that he considered himself as a student of Källén and learned his field theory from him. He said that he had come to Copenhagen taking with him the book of Heitler[2] to learn field theory. But he hardly had opened the book as Källén was nearby acting as his teacher (see Chap. 61).

Indeed, there can be no doubt that Källén understood, better than anyone else in Sweden, the significance of the contributions of various people to the development of quantum electrodynamics. Therefore, it would have been great to know his opinion about the evolution of quantum electrodynamics, his area of expertise. For example, were the theoretical discoveries monumental enough to justify a Nobel Prize? If so, who were the proper candidates to be honored? Fortunately some of these questions can be answered because Källén published a popular article (paper [1965d] on his list of publications) on the subject. Here below we give some excerpts from his article (translated from Swedish). The title is: "1965 year's Nobel Prize in Physics".

Källén considers the treatments of the energy levels of the hydrogen atom by Schrödinger and Dirac equations respectively and their great impact on

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[1] These annual lectures, in honor of the eminent Swedish theorist Oskar Klein (1894–1977) were started by Gösta Ekspong and myself (CJ). Klein's name appears in Klein-Gordon equation, Klein's paradox, Klein-Nishina formula, Kaluza-Klein, etc. Note that the compactification of the fifth dimension, in the Kaluza-Klein formalism, is due to Klein who introduced it as a possible explanation of the quantization of the electric charge.

[2] Walter Heitler (1904–1981) had written a much appreciated book "The Quantum Theory of Radiation" which from 1936 on has appeared in several editions.

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understanding of the spectrum. However, he points out, that measurements by Lamb[3] at the end of 1940's attracted a tremendous attention, as these could not be explained by the above equations.

Then he adds (translated from Swedish):

> These [Lamb's results] were vehemently discussed, among other places, at a physics congress at Shelter Island in June 1947. Several participants were of the opinion that this deviation was not due to new kinds of interaction but depended on the inadequacy of the employed method of approximations.

After this follows a two page discussion of how perturbation theory should be applied, the treatment of virtual photons, the relevant Feynman diagrams, and divergences. Then, he continues:

> An initial step toward getting out of these difficulties was taken by Kramers[4] and Bethe soon after the aforementioned Shelter Island Conference. The major idea was to interpret certain parts of these [results in perturbation theory] as the electromagnetic contribution to electron's fundamental properties such as its mass and charge.

After a rather detailed discussion of this point, Källén notes that Lamb shift is not the only effect that quantum electrodynamics, in its modern form, is able to explain. Another important application is the explanation of the measured value of the magnetic moment of the electron.

Concerning the Nobel Laureates' contributions he writes:

> The great achievement of this year's Laureates has primarily been the formulation of practically useful methods for carrying through the above calculations.
>
> Tomonaga started his work already during the second world war, before Lamb's measurements. His two American colleagues came in somewhat later. Schwinger's first result was the term $\alpha/2\pi$ in the anomalous magnetic moment of the electron. … Schwinger, in his formalism, goes considerably beyond Tomonaga and his methods are much more applicable to practical calculations than Tomonaga's. Moreover, he obtained, in a much shorter time span, several valuable results.

Källén praises Schwinger's achievements, such as computation of the radiative corrections to electron-proton scattering, important for obtaining information

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[3] Willis Lamb received the 1955 Nobel Prize in Physics for these measurements.

[4] In this book we have included a special chapter about Kramers because not only Källén but also Møller and Weinberg in their articles refer to him as a pioneer in connection with renormalization in field theory. We feel fortunate to have obtained a first-hand information on this matter from a student of Kramers. See Chap. 67.

about the electromagnetic structure of the proton which had been honored by the 1961 Nobel Prize to Robert Hofstadter. Källén notes that:

> … even Schwinger's methods are rather complicated and time-consuming. One of Feynman's most important contributions to the theory of quantized fields is his graphic approach which leads to substantial simplifications and makes it possible to go further and do more complicated calculations. Many current computations, would have been impossible without the help of the new methods.

He concludes:

> It is, therefore, exceedingly gratifying that the Academy of Sciences, with the 1965 Nobel Prize in Physics, has crowned the greatest and most crucial achievement of the past two decades, in theoretical physics.

In a nutshell, Källén thinks Schwinger is GREAT, indeed the greatest of the three in this area. The attentive readers perhaps already have the premonition of what was to come afterwards. Källén loved to knock down great scientists from their pedestals. The greater the better. See further the next chapter.

Källén on Popular Presentation of Science

Before discussing the Källén-Schwinger relationship, this is perhaps the right place to insert an aside about an interesting consequence of Källén's above article as it tells us about his opinion on the popular presentation of theoretical physics.

In 1966, Källén received a letter, dated 19 October, from Jesse W. M. DuMond (1892–1976) at Cal. Inst. Tech., who informed him about a recent article that he had written, with the title "Our Knowledge of Fundamental Constants of Physics and Chemistry in 1965" (see Rev. Mod. Phys. 37 (1965) 537). He referred to Schwinger's calculation of the anomalous magnetic moment of the electron (the famous $\frac{\alpha}{2\pi}$-term) and stated:

> "This famous triumph of the theory of quantum electrodynamics plays an important role in our most recent evaluation of the fundamental physical constants. …"

DuMond had been informed by Kai Siegbahn[5] about Källén's article on the 1965 Nobel Prize. In his letter he expressed the opinion that Källén had *not* gone far enough in his Kosmos 65 article to satisfy his curiosity [about where Schwinger's factor $\frac{\alpha}{2\pi}$ comes from]:

> "... I feel rather hopeless about trying to get help from Professor Schwinger or Professor Feynman, I am wondering if you would consent to try to write, in suitably simple semi-popular language, a more satisfactory explanation of this matter. It seems to me that the physical ideas sufficient to explain how Schwinger arrived at just the term $\frac{\alpha}{2\pi}$, should suffice, at least to satisfy the reader's craving for understanding a little better."

DuMond was asking Källén to write a new more detailed article for the Swedish journal Kosmos. Källén replied that he would certainly be unable to do so:

> "One has to realize in this connection that the Kosmos is mainly intended for readers with a very scanty, if any, knowledge of advanced theoretical physics. It certainly is possible to write about new experimental discoveries for such an audience but, to the best of my judgement, it would need a pedagogical genius to give an account of the development of modern theory on this level, supposing that one does not limit oneself to very general and vague statements as I did in my paper about the 1965 Nobel prize. I hope you realize that I should very much like to write the account that you want if I thought I had even a fraction of the pedagogical ability which would be necessary to do such a thing. Actually, about a year ago when I wrote about the 1965 Nobel prize, I was thinking very hard about how much of the technical side of the matter I could present. I finally decided that it was a hopeless task to even try to give an adequate description of the situation and that I had to be content with a very superficial account which is certainly not very satisfactory to the curious mind. I am myself very much aware of my own limitations and I know it is hopeless for me to try to derive $\frac{\alpha}{2\pi}$ for the readers of the Kosmos and I hope you will forgive me for this."

Nonetheless, Källén wrote a couple more popular science articles (papers [1956a] and [1964a] on his list of publications in Part 5 of this book) but without going into subtle details.

[5] Kai Siegbahn (1918–2007), Nobel Laureate in Physics 1981, took active interest in the determination of the fundamental constants. See, for example, E. B. Karlsson and H. Siegbahn, Nuclear Instruments and Methods in Physics Research A 601 (2009) 1.

15

Källén-Schwinger Clash of Personalities

In his article on 1965 Nobel Prize in Physics (see the previous chapter) Källén highly praised Schwinger's achievements, and in fact, more than those of the other two Nobel Laureates of that year, Feynman and Tomonaga.

Källén's article reveals an important aspect of his personality. He who was not[1] particularly fond of Schwinger did not let his personal feelings interfere with his scientific judgement. After the 1965 Nobel Prize was announced, in a letter dated 21 October 1965, he wrote to Schwinger:

> "Dear Julian,
>
> It is with the greatest pleasure we have learned that you are one of the Nobel prize winners this year. From your point of view the award is, of course, long overdue but better late than never. In any case, please accept my heartiest congratulations!"

Källén then asked Schwinger to fit a visit to Lund into his schedule:

> "Both Gunnel [Mrs. Källén] and I are most eagerly looking forward to this opportunity of seeing you and Clarissa [Clarice] here."

After the Nobel ceremonies in Stockholm, the Schwingers were received by the Källéns in Lund and their relations were cordial. In Lund, on Saturday, 18 December, Schwinger presented a lecture on "Magnetic Charges and Quantum Field Theory", in other words on his work on the anomalous magnetic moment of the electron, for which he had received the Nobel Prize. However, already the title shows a trait of Schwinger's personality that Källén generally disliked in physicists, i.e., often incomprehensible elegant formulations. He suspected that, in general, the physicist using elegant formulations was trying to hide his lack of knowledge. This was, of course, not the case with Schwinger but his papers were considered to be very difficult to read partly because of their elegant formulations. Raymond Stora recalled once at CERN that in France whenever a new article by Schwinger arrived the theorists would get

[1] Källén's resentment had its origin in the history of the Handbook Article; see Chap. 19.

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together in order to collectively decipher what the author had in mind. Källén adhered to the opposite style – blunt and straight to the point. Thereby, the potential reader didn't have to "waste" a lot of time debating what the author was trying to communicate. John Polkinghorne[2] recalls an interesting Källén-Schwinger encounter at a "Rochester Conference" in 1957 [1]:

> "Schwinger presented a paper at Rochester 7 in which, in his rather high-flown style, he essentially made that claim for a particular set of amplitudes. In the Proceedings we are told concerning the aftermath of that particular talk that part of the discussion was lost. I suspect that was a diplomatic move. I recall that as Schwinger's ringing tones died away, Källén rose to his feet. He said he didn't know very much about the problem but he knew enough to be able to say that the previous speaker was totally wrong. An instant chill descended on the meeting at this stinging rebuff delivered to a great physicist. Källén was right, all the same. The singularity structure of scattering amplitudes was to prove to be very rich and subtle, beyond naive expectation.
>
> That was a topic I was later to spend several years working on. …"

For Källén social friendship and fierce scientific criticism were perfectly compatible. His friendship with Schwinger, during Schwinger's visit in Sweden in 1965, did not stop him from attacking him later on. He was not intimidated by giants in science and always spoke up his opinion. He also readily admitted his own mistakes, as soon as he discovered them. For him, prestige, credibility as well as other similar concepts, had no infallible place in science. He enjoyed a scientific battle.

Schwinger seems to have been unable to understand the above trait of Källén's personality. According to Ref. [2], later Schwinger had made the following statements:

> "I thought we were quite friendly. Then, in 1967 at Rochester, and at the Solvay Conference in Brussels, Källén came and listened to the lectures. I thought he listened. He must have gotten the idea I was trying to get across and then later he rose and attacked the whole idea. I thought that was treachery of a high order. Källén got up to say, 'Well I've just received word that this has been measured and the value is 1.24 so Schwinger with his value of 1.18 is absolutely wrong.' You know I had quite a history of being confronted with experiment. Everybody loudly proclaimed as being right that later turned out to be wrong. So that was not a very important remark to me. But Källén was being unnecessarily offensive, really. However, the currently accepted value is 1.27."

[2] John C. Polkinghorne (born 1930) is a famous mathematical physicist, whose career took a very unusual turn. There is a great deal of material available about him on the internet.

[This discussion was about the value of the ratio of axial-vector and vector coupling constants, in beta decay of the neutron.]

Concerning the clash of scientific personalities – not understanding one another's behavior – it is interesting to recall a correspondence between Wolfgang Pauli and Madame Wu[3]. On 6 October 1958, she wrote to Pauli (reproduced as letter [3077] in the Pauli Collection) informing him that she has been invited to contribute to a volume commemorating the 80th birthyear of "Miss Meitner, Otto Hahn and Max von Laue". Her plan was, as she put it:

> "Since Miss Meitner has contributed so much in straightening out the complications of the β-decay phenomena in the early period of perplexity, I am going to write a story to emphasize her contribution. However, the central figure of β-decay is the elusive particle which you created more than twenty years ago. …"

She was asking Pauli to provide her with historical information that she could use:

> "Any incidents pertaining to your neutrino hypothesis would be very interesting …"

Pauli answered (letter [3082] in the Pauli Collection, dated 9 October) that also he was writing a historical article on the subject and gave some details, among them the conflicting results on β-decay spectrum in 1922, by Lise Meitner and her competitor C. D. Ellis:

> "Trying in vain to be diplomatic with L. Meitner, I finally confessed to her 'I believe that Ellis is right.' She got a red head and we had a long discussion. …"

Of course, had Meitner been right, there would have been no need for Pauli's "baby" – the neutrino. Pauli also informed Wu about a lecture he had given in June 1931 at a meeting in Pasadena on his neutrino hypothesis. He wrote:

> "My great opponent was Bohr, who – even after wave mechanics, maintained the view[4], that in β-decay the energy law holds only statistically. …".

On November 5, 1958, Wu sent a copy of her article to Pauli informing him that (letter [3106] in the Pauli Collection):

[3] Chien-Shiung Wu (1912–1997), usually referred to as Madame Wu or the First Lady of Physics, was a distinguished experimental physicist. She was also the first female President of the American Physical Society.
[4] Here Pauli refers to a paper by N. Bohr, H. A. Kramers and J. C. Slater, published in 1924,

Figure 15.1 Julian Schwinger with Källén's daughters Elisabeth and Kristina. Courtesy of Kristina Källén

> "However, I omitted to mention the opposition role played by Niels Bohr because I do not want to remind him of this incident."

Pauli's answer (letter [3111] in the Pauli Collection) dated 17 November, includes a number of comments on Wu's article and the statements:

> "That you leave Bohr's opposition role to me, is perhaps quite good. It is not the same, if I say it than if you say it. Of course I treated this in my article at length. Bohr and I were accustomed to this kind of struggles in all friendship: sometimes he was right and sometimes I."

On 15 December 1958, i.e., less than a month after Pauli wrote this letter, he passed away. It should be mentioned that Lise Meitner and Niels Bohr were among those whose friendship Pauli cherished until the end of his life.

References

1. J. Polkinghorne, "Rochester Roundabout, the Story of High Energy Physics" (W.H. Freeman, 1989) pp 66–67
2. J. Mehra and K. Milton, "Climbing the Mountains: The Scientific Biography of Julian Schwinger" (Oxford U. Press, 2000) p. 476

16

Källén and Not Richard Feynman But His Diagrams

Källén didn't approve of Schwinger because of what he considered to be his "unethical behavior" in their planned collaboration. I (CJ) heard him say so! What about Feynman? In his correspondence (the Källén Collection) one finds no answer to this question and there is no evidence that he ever criticized Feynman. However, he would sometimes warn the students about inadvertent use of Feynman diagrams. Field theory for him was more that just a collection of such diagrams. The diagrams were useful, as he noted in his article about the 1965 Nobel Prize in Physics and in the last few years of his life he used them, for example, in his lectures at 1966 Schladming School. His point was that you can get, for example, absurd values for the photon mass by basing your computation on the corresponding Feynman diagram. In the Källén Collection a number of letters and reports deal with this topic. As an example, there is a copy of a letter from Herbert M. Fried, dated 24 Nov. 1959, and addressed to the editor of Physical Review, S. Goudsmit, in which he writes:

> " … Finally, I would like to thank the referee for pointing out two glaring errors in the original manuscript. …
>
> The final remark[1] of the Referee concerning the operator gauge transformation involving the constant L is also gratefully acknowledged."

Herbert M. Fried's Recollections

After finding the above letter, I (CJ) contacted Herbert Fried [1] who communicated the following:

> "I never knew Källén personally, but I do remember the sense of his referee's comment to me; and it was exactly right". …

[1] Note added by me (CJ): The final remark had to do with the fact that $\sum C_i log(\frac{m}{m_i})$ does not vanish by imposing Pauli-Villars regularization conditions, $\sum C_i = \sum C_i m_i^2 = 0$

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The only correspondence I can remember with Prof. Källén (GK) happened more than a half-century ago, and my memory of that small interaction may not be entirely accurate: but what I remember is the following. GK had written a paper to the effect that "at least one of the renormalization constants of QED is infinite". My thesis advisor, Prof Don Yennie (DY) – at that time at Stanford, then at Minneapolis, and finally at Cornell – found a possible error in GK's argument, because, if my memory is correct, GK had interchanged two limiting procedures. This was relevant to me only because DY had shown me some of the correspondence between himself and GK: and the handwriting – small and curly – in GK's letter(s) was very distinctive.

A year or two later, I was a young theorist with a Ph.D., out on my own, and thinking deeply about QED; and I wrote and submitted my first solo paper on the subject to the Physical Review, on the question of the quadratically-divergent photon mass, which appears mysteriously upon evaluating the relevant Feynman graph of a simple, closed-fermion-loop. In due time, back came the Referee's response, hand-written; and I could immediately tell it was from GK. As I recall, he told me not to waste my time doing that sort of calculation, for that problem was well-understood; and I was offended, humiliated, my pride rubbed in the dirt. But, as I learned later from Schwinger, GK was quite correct. I had believed, completely and without question, in the veracity of Feynman graphs; and did not understand that their appalling divergences caused the requirement of current-conservation to be violated, which in turn led to the appearance of an absurdly divergent photon mass. That, at least according to my memory, was my only interaction with Prof. Källén; and, as I now look back over a five-decade career, I do wish that there had been more."

Källén did not live to "enjoy" another remarkable example of ambiguities of Feynman diagrams, demonstrated in the computation of the axial anomaly [2] and its important role in the development of physics. Actually, long before the question of anomalies came up, Schwinger had correctly computed [3] the anomaly (for $\pi^0 \to \gamma\,\gamma$) without using Feynman diagrams but by paying attention to gauge invariance.

References

1. H. M. Fried, private communication to CJ. We are grateful to Professor Fried for sharing his experiences with us.
2. S. L. Adler, Phys. Rev. 177 (1969) 2426; J. S. Bell and R. Jackiw, Nuovo Cim. A 51, (1969) 47
3. J. Schwinger, Phys. Rev. 82 (1951) 664 (see especially Section V of this paper)

Status, Passion for Teaching and Disciples; His Mortal Crash – a Preview

> "I doubt that folks outside Sweden really appreciate the magnitude of the impact that Källén made."
>
> James D. Bjorken (2010)

The previous chapters of this Part have dealt with Gunnar Källén's childhood and youth, his professional career and some aspects of his personality. The following chapters give a description of how he was perceived on the international scene and his scientific legacy – including his Handbook Article on quantum electrodynamics, his book on "Elementary Particle Physics" as well as their impact.

We present his "disciples" and describe his passion for teaching and flying. Moreover, a person of utmost importance in his life – his wife – is introduced to our readers, and his oldest son, Erland tells us about the tragic accident that took his father's life.

Things were going his way, when Källén died. Ever increasing computer power was providing physicists with new tools to attack problems previously beyond their reach. His beloved field theory had come back from the cold and was on its way to occupy the position of the unique superstar on the sky of theoretical particle physics. There was a non-Abelian "gauge-revolution" just around the corner that he sadly missed. He would have loved to get engaged in the subtleties and in, for example, renormalization of complicated gauge theories – the more difficult the better.

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On the International Scene

1960's was a golden age for theoretical (particle) physicists, in the sense that the universities were competing to become the very best in the area and were looking for the most competent and promising scientists. Källén was an obvious candidate. It was known that Källén, as he himself had stated, could not be tempted to take up a permanent position anywhere outside Sweden. But perhaps he could be tempted to come for some time? His correspondence (in the Källén Collection) shows that he had quite a few invitations to go to several places in the USA. Here below, we quote a few of his last "offers". For example, he received a letter of invitation, dated 30 October 1967 and signed by Alfred Schild, Austin, Texas (with copies to Yuval Neeman and Harold P. Hanson) stating:

> "I am writing now to ask you if you would like to come during the next academic year [1968–9], preferably for both semesters, otherwise for one. If you do, I am confident that the University of Texas would make an attractive offer."

From Wisconsin he received the news (from Hiroomi Umezawa, dated 24 April 1967):

> "The University of Wisconsin-Milwaukee has become extremely interested in establishing a strong group of theoretical physicists. … We would be extremely happy if you would agree to come to Milwaukee on this visiting professorship."

T. A. Pond, the Chairman of Department of Physics, State University of New York at Stony Brook wrote to him on 3 November 1967 (with copies to President Toll, Professor Yang, and Professor Dresden):

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> "We hope we are correct in assuming that you will be able to take up your standing summer invitation here next year. … I expect that we could also support a student if you would want him to accompany you.
>
> Please do come, and bring Gunnel[1] and the children too."

A couple of years earlier, in a letter dated 24 June 1965, Pond had transmitted the great atmosphere of the time to Källén:

> "I am sure that John Toll[2] has told you of our hopes and plans to make Stony Brook one of the world's leading centers for theoretical physics. A visit from you would be a wonderful step towards that goal."

Pond offered Källén the position of "Distinguished Visiting Professor of Theoretical Physics".

Indeed, as Toll informed Källén, in a letter dated 29 June 1965, the situation looked very good:

> "The Einstein Professorship has just been awarded to Stony Brook. We were the only University to receive one in a competition of all of the universities, public and private, in the State of New York, so we are feeling very pleased! I am grateful to you for your letter, which helped bring about this happy result. The appointment of Yang to the position must remain confidential until it is confirmed by the Board of Trustees, but I am sure all physicists realize what we have in mind!"

An important issue for Källén was the question of his children's education. When invited to spend an extended period of time (as long as he wanted) at MIT during 1968–1969, he explained why he was reluctant to accept. On 26 January 1968 he wrote to his prospective host, Irving Segal:

> "Finally, as far as the possibility of staying for a whole term or so goes, there is always the problem of children's school to be considered. Please, don't misunderstand this point. I will not in any way say that the American schools are inferior to the Swedish ones, it is just that the change of school for a child (at least for ours) is somewhat difficult. We know from previous experience (when we spent a term in Washington 1964) what problems appear in this connection. As the children are a few years older by now, I am afraid that the problem involved in switching schools and catching up in the old ones afterwards would be more serious now than it was in 1964.

[1] Mrs. Källén, see Chap. 29.

[2] John Sampson Toll (1923–2011) was one of Källén's collaborators and closest scientific friends. The reader will meet him several times further down in this book. He was appointed as President of the State University of New York, effective from 1 September 1965. See Chap. 50.

> In summary, I am very grateful for your invitation and I should have liked very much to accept it if it had been at all possible. However, I hope you understand from the description above that I just don't see any practical possibility to come to MIT for any extended period of time.
>
> Best regards to the whole family from all of us."

In the Källén Collection, one finds several earlier offers as well. In addition to the USA, he was also very popular in France. He had been officially appointed, by the Minister of Education, as (translated from French): "first class professor".

Källén was expected to spend at least one month per year at l'Université d'AIX-MARSEILLE. Among his friends in Marseille were Antoine (Toni/Tony) Visconti and Daniel Kastler with whom he corresponded during many years. The Külléns, especially Gunnel, loved France. In the spring of 1960 Källén wrote to his friend Toni:

> "… As a matter of fact we intend to make the summer 1960 a French summer. … We had intended, however, to spend the month of July in Marielyst, a small holiday place in Denmark, but I am quite willing to be persuaded that the swimming in the Mediterranean is very superior to swimming in the Baltic. …"

In Paris, he was always welcome at IHÉS (L'Institut des Hautes Études Scientifiques, an Institute for advanced studies in mathematics and mathematical physics) where his friend Louis Michel was a staff member. I (CJ) recall that Källén once said that Michel was the best theoretical physicist in France.

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Källén's Scientific Legacy

An inherent feature of physics is that as time passes many of its great discoveries, inventions, and vital theoretical constructions are at best relegated to the museum of historical curiosities. Some barely make it into the footnotes in textbooks and in the worst case, they are sadly buried and forgotten. In theoretical physics, one tends to present the latest version, often neglecting the decisive steps that had to be taken to reach it. Deleting the intermediate steps makes life simpler. Often, it is not the pioneers – those who by their new ideas and hard work contributed to building the great "cathedral" of theory – who get the credit but a few who finish up the work. We also see that every young physicist has heard of Einstein and Newton but some hardly know anyone else, in the field of theoretical physics, except perhaps a countryman or two. Mathematicians are better off. Their theorems, if correct within the assumed framework, are eternal. In theoretical physics, on the contrary, one is dealing with "unfinished business", work to be refined, improved or modified as time goes by. The concept of "TOE – theory of everything" is amusing but, of course, pure nonsense. There is no guarantee that a subsequent experimental discovery will not find the claimed TOE to be incorrect. In the case of Källén we wish to make the following remarks:

- Källén was a pioneer in the field of quantum electrodynamics that has been absorbed by the electroweak theory, which in its turn has become a part of the Standard Model of particle physics.

His name is associated with:

- Källén-Yang-Feldman formalism;
- Källén-Lehmann representation;
- Källén-Sabry "potentials" – work on the fourth-order vacuum polarization which is used not only in quantum electrodynamics and chromodynamics, but also in other fields, such as atomic physics and exotic atoms;
- Method for non-perturbative renormalization in field theory, especially quantum electrodynamics.

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He was a leading figure in all the above areas, and astonished his contemporaries by the depth of his knowledge.

- Analytic properties of the three-point function, was another area to which Källén made an impressive contribution. His definitive work, together with Wightman, on the analytical properties of the vacuum expectation value of the product of three scalar local fields (three-point function) is an impressive piece of work.

Källén, in spite of often expressing misgivings about what he called "epsilontics" in mathematics, actually loved that subject. His mathematical contributions include, in addition to the Källén-Wightman papers, also the articles Källén-Wilhelmsson and Källén-Toll. His correspondence shows that he also found a great deal of joy and satisfaction in his (unpublished) work on the connection between the Bergman-Weil and Cauchy integrals. This paper has been reproduced, at the end of Part 4 of this book.

Källén was a born teacher. It is difficult for us to judge how much this had to do with his background – both his parents were teachers. But what is certain is that he invested a substantial amount of time and effort into spreading his knowledge among the younger generation. He took his teaching at the university very seriously and would produce typed lecture notes on a variety of subjects. This was long before the age of modern technology. Typewriters were used; cutting was done with scissors and pasting with real glue or Scotch tape. As we shall discuss soon, he also produced his Handbook Article and a book on particle physics, at a time when the relevant literature was largely lacking. His solid and systematical treatment of the material and no-nonsense style saved a great deal of time for those who were trying to learn the subject. Surely, the 1999 Nobel Laureate in Physics, Martinus (to many Tini) Veltman[1] was not alone in his experience that [1]:

> "Källén's article in the handbook was my way of entry into the subject. … His more phenomenological work, such as his book on elementary particles, was appreciated by me at the time, but it is completely out of date now. Today everything is gauge theory, in Källén's time there was virtually nothing about that."

Last but not least, he cared about his "disciples", his scientific "children" as long as they needed him. See, for example, Steven Weinberg's testimony [2].

Perhaps that is the greatest legacy of all?

[1] See Veltman's autobiography, written in connection with his Nobel Prize. He has also written a delightful popular book called "Facts and Mysteries in Elementary Particle Physics" (World Scientific, 2003).

References

1. M. J. G. Veltman, private communication
2. S. Weinberg, contribution to this volume, Chap. 61

19
The "Handbook" Article

The story of the Handbook Article begins as follows:

In May 1954 (letter [1791] in the Pauli Collection), Pauli informs Källén that the German publishing company, Springer-Verlag, will soon ask Källén to write a review article on quantum electrodynamics for their encyclopedia of physics (Handbuch der Physik). The editor, not having found someone more famous, was going to follow Pauli's suggestion and ask the young Källén to do the job. Indeed the invitation came and Källén did the work with an astonishing speed and already in September 1954 he sent a copy of his manuscript to Pauli. But his article, which we shall refer to as the Handbook Article [1], in its final version, was published *four years later*! What had gone wrong?

This chapter is devoted to the Handbook Article as its history provides further insight into the personality and scientific style of Källén. Most of the material, here below, comes from the Källén Collection. For the benefit of our readers, whenever an item has also been reproduced in the Pauli Collection we also give the corresponding reference.

A Volume by Pauli, Källén and Schwinger?

The Encyclopedia of Physics, Volume V. Part I (original title "Handbuch der Physics Band V.Teil I") bears the English title "Principles of Quantum Theory I" and was published in 1958. In this Part I there are two articles, the first one by Pauli, on General Principles of Quantum Mechanics, and the second one by Källén, on Quantum Electrodynamics. Both articles are in German.

Having Volume V Part I in your hands, you may be tempted to look for Part II. Don't – you will not find it. It's only role in the history seems to have been to delay the publication of what came to be called Part I.

Volume V was intended to be a true scientific jewel – starting from quantum mechanics and ending up with the most modern aspects of quantum field theory. Indeed the first article in the book is on quantum mechanics written by Pauli [2]. The very first footnote of Pauli's article informs the reader that this article is an updated version of an earlier one from 1933 (i.e., 25 years earlier) and he adds that compared to his earlier article (translated from German):

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> Here some minor changes have been made and the last 30 pages have been left out. Instead detailed articles by J. Schwinger and G. Källén are included.

In retrospect, this is an amusing remark that Pauli must have forgotten to change before publication. It shows what Pauli had expected, but not what actually did happen. His last 30 pages were instead "replaced" by a 196 pages long article (including 48 sections) by Källén and no trace of any contribution by Schwinger.

The original plan was indeed to have three articles in Volume V of the Encyclopedia. Schwinger had agreed to deliver an article on quantum field theory.

Källén, being a man of action, finished his contribution in 1954, while he was a staff member of "CERN Theoretical Study" in Copenhagen. He deposited a copy of it in Lund [3] and also sent a copy to Pauli [4].

Removing 30 pages and introducing minor corrections were trivial tasks for Pauli. Pauli and Källén were finished and all they had to do was to wait for Schwinger. They waited and waited – for several years. Pauli had actually met Schwinger at a meeting in Pisa (12–18 June 1955) and Schwinger had told him that he wished to contribute to the Handbook with an article on "General Principles". Moreover, the publishing company considered Schwinger's promised contribution as such an invaluable jewel that it was, no doubt, worth waiting for. This irritated Källén.

While waiting for Schwinger, Källén kept on enlarging and updating his Handbook Article (for example, by including comparison with the latest data – the field was moving quickly in those days). Pauli notes in a letter dated 29 February 1956 ([2250] in the Pauli Collection) to Jauch that (translated from German):

> Källén has finished and sent off his Handbook Article. I haven't read it. Surely, everything in it is going to be correct but I doubt that his way of presenting the material is going to open up the simplest entry into quantum electrodynamics. Too large is the number of his personal hobby-horses.

As time went by and there was no sign of any activity from Schwinger, not even any news about whether he had actually started writing his article, Källén got impatient. In a letter[1] (letter [2455] in the Pauli Collection, dated 17 January 1957) Pauli tried to get his former assistant Weisskopf, then professor

[1] This letter is the one in which Pauli makes his famous statement: I don't believe that the Lord is a weak left-hander. In other words, Pauli at that time, did *not* believe in parity violation and was willing to bet against it. Later on he stated several times that the reason had been because he couldn't understand why parity is violated in weak but not in strong interactions.

at MIT, to find out the status of Schwinger's article. Did it exist at all?, he asked. Weisskopf didn't think that the article yet existed (letter [2493] dated 5 February 1957 in the Pauli Collection) but promised to talk to Schwinger. For Pauli, during this period, the status of parity violation in weak interactions had become more important than the fate of Källén's Handbook Article. His own article was only an update and its publication was by no means urgent.

Real and Imaginary Parts of Volume V

Källén could not convince the publishers to go ahead and publish his and Pauli's contributions, but he did not hesitate to express his dissatisfaction to Pauli several times. Finally, Pauli took the matter seriously and on 10 October 1957 wrote to Källén (translated from German):

> … there is no point in waiting any longer. The time is ripe for taking an action. I suggest that we write a joint letter directly to Springer-Verlag (with a copy to Flügge [the editor]) with the following content:
>
> 1. The article of Schwinger is superfluous and we refuse to wait any longer for it.
> 2. The proof-pages of your article must immediately be forwarded to the printers so that the entire volume is published promptly, *without* Schwinger's contribution.

If needed, Pauli assured Källén, that he intended to use his personal contact with Jensen[2] to put pressure on the publishing company.

And indeed, Prof. Dr. W. Pauli and Dr. G. Källén did write a joint acid letter to the publishing company, essentially accusing the chief editor of incompetence and holding him responsible for the delay.

In spite of this and some following correspondence, the editor, still hoping to receive an article from Schwinger, tried to delay the publication of Källén's article. But Källén, who had actually seen Schwinger at a meeting in Lille, France, in the beginning of June 1957, strongly suspected that Schwinger had not even started writing his article. The option of dividing volume V into two parts was brought up. Finally, in mid December 1957 Källén gave the publishing company an ultimatum: he would withdraw his article from publication if he did not receive a definitive answer by 15 January 1958!

[2] A few years later, Hans Jensen became one of the 1963 Nobel Laureates in Physics.

This threat worked and already before the end of the year the decision was made to publish the articles by Pauli and Källén in Part I of volume V. Schwinger's article was to constitute Part II.

The last news about Schwinger's contribution comes from Schwinger himself, when he takes part in the "1958 Annual International Conference on High Energy Physics" at CERN (30 June–5 July), and gives a plenary talk on "Four-Dimensional Euclidean Formulation of Quantum Field Theory". In the written version of his talk[3] he gives five references, all of them to himself, but where in one case he also quotes a paper by Jost. His last reference reads:

Schwinger, J. Quantum Theory of Fields, in: Handbuch der Physik; volume V/2, Berlin, Springer (to be published).

He has also a sentence that carries a footnote [⋆]. The sentence reads:

> "A large variety of equivalent forms can now be devised for the Green's functions, based primarily upon the well-established transformation and representation theory⋆ for canonical variables of the first and second kind."

The ⋆ informs the reader that an extended discussion is contained in his Handbuch article!

The physicists had some fun by referring to Part I of volume V as its **real part** and Part II as its **imaginary part**. These nicknames were proposed by Weisskopf who, according to Pauli, was a master of such inventions.

It is difficult to understand why Schwinger behaved the way he did. Why didn't he inform Pauli and Källén that he no longer was interested or that he would be much delayed, whichever was the case. Källén, who abided by formal agreements could not understand Schwinger's behavior and considered it as unethical. I (CJ) recall that he said so himself.

The Handbook Article in a Nutshell

As Pauli noted above, Källén's Handbook Article is not for the beginners to learn field theory from. But for a more advanced reader it has some wonderful features as it is written in a very systematic fashion, introducing the main concepts explicitly in the first few sections and then proceeding to the domain of quantum electrodynamics. The article gives many examples and does not just say "it can be shown" but actually shows how the calculations are to be performed and presents comparison with experiments. For Källén's doctoral students, the Handbook Article was compulsory reading, followed by an oral

[3] The Proceedings of this Conference were published by CERN, Scientific Information Service.

examination in which Källén would ask the student to derive some results on the blackboard. Reading the Article by oneself, which was what was usually done, was not easy and took quite some time (at least for some students) due to its wealth of new concepts and somewhat heavy mathematical machinery. But after going through some pain there was a wonderful reward. The ideas were rich and the formulas beautiful.

Pauli, who by nature was critical[4], wrote to Källén (letter [1886] in the Pauli Collection, dated 7 October 1954) that Källén had been too restrictive (translated from German):

> The worst thing you can do in theoretical physics, is to always dig yourself down into a *single* special formalism. The canonical [quantization] formalism is indeed particularly unphysical due to its strong singling out of equal times.

Källén answered (letter [1893], dated 15 October 1954, in the Pauli Collection) that he totally agreed with Pauli's opinion that one should not use only one formalism and continued to inform Pauli that (translated from German):

> I have indeed made an effort to present various methods in [my] Handbook Article. (Both Fermi and Gupta-Bleuler as auxiliary conditions, perturbation calculations both in Heisenberg representation as well as in interaction picture.)

Källén then noted that since there were to be three articles in the Handbook he had chosen to leave out certain topics.

As was mentioned in Chap. 8, Källén was upset by Freeman Dyson's review of his Handbook Article [see Freeman J. Dyson, Physics Today, July 1958, p. 26]. Here below, I quote from what Dyson had written:

> "It is clear from the content of Källén's article that it must have been written and essentially completed in 1954 or 1955. Its publication was delayed by the editors of the *Handbuch* in a vain attempt to avoid the division of Volume 5 into two parts. The delay is not Källén's fault, but it is none the less regrettable. It has the effect of making the article look old-fashioned. Källén has been careful to revise his numerical results and to put in references to electrodynamic calculations published as recently as 1957. But the point of the article is still definitely 1954. Of the "New Look" which came into field theory after 1954, with various attempts to abandon altogether perturbation theory and the renormalization program, the article reflects no trace. While the recent attempts to rebuild the theory upon a more secure mathematical

[4] Giving compliments did not come easy to Pauli. Indeed, he praised Källén often and a great deal, but only in letters to others.

> foundation have not been notably successful, still it is a disappointment not to find them subjected to Källén's critical scrutiny."

This indeed reminds one of a criticism of Källén's book [on "Elementary Particle Physics"] by one of the referee's on the grounds that it didn't deal with Regge poles (see Chap. 22):

> "Even though they [Regge poles] are no doubt much over-emphasized at the present time, I expect that some aspects of the subject will remain as a useful way of looking at things."

The point is that Källén, as a matter of principle, was *against* including speculations in a textbook or monograph for students! He wanted to teach them longer-lasting basic principles and techniques, in order to help them pursue their research. Textbooks and pedagogical monographs were not the right fora for critically scrutinizing the latest fashion and speculations. Generally, almost all speculations end up in the trash can of history. Only a tiny fraction succeeds in making the transition speculation $\rightarrow$ fact.

The Handbook Article encountered other problems. It was written in German, which was Källén's first foreign language, as he took for granted that it will quickly be translated into English. This did not happen, due to the above mentioned delay. At the time, when the article was written, many physicists had a good reading knowledge of the German language. However, the situation changed dramatically some years later and the students were reluctant to use textbooks in foreign languages, with the exception of English.

The delay in the publication of the German version also meant that the translation into English of the Article started later than Källén had expected, and in fact finished after he had passed away. He considered Julian Schwinger to be responsible for the delay and could not forgive him.

The English Translation of the Handbook Article

Källén agreed that his Handbook Article be translated from German into English and had some correspondence on this matter with Springer-Verlag in Germany. In 1966 he expressed his dissatisfaction with the work that was being done. He gave many examples where the translator had made serious linguistic errors, such as mixing up singular and plural forms and giving wrong English translations. Being very sensitive to linguistic matters (even in Swedish) he insisted that Springer-Verlag find a better translator. He suggested that the best solution was to find someone who understood the physics involved and mastered both languages. If this was not possible, he argued that the translator should have English or German as mother tongue so that at least half of the misunderstandings could be avoided. He left it to the Springer-Verlag to find a suitable person.

Due to his untimely death, Källén could not oversee the translation of his Handbuch Article. He most probably would have updated some of the material in it, as he did in the second (German) edition of his book on elementary particles (see Chap. 22 and 23).

Unrealized Future Plans

Källén's was invited by Springer-Verlag to write a supplement to his Handbook Article. Indeed, he had intended to produce such updates at regular intervals. As a first step, he submitted a 70 pages long article, “Radiative Corrections in Elementary Particle Physics” (paper [1968c] on his list of publications, see Part 5). He had also planned to write a more mathematical article at a later date. However, he did not specify what that meant. Perhaps a review of the n-point function program? There can be no doubt that Källén, had he lived, would have kept on modernizing his earlier work to meet the needs of the new generations of physicists.

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2. W. Pauli, *in* Handbuch der Physik, Band V.Teil 1, Prinzipien der Quantentheorie 1, page 1, footnote 1
3. “Institution for Theoretical Physics” (Lund), Publications, Vol. III (1953–1955), article number 104
4. Letter from Källén to Pauli dated 30 September 1954. This letter that has been listed as missing in the Pauli Collection, is fortunately found in the Källén Collection

20

R. Stora: Källén's Constant M

In his Handbook Article [1] G. Källén states the asymptotic condition in quantum electrodynamics

$$A_\mu(x) \longrightarrow A_\mu^{in} + M\partial_\mu\partial_\nu A_\nu^{in}$$

where the arrow stands for the "weak" asymptotic limit a la LSZ and M is a finite computable constant expressible in terms of the Källén-Lehmann weight function $\Pi(a)$ for the photon two point function.[1]

All the time, there were conflicting points of view between Källén and LSZ, the former insisting on the canonical formulation (which is sick for coupled fields) and the latter who insist on the asymptotic condition.

Amusingly enough followers of LSZ in the framework of LSZ assume [2]

$$A_\mu \rightarrow A_\mu^{in}$$

which turns out to be inconsistent with perturbative renormalizability.

Now Källén's asymptotic condition is perfectly compatible with LSZ since the free vector field $A_\mu^{in}(x)$ is associated with a reducible representation of the Poincaré group.

The best way to sort this out is to look at massive QED (add a mass term to Källén's Lagrangian in the Handbook Article, see below).

The constant M produces in the canonical commutation relations anomalous additional gradient terms, later called Schwinger terms because Schwinger showed [3] generally that such terms were required by Lorentz covariance, in even more general contexts (including gauge theories). In connection with M, Källén quotes Goto and Imamura.

A few comments about massive QED are in order here, because in the past there was some confusion about this subject. It turned out that Källén's Handbook Article and Bogoliubov-Shirkov were the only references where it was enough to add $\frac{1}{2}\mu^2 A_\mu A_\mu$ in the Lagrangian and still get a consistent theory. In fact the two irreducible components of A_μ are renormalized differ-

[1] Note added by me (CJ): Mailing address: PH/TH Department, CERN, CH-1211 Genève 23, Switzerland

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ently. The mass of $\partial_\mu A_\mu$ which is a free field is not renormalized whereas the mass of the transverse part is renormalized.

Massive QED is described in the textbook by Itzykson and Zuber in a non optimal way. See J. H. Lowenstein and B. Schroer [4] for the best treatment in the BPHZ framework. For the old and counterterm approach, see the article of de Calan et al. [5]

References

1. G. Källén, Handbuch der Physik, Volume V – Part 1 (1958) pp 169–364
2. H. Rollnik, B. Stech and E. Nunnemann, Zeits. f. Phys. 159 (1960) 482
3. J. Schwinger, Phys. Rev. 127 (1962) 324
4. J. H. Lowenstein and B. Schroer, Phys. Rev D 6 (1972) 1553
5. C. de Calan, R. Stora, W. Zimmermann, Lett. Nuovo Cimento 1 (1969) 877

21

Letter to K. Nishijima

This letter gives an example of how Källén would communicate with those who sent him their preprints. It is particularly appropriate to reproduce it here as it concerns the constant M. Note that Källén uses the Pauli metric, with indices running from one to four.

On 25 January 1960 Källén writes a letter[1] to K. Nishijima[2] at Urbana, Illinois, which reads as follows:

Dear Dr. Nishijima:

Thank you very much for the preprint of your paper "Asymptotic conditions and perturbation theory" which I received today. Perhaps you will permit me to make a comment about your applications to quantum electrodynamics and, in particular, the question of gauge for the electromagnetic field. I should like to suggest that you have used *different* gauges in Eqs. (5.18) and (6.11). As far as I can see your Eq. (6.11) implies that the matrix element of the electromagnetic potential from the vacuum to a one photon state is given by the corresponding matrix element for the free field. Of course, this is a perfectly permissible gauge, but, as far as I can see, that gauge can never yield Eq. (5.18a). The general form of this matrix element is given by

$$< 0|A_\mu(x)|k >= [\delta_{\mu\nu} - Mk_\mu k_\nu] < 0|A_\mu^{(0)}(x)|k > \tag{1}$$

where the value of the constant M does depend on the gauge. From this formula one gets the following expression for the commutator between two potentials

$$< 0|[A_\mu(x), A_\nu(x')]|0 >= \frac{-1}{(2\pi)^3} \int dp e^{ip(x'-x)} \varepsilon(p)\delta(p^2 + a)F_{\mu\nu}, \tag{2}$$

$$F_{\mu\nu} = \delta_{\mu\nu}[\delta(a) + \frac{\pi(-a)}{a}] + p_\mu p_\nu[\frac{\pi(-a)}{a^2} - 2M\delta(a)]. \tag{3}$$

[1] This letter, found in the Källén Collection, has been retyped by me (CJ). I have checked that it has been correctly reproduced.
[2] K. Nishijima (1926–2009) was a distinguished Japanese theorist. For example, his contribution named the Gell-Mann-Nishijima formula played a very important role in the development of modern particle physics.

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(I use the metric $p^2 = \bar{p}^2 - p_0^2$ etc.) The first term in the first square bracket and the last term in the last square bracket both lie on the mass shell $p^2 = 0$ and come from the one photon matrix elements indicated above. The function $\pi(p^2)$ is the usual weight function with contributions from all other masses and which is positive definite. The very last term in the expression for $F_{\mu\nu}$ comes from the expression involving M in the one photon matrix element. The first term containing $\delta_{\mu\nu}$ gives no contribution for equal times while the second term involving $p_\mu p_\nu$ gives something for one of the indices equal to four and the other one space like.

The corresponding contribution to the commutator is proportional to

$$\frac{\partial}{\partial x_k}\delta(\bar{x}-\bar{x}')\int_0^\infty da\left[\frac{\pi(-a)}{a^2} - 2M\delta(a)\right]$$

Obviously, the particular gauge where $M = 0$ cannot give zero for this expression because of the positive definite character of the function π. Further, there is just one particular value of M for which this expression vanishes and where one has the conventional commutator. However, I can see no possibility to have both your Eqs. (5.18a) and (6.11). The conventional gauge of perturbation theory is the gauge where one has your Eq. (5.18a). In that case one finds $M = \frac{\alpha}{30\pi m^2} + O(\alpha^2)$.

Of course, I realize that the thing I mentioned here is a very small point and can presumably be changed in your paper without altering any of the main conclusions. However, I like to mention it to you anyhow.

Perhaps you will also permit me to draw your attention to the paper[3] in H.P.A. 26, 755 (1953), which was published a couple of years before the Takahashi paper in your footnote 7. A copy of that paper is mailed to you separately.

Best regards

Gunnar Källén

Note that Källén, as usual, is very quick in finding the error and informing the author. Further, the tone of the letter is very polite as it is written to a Japanese gentleman. In his published paper (Phys. Rev. 119 (1960) 485) Nishijima has "a note added in proof" in which he thanks Källén for suggesting the use of the special gauge which gives the correct result.

[3] In this book, denoted by paper [1953a] and reprinted in Part 5.

Källén tried to respect what he thought were the receivers' norms. He was informal in dealing with Americans, very formal but tough in his correspondence with Pauli, etc., but always to the point and never hiding anything under the rug.

22

Källén's Book: "Elementary Particle Physics"

In about 1963, during one of the daily departmental tea gatherings, Källén said that he had been, as he put it, "inactive lately", and hadn't "published anything". He was immediately corrected by one of the students who exclaimed: you have written a whole book! a fact that he seemed to have had forgotten! Källén laughed and looked happy again, as he used to do.

Here, we give a somewhat detailed account of the coming into existence of this book [1] and how it was received. We also describe Källén's amicable as well as quarrelsome encounters with the publishers and their referees.

Källén, a field theorist, wanted to learn particle physics. This he did by lecturing about the subject during the academic year 1961–62. The word got around that he, perhaps intended to write a book, based on his lectures and in no time at all he was approached by publishing companies. His archive shows that already in October 1961 an agent from Addison-Wesley Publishing Company visited him in Lund and shortly after the Company wrote to him that they were: "delighted to hear of your proposed text, Elementary Particle".

Källén and the Publishing Company

Källén's idea was to write a book for students, its purpose being:

> "… to provide the general background necessary for students who want to specialize in the field of elementary particle physics. …
>
> We concentrate on methods and techniques which we feel to be of methodological interest and apply them to specific problems mainly as illustrations."

The agreement between Källén and Addison – Wesley was signed in July 1962. In November he was informed by the Company that:

> "Most physicists also agreed that if the book is to be widely used as a text in this country[1] it cannot be too formal."

[1] The country being USA.

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Apparently, the Company was afraid that the book could be "too mathematical". Källén quickly provided a list of topics he intended to discuss, which was sent to referees. He pointed out that the important topic of $SU(3)$-symmetry (what came to be known as the eightfold-way) is missing but that he had to "draw the line somewhere".

One referee wrote:

> "Another subject I notice as missing is that of Regge poles. Even though they are no doubt much over-emphasized at the present time, I expect that some aspects of the subject will remain as a useful way of looking at things. It certainly adds another dimension to the high energy phenomenology. These criticisms aside, I think the project is excellent."

Fortunately, Källén did not take the referee's advice. Indeed, in just a few years experiments showed that these models were *not* describing data, whereby one had to complicate the picture by introducing, for example, Regge cuts, sense-nonsense signatures, etc.

Källén personally delivered his complete manuscript to Addison-Wesley Publishing Company in June 1963, during a visit to United States. The book was sent to a referee (the 1961 Nobel Laureate Robert Hofstadter). Källén, always acting very fast, was dissatisfied that the reviewer took inordinate amount of time to do his review. Finally, the referee returned the manuscript along with his remarks that he had "no criticism and that the book was of the highest quality". Källén did not appreciate this slow procedure. He expressed this in a letter, dated 24 Oct. 1963 to Addison-Wesley:

> "... (I am) seriously annoyed about what appears to me to be an absurd time delay in the handling of my manuscript by Addison-Wesley. ... I and I alone am responsible for the content of my book. No reviewer in the world could have made me change anything of importance even if he had wanted to."

Addison-Wesley wrote back, on 1 November 1963,

> "You are quite right to be annoyed at the delay. We were also. Nevertheless, it would be quite irresponsible for any publisher to release a textbook directly into production without at least one technical specialist reading it."

After having received the proofs, Källén wrote an angry letter to Addison-Wesley:

> "Someone has tried to 'update' the foreword by changing 1963 to 1964 everywhere. I strongly object to this procedure. The book and the foreword were

> written in 1963 and this is evident to every expert looking at it. I do not want my professional reputation to suffer from the incompetent handling of my manuscript by Addison-Wesley. I was originally promised a publication time of 6 or at most 7 months and I cannot help that this time has been doubled. It will do more harm than good to me and to the book to try to cover up this situation by a phony date!"

On 16 January 1964, Addison-Wesley, no doubt, pleased him with a kind letter and the words:

> "The speed with which you handled the return of galley proofs on the first four chapters amazed us."

References

1. G. Källén, "Elementary Particle Physics", Addison-Wesley (1964) 546 pages

23

More on His Book and How It Was Received

Källén's book mirrors his general approach to physics. It starts with "down to earth" issues that every particle physicist should master, such as how does one determine the masses, spins, etc. of various particles. How to deal with relativistic kinematics is another such basic topic. Then there are treatments of strong, electromagnetic and weak interactions of elementary particles, calculations of cross sections, etc. The book treats both formal theory and comparison with experiments.

The book was published at a time when books on elementary particle physics were rare. Källén introduced new elegant concepts, from mathematics into physics, that were quite useful, such as what young people started calling the "Källén λ-function", $\lambda(x, y, z) = x^2 + y^2 + z^2 - 2xy - 2yz - 2zx$, that is used to determine the area of a triangle with sides x, y, z and thus enters into two-body kinematics.

From the beginning, Källén had realized that the field of elementary particle physics was progressing very rapidly and his book would need future revisions. There can be no doubt that he had intended to do so (see below). However, methods and techniques would often be useful, even if the field changed direction.

Källén's book, finished in June 1963, appeared in 1964 and was very well received, especially by students, experimentalists as well as most theorists. For students it served just the purpose Källén had intended. During my numerous visits to CERN later on, I (CJ) heard from *many* that they had learned particle physics from Källén's book. They had appreciated his direct bottom-up style, starting from foundations and ending up with comparison with the latest data.

Reviews of Källén's Book

I now present a few reviews of Källén's book, found in the Källén Collection. [Hopefully, he did not throw the "bad ones" away, if there were any. It would have been interesting to include them.]. The reviewers wrote:

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- Eugen Merzbacher, Physics Today, June 1965:

  "Generally, the great virtue of Källén's book is that no topic is taken up unless the author can arrive at an angular distribution or a decay rate which can be compared with experiments."

- Yuval Neeman, Mathematical reviews, May/June 1965:

  "Indeed, from many points of view the book is really refreshing: it is extremely close to experiment and is probably the best available description of the phenomenology of high-energy physics."

- Paul T. Matthews:

  "The appearance of this book by Professor Källén is one of those extremely agreeable surprises, only surpassed by the now famous Feynman lectures in general physics. … Professor Källén has a keen, critical mind and writes with confidence and precision. One reads with the feeling that he personally has checked in detail every result which he quotes. This is the best book of its kind at present on the market."

- Several physicists wrote to Källén and thanked him for his book, for example, Jack Steinberger[1] from CERN wrote to him:

  "Dear Källén:

  This is a Fan letter.
  I am preparing some lectures for Zürich this spring and am having a chance to read your book.
  It is beautiful.

  Best wishes

  Jack Steinberger"

  Coming from Jack Steinberger, this is indeed a great compliment as he is known to be a critical person with very high standards.

[1] Steinberger received a Nobel Prize in Physics in 1988.

The German Edition and its Revised Version

The German translation of Källén's book was done quickly and the German version was published in 1965 [G. Källén, "Elementarteilchenphysik", Bibliographisches Institut (1965)]. All along Källén had kept on working on a revised version of his book, which was to be published first in German in 1969. The most important new elements were the addition of a chapter on SU(3)-symmetry, introduction of helicity formalism, and the phenomenon of CP violation in the kaon system.

After Källén's death six of his scientific "children" and one "grandchild" decided to finish the work. These were: Bo Andersson, Peter Arrhén, Jan Bohman, Lars Gislén, Gösta Gustafson, Cecilia Jarlskog, and Mats Lyberg (the first two have sadly passed away). In fact the changes made by these people were insignificant. Moreover, Jack Steinberger, who had done fundamental work on CP violation, kindly agreed to expand upon and update Källén's original writing on that subject. The second edition, in German, was published in 1974.

Källén's book was also translated into Russian.

Källén's correspondence shows that he had planned to update his book by including more recent developments in particle physics.

Finally, Källén was also approached by the North-Holland Publishing Company to write a book on Field Theory. The Managing Director of the Company wrote to him, on 11 January 1968:

> "As you may remember I am prepared to offer you the best of terms, because we really want to publish your book, which we regard as a most valuable European contribution to the world literature."

Regrettably, Källén passed away already in the October of that year!

Passion for Teaching and Disciples

> "The enthusiasm of the lecturer is perhaps even more important than the actual material presented."
>
> Källén to Sabry (1964)

Källén loved teaching and took it very seriously. He strived to give young people a solid foundation in theoretical physics to stand on. It was not a question of "it has been shown" but rather actually being able to derive the results one was using. He modernized and introduced new courses in Lund which influenced the standard of education elsewhere in Sweden as well.

In the next few chapters, we describe him as a teacher and supervisor. We are also helped by Bengt E. Y. Svensson and Karl-Erik Eriksson who recount their experiences.

Källén's most distinguished "disciple", according to our definition, is Steven Weinberg. His depiction of Källén as a supervisor is presented in Part 3 of this book.

Figure 23.1 In accord with the former Swedish academic traditions, Källén (far right), assisted by Erik Turner Karlson (middle) and Hellmuth Hertz, cross-examines his first PhD student (Arne Claesson, not shown) on 23 November 1957. Two weeks later Karlson, Källén's second PhD student, defended his thesis, again with Källén as the major cross-examiner but then with Claesson in the jury. Both Karlson and Claesson became professors, in Uppsala and Umeå respectively. Hellmuth Hertz (1920–1990), an experimentalist, was a professor of electrical measurements at Lund University and one of the most gifted inventors in its history. He is famous – among other things – for his ground-breaking work on ultrasound. (Courtesy of Erik Turner Karlson)

24

Källén Addresses the Student Union

At his professorship inauguration ceremony in 1958, Gunnar Källén delivered a speech on elementary particles. Afterwards, he addressed the students. It befits to recount what he had to say to them (translated from Swedish):

> The tradition calls that one at an occasion as this also says a few fatherly[1] admonitory words to those members, who happen to be present, of the LUND'S STUDENT UNION.
>
> I feel a little unsure about this task but, due to lack of something better to say, I would like to take this opportunity to praise the untrammeled studies that have been pursued following old tradition and still <u>can</u> be pursued at least at the Swedish universities. Nowadays there is a certain tendency to consider the free studies as highly ineffective, and there are attempts from essentially all relevant instances to constrain the academic education to regulated lectures and exercises, where all the material of a given course is covered in detail. Presumably, one has no difficulty in establishing statistically that such method of teaching gives better results taking the measure to be the number of examinations per student and per unit time – or whatever the proper unit could be. In spite of this I wonder if one does not sometime underestimate the disadvantages caused by this system. Let me try to illustrate my point by telling you a little story whose truth-value I can in no way guarantee – "a story is not necessarily a lie just because it perhaps has not happened". It so happened that under such a strict course, once one of the students was absent from one lecture. When he appeared next time, the lecturer threw a question at him, related to a particular point that he had gone through at the previous lecture. When the student couldn't answer, the lecturer firmly fixed his eyes on the sinner and said: Either you gentlemen should be <u>here</u>, or <u>in addition</u> you should know something.
>
> Indeed, it is often convenient to just "be present" and be spoon-fed <u>everything</u>, but isn't it sometimes at least as stimulating to bone up on a problem, by oneself, and thus "know something"? Of course, there are always more difficult parts of a course, that are not so easy to get through by oneself without any

[1] At the age of 32, he was a "young father", especially since in those days there was no limit to the number of years one could be registered as student and the university had a number of "eternal" students.

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help at all. But the gap is a large one between, if I may say, “old fashioned” lectures on such parts of the course and a thorough analysis of every individual detail all the way up to a master degree. Often it is perhaps so that the knowledge one acquires by oneself, by thinking about how things are related, sticks better than that obtained by uncritically accepting what someone else has come up with. Now, if I may address those of you who plan to continue to higher academic degrees, perhaps defend your thesis and continue doing scientific research, I don’t believe that independent studies are less effective in the long run than spoon-feeding – in fact it is just the opposite.

25
As a Teacher and Supervisor

In spite of his advice to the students, in the previous chapter, that you don't necessarily need to go to lectures, Gunnar Källén took his teaching at the university very seriously and loved it. He wanted to give the students a firm ground to stand on, but he tried to avoid spoon-feeding them.

Källén was very conscious of the value of basic academic courses. He would give courses on a variety of topics, such as classical mechanics, electricity and magnetism, quantum mechanics, field theory, group theory, etc., but did not repeat the same course. The lecture notes were taken by students and typed by his secretary. This was, of course, long before the age of latex and internet!

His style of teaching was as follows: when Källén entered the lecture room the audience would stand up. He would greet his listeners by bowing down; a faint smile would appear on his face and the lecture would start. Once I (CJ) chose to remain seated when Källén entered the lecture room. Being pregnant and expecting a child any day, I decided that standing up was too much of an effort, due to spatial boundary conditions. Källén looked at me and simply waited until I, with some effort, did stand up. The order was re-installed; Källén looked happy and the lecture started.

Källén was pedagogical and systematical. He presented the material neatly and joyfully. He would use the blackboard efficiently, piling up properly numbered equations with text in between. At times, he had already, before the start of the lecture, written some of the important equations from earlier lectures on the blackboard to remind the students of the highlights. He loved to do detailed analytic computations on the blackboard and to use all kinds of mathematical tricks. At the end of the lecture sometimes he would exhibit his satisfaction by writing "voilà" on the blackboard. Even if one didn't understand what he had been talking about (the topic being too advanced) one important message was transferred to the student: it must have been something truly exciting. He was an inspiring lecturer. What he, however, did not do was to discuss the "history" of the subject that he was discussing, or tell stories, as was extensively done by his colleague Gustafson.

In June 2011 I had the privilege of participating in a scientific symposium in honor of Martinus (Tini) Veltman, on the occasion of his 80th birthday.

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One of the speakers, Bernard de Wit[1], gave an account of what the situation had been at Utrecht when Tini, as a newly appointed professor, arrived to Utrecht and how for those who were students at the time, "this marked the beginning of a new and exciting period". He himself was one of those students and one of his very first experiences had been:

> "Shortly after that, I attended the theoretical physics colloquium for the first time. Tini had invited Gunnar Källén, who gave a seminar on radiative corrections to beta decay. Tini respectfully introduced him, but then we got our first shock when the speaker moved one of the blackboards, uncovering another blackboard full of formulae summarizing everything that was known at the time about beta decay. We were completely lost, and after a while the only thing we could do was to study the reactions of the various staff members to the talk. During the break we concluded that none of them, with the possible exception of Tini, understood anything of the talk. After the break, the seminar continued and Tini took off his jacket, so that we decided that even for him, it required a major effort to follow the lecture, a conclusion that made us feel a lot better."

Similar stories have been told by several other physicists as well. The students didn't understand much but were impressed – Källén was certainly talking about something worth learning and understanding! They would remember such events for many years.

The recommendation letters that Källén has left behind bear ample evidence that he had a genuine fatherly interest in helping young scientists, not only his own students but also those of other supervisors. He would as readily answer letters from novices in the field as he would those from experts (see also Chap. 9). Often, his answers were several pages long and contained technical details, hand written equations and thorough discussions. The questions addressed to him were not only about field theory but fell into a broad range, such as how does a maser work, angle and angular momentum uncertainty relations, etc.

For a reader who does not read this book from cover to cover, we would like to repeat that in contrast to his often incredible kindness to young people, Källén was very tough on established scientists and colleagues as well as on himself (see also Chap. 8). He enjoyed bringing down a physics superstar from his high pedestal. He demanded total honesty and a clear separation of facts and guesses. A conjecture could be OK provided it was presented as such and not as a fact.

[1] In due time, Bernard de Wit himself became a professor of theoretical physics at Utrecht University. Several of Veltman's PhD students have risen to very high academic ranks.

Sometimes the students would look forward to the "battle of giants" at seminars or conferences where Källén was present. Of course they couldn't know who was right but they would remember the occasion and talk about it. What was the issue when, for example, an established scientist wanted to hide a small charge behind the moon and Källén was wildly protesting? What about the legendary Källén-Johnson fight at the Schladming winter school in Austria, that everyone was talking about?

Studying Theoretical Physics During Källén's Time

Källén would receive enquiries from young people about the possibility of doing a doctorate degree with him in Lund. His typical answer, according to his correspondence, was:

> "… Our university system is not as formal as is the American system. In particular, there is no very fixed program for people who study for the equivalent of an American Ph.D. Occasionally, lectures are given about subjects of interest for these students but mainly the student is supposed to learn by himself by reading from books. A consequence of this set up is that practically anyone who wants can be admitted, provided he can show formal documentation showing his having basic degree from either a Swedish or a foreign university. If and when you come here, you are perfectly free to try immediately to pass the exams for whichever of our requirements you feel you can meet."

How could it have been "as formal as is the American system" as Källén puts it? He was essentially the only professor at the Department of Theoretical Physics in Lund who could give high-level courses and it was impossible for him to cover "everything" that the students needed. He would give each course, including lower level courses, only once.

It turns out that the situation in Lund was not unique. For example, also in Uppsala, where the eminent theorist Ivar Waller "reigned", there were hardly any formal PhD courses. Nonetheless, the system did produce a number of distinguished scientists. The professor inspired and the students learned by themselves and from each other.

Källén's correspondence also shows that he put a great deal of effort into improving the standard of education in theoretical physics, not only in Lund but also elsewhere in Sweden. His idea was that institutions in Lund, Gothenburg, Stockholm and Uppsala, locations where theoretical physics was growing, should collaborate and exchange lecture notes. In his correspondence, there are several letters exchanged between him and Ivar Waller (Uppsala), Oskar Klein (Stockholm) and Nils Svartholm (Gothenburg). Källén sent stencil copies of his lectures to these colleagues.

A colleague, Professor Bengt Nagel from Stockholm, recalled after Källén's death that (translated from Swedish):

> It was due to his initiative that modern physics comprising theory of relativity and quantum mechanics was introduced in the curriculum at an early stage at Lund University, a reform that later was implemented in all other Swedish universities.

Nowadays the situation is completely different. There are quite a few professors at each department and the PhD education is highly formalized.

26

Young Scientists Supervised by Gunnar Källén

A typical acknowledgement given to Gunnar Källén, in an article written by a young scientist reads:

> **"I am deeply indebted to Professor GUNNAR KÄLLÉN for suggesting this investigation and for his invaluable guidance and persistent help through all stages of this work."**

Another typical one is where the author thanks Källén

> **"for suggesting the investigations mentioned above, for his stimulating and generous advice and many helpful suggestions."**

We have taken such acknowledgements as the definition that the young person in question has been supervised by Källén and is thus a "disciple" of him.

Källén was an astonishing supervisor. Like a magician having a large reservoir up his sleeves, he quickly produced research topics within a broad repertoire, for the students to work on. In a letter (dated 31 January 1964) to Asim Barut who had invited him to a symposium at Boulder, Colorado, Källén writes:

> "I see that one of the topics on your list is 'Analytic properties of Lorentz invariant amplitudes'. This is a subject which is very near to my heart, and if it fits with the general scheme of the symposium, I think I should like to give a couple of lectures about the general situation in this field."

Indeed quite a few of his students did work in this field. But he also distributed other completely different topics to his disciples. These could fall within particle phenomenology (for example Delbrück scattering, radiative corrections and photo-production) or beyond particle physics (for example computation of Slater integrals for Argon II, or computation of the intensity of forbidden lines in some atoms).

Asked by Sigward Nilsson, from Stockholm University, to supply statistics for a committee of CERN, Källén on 2 May 1966 listed 10 research students in "high energy physics" and three in other areas, all under his supervision.

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The list of those who were supervised by Källén, according to the above definition includes:

Bertil Almgren	Bo Andersson	Gunvor Arrhén
Peter Arrhén	Jan Bohman	Arne Claesson
Göran Eriksson	Göran Fäldt	Per Grönlund
Roland Gudmundsson	Gösta Gustafson	Cecilia Jarlskog
Keijo Kajantie	Erik Turner Karlson	Bengt Kjöllerström
Benny Lautrup	Nils Henrik Möller	Irja Nieminen
Poul Olesen	Bengt Petersson	Bengt E. Y. Svensson
Hans Svensson	Steven Weinberg	Tomas Wennström
Hans Wilhelmsson		

More than half of the people listed above ended up as university professors. The most prominent of Källén's disciples on the above list is the Nobel Laureate Steven Weinberg. His account of what Källén meant to him is found in this book (see Chap. 61). The above list has been compiled by me (CJ), by going through the publications stored at the Department of Theoretical Physics in Lund as well as Källén's correspondence. For example, Benny Lautrup and Poul Olesen, both from Copenhagen, had an extensive correspondence with Gunnar Källén. Why and how this happened is recounted by them as follows. Benny Lautrup writes (translated from Danish):

> Poul and I both corresponded with Gunnar Källén, because he was the only person in our vicinity who understood quantum field theory. He was very kind to me, I visited him in Lund and also at his summer house …

Poul Olesen's testimony reads:

> "It is reasonable that I be included on the list. As you point out, I had extensive correspondence with Källén when I was a student. The problem in those days was that there were no teachers here in Copenhagen that knew anything about field theory. Thus in reality Källén was my only supervisor, even though this relationship was not formalized.
>
> My initial contact happened when I was a second year undergraduate student, when I read his famous Handbuch article. I had several questions, and I wrote to him concerning things I did not understand. Today I am really impressed that he answered these questions in detail, often sending me copies of his handwritten notes (this was before Xerox copies became available). If I

> had made a slightly independent contribution to something he would always encourage me to think more about it or in a gentle way indicated that this was perhaps not so interesting. I learned a lot. Källén was a great teacher."

Of course, it is possible that not *all* of Källén's disciples are on the above list. Did he suggest research themes and supervise young theorists during his visits abroad? I don't know the answer to this question. Källén was only 42 years old when he was killed in a plane crash. I wonder what the number of his disciples would have been by now, had he still been alive.

27

Bengt E. Y. Svensson: Some Reminiscences of Gunnar Källén as Supervisor

Gunnar Källén started his professorship in Lund in the fall of 1958 with great expectations. He clearly realized that he was settling in an environment which required him to build up a scientific milieu in his field of physics largely from scratch.[1] His feelings and intentions at that time can be captured in a story I have from Gunnar's fellow student and colleague here in Lund, the eminent nuclear theorist Sven-Gösta Nilsson. Sven-Gösta had expressed his astonishment that Gunnar, who had a position at NORDITA – then attached to the Physics Institute in Copenhagen – wanted to transfer from the sprawling Niels Bohr's institute to the calmer Lund. "You will find nothing in Lund" said Sven-Gösta. To which Gunnar replied: "Sven-Gösta, zero is larger than minus one!"

Gunnar immediately started to build up a group. We were a handful of aspiring young students who had approached Gunnar to have him as supervisor for our PhD studies. Gunnar's main research interest at that time concerned fundamental problems within quantum field theory. Based among other things on an earlier collaboration with Arthur Wightman, he wanted to extract as much information as could be squeezed out from as small a number of fundamental assumptions as possible. One important such assumption was formulated in terms of analyticity properties of the so-called n-point functions. I was assigned the problem of finding an integral representation for the three-point-function, i.e. the vacuum expectation value of three (scalar) fields extended to complex values for its three independent, invariant kinematic scalar variables. The basic tool for solving that problem was the so-called Bergman-Weil integral.

The role as supervisor fitted Gunnar. He undertook it with his usual frenzy. He was known as an *enfant terrible* on the international research arena, not showing any mercy in his criticism of people and ideas he thought wrong. In this respect he was a worthy follower of his supervisor, collaborator and friend,

[1] Bengt E. Y. Svensson: Department of Astronomy and Theoretical Physics, Lund University, Lund, Sweden.

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Wolfgang Pauli. But with us students he was very different. He of course had no problem in finding appropriate research problems to give us to work on. The door to his office stood always open, *nota bene* when he had no other visitor. He was completely up-to-date with – or rather, presumably, well ahead of – the current status of just my problem; all of us students had exactly the same experience. He gave generous advice as how to proceed. Of course, he was demanding too, critical when we did not perform as he had expected and always requiring us students to stick to any time limits set.

The social life at the department was not neglected either. It was more or less mandatory – but of course also a great favour! – for us, the research students, to attend the departmental daily afternoon tea gathering where any problem could be taken up. And he extended his social obligations to outside the department. With his wife, Gunnel, he regularly and generously invited us students, with our respective spouse, to his house for parties. These were enjoyable evenings although, even on those occasions, Gunnar did not compromise on his role as the professor and supervisor.

Källén was not only the established scientist and the demanding but supporting supervisor, he was also a great teacher. It sat partly in his genes: His father was a brilliant and successful high school teacher, at least for those students who could live up to his very high demands. I know, since I had Källén senior as my high school maths teacher for two years. Gunnar undertook to reform the undergraduate curriculum in theoretical physics in Lund from the ground up. His lectures in basic relativity theory and quantum mechanics appeared – and still appear – as outstanding examples of clear and lucid presentation. And my notes from his lectures on a more advanced level, be it statistical physics, quantum electrodynamics or group theory or any other field he introduced us students to, have a permanent place on my bookshelf where, up to this date, I consult them every now and then.

By and large, I think Gunnar was a little disappointed of the general performance of his first "real" group of students; he had been involved in supervising doctoral students before but then on a more ad hoc basis. True, all succeeded to pass the intermediary degree, the fil lic[2] that the Swedish system required at that time on the way to the PhD. But only two of the more than a handful of us who started with him in the fall of 1958 did proceed further to the PhD. He was much more successful with his next generation of students.

Having passed my intermediary degree, the fil lic, I was encouraged not by Gunnar but by the departmental head, professor Torsten Gustafson, to apply for a fellowship at CERN. I do not know how much Gunnar knew of my plans. But when I was accepted at CERN I of course had to tell him. His

[2] fil lic stands for the Swedish academic degree "filosofie licentiat".

characteristic remark was “Well, I wish you good luck. By the way, are there any good theoreticians at CERN nowadays? Let my think: Glaser is there. Is there anyone else?”

So I came to spend two good years at CERN, switching field to more phenomenological problems than what had been on Gunnar’s agenda up till then. (As we know, he soon realized that his program of solving problems in field theory using general properties of n-point functions was too optimistic. So he later switched to elementary particles, among other things writing a very successful textbook on elementary particle physics.) And of course, I came to know that CERN was full of good theoreticians as well as good experimentalists. On one of my visits back home during those years, being very proud of what I had accomplished, I told Gunnar about it. He was not terribly impressed, so I mobilized all arguments I had and told him that Leon van Hove had approved of my work. “Well, it could be good anyhow”, he mumbled.

To Gunnar’s advantage it must be said that I had no problem whatsoever to get my PhD with him in Lund mostly on the work I did at CERN. He also arranged for me to get a position as assistant professor in Lund. I was on leave of absence from that position for a year in Berkeley, when the tragic news of his premature decease in the flight accident reached me. This event changed many lives, also mine.

28

Karl-Erik Eriksson: Some Reminiscences of Gunnar Källén's Role in Sweden

It is difficult to remember how I met Gunnar Källén the first time.[1] I had heard stories about him. It was said that he could be quite polemic and even arrogant in scientific discussions, in particular in discussions with physicists of high international standing. I probably first met Gunnar when he visited CERN in 1960 during my time as a fellow at the Theory Division. I was a graduate student from Uppsala University where Ivar Waller was my professor. I did not notice the slightest sign of arrogance when I met Gunnar. He was only friendly and encouraging.

My wife Kristina and I went to spend Christmas 1960 in Sweden. Along the way home, we stopped in Lund to visit Gunnar. Thinking back on this, I realize that Gunnar's attitude to younger colleagues must have been quite unusual. I was an Uppsala student working at CERN, whom he had met only quite briefly, and he took the trouble to invite me and Kristina to visit him and his wife Gunnel in Lund. We had a good supper in their home and a nice friendly conversation. At this time Kristina was expecting our first child. The next year, in Genève, Kristina gave birth to a boy, who for several reasons was given the name Gunnar. I remember very well a comment by Gunnar at the supper in Lund. Over the years, Kristina has reminded me about it from time to time. Gunnar said that it is quite common among scientists to get stuck in the topic of one's thesis and to stay there for the rest of one's research career without widening the field of interest.

In recent years, I have had reasons to think again about this remark by Gunnar Källén. I have not stayed in the field of my thesis, but I have lately found reasons to return to some of the work I did in my thesis, and it has turned out to be quite useful in a different context.

After my time at CERN, I returned to Uppsala in 1962. Gunnar visited our institute in Uppsala and I was his host. I remember that we went to Sigtuna for

[1] Karl-Erik Eriksson – current address: Karlstad University, Karlstad, Sweden.

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lunch. Alf Sjölander[2] took part in this lunch, probably also Stig Lundqvist[3]. The following year, I got an appointment as professor of theoretical physics at Göteborg University, and I moved with my family from Uppsala to Kungsbacka.

The chair in theoretical physics at Göteborg University was a completely new one. There had been elementary courses in mechanics, but it was my task to set up new courses in theoretical physics at all levels. I knew that Gunnar Källén had reformed the theoretical physics curricula in Lund and introduced quite a lot of modern physics also at the lower levels. I decided to follow the example set by Gunnar. But there was a problem in this. I had neither the breadth nor the depth of Gunnar in my understanding of physics. My experience was very limited and not sufficient to weigh different parts of physics against each other for a limited curriculum space. As a result, I pushed Gunnar's ideas further than he himself would have done, and my course curricula soon had to be revised after criticism from my Chalmers colleagues.

During the last few years of Gunnar's life, I met him at national and international physics meetings, but I do not remember specific encounters or discussions. However, I do remember that we had a relationship based on confidence, and this was very important for me in the period when when I had to build up a professional experience. During my first years in Göteborg, I was really too inexperienced for my job. Gunnar could have had reasons to be critical, but he was only helpful.

Step by step, I also got to know colleagues in Gunnar's group in Lund; first among them was Bengt E. Y. Svensson.

My teacher in Uppsala, Ivar Waller had his seventieth birthday in June 1968, and it became my duty to edit a volume of scientific papers dedicated to Ivar Waller, including a paper by Gunnar Källén. On the birthday, I travelled to Uppsala and handed over the volume to Waller in his home. Unfortunately, I do not have any copy left of this volume.

The last time that I met Gunnar was at the large high-energy physics conference in Vienna in 1968. I remember that our plane to Vienna had to make a detour because of the Soviet-led invasion of Czechoslovakia. Later I have been thinking that it might have been difficult for Gunnar who flew there in his own aeroplane.

When Gunnar died suddenly and tragically, I felt this as a personal loss. Almost immediately, I felt that I ought to write something about Gunnar for

[2] Alf Sjölander (born 1927) was a doctoral student of Waller in Uppsala. Later on he became a much respected professor at Chalmers Institute of Technology.

[3] Stig Olov Lundqvist (1925–2000) was another student of Waller who became a professor at Chalmers. He had strong international ties and played an important role in promoting condensed matter physics not only in Sweden but also at ICTP in Trieste and NORDITA in Copenhagen.

the newspaper. Afterwards, I have thought that it was not at all clear that I was the person closest at hand for doing this, but this was how I felt at this time. I also felt uncertain, and therefore I suggested to Jan Nilsson[4], my closest colleague in Göteborg, that we write together. He agreed and our obituary, or rather our note of commemoration, was published in the Dagens Nyheter. Immediately after this, Gunnar's father, Yngve Källén, called me to thank us. I remember his phone call as very touching. Jan Nilsson went to Gunnar's funeral. I did not, and I do not remember what prevented me from going, but I was grateful to Jan who went as a representative for the Göteborg colleagues.

After this, I got the opportunity to meet Gunnel Källén once more in Lund before she also died, much too early.

Notes Added by CJ:

Källén's correspondence contains several letters that he wrote to Karl-Erik Eriksson. These letters show that he had a very high opinion of Eriksson. Some of their correspondence concerns purely scientific matters but Källén's letters also reveal his deep concern about physics education, and in particular the status of theoretical physics, in Sweden. He is worried about "signals" coming from certain places in Sweden, proposing that theoretical physics be removed from the curriculum of physics teacher, etc. The relevant Chancellery had appointed two professors (Ture Eriksson[5] and Ingvar Lindgren[6], from Gothenburg (Göteborg)) to draw up a proposal for reforming the system (physics curriculum) at the Swedish universities. Källén wishes to be informed about what is happening. As he is traveling a great deal and has very little time to get actively involved, he entrusts in Karl-Erik Eriksson to act as his "ambassador", informs him of his opinions and urges him to take action.

[4] Jan S. Nilsson (1932–2010) was a professor at Chalmers Institute of Technology. His capacity as a "problem solver" was much appreciated and hence he was trusted with several very high official duties, such as the presidency of the International Union of Pure and Applied Physics (IUPAP) and the Royal Swedish Academy of Sciences. He was also the Vice Chancellor (Rector) of the University of Gothenberg.
[5] Ture Arnold Eriksson (1926–2010) is another of Waller's students who ended up in Gothenberg, where his lucid presentations and course literature were much appreciated.
[6] For the remarkable CV of Lindgren see, for example, http://fy.chalmers.se/~f3ail/.

Gunnel Källén, Last Years and Mortal Accident – a Preview

In the final chapters of this Part, we introduce Mrs Källén to our readers – a wonderful lady who was much appreciated by those who knew her. Scientific "foes" of her husband became his friends thanks to her diplomatic skills.

Then we describe Källén's participation in his last conference (Vienna, 1968). Finally, Källén's oldest son, Erland, recounts his father's passion for flying and his mortal accident.

29

Gunnel Källén

Mrs Källén, Gunnel (22 July 1929–6 April 1969) is remembered by those who knew her in the academic circles, as a wonderful lady. She was a language teacher (English, French and Russian) [1] at the upper high school (in Swedish referred to as "gymnasium").

Gunnel Källén's parents were teachers. Her father, Eric Bojs (1903–1992), had in addition a range of other activities as writer, scriptwriter, reporter, draftsman and painter (water colors). Gunnel's brother Anders Bojs recalls [2] (translated from Swedish):

> Gunnel was a delightful child who, unlike her brothers, caused no "problems" for her parents. But to their dismay, she attracted several "unsuitable" admirers, one of them being a young French student who had been drafted to the war in Indochina. Her parents' joy was thus much enhanced when a promising young man, by the name of Gunnar Källén, appeared on the horizon.

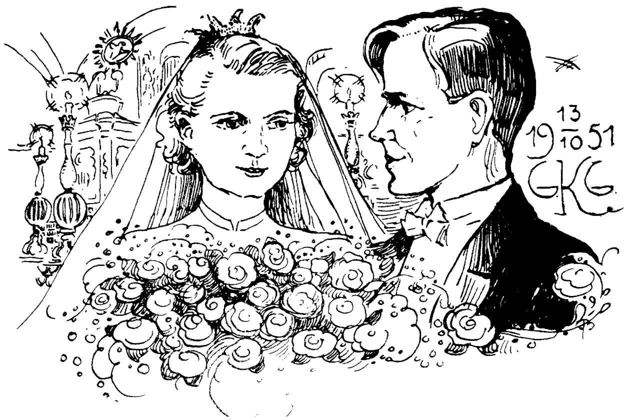

Figure 29.1 Gunnel and Gunnar Källén drawn by Gunnel's father Eric Bojs.

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Bengt Källén gives the following insight into the personality of his sister-in-law [3]:

> "Gunnar met his future wife Gunnel Bojs at a course in pedagogic. According to Gunnel, she had to make a series of manoeuvres to get his attention but finally it worked and they got married a few months after us and stayed happily married until death did them part."

Källén would proudly declare his delight for having succeeded in getting Gunnel to make two great sacrifices, just for his sake, when they married: she gave up smoking and using make-up. At one tea gathering at the department, Källén gave the following account of his wife's manoeuvring technique. Having given birth to their second child, Gunnel was at the hospital. Källén visited her and the newborn baby girl, and the couple had a long discussion on what to call her. After going through a large number of options Källén was proud to have proposed a really good name (Anna Kristina) that Gunnel had approved of. And so they had agreed on that name. On returning home from the hospital, a neighbor in the adjacent garden, had asked him about the gender of the baby to which he had replied: it's a girl, whereby the neighbor had immediately exclaimed: so she is going to be Anna Kristina[1]. Källén was much amused by this event.

Gunnel Källén is often present in her husband's scientific correspondence. Thanks to her personality, many of his scientific contacts became friends of the Källén family. For example, Källén disliked Schwinger. To Källén, who had no hidden agendas and abided by "gentlemen's agreement", Schwinger had behaved unethically (see Chap. 15 and 19). He had promised to contribute to a volume of the series "Handbuch der Physik", together with Pauli and Källén. All he had done was to make the latter two authors wait for several years for an article that he had no intention of writing! In spite of all this, Källén invited Schwinger to visit him in Lund, after Schwinger received a Nobel Prize in 1965. To us students he said that he had been "ordered" to do so by Gunnel, who liked Schwinger's wife, Clarice. There is an account of this visit in a biography of Schwinger [4] where the authors write:

> "[The Schwingers] drove Clarice's new Volvo to Lund afterward to visit Gunnar Källén. Clarice again enjoyed meeting him very much. Although he was famous, like Pauli, for having a sharp tongue, Clarice had no problem with him. His wife and Clarice became very good friends when they met in Tri-

[1] The name of Gunnar and Gunnel Källén's second child, born in July 1955, is Anna Karin Kristina. However, I (CJ) who vividly recall the above story, do not remember the name Karin.

> este in 1962, so after the ceremony in Stockholm they went to Lund to visit them."

Indeed, Gunnel Källén would ease hostilities and establish good relationships.

Källén's correspondence shows that he travelled as often as he could with his wife. As an example, the Källéns were invited to visit K. V. Laurikainen, in Turku Finland, where he was to give a series of lectures. On 29 September 1960 he wrote to Laurikainen (translated from Swedish):

> … I will be arriving on 2 October. I will comply with all your suggestions. Unfortunately my wife can't come until somewhat later. She has had some difficulty in getting a babysitter. She will come on Friday the 7th and will stay for one week.

After the visit, as was his habit, he wrote a "thank you letter" to Laurikainen, on 20 October, informing him that (translated from Swedish):

> My wife claims that I have gained weight during my visit in Finland. Evidently I got far too many good lunches and dinners, but I myself don't complain.

Whenever possible, the couple took their children along on their journeys, as amply witnessed by the letters in the Källén Collection. (Källén used to say that his four children, born in the period 1954–1958 were "quantized".) These family trips required detailed planning, often well ahead of time. As an example on 16 September 1959, he writes to the well known French scientist Roland Omnès, telling him that he intends to bring the whole family to the Les Houches School (in the French Alps), i.e., his wife and four children (ages 6 and below), as well as a babysitter. In addition, he writes that they would like to stay at a chalet.

Gunnel Källén's opinions did matter. As an example, in 1967, in a letter addressed to G. E. (Gerry) Brown who had invited him to lecture at a summer school in Copenhagen, Källén wrote:

> "Dear Gerry,
>
> … the summer of 1967 is already rather well organized for me. I am going to spend the months of May, June and July at CERN in Geneva. Sometime around August 8th comes the Montreal meeting. After that, I am going to a meeting in Rochester, New York (the 'new' Rochester conference) and the time between these two meetings will be used up in some traveling in the US. This brings us to about September 1st with no other 'free' time than roughly the first week of August. Actually, I hinted to Gunnel that maybe it would be a good idea to spend at least part of that week in Copenhagen but she

said a definite 'no'. She declared very firmly that I have to have some vacation also this summer. Therefore, I must tell you that orders from high command strictly forbid me to come."

Källén liked languages. German was his first foreign language and he felt at ease with English as well. In the last few years of his life he was constantly improving his French, with the help of his wife. He would inform the recipient of his letters in French that Gunnel had corrected his writing.

On 3 May 1968, Maurice Jacob[2] from CERN, visited Källén in Lund and gave a talk on "Recent analysis of high energy photoproduction". Afterwards, there was a lunch in honor of the guest at Källén's home. On such occasions, Källén would invite a couple of students to join. On this particular occasion, I (CJ) was fortunate to have been invited to this event. Gunnel Källén was, of course, fluent in French. To the great surprise of those present, including Källén's parents, not only Gunnel – the language teacher – but also Gunnar Källén was talking to the guest in the latter's mother tongue – French! And perhaps even more surprising was that they were discussing, among other things, the 1968 Winter Olympics in Grenoble, France, and the great performance of the French skier Jean-Claude Killy who had won three gold medals (the maximum number possible in those days) in down-hill skiing. Källén's father seemed utterly surprised. He turned to his son and exclaimed (in Swedish, of course): Do you speak French? Gunnel Källén must have been a very efficient teacher.

Hospitality was a trade mark of the Källén family. (See also the article by Bengt E. Y. Svensson, Chap. 27 in this Part.) They would often invite people from the Institute to their home in Lund and to their summer residence in the west coast of Sweden. On such occasions, the invited student, who could bring along his/her family, was expected to present his/her work. In the summer of 1968 I (CJ) did so. The scientific discussion took place while Gunnel Källén took my husband as well all the children to the beach. After returning, she served a delicious meal to the whole lot. To get such a special treatment from a great scientist was simply wonderful and an unforgettable experience.

Christian Møller, who had known the Källén family during their long stay in Denmark, recalled that

"… [Källén] developed a totally harmonious and well-balanced personality. A contributing factor in this respect was undoubtedly his happy family life with his charming children and his lovable wife Gunnel. She was an intelli-

[2] Maurice Jacob (1933–2007) was a theorist at CERN. More information about him can be found, for example, in July 2007 issue of CERN Courier, and in an article written by me (CJ) ["On the wings of physics", Physica Scripta, Vol. 78, Issue: 2 Article Number: 028003 (2008)]

> gent woman with a strong personality. The bravery and courage, which she showed when her husband suddenly was taken away, aroused the admiration and compassion of all her friends. On the day of the funeral she said to my wife: 'After this I am not afraid of anything in the world, my only ardent wish is that I may keep my health'. As you know this wish was not fulfilled, only half a year later she followed her husband into the grave."

See Chap. 65 in Part 3.

After her husband passed away, Gunnel Källén let it be known that she would like to keep contact with the members of the Department. I (CJ) invited her to my home. She came and brought presents for my children. She looked strong and wonderful. I didn't know that she was sick and, therefore, her death a few weeks later came as a shocking surprise to me.

Källén on his Wife's Health Problems

In his correspondence Källén alludes several times to his wife's health as he feels obliged to explain why he has to cancel his visits. For example in a letter dated 28 November 1964 and addressed to Léon Motchane, the founder of the IHÉS (L'Institut des Hautes Études Scientifiques, an Institute for advanced studies in mathematics and mathematical physics, in Paris), he writes (translated from French):

> … Unfortunately we will not be able to come to Paris this Christmas. Mme Källén is now at the hospital. A few days ago she underwent a very serious operation. The operation was successful but she is very weak …

The whole family was supposed to spend the Christmas vacation in Paris. The letters show that the Külléns were living between hope and despair. At times Källén was very optimistic that the troubles were over and done with but then the disease attacked again. Källén had to cancel several trips.

On 30 March 1966, in a letter (in German) to Franca Pauli, he thanked her for the two copies of her husband's lectures on "Optik und Elektronentheorie", i.e., Optics and Theory of Electrons, that he had received. Källén was especially grateful for these being Pauli's own annotated copies. Furthermore, he informed her that:

> My wife is quite well and has completely recovered from her operation.

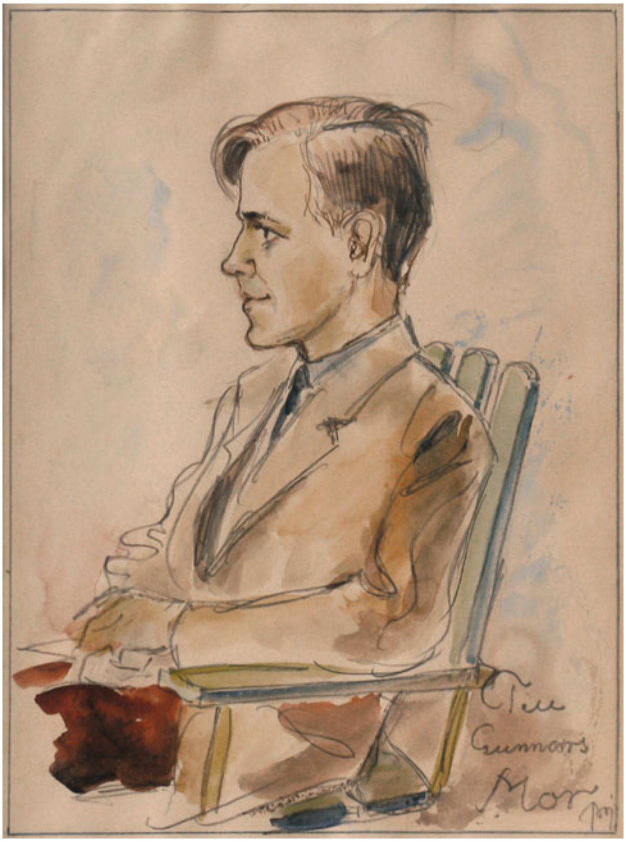

Figure 29.2 Gunnar Källén painted by his father-in-law, Eric Bojs.

On 22 July 1968, he wrote to Afaf Sabry:

> "… Gunnel and the children are with me here on Long Island. We shall stay until the end of August. Just before we came here, Gunnel had a second operation for her cancer. She seems to recover quite well and we can only cross our fingers and hope that it will not come back again."

On August 9, 1968, Källén sent some material from USA to his mathematician colleague in Lund, Lars Gårding, and in a letter, written in English, informed him that:

> "Everything here is fine. Gunnel is recovering very well from her operation. This, I am sure, is to a large extent due to the bracelet! I hope you have a nice summer with lots of SWEDISH RAIN … "

Figure 29.3 Källén's children in late 1958. From left to right: Kristina, Erland, Arne and Elisabeth.

This was only about two months before Källén died, not knowing that Gunnel didn't have much longer to live either.

References

1. Kristina Källén, private communication to CJ
2. Anders Bojs (Gunnel Källén's brother), private communication to CJ
3. Bengt Källén, Chap. 1
4. J. Mehra and K. Milton, "Climbing the Mountains: The Scientific Biography of Julian Schwinger", Oxford U. Press (2000), p. 448
5. C. Møller, Proc. of the Lund Int. Conf. on Elementary Particles (June 25–July 1, 1969), Ed. G. von Dardel (Published by Berlingska boktryckeriet, Lund, Sweden 1969); see Chap. 65

Figure 29.4 Källén's children entertaining guests. From left to right: Arne, Erland, Kristina and Elisabeth

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Källén's Last Conference, Vienna 1968

In 1968, the "14th International Conference on High-Energy Physics" was held in Vienna, 28 August–5 September. About 1000 physicists attended this meeting. Walter Thirring, professor at the University of Vienna and the Director of CERN Theory Division, was the Chairman of the organizing committee and the proceedings of the conference were published as a CERN publication, edited by the CERN physicists Jacques Prentki[1] and Jack Steinberger[2]. This was to be Källén's last conference.

Källén came to the conference, in a small plane piloted by himself. He had only one passenger – his former student Bengt E. Y. Svensson who flew both ways with him. Svensson recalls[3] that Källén hardly spoke during the flight. He was not so experienced yet and therefore was very careful not to commit any errors.

T. T. Wu[4], who also attended the conference recounts (private communication):

> "... the only time Källén and I had long conversations about physics was at the Vienna conference. That was also the time when he invited me to fly around Vienna with him. However, that did not actually happen. With me in his plane, Källén taxied on the runway and was getting ready to take off, but he turned back without getting into the air, explaining to me that one of the two sets of spark plugs was not working properly."

There were several distinguished theorists at the conference with whom Källén had had strong scientific relationship, among them Vladimir Glaser, T. D. Lee, Steven Weinberg and Arthur Wightman.

Wightman was rapporteur at a session on "Fundamental Theoretical Questions", primarily reviewing a number of papers submitted to the conference (p. 431 of the Proceedings). He noted that:

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[1] Prentki (1920–2009) was a central figure at the CERN Theory Division for many years. For more information about him, see the February 2010 issue of CERN Courier.
[2] Steinberger received a Nobel Prize in Physics in 1988. He has written a book about his life "Learning About Particles – 50 Privileged Years" (Springer 2005).
[3] Private communication to CJ.
[4] T. T. Wu is Gordon McKay Professor of Applied Physics & Professor of Physics at Harvard University.

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"It is a standard feature of S-matrix theory that analyticity, crossing symmetry, and unitarity are together enormously restrictive"

and then discussed new results on Mandelstam representation.

Källén was very sceptical about the general validity of this representation. Once he made that very clear by exclaiming, in his typical manner:

If you don't know what Mandelstam representation is, you should be proud of yourself!

Knowing his way of thinking, this statement could be translated into: Mandelstam representation is a conjecture and not a proven fact and thou shall not mix up the two. Indeed Wightman reported on new results, some showing that the Mandelstam representation is not valid.

Another area that Wightman dealt with was "model quantum field theories", again a matter close to Källén's heart. Wightman referred to work by a number of "axiomatic field theorists", among them Glimm and Jaffe. We shall not dwell on issues discussed by Wightman but would like to note that after Wightman's presentation Källén made his only documented intervention during the conference:

Källén: Concerning the results of Jaffe[5] et al., what is the difference with the old field-theoretical ideas of the fifties, say, the work of Kristensen[6]? They gave up because of problems of convergence, I believe. What is the new ingredient that makes things work now?

Wightman: I think that the difference is that now we do not impose anything on the point limit that enforced conditions on the form factors. I think these form factors would violate that condition. This is an impression; I have not looked at the matter carefully.

At this conference, Källén's work on radiative corrections was mentioned, but not discussed, in an article that is marked "Appendix", written by Alberto Sirlin (p. 321 of the Proceedings). See also Sirlin's article in Part 4 of this volume.

[5] Arthur Jaffe (born 1937) is a mathematical physicist and professor at Harvard University. He was a doctoral student of Wightman. For more information see the internet.

[6] Poul Kristensen, called Pablo, was a field theorist working in Copenhagen and later in Aarhus, the second largest city in Denmark. Here Källén is referring to his work on non-local field theory, done together with Christian Møller.

31

Erland Källén: My Father's Passion for Flying and His Mortal Accident

My father was fascinated by flying since his childhood in Göteborg.[1] He finished school in 1944, a time when many young boys in Sweden aspired to join the Swedish Air Force as pilot cadets. I asked my father once why he never did this, he answered that he would have liked to do it but he thought that his physical abilities were not up to the standards required by the military. In the early 1960's he was invited by a colleague in the US to come along on a weekend flight with a Cessna airplane. I think this was the triggering event that made him decide to try it out himself. In 1964 he took up flying lessons at an airport in nearby Malmö where an aeroplane club offered the possibility to rent single engine small aircraft. Initially he mostly regarded flying as a hobby, but he was also very keen on developing his flying skills so he could use flying for transport in connection with business trips. To achieve this it was necessary to obtain an instrument rating, i.e. to be able to fly in weather conditions where you cannot see the ground or other visual references. It took some years to reach this goal, in addition to training and theoretical exams he also needed to accumulate flying hours. I frequently accompanied my father on evening flights and other activities that he undertook in order to gain experience and collect flying hours. I became fascinated by flying, in addition to experiencing the technical and visual excitement I also came closer to my father. We both shared the fascination for flying.

The airplane crash in Hannover on 13 October 1968 that killed my father was of course a traumatic experience for the whole family. My mother, who accompanied my father on this trip, was only slightly physically injured. The children; myself, two sisters and a brother, were alone at home in Lund. We received the news about the crash in the evening of 13 October but it was not until the following day that our grandparents came down to Lund and told us that our father was dead. Our mother returned home a few days later and our

[1] Erland Källén – Department of Meteorology, Stockholm University, Sweden, and from July 2009 Director of Research at the European Centre for Medium Range Weather Forecasts in Reading, England.

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grandmother stayed on to help us cope with the situation. Our lives changed dramatically in the year following the airplane crash, our mother Gunnel also died about half a year later. She did not die from the minor injuries she received in connection with the airplane crash. Some years earlier she had been treated for cancer. The treatment was unsuccessful and she died of cancer on 6 April 1969. We, the children of Gunnar and Gunnel Källén, were now taken care of by our uncle Bengt and we moved in with his family. Today we are all doing well with families and children, in total we have 16 children and some of them are studying physics.

The Crash

On Sunday 13 October 1968 my father took off from Bulltofta airport in Malmö for a flight to Geneva. He planned to attend a meeting at CERN, I have later been told that he took over the task of representing Sweden at this meeting from a colleague in Stockholm. My mother and a friend of hers also accompanied my father on this trip. They looked forward to a couple of days of sightseeing in Geneva, my mother loved the French culture and language and her friend had lived in Geneva for many years. The weather forecast for the trip looked good, broken clouds at a few thousand feet and a moderate wind from the west[2]. The aircraft was a Piper Cherokee Arrow, a single engine light aircraft with a retractable gear. It was manufactured in 1967, recently purchased by the flying club "Aeroklubben i Malmö" and in excellent mechanical condition. My father had planned the trip with a refueling stop in Hannover, about half way between Malmö and Geneva. At twenty minutes to three in the afternoon my father contacted the approach control in Hannover and was instructed to descend from his cruising altitude down to 3000 feet. He was cleared for an approach to the main east-west runway and continued to descend down to 1500 feet. Due to the moderate wind it was probably somewhat turbulent at this low altitude, my mother later told me that father appeared nervous and tense when flying in towards the airport. The airplane was only about 10 kilometers from the airport when my father announced over the radio that he was losing engine power and had to make an emergency landing. At five minutes past three in the afternoon his last radio message reported that he still had only limited engine power and that he had selected an open field for an emergency landing. With the landing gear extended he touched down about half way into the field, most likely with a landing speed

[2] All flight and accident details are taken from the accident report published by the German civil aviation authorities in 1970.

that was much too high. The nose wheel touched down first, before the main gear. This shows that the airplane had a nose down attitude at touch-down, consistent with a high landing speed. The landing roll on the ground was quickly aborted, my father must have been struck by panic when he saw a line of trees at the far end of the field approaching quickly. He applied whatever little power the engine could deliver and pulled the aircraft into a steep climb. Due to the high landing speed he had a surplus of momentum which he used to get airborne, the engine power must have been insufficient to contribute very much to the take-off. The airplane was unable to climb over the trees, it collided with a tree and crashed onto the ground. The pilot's seat was torn off the floor and as my father only had a seat belt around his waist he suffered severe injuries when crashing into the instrument panel. He died in the hospital a few hours later. My mother and her friend were sitting in the back seat and survived the crash with only minor injuries.

Engine Trouble

Why did the engine loose power and why did my father take the decision to make an emergency landing when he was so close to an airport? The accident report found a most probable cause for the loss of engine power, if the emergency landing was necessary or not is a question that remains unanswered. If the engine problems were apparent already at cruising altitude it would have been appropriate to remain at a high altitude for as long as possible and to find a nearby airport where a safe landing could be made. If the engine problems only showed up when the approach to Hannover was initiated, why did my father not report them immediately to air traffic control so he could have been guided to take a short cut for the landing at Hannover airport? I will never know the answers to these questions.

After the crash the engine was examined for failures and some severe engine malfunctions were found. One of the four cylinders was inoperative, due to overheating both spark plugs in the failing cylinder were badly damaged. In the other cylinders there were also clear signs of overheating. The most likely reason for the overheating was that the engine had been run with a too lean air-fuel mixture. On piston engine airplanes there are several controls to handle engine output power. The most basic, the throttle, determines the total air-fuel flow into the cylinders. In addition there is a mixture control, it regulates the amount of fuel injected into the cylinders. Finally there is a propeller control, it regulates the blade angle of the propeller which determines the propeller rotation speed. The mixture control is necessary to have in a piston engine as the air to fuel ratio changes with air density. Air density drops exponentially with altitude, at 5000 feet it is about 80 % of its value at sea level. The engine fuel flow must be adjusted with altitude in order to obtain the right mixture between air and fuel. At sea level the mixture control is normally set to a maximum fuel flow, the higher up you go the more you have to reduce the fuel flow by adjusting the mixture setting. In the late 1960's this setting was done without any proper instruments indicating how the engine reacted to a changed mixture setting. The engine sound was used as a subjective indicator, if the fuel mixture was too rich or too lean the engine would run unevenly. The power output would drop and this could immediately be noticed in airplane speed and climb rate.

The procedure to find the right mixture setting was as follows: When reaching cruising altitude the mixture is leaned until the engine starts to run rough. Now the mixture is enriched until the engine runs smoothly again and it is enriched a little bit more to prevent engine overheating. If the engine runs at a minimal fuel flow with a high power output there is a danger that the cylinders overheat. After about 1975 all piston engine airplanes are equipped with a temperature gauge for the exhaust gases. Nowadays many airplanes are equipped with temperature sensors for every cylinder. With a temperature sensor it is possible to monitor the engine temperature accurately and to avoid engine overheating due to running with a too lean mixture.

My father approached flying in a very systematic way. He was also keen on flying as economically as possible. I accompanied him on flights where he experimented with optimal power settings at various altitudes and he strived to minimize fuel consumption. According to the mechanic of "Aeroklubben i Malmö" he was the club pilot with the lowest fuel consumption. The most likely reason for the engine failure is that he flew the airplane with such a lean mixture setting that the engine was overheated. It is unclear when he noticed that the engine was not delivering full power, did it show up already at cruising altitude or was it only apparent when he reached the approach segment of the flight at a lower altitude? The engine must also have been running very rough, with one cylinder inoperative it must have been shaking and rambling quite markedly. I would have liked to ask my mother about these things, unfortunately the full accident report did not appear until 1970 and then it was too late to ask her. She never understood the actual reason for the emergency landing, I remember her saying that this was a secret that my father had taken with him. As my father had done many flights at altitudes sufficiently high to motivate lower mixture settings, why had he not experienced problems with engine overheating before? Well, my guess is that he knew that he had to enrich the mixture a little beyond the setting that would give minimal fuel consumption but he wanted to make this adjustment as small as possible. In the end he made it too small. The aircraft he flew was equipped with a fuel flow meter and he could thus determine the mixture setting that gave a minimal fuel flow. On his last flight he was apparently too economical with the mixture setting, once the engine starts overheating there is no way to get it back to operate normally again.

Flying and Physics

Is there any connection between my father's keen interest in flying and his professional interest in physics? I think so, he explained at some point to his father (my grandfather) and me that his interest in theoretical physics was not so much about mathematical logics and formalism but more about how mathematics can be used as an efficient tool to describe and rationalize observations of reality. An airplane is a rather complex piece of machinery that can be controlled by a limited set of control inputs, manageable by a human being. The laws of physics tell us how we can control the aircraft in a stable and predictable manner. Taking control of something, be it either an airplane or a beautiful theory that can describe and predict observations, gives a feeling of satisfaction. In addition flying involves theoretical aspects such as navigation and flight planning. Once in the air you can compare what you predicted in your flight preparations with how the flight actually evolves. Flying on instruments also involves precision work where you for example follow a set of standard procedures to descend and approach an airport through clouds. Suddenly you see the runway appearing right in front of your eyes at the right time and altitude. It gives you a feeling of satisfaction having managed to bring the small airplane back to the ground in a predictable and deterministic manner.

Weather is a very important part of small airplane flying. Strong winds, low visibility or freezing conditions severely restrict the possibilities to operate small aircraft. The syllabus for a pilot's license involves a fair amount of meteorology and taking an instrument rating you have to learn even more. I remember my father commenting on his meteorology lessons in connection with his flight training. He said that this was really an interesting part of the education. I think he enjoyed how basic physical principles can be applied to understand atmospheric phenomena such as cloud formation, links between wind and pressure fields and weather forecasting. Using physics to formulate a mathematical model that can be used to predict the weather some days ahead knowing the observed state of the atmosphere in the past I think is one of the most fascinating applications of classical physics. I have become professionally interested in weather and meteorology, I am presently a university professor of dynamic meteorology at Stockholm University.

Concluding Words

This short essay was triggered by some remarks made by Steven Weinberg when he visited Lund on 13 February 2009. He asked me about the plane crash that killed my father, why did it happen and was my mother killed in the crash? It is the first time I have put down this story in writing, I am grateful for having had the opportunity to do this. My father's plane crash and my mother's subsequent death was naturally a traumatic experience for me, I was only 14 years old when I lost both my parents. I am sure that these events have been crucial for the way my life has evolved, but it is difficult to know how things otherwise would have been. My fascination for flying led me to studies in meteorology which have determined my professional career. I also took up flying, I did my military service in the Swedish Air Force as a meteorology cadet which included flight training. Later in life I met one of my father's class mates from his school years, Sture Wickerts. Sture went through military flight training towards the end of World War II and later became an air force meteorologist. I asked Sture about his impression of my father during the school years, did he remember that my father had any interest in flying and perhaps talked about following Sture to join the military flying school in Ljungbyhed in 1944? Sture said that he had no such memories, he was in fact very surprised when he learned that my father had taken up flying in the 1960's. Obviously my father kept his dreams about flying to himself. I think that my father was often seeking new things to do in life and he was a bit restless. He loved traveling around the world to attend various physics meetings and often he took the family along for extended visits abroad. Some of his students have witnessed that he could get extremely excited by an intriguing physics problem, almost like a little boy having found a new toy to play with. He enjoyed being in debates, in particular ones with senior and more established colleagues. If he could show that he was right and they were wrong he was very pleased.

Taking up flying was adding another dimension to an adventurous life.

32
The Future That Never Came

Källén was an adherent of modern technology. After all he was a certified engineer. Early on, when many scholars regarded mathematical work on computers with contempt and suspicion, Källén was, on the contrary, a great supporter. His optimism and joy in getting his computer programs to work shine through several of his letters, for example when he informs his friend and collaborator John Toll, in 1960, about:

> "… what is happening with the DANAD-curve. As I wrote earlier, I have put the curve into the electronic computer we have in the basement. I now have about 20 cases computed for various combinations of the z:s. …"

One should keep in mind that the "computer in the basement" that Källén is talking about, was a machine called SMIL (Digital Machine in Lund). It was one of the first computers (in the civil sector) in Sweden and its inception was such a remarkable happening that the Swedish Minister of Education and Religion was present at its inauguration ceremony, in 1956.

Later on Källén was a strong supporter of the upgrading of SMIL. In a letter dated 9 February 1960, addressed to the "Knut and Alice Wallenberg Foundation",[1] he strongly supports a proposal, made by his colleague[2] in charge of SMIL, that funding be provided to increase the capacity of SMIL. He informed the Foundation that he is doing advanced computations and needs to have a computer "*with very large capacity*". The application was for an increase of the capacity "from 2048 to 4096 places", as Källén puts it.[3]

Undoubtedly, the developments in the computer sector and the new tools provided thereby would have pleased him enormously.

In August 1968, in a letter, Källén thanked CERN for:

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[1] Knut and Alice Wallenberg Foundation, was founded in 1917 to "promote scientific research, teaching and/or education beneficial to the Kingdom of Sweden". It has been and is one of the most important supporters of basic science in Sweden.

[2] The colleague in question was Carl-Erik Fröberg (1918–2007), a former theoretical physicist and one of the men that had been sent to Pauli in Zürich (see also chapters 4 and 12).

[3] By the present standards, of course, SMIL was no more than a miniature dwarf – 2048 is only 2^{11} ! but as sometimes rightly claimed: "The first step is the hardest".

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> "the invitation to the conference about weak interactions in January 1969. This looks like a very interesting meeting and I shall do my best to be able to attend."

In the meanwhile he had died and I (CJ) was asked to participate instead of him in the above conference. It was evident to me that he was sorely missed at the conference.

Källén had become deeply interested in fundamental issues in particle physics, primarily in current algebra – his latest contributions had been about "gradient terms" (usually referred to as Schwinger terms, a terminology that Källén found inappropriate). To penetrate deeper into subtleties of current algebra, Källén started a series of weekly seminars in Lund focused on a newly published book by Adler and Dashen [1]. The plan was that each week one person should present the material in one of the chapters of this book. I was selected to present the material in Chapter 1, especially the derivation of the Adler-Weisberger relation. Early in the morning of the scheduled day of my presentation, I received a phone call from Källén in Stockholm and was informed that he had planned to fly back on a small plane, piloted by himself, but due to technical problems he was not going to make it in time. Therefore, the talk had to be postponed. A few days later I gave my presentation, in front of Källén and the whole "class". Källén was not happy at all, not because of the content of the talk, but due to the fact that, following the book, I used the Feynman metric, instead of the compulsory Pauli metric[4] of the Department. Källén said: don't ever change the metric. Unfortunately, I could not follow his advice. A few years later, when I was a fellow at CERN, I discovered to my great surprise that (to the best of my knowledge) the only people who used the Pauli metric were John Bell, T. D. Lee, and Murray Gell-Mann. I felt that I did not belong to such a prominent society and therefore changed my metric, from Pauli to Feynman. That made my collaboration with other theorists much simpler.

It is difficult to "predict" if Källén would have adapted to the growing uniformity pressure exerted by the scientific community. His books, most probably, would have sold better and reached a larger number of readers if he had changed his metric. But he had an exceptionally strong will power and it is impossible to know what he would have done. It is, however, true that Pauli metric is "safer" in field theory as one need not distinguish between up and down indices. But, it needs to be modified, when applied to general relativity.

[4] The metric with $x_\mu = (x, y, z, x_4 = ict)$, and Dirac matrices γ_μ, $\mu = 1 - 4$, etc.

Källén's correspondence shows [2] that he was interested in starting a collaboration with Martinus (Tini) Veltman. The two men had discussed physics at a "Wouthuysen party" in 1966 at Brookhaven and had continued their discussions during a visit by Källén in Utrecht and afterwards while Veltman had driven him to Louvain, Belgium. They had continued their discussions at CERN. In November 1967 Källén wrote to Veltman that he was very busy but that:

> "I will contact you again to take the matter up more seriously. I don't expect this to happen until, possibly, February next year. If you get any bright ideas in the meanwhile, please let me know."

Alas, this was less than a year before Källén died. There can be no doubt that he would have loved gauge theories with all their subtleties. They were waiting just around the corner to make a triumphant entry into theoretical physics.

References

1. S. L. Adler and R. F. Dashen, "Current Algebras" (W. A. Benjamin, 1968)
2. Several letters exchanged between Veltman and Källén (1966–67) *in* the Källén Collection

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Appendix I-A: More on the Källén and Pauli Collections

The Källén Collection

The Collection [1] consists of 18 “boxes”. The material in the Collection has been labeled by the archivists as follows (translated from Swedish):

- Boxes 1–4 letters from GK 1959–1968
- Boxes 5–8 letters to GK 1960–1968
- Box 9 letters from Pauli
- Box 10 other material and collected works
- Boxes 11–12 articles 1–55
- Box 13, manuscripts
- Boxes 14–16 his statements on scientific works
- Boxes 17–18 papers/publications

Actually, a closer look into the Collection shows that five of the boxes (11–15) include hundreds of reports from the period 1952–68. These are articles of varying length written by Källén *in German*, for the journal “Zentralblatt für Mathematik” and sent to an office in the city of Würzburg, Germany. The current homepage of this journal informs us that “Zentralblatt MATH is the world’s most complete and longest running abstracting and reviewing service in pure and applied mathematics”. Unfortunately, however, only a very small fraction of Källén’s reports are retrievable from the otherwise user-friendly webpage of the journal. The reason is that [2] there is a *gap*, between volumes 100 and 200 of the journal, where the reviewers are not searchable, at least not yet. The relevant card catalogue which was kept in East Berlin was somehow lost, when the government of East Germany closed the office at the end of 1977.

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The Pauli Collection

Turning to Wolfgang Pauli, all his correspondence of interest to us has been edited by Karl von Meyenn and appears in several volumes as listed here below. The all-embracing title of these volumes, published by Springer-Verlag is: "Wolfgang Pauli, Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a." [3]. Actually, Pauli's much more extended correspondence with Markus Fierz, than with Bohr or Einstein, as documented in these volumes, is particularly interesting and instructive.

As mentioned before, the letters in these volumes are conveniently labeled in the form [number], where, for example, [1234] denotes letter number 1234 in the Collection. Thus, the reader will find it in Vol. IV, Part I.

References

1. Gunnar Källén Collection, "Manuscripts & Archives", Lund University Library
2. Barbara Strazzabosco, Office of the Zentralblatt MATH, private communication to CJ
3. "Wolfgang Pauli, Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a.": Vol. **III** (published in 1993) contains letters numbered [588]–[1071] from the period 1940–1949;
4. "Wolfgang Pauli, Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a.": Vol. **IV, Part I** (published in 1996) contains letters numbered [1072]–[1500] from the period 1950–1952;
5. "Wolfgang Pauli, Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a.": Vol. **IV, Part II** (published in 1999) contains letters numbered [1501]–[1963] from the period 1953–1954;
6. "Wolfgang Pauli, Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a.": Vol. **IV, Part III** (published in 2001) contains letters numbered [1964]–[2428] from the period 1955–1956;
7. "Wolfgang Pauli, Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a.": Vol. **IV, Part IV-A & B** (published in 2004) (**Part IV-A** contains letters numbered [2429]–[2816] from the year 1957, **Part IV-B** contains letters numbered [2817]–[3142] from the year 1958)

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Appendix I-B: A Brief Källén Chronology

Anders Olof Gunnar Källén was born on 13 February in the city of Kristianstad in the south of Sweden. His academic path is summarized here below:

1944	27 May, graduates from "Vasa högre allmänna läroverk" [high-school], Göteborg (Gothenburg)
1948	31 January, receives engineering degree from Chalmers Institute of Technology, Göteborg
1948	5 February, registers as student at Lund University
1949	Is sent to Zürich (April-July) to attend lectures by Wolfgang Pauli – an event of utmost importance for his academic career
1950	June, starts one year (compulsory) military service
1950	24 November, defends his doctoral thesis "Formal Integration of the Equations of Quantum Theory in the Heisenberg Representation"
1951	31 May, receives PhD from Lund University
1952	1 February, is appointed as research fellow at Lund University
1952	Becomes a fellow of "CERN Theoretical Study Division" in Copenhagen
1953/4	September-April, guest scholar at Institute for Advanced Study at Princeton
1954	1 October, becomes staff member of CERN, Copenhagen
1957	1 October, becomes staff member of NORDITA, Copenhagen
1958	1 August, starts his (personal) professorship in theoretical physics at Lund University
1964	Spends spring term at the University of Maryland
1968	13 October, dies in a plane crash near Hannover, Germany

The above chronology does not include Källén's numerous shorter trips, for research, giving talks at conferences and lecturing at various schools. He was a highly esteemed visitor in many places and had a standing invitation to several universities and research centers, such as University of New York at Stony Brook, MIT, IHÉS in Paris, and the Theory Center in Marseille. See Chap. 17.

On the private front, on 13 October 1951, Källén married Gunnel Bojs. They had four children: Anders Olof Erland (b. 1954), Anna Karin Kristina

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(b. 1955), Anna Hilda Elisabeth (b. 1956) and Anders Erik Arne (b. 1958). Gunnel Källén died a few months after her husband, on 6 April 1969.

Källén used to say (jokingly) that 13 was his lucky number – perhaps just to counterbalance the superstitious people who are afraid of number 13. There must be quite a number of such people as, for example, in the Scandinavian commercial planes there is no row numbered 13.

Part 2

Correspondence With Pauli, Heisenberg and Dirac; Källén in Action

Preface

> Your mischievous Remarks are quite good, but my own, when I was at your current age, were probably better – and sharper (translated from German).
>
> Pauli to Källén (1955)

> I am glad that you now again write in a plain language and do not only make obscure insinuations that are essentially incomprehensible to me (translated from German).
>
> Källén to Pauli (1958)

This Part deals with Källén's correspondence, with Pauli, Heisenberg and Dirac. We introduce some of Källén's collaborators and follow him to a few conferences and schools.

In the first few chapters of this Part, we limit ourselves to giving an overall picture of the Källén-Pauli correspondence. Some of their correspondence fits more naturally into the chapters concerned with Källén's work and is, therefore, deferred to Part 4. Then we describe Källén's extended *indirect* correspondence with Werner Heisenberg. The latter avoided Källén, in spite of the fact that Pauli urged him to send his articles to Källén to be scrutinized. Thus, a Heisenberg-Pauli-Källén network was established with Pauli as the central node, exchanging information between Heisenberg and Källén.

We describe Pauli's (sometimes rather rude) treatment of his friend Heisenberg and Källén's views on Heisenberg's work, cheered and scorned by Pauli and sometimes in collaboration with him.

The correspondence with Dirac, Källén's collaborators and his participation in conferences and schools will be introduced in due time, further down in this Part.

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A Brief Overview of Källén-Pauli Correspondence

This book is *not* about Wolfgang Pauli (1900–1958). As mentioned in Part 1, he was a superstar on the scene of the 20th century physics and his remarkable science and unusual personality have been documented in the monumental work by von Meyenn[1] (referred to in this book as the Pauli Collection), a true goldmine for those who are interested not only in Pauli but also in a large number of other scientists (as well as non-scientists) who corresponded with him, among them Gunnar Källén.

After Pauli's death, his widow Franziska (called Franca) Pauli collected her husband's letters and created a Pauli Archive that she later donated to CERN and *not* to ETH[2] in Zürich!

The Källén-Pauli correspondence[3] is in German and can be found at the University Library in Lund, in their "Gunnar Källén Collection". The Collection contains the original hand-written letters of Pauli and copies of Källén's letters. The latter are generally typed but the equations in them are sometimes hand-written. About 140 of the letters have been reproduced in the Pauli Collection, while in Lund there are about 20 more[4].

The Källén-Pauli correspondence shows Pauli's appreciation of Källén as a physicist and problem solver. Källén was famed for his sharp intellect and the speed with which he could spot inconsistencies in reasoning or equations, even in a complicated calculation. The relationship between the two men was cordial but at the same time very formal, as was not uncommon in those days. The age difference (26 years) was too big for them to address each other in the familiar (German "du") form or to use first names. Actually scientists that had the honor of being on the first-name-basis with Pauli were at most a hand-

---

[1] This work, which is primarily in German, is properly quoted in Appendix I-A at the end of Part 1.

[2] Pauli was a professor at ETH, from 1928 until his death in 1958, with some interruptions as guest professor elsewhere.

[3] Looking through their correspondence one is amazed by the efficiency of the postal delivery in those days, in Europe as well as across the Atlantic Ocean.

[4] The originals of Pauli's letters are in Lund and show that Pauli's hand-writing was often "terrible". The only scientist who, to the best of CJ's knowledge, had a more difficult-to-read hand-writing was the 1904 Nobel Laureate in Physics, John William Strutt – the characters in his hand-written letters, in the Nobel Archives, are "Rayleigh-scattered" all over! But, fortunately, von Meyenn has managed to decipher the Pauli letters, thus making them easily available, at least to those who read German.

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ful.[5] Bohr tried to join this exclusive club by sending him a letter in Danish addressing him "Kære Wolfgang", i.e., Dear Wolfgang (letter [2218] in the Pauli Collection). Pauli's next letter to him (letter [2293] in the Pauli Collection), however, restored the order as it began with "Dear Bohr". And yet Bohr and Pauli were close scientific friends and enjoyed one another's company very much indeed. Bohr was about 15 years older than Pauli and thus addressing him as Niels was out of question for Pauli.

Some of the prevailing social rules of those days may be difficult for contemporary people to understand, especially Americans. For example, I never addressed Källén by his first name. One did not address a professor by his first name! However, some of his students who had accompanied him on his visits to the USA, learned quickly to call him Gunnar. He didn't mind and actually seemed to enjoy it!

It is also interesting to note that women were addressed by their first names much more readily than men. For example, Pauli who never addressed Niels Bohr, Victor Weisskopf, or Oskar Klein by their first names would in his letters to them refer to their wives respectively as Margrethe, Ellen and Gerda. His own wife was known to most physicists as Franca. In his letters to Lise Meitner, Pauli would address her by "Dear Ms. Meitner", but when writing to others he sometimes would refer to her as "our good Lise".

Källén-Pauli correspondence falls into following main categories:

- Their perception of contemporary science and happenings in physics, as well as their opinions about other physicists. See the next chapter.
- Källén work using Heisenberg representation in quantum electrodynamics. This is described in Chap. 71.
- Their joint paper on the Lee Model and related issues. This is described in Chap. 61 and 77.
- The Källén and Pauli articles in the Encyclopedia of Physics (Handbuch der Physik) - waiting for "His Majesty Julian[6]". This has been described in Chap. 19.
- Heisenberg's attempts to do, as Pauli put it, something truly original, and wishfully revolutionary. See Chap. 39.
- Pauli's disapproval of Källén's work on the n-point functions. See Chap. 81.

[5] Among them Max Delbrück, Gregor Wentzel as well as Robert Oppenheimer. The latter doesn't really count because the Americans were informal. For them addressing someone by the first name was not a big deal. Pauli had spent a large amount of time at Oppenheimer's Institute in Princeton. In fact, since 1946 he was an American citizen. This was about three years before he finally, in 1949, "made it" and was granted Swiss citizenship.

[6] Pauli's nickname for Julian Schwinger.

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Källén's and Pauli's Views on the Status of Theoretical Physics

As was mentioned in the beginning of this book, in theoretical physics, history is largely unfair to great minds. In times when there are urgent problems to be solved, it quickly creates a number of "great scientists", crowns them with glory and everlasting fame. This was certainly the case during the first decades of the 20th century. There were experimental puzzles crying to be solved and in a relatively short time the remarkable fields of special relativity and quantum mechanics were born. Many were those who went to the history as great scientists, by contributing to different aspects of this revolution. Without hints from nature, it is very difficult for a theoretical physicist to "show the whole world" how great he/she is, even if he/she were the smartest and most creative scientist of his/her time. Creating a new wave, without experimental hints, is a tremendous challenge. The best (and only?) example is the creation of general relativity, where Albert Einstein "single-footedly" climbed up all the way to the top of the ladder of fame by single-handedly proposing his theory of general relativity. But he could *afford* it, as he was already at the top due to his other contributions which had been prompted by experimental observations. One wonders what would have happened if Einstein had *only* postulated his theory of general relativity and nothing else. Had the scientific community noticed it and cared enough about it to send expeditions to Brazil and Africa to check his "speculations"?

Källén sometimes expressed his regret for having been born "too late", as he put it. He had come to Lund University in 1948 as a full-fledged 22 year old electrical engineer who wished to re-orient himself toward theoretical physics. In no time at all he had acquired an incredible amount of knowledge, as it is testified by his very first paper published already in 1949. In no time at all he was "writing poetry in the language" of field theory. However, he was born in 1926, while the founding fathers of quantum electrodynamics, with whom he compared himself, had been around years before he appeared on the scene. They had almost done it all!

Källén had the big fortune of being sent to Zürich to attend Wolfgang Pauli's lectures, during about three months in 1949 and thus to be "discovered" by Pauli. He was not a person who needed any technical supervision, such as to learn from Pauli how one calculates a physical quantity. Actually,

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Pauli considered him as more capable than himself in that respect. Much more importantly, Pauli became his source of inspiration and defined the general area of his research. Källén returned home as a transformed man, knowing what he wanted to do in physics.

In his correspondence with Källén (1949–1958), Pauli often laments over the stagnant state of theoretical physics. There was nothing on the horizon even remotely comparable to the glorious periods of (general) relativity, that he had experienced already as a teenager, and quantum mechanics. Pauli, placed in the node of his correspondence, was following the developments in several areas such as foundations of quantum mechanics, field theoretical models, superconductivity and thermodynamics, but he himself was not doing much original work. He was bored! The situation changed dramatically in 1956, as evidenced by his announcement at the 1956 CERN Conference [2]. He had just received a a telegram from Fred Reines[1] and Clyde Cowan (Los Alamos) informing him [3]:

> "We are happy to inform you that we have definitely detected neutrinos from fission fragments by observing inverse beta decay of protons. ..."

Pauli added:

> "I make this announcement because otherwise everybody would ask me separately."

Neutrino was Pauli's "baby", postulated by him in 1930. Evidently, he was pleased to know that the baby actually existed and was curious about its nature. Pauli's excitement was enhanced in the following year by the announcement of the discovery of parity violation. After a short period of scepticism, he became very interested in the subject, to the extent that on 8 April 1957 he gave a talk with the title "Some remarks about parity conservation and weak interactions" at the CERN Theoretical Study Division in Copenhagen [4], where Källén was a staff member. Pauli also tried, together with Heisenberg, though not with any success, to make models to describe the interactions of elementary particles. Thus, he did experience a period of great excitement in physics at the end of his life, and one could perhaps say that, from this point of view, he "died happily" in December 1958.

For Källén the situation was rather different. He had entered a field that already during several years had been plowed by others. He had to make his mark by digging deeper and dealing with much harder yet unsolved problems.

[1] Reines received the Nobel Prize, in 1995, for this achievement. Cowan (1919–1974) had already passed away, and according to the rules could not be considered for the Prize.

In fact, he was attracted to solving difficult problems while solving "easier" ones, at that time, would have been more rewarding. He was aware of what was happening in particle physics but had already got involved in the highly challenging studies of the mathematical structure of the "n-point functions", i.e., the vacuum expectation value of the product of *n* field operators. He was not willing to drop that ambitious mathematical program, which he really loved and was good at, and turn to say particle physics phenomenology – a field which turned out to be where the "gold-mines" were hidden in those days, and waiting to be discovered by much less mathematical effort.

References

1. A. S. Wightman, Comm. Math. Phys., Volume 11, Number 3 (1968–1969) 181
2. Proceedings of CERN Symposium on High Energy Accelerators and Pion Physics (CERN 56-26, edited by A. Citron et al.)
3. W. Pauli, *ibid* p. 258; see also Chap. 53
4. CERN Annual Report 1957, Appendix C (p. 85)

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Pauli on Källén's Sharp Tongue and "Nasty Theorems"

This is what Pauli had to say about Källén's sharp tongue, in a letter to him ([1984] in the Pauli Collection) dated 21 January 1955 (translated from German):

> Your mischievous remarks are quite good, but my own, when I was at your current age, were probably better – and sharper.
>
> Once *Ehrenfest* had written a letter full of viciousness to *Einstein* – it was about the Bose-Einstein statistics, especially on the condensed phase. Einstein replied: Your jokes are great, your arguments weak![1]

The above statements were delivered by Pauli after Källén had written the draft of their joint paper and subsequently had bluntly objected to some of Pauli's critical remarks on the manuscript. Otherwise, in his letters to Pauli, Källén was usually objective and concentrated on purely scientific issues. There is relatively little "gossip" in the letters and if there is, it is not so unkind.

Nonetheless, in one case Källén goes completely off his usual track and makes a number of sweeping generalizations (letter [2123] in the Pauli Collection). One wonders what was going on in his mind when he wrote this letter to Pauli, dated 30 June 1955. In it he attacks Walter Thirring in an unpleasant way and formulates a number of "theorems". One of his theorems reads (translated from German):

> *Theorem 3*: The great men never understand what they themselves have done.
>
> Proof: (in alphabetic order)
>
> *De Broglie* doesn't understand wave mechanics.
>
> *Dirac* doesn't understand the relativistic theory of electrons but instead wants to make it classical.
>
> *Heisenberg* doesn't understand matrix multiplication but instead introduces Hilbert Space II.

[1] Original version in German reads: "Deine Witze sind großartig, Deine Argumente schwach!"

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> *Pauli* doesn't understand regularization but relies on Thirring.
>
> ...

Pauli was not impressed by this theorem. He responded soon after (translated from German):

> After having been bored for years, by the young generation, came a ray of hope with your letter of 30 June, which no doubt possesses some fictional value as entertainment literature. Up to now, I believed that such letters could only be written by jealous women, which now, due to existence of an author of male gender, has been proven to be wrong.

Pauli's nickname for Källén was the Danish word "sagføreren" (in English: the Lawyer). He wrote, for example, to Møller, the head of the CERN Theoretical Study in Copenhagen, where Källén was a full-time staff member (letter [2203], dated 3 December 1955, in the Pauli Collection):

> "All good wishes, also, to sagføreren, who, as I hope, from now on will enjoy a long, undisturbed (though undeserved) rest in his ivory tower."

Pauli, was disturbed by Källén's very sharp criticism of Thirring. He worried that perhaps his "discovery" was entering into a long epoch of uncompromising silence. This was his reason for wishing him "a good rest in the ivory-tower". He had signed his latest letter ([2197] in the Pauli Collection) to Källén: Your old, but now defected Club member W. Pauli! Nonetheless, it didn't take long before Pauli forgot that he had defected and again "disturbed" Källén by sending him letters and asking his opinion about various matters.

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The Last Källén-Pauli Letters

Let us jump ahead of ourselves and have a look at the last letters exchanged between Källén and Pauli, in the year Pauli passed away. During the two preceding years, their correspondence was mostly related to Heisenberg's work which we shall discuss further down in this Part.

By the beginning of 1958, the volume of the correspondence between Källén and Pauli had diminished considerably. They had no urgent matters of common interest to discuss. Pauli was working on particle physics phenomenology and Källén had deeply immersed himself in the n-point function program. Pauli's last letter ([3093] in the Pauli Collection) to Källén, dated 22 October 1958, has the following content (translated from German):

Pauli thanks Källén for having sent him a copy of his Handbook Article with its, what he calls, beautiful dedication. Källén had added: Hopefully, this will not be left lying in the refrigerator[1]. In addition, Pauli writes (translated from German):

> I believe I should now give up and establish a special cabinet for your article. *Not* a refrigerator but a cabinet for curios with symbolic value. ...
>
> What afflicts me concerning your various dedications mentioning the refrigerator is that you seem to plan to keep on working for this refrigerator[2] {i.e., without anything, in your work, of direct relevance to physics (such as your famous conjecture about the Born approximation)}.[3]
>
> This week I start my lectures on many body systems and I will try to forget all about field quantization for a long period. ... In September, I wrote a rather long article (about 40 pages) on the early and more recent history of neutrinos.

[1] The refrigerator was a concept that Källén had "invented" for Pauli to preserve his articles without reading them.

[2] Pauli, on numerous occasions expressed his aversion to Källén's new direction of research – the study of the vacuum expectation value of products of field operators. He considered that field to be sterile and urged Källén to do "real physics", such as model building. For him, Källén's articles in that field found their natural domicile in his refrigerator.

[3] Källén would argue that at "high energies" two particles colliding with each other would have little time to interact. Therefore, the lowest order in perturbation theory (i.e., the Born approximation) should give an accurate description of their interaction.

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Källén answers from Copenhagen on 27 October (letter [3096] in the Pauli Collection), this being his last letter to Pauli. In his letter, after answering Pauli's questions he adds (translated from German):

> It is hardly worthwhile to answer all your vicious remarks in detail. I am only a bit surprised when you say that you would now like to forget your knowledge of field quantization. Actually, I thought you already had done so a long time ago.
>
> Many regards
>
> your (in spite of it all) much devoted Gunnar Källén

Coming from a more than a quarter-century younger colleague, the above text sounds unduly harsh. But here, as usual, Källén is responding in kind. Indeed he had the courage to treat Pauli the way Pauli treated others and Pauli didn't seem to mind.

Correspondence with Heisenberg
Through Pauli and Objections to Their Work In the following few chapters we present, figuratively speaking "the triangle" with the corners Heisenberg, Källén and Pauli, with Heisenberg in Göttingen, Källén in Copenhagen, and Pauli in Zürich. Heisenberg and Pauli were strongly linked to each other through their common scientific heritage, long acquaintance and (different) personalities: Heisenberg prolific in ideas, mild and polite; Pauli often rude but genuinely interested in his friend's work. And there was Källén who was often called upon by Pauli as a judge of Heisenberg's work, a role that Källén didn't always appreciate.

To begin with, we look into Heisenberg-Pauli relationship during the relevant period, for a better understanding of Källén-Heisenberg correspondence. Then we give an account of Källén's role, as well as his opinion on the work by Heisenberg, at times done in collaboration with Pauli.

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Wolfgang Pauli on His Friend and Collaborator Werner Heisenberg

In order to appreciate the Källén-Heisenberg correspondence, it helps to know a little about the latter's personality and his relationship with Pauli.

Heisenberg and Pauli were close friends and at times collaborators. They had known each other since they were about 20 years old and fellow students in Munich, studying under their common supervisor Arnold Sommerfeld.[1] Pauli was very proud of having been a student of Sommerfeld and would keep on reminding Heisenberg of their common scientific ancestry and the obligation imposed by it: to do excellent science.

There can be no doubt that Heisenberg (1901–1976) was one of the most creative physicists of the 20th century and aptly received in 1933 the 1932 Nobel Prize in Physics, together with the most Noble of all Nobel citations in science ever:

"for the creation of quantum mechanics ..."

He was thus raised to the "Nobel Heaven" above all scientists of all [Nobel] time, provided one does not read what the dots stand for. They were reminder of the fact that, according to the Nobel rules, the act of "creation", no matter how mighty it may be, is not enough for a Nobel Prize – it has to have a down-to-earth testable application.

Victor Weisskopf[2] (1908–2002), known to many as Viki, during several years used to give a series of lectures to the young summer students at CERN, never forgetting to transmit his great admiration of Heisenberg and Pauli. Having been a postdoc under Heisenberg in Leipzig, and an assistant to Pauli in Zürich, he knew what he was talking about. According to Viki, Heisenberg was extremely competitive; he simply wanted to be *number one* in "everything". In Leipzig, where the young Heisenberg was a professor, he used to beat everyone in frequent table tennis competitions and was happy, until a Japanese

[1] Arnold Sommerfeld (1868–1951) was a great scientist and a remarkable teacher and supervisor.

[2] Weisskopf was also the Director-General of CERN (1961–1965). After his death a special Symposium was organized at CERN [actually by me (CJ)]. A written account of the talks given was published in a special issue of CERN Courier, December 2002. In it J. D. Jackson describes the impressive scientific profile of Weisskopf. See http://www-theory.lbl.gov/jdj/VFW-CernCourier.pdf.

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guest[3] came along. According to Viki, when Heisenberg was defeated by the guest he went home and didn't show up for several days!

While having a coffee on the terrace of CERN cafeteria with Weisskopf and Val Telegdi,[4] I (CJ) witnessed a heated debate, almost a verbal fight, between the two men, on Heisenberg. The two agreed on one thing: both had immense admiration for Pauli. But concerning Heisenberg, they had completely opposite opinions. Telegdi, was attacking Heisenberg for his activities during the second world war but Weisskopf (who sometimes referred to himself as a Viennese Jew) would have none of it. After a while I went away, with the impression that the status of the battle was: advantage Weisskopf.

Historically, Heisenberg before the war and Heisenberg after the war emerge as two different individuals. One could call them Heisenberg-I and Heisenberg-II, in analogy to his work, where he insisted on dividing the Hilbert-space into Hilbert-space-I and Hilbert-space-II, respectively. Heisenberg had, as Pauli put it, an irrepressible urge to be in the limelight[5], a trait that he didn't approve of. He described Heisenberg's behavior in a letter to C-S. Wu in March 1958 (i.e., the year in which he died) as follows (letter [2926] in the Pauli Collection):

> "Heisenberg's newspaper and radio advertisement which is of very bad taste, I have been provided already with some clippings by Dr. Källén (Copenhagen). In some of these I had been, unfortunately, mentioned (not in the one sent by you), but fortunately, in a 'mild' form as a secondary (or tertiary) auxiliary person of the Super-Faust, Super-Einstein and Super-Man Heisenberg. (He seems to have mentioned his dreams on gravitational fields – about which one has *not* worked at all in Göttingen recently – and his revival of the old idea of a 'world-formula' – which was never successful – in a quantized form.)
>
> Heisenberg's desire for publicity and 'glory' seems to be unsatiable [insatiable], while I am in this respect completely saturated. I only need to have something in science which interests me sufficiently and with which I can play (without being a hero in the limelight of the 'world').

---

[3] The champion seems to have been Yoshio Nishina (1890–1951) who is well-known for his work on Compton scattering (the Klein-Nishina formula). See also the homepage of the Nishina Memorial Foundation.

[4] Valentine Telegdi (1922–2006) was a well-known experimental particle physicist who had a fantastic memory, which he was very proud of. He used to joke that he would remember the names of physicists' former girl friends, long after they themselves had forgotten them. He had known many distinguished physicists and would gladly tell stories about them and imitate them. Telegdi was a member of quite a few distinguished societies and there is a wealth of information available about him on the internet.

[5] Heisenberg's behavior reminds one of some plays (e.g., The Master Builder) by the Norwegian playwrite Henrik Ibsen who describes an ageing man, with a brilliant past career, but who constantly fears to be considered as a relic of a departed era.

> Heisenberg's opposite attitude, with which he certainly wishes to compensate earlier failures, may have many reasons lying in the whole history of his life. (Rabi[6] may analyze that.) I reacted to it with jokes which I sent to different places (one of these to my friend Panofsky[7] – the father of the physicist in Princeton, one to Weisskopf, who always likes to spread something – and it is much better to spread my jokes on Heisenberg than to spread wild rumors). . . . "

In his very last letter, dated 5 December 1958, addressed to C-S. Wu, Pauli wrote (letter [3125] in the Pauli Collection):

> "... the statement that I have changed my mind on his [Heisenberg's] 'theory' since the CERN conference[8], seems to be on his record now. Wishful thinking is always with Heisenberg.
>
> My prediction is, that Heisenberg will soon get in touch with the dictator Nasser in Egypt, to convince him of his point of view. Nasser has much more power than the Queen[9] of Greece!
>
> With warmest regards from both of us, also to Yang and Lee and to Rabi
>
> Very sincerely your W. Pauli"

This was to be Pauli's last day in office and his last words about his closest scientific friend and collaborator Werner Heisenberg. The next day he was taken to the hospital and on 15 December he passed away.

[6] Isidor Rabi (1898–1988), Nobel Laureate in Physics 1944 and a colleague of C-S. Wu at Columbia University.
[7] Pauli is referring to Erwin Panofsky at Princeton, who was an art historian and the father of Wolfgang Panofsky – the famous physicist at SLAC.
[8] Pauli is referring to the International Conference on High Energy Physics at CERN, which took place in the summer of 1958.
[9] Pauli is referring to an earlier comment by Wu who had heard that Queen Frederica of Greece (1917–1981) had visited Heisenberg and had been much impressed by his unified theory.

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Correspondence With Heisenberg Through Pauli

Most but not all of Källén's correspondence with Werner Heisenberg goes through Wolfgang Pauli. During the period in question Pauli is in Zürich, Källén in Copenhagen and Heisenberg in Göttingen, situated about half way between Zürich and Copenhagen. (Heisenberg moved to Munich in the fall of 1958, just a few months before Pauli passed away.)

There is a pattern in the Källén-Heisenberg correspondence. Pauli receives a letter from Heisenberg, and gets excited about his often wild ideas and, as he puts it, "sloppy" calculations. He is usually quick in answering his friend. Sometimes he reminds Heisenberg of their proud common "Sommerfeldian" heritage. How come then Heisenberg is so afflicted with unworthy sloppiness and wishful thinking? He then raises some criticism and sends off his answer. Quite often, he sends Heisenberg's calculations to Källén for careful scrutiny. Källén often finds errors in Heisenberg's calculations and informs Pauli about it, who in turn passes on the information to Heisenberg.

Pauli and Källén treat Heisenberg very differently. Pauli keeps on criticizing Heisenberg, sometimes rather brutally. For example, in a letter (letter [2490] in the Pauli Collection) dated 5 February 1957, he writes (translated from German):

> It was my belief in your academic honesty and the remembrance of old times which led me to sacrifice a lot of time on you. (I know that practically nobody reads your papers and that your students gladly run away from you.)

In spite of this, Pauli never ignores Heisenberg, as if between his letters he pushes a special delete-key in his memory and restarts again. Heisenberg too, seems to have his delete-key ready to push and remove all of Pauli's spiteful comments. He quickly makes some corrections and sends a new version of his work to Pauli. To an outsider, this looks very much like a "game" that the two friends keep on playing, perhaps to the benefit of both of them. Indeed, Pauli keeps on working on Heisenberg's ideas. For example in a letter (letter [2774] in the Pauli Collection) dated as late as 6 December 1957 (i.e., about one year before he died) he writes to Källén (translated from German):

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> I have on purpose given a talk, in the Zürich seminar series, about Heisenberg's work on the Lee Model, in order to go through the whole thing for myself. The result is that I am beginning to take the case of complex roots seriously …
>
> I don't know much about Heisenberg's non-*linear* spinor equation, because I have no feeling for the Tamm-Dancoff[1] approximation. But I am going to try …

Here the last dots stand for what he plans to do. Indeed both Pauli's interest and Heisenberg's persistence are remarkable! They both care about physics!

Källén, however, contrary to Pauli, does not take Heisenberg's work so seriously because he makes too many mistakes. For Källén, Heisenberg is indeed a giant, a great monument, but of the past.

Heisenberg is reluctant to communicate with Källén. The impression one gets is that Heisenberg is not the least bothered by Pauli's "nasty" remarks but doesn't want to take the risk of being offended by the quarter-century younger Källén. In direct contacts with Heisenberg, Källén is as blunt as Pauli, but more respectful and much less patient. Here below, we give a few examples, to illustrate what Källén thinks of Heisenberg.

In a letter, dated 2 January 1957, Källén writes (letter [2432] in the Pauli Collection) to Pauli (translated from German):

> Dear Professor Pauli,
>
> Thank you for your two letters. In your first letter (from 14 December) you, together with Heisenberg, ask the question: "Can one in the Lee Model, above the critical value of the coupling constant, arrange it so that the ghost-state becomes metastable?" He [Heisenberg] has addressed this very same question to Møller and Glaser, but *not* to me[2]. (I don't know whether he doesn't have the courage, or he finds that my opinions about it are uninteresting.) In any case, the answer is unequivocally "no".

Källén then gives a detailed explanation of what happens.

Actually Pauli, urges Heisenberg to contact Källén directly, for example (*in* letter [2433] in the Pauli Collection, dated 4 January 1957) he writes (translated from German):

---

[1] Named after Igor Y. Tamm (1895–1971), who is famous as he received a Nobel Prize in 1958 (see his autobiography), and Sidney M. Dancoff (1913–1951) who in 1939 had invented an improved perturbation method that played an important role in Tomonaga's work which eventually earned him a Nobel Prize in 1965. See Tomonaga's Nobel lecture.

[2] Christian Møller and Vladimir Glaser, as well as Källén were at the time in Copenhagen.

> Dear Heisenberg!
>
> If you wish to get an answer from letters to Copenhagen, I give you the practical advice to address them not to Møller or Glaser, but to Källén. To wit, he gladly writes long letters (especially when he believes he can prove you wrong, which is almost always the case).

Indeed, having gone through Källén's correspondence one sees that Källén often answers with equations and detailed calculations. These don't leave much room for discussions, speculations, and wishful thinking. Källén stands up to both Heisenberg and Pauli. For example, he writes to Pauli, concerning Heisenberg's work which has been sent to him by Pauli, again together with a new set of questions (translated from German):

> In my opinion, Heisenberg should now discuss these extra conditions and if possible show that they are satisfied. I don't see why I must show that they are *not* satisfied.

In spite of not believing in Heisenberg's work, years later in 1965 when Källén was informed that Heisenberg was visiting Copenhagen, he invited him to visit Lund. Heisenberg came and gave a talk about his computation of the fine-structure constant from first principles. Källén urged the students to go to the talk and see the great man, an event they would remember for the rest of their lives, but warned them that he was going to talk nonsense. It was indeed a memorable event. Heisenberg was full of youthful energy and enthusiasm. He looked friendly and happy. The talk was incomprehensible enough that one didn't even know what kind of "nonsense" he was talking about – if that is what he was doing. Källén was polite to his guest and did not ask any tough questions or make "unfriendly" comments.

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On the Work by Heisenberg and Pauli – I

Heisenberg, in his non-linear spinor theory, tries to make a unified model for particle interactions based on postulating just one single fundamental spinor field. He hopes that even the photon can be constructed[1] out of his spinor. However, the model fails and more ingredients are necessary in order to be able to move on. Pauli is intensely involved in the development of this model, which is extended by including isospin, degenerate vacua and parity doubling (mirror particles) to explain parity violation. There is a major difference between Heisenberg's and Pauli's approaches to this problem. Heisenberg wishes to publish the results quickly. Pauli, on the other hand, likes to circulate their intermediate results to get input from others, among them especially Källén. Pauli urges Heisenberg, over and over again, to be patient. For example, in a letter ([2849] in the Pauli Collection) to him on 1 February 1958 he writes that the order has to be (translated from German):

> … First understand oneself, then publish and not the other way around.

Källén has several objections, such as the assumed γ_5-invariance entails zero mass and that not all particles appear in isospin doublets. Doubling the number of states is not going to be enough. When Pauli objects without giving any reason, Källén writes back (letter [2853] in the Pauli Collection) on 3 February 1958 (translated from German):

> Dear Professor Pauli!
>
> Thank you for your letters of 10 and 16 January. Unfortunately, I understand virtually nothing of what you have written. The only thing that I am to some extent sure about is that the initial sentence in your letter of 10 January stating that "Your solution [i.e., Källén's] in your letter of 9 [of January] is essentially the same as mine" *can't* be correct, and what you have done must be quite different from my simple observations in my letter of 9 January. …

[1] This was not the first time that economy lured the physicists to employ such a scheme. Earlier, there was the so-called neutrino theory of light, where the photon was assumed to be made of a pair of neutrinos. This theory was never successful.

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> I have just come back from Switzerland. There, I understood that you have left not only me but the whole Switzerland in the state of great confusion. But in Zürich I heard that there is soon going to be a preprint[2] and I hope that I perhaps will be able to understand more from it.

Pauli answers from Berkeley (letter [2855] in the Pauli Collection), already on 6 February (translated from German):

> Dear Mr. Källén!
>
> Thank you for your letter of 3 February.
>
> I didn't know how to avoid a state of confusion: as at that time many more things were unclear to me than now (a lot of things are still unclear to me). ...

Pauli goes on to explain some of his problems. He doesn't believe that their Lagrangian formalism is quite appropriate, etc. At the end of his letter he has the following statement (translated from German):

> I believe that, irrespectively of the number of fields introduced in a model, the consecutive decrease of the amount of symmetry – first by going from strong to electromagnetic interactions, and then again by going from the electromagnetic interactions to weak interactions – is not due to arbitrary additional terms in the Hamiltonians or Lagrangians, but in fact arises through the metric in the Hilbert space due to mathematical necessity for a convergent theory. ...
>
> If you in the meanwhile can alleviate the confusion, I will be happy (I don't know what more is going to be done in Göttingen.[3])

It is interesting to note that Källén does not shy away from urging Pauli to express himself clearly. For example in a letter ([2859] in the Pauli Collection) dated 7 February 1958 he writes (translated from German):

> Dear Professor Pauli,
>
> Thank you for your letter of 2 February from New York[4]. I am glad that you now again write in a plain language and do not only make obscure insinuations that are essentially incomprehensible to me.

[2] Källén is referring to a draft of a paper by Heisenberg and Pauli, "On the Isospin Group in the Theory of Elementary Particles", which was circulated later in 1958.
[3] Where Heisenberg was until the fall of 1958 when he moved to Munich.
[4] Pauli was on his way to Berkeley where he spent the Spring term 1958.

In a letter to André Petermann[5] at CERN, dated 7 November 1958, Källén writes:

> "… On Monday Nov. 17 we have Heisenberg [in Copenhagen], and he is supposed to give a lot of talks during that week. Personally, I am not looking forward to his visit, but there is nothing to be done about it now. …"

One can be sure that Heisenberg's lectures on his spinor theory did not impress his distinguished audience in Copenhagen.

[5] André Petermann (1922–2011) was one of the first permanent staff members of CERN. He was famous for his work on renormalization group and once Källén said Petermann is not only an excellent physicist but also an accomplished Alpinist who once had survived a severe accident.

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On the Work by Heisenberg and Pauli – II

On the 22 February 1958, Källén writes (letter [2882] in the Pauli Collection) to Pauli who is at Berkeley (translated from German):

> Dear Professor Pauli!
>
> In the last few days, I have seen a photocopy of your article with Heisenberg. (This does *not* mean that I have received the manuscript directly from Heisenberg. The photocopy was shown to me by Aage Winther[1]. He had got it from Pablo Kristensen[2] in Aarhus, who is said to have received it from Swiatecki[3]. I don't know the earlier history of the photocopy. However, one can see Dyson's name in a corner. I find it very amusing that I have received your and Heisenberg's manuscript in this complicated fashion!) Your manuscript has helped insofar as, I am afraid, I now understand your argument concerning compatibility of the γ_5-invariance and non-zero masses. From psychological point of view it is quite interesting to study your reaction to my letter of 26 December. On 2 January, first you say [to me] "What you have written is known to me since a long time". Then comes the argument on degeneracy, which I have understood and is surely correct (see below). At the end of the same letter, however, there is in fact a "Brask-note" {Do you know the gentleman Brask?[4] He was (in Sweden) a famous Quisling[5] during the war between Sweden and Denmark in the beginning of 17th century. He was the only one who was not beheaded in Stockholm by Christian [Kristian] the Tyrant. This he achieved by a cunningly written note. I have found out that he is less well-known in Denmark} "This letter is naturally only preliminary". (In other words, although you since a long time know this difficulty, you only give a preliminary answer!) Then the question concerning Λ_0 came up, after which you first on 10 January driveled about anti-Λ_0 in order to, on January 16, make a completely different calculation according to which the compati-

---

[1] A Danish nuclear physicist in Copenhagen.
[2] Poul Kristensen, called Pablo, was a field theorist working in Copenhagen and later in Aarhus, the second largest city in Denmark. He worked among other things, on non-local field theory, partially together with Christian Møller.
[3] Wladyslaw J. Swiatecki, a nuclear physicist who at the time was affiliated with Berkeley.
[4] Hans Brask (1464–1538), Swedish bishop, who is said to have saved his own life by adding a sentence to his signature, noting that he was compelled to sign.
[5] Quisling, after Vidkun Quisling (1887–1945), an infamous Norwegian. Here Quisling means traitor.

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bility of a γ_5-invariant spinor model with the exact [description of the] world is to be established. This computation was until now for me incomprehensible. In all your following letters you have railed against the γ_5-argument and called it "False" and so on. Just this shows, however, that on this matter you feel remorse (consciously or subconsciously), and I know now the reason. In the manuscript I see now clearly that you begin to do the following. You start with the usual [Dirac] equation[6]

$$(\gamma \frac{\partial}{\partial x} + m)\psi = 0 \tag{1}$$

and then write

$$\psi' = \frac{1}{\sqrt{2}}(\psi + \gamma_5 \psi^C); \quad \psi'^C = \frac{1}{\sqrt{2}}(\psi^C + \gamma_5 \psi)$$

ψ'^C satisfies the equation

$$\gamma \frac{\partial \psi'}{\partial x} + m\gamma_5 \psi'^C = 0$$

and is γ_5-invariant, i.e., invariant under the transformation $\psi' \to \gamma_5 \psi'$, even when $m \neq 0$. This is correct and also understandable, because *the γ_5-invariance for ψ' corresponds to the charge conjugation for ψ*. Indeed, also under charge conjugation ($\psi \rightleftarrows \psi^C$) equation (1), with $m \neq 0$, is invariant. The converse is also true, i. e., the γ_5-transformation of ψ corresponds to charge conjugation of ψ' (apart from a sign, which is uninteresting). Since the γ_5-invariance for the original ψ has as its consequence $m = 0$, one would expect that charge conjugation of ψ' implies $m = 0$. This can be checked simply, and comes out. [Källén shows this by doing a simple calculation.]. ...

Therefore it is not correct the way you express it in your letter of 2 February, that the prediction $m = 0$ depends on how the theory is written down. Also, it was not *my* argument that was wrong and in this connection had contributed to the confusion. ...

Pauli answers Källén from Berkeley on 25 February 1958 (letter [2888] in the Pauli Collection), defending himself (translated from German):

The manuscript that you have got, I regret very much to say, was *not good enough* for people like you (perhaps neither will be the new one that you will

[6] If the reader does not quite recognize this equation, it could be due to the fact that it is written in the Pauli metric, which Källén always used. The signature of the metric is such that, for a particle of mass m and four-momentum p, one has $p^2 = -m^2$.

> finally get); therefore, I have systematically prevented it from being sent out. The calculations that you allude to are no longer in the new manuscript. …
>
> We have now deliberately added vacuum expectation value terms that break the γ_5-invariance. …
>
> …
>
> I am not at all convinced by the spinor model, but *also not yet* [sure] that it is impossible.
>
> The new manuscript is also not yet for publication.
>
> I am still for [the idea that] vacuum expectation values could have a lesser degree of symmetry than the Lagrangians, in a framework with indefinite metric (independently of the spinor model). What do you think?

Near the end of February 1958, Heisenberg gives a colloquium, with the title "The World Formula" about his and Pauli's joint work (letter [2896] in the Pauli Collection). This unleashes a "thunderbolt" in the physics sky, at least as viewed by journalists. Finally Einstein's dream of a unified theory has been realized by Heisenberg and his collaborator Pauli. At the end of his presentation, Heisenberg had made in a humorous way an unwise statement (translated from German):

> Leibniz labeled the world as the best of all possible worlds. He [Heisenberg] was not so sure one could say that. But it looks as if it possibly is the simplest of all worlds.

Reacting to Heisenberg's "advertisement", Pauli quickly makes a cartoon. He draws a rectangle, where the sides are marked but there is nothing inside. The figure caption of this empty rectangle reads:

> "This is to show the world, that I can paint like Tizian[7]:
>
> Only technical details are missing."

Heisenberg's explanation to Pauli (letter [2908] in the Pauli Collection) of what had happened, was the following (Translated from German):

> I had already given a couple of talks about our work at our Institute and nothing had happened. Then Hund[8] asked me to talk about it at the official university colloquium. An extraordinary number of people showed up,

[7] The famous 16th century Italian painter, also known as Titian.
[8] Friedrich Hund (1896–1997). Among his prolific scientific production one finds a delightful little book "The History of Quantum Theory" Barnes & Noble (1974).

> among them, without me knowing it, also journalists. It is they who have spread the hair-raising nonsense such as "The end of physics" and so on. Then I started getting hundreds of phone calls. Finally, I dictated a couple of sentences that my secretary could say on my behalf; the most important being that our work "new proposals for a unified field theory", whose validity can only be decided first after a few years of research. (Unfortunately I had not added the epithet "Nobel Prize" to your name, whereby to my annoyance you were not mentioned on a symmetric [equal] footing with me, at least not everywhere.) Afterwards, the absurdity somewhat ebbed away; but then Landau in Moscow (certainly unintentionally) must have poured oil into the flame of the journalists' ecstasy. Anyhow, by referring to Landau's talk in Moscow, it took off with an intensified degree. …

In March 1958 (letter [2901] in the Pauli Collection) Heisenberg and Pauli decide to send the new version of their work to about 70 distinguished scientists in several countries, not only in Europe but also in USA, Russia, India, Japan, Brazil and Turkey. One is, however, struck by the fact that the list doesn't have a single person working in France (for example Louis Michel) or in Netherlands, in spite of the fact that Pauli had just been elected to the Royal Dutch Academy of Sciences.

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On the Work by Heisenberg and Pauli – III

We are now in the spring of 1958. Pauli is at Berkeley, Heisenberg in Göttingen and Källén in Copenhagen.

Heisenberg and Pauli continue their work on the spinor theory. However, none of them is versed in phenomenology. For example Pauli believes (letter [2914] in the Pauli Collection) that the decay of charged pions goes via electromagnetic interactions! How to deal with strange particles is another major problem. The most important issue is how to compute the eigenvalues, i.e., the masses of elementary particles. Pauli describes his collaborative work with Heisenberg, to Källén in a letter from Berkeley dated 12 March 1958 (letter [2918] in the Pauli Collection). In this work strangeness is not present at all. Källén answers (letter [2931] in the Pauli Collection) on 18 March (translated from German):

> Naturally you are right when you say that the Fieldverein[1] LSZ has not given a method by which one can compute the mass spectrum. It seems to me that it is not quite right to rail about that too much, because those people never did claim that they have done or are able to do so. However, this is not the case with Heisenberg, who claims that he has computed the eigenvalues in his model, within a certain approximation. In particular, since one sees easily that all his computations are nonsensical due to γ_5-invariance.

About three weeks later, on 7 April, Pauli writes to Heisenberg that he no longer believes in their joint work and has decided to pull out of the collaboration. This is not because of Källén, who had already found that the foundations of the model were shaky. Pauli thought that Källén's objections could somehow be dealt with later on. The model had simply too many problems. In fact it explained nothing. On 8 April, i.e., the day after Pauli quits working with Heisenberg, he wrote to Källén (translated from German):

[1] The German word "Verein" (meaning club, association or society) is often used in physics. Here it refers to the trio Harry Lehmann, Kurt Symanzik and Wolfhart Zimmerman (LSZ). Earlier they had worked together in Göttingen and sometimes Pauli would refer to them as Heisenberg's magicians.

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> Dear Mr Källén!
>
> I gave myself some time before answering your letter of 18 March. When it arrived, I had already strong suspicion that the interpretation of the strange particles, with half-integer difference between spin and isospin, through degenerate vacuum – in short – is an act of deception. …
>
> With this the logical situation changes, that was the basis of our controversy, and indeed – it looks to me – in a way, that it allows all the involved self-willed people to be satisfied: I *agree* with the sentence in your letter of 18 March:
>
> Especially, it looks to me logically impossible that the discovery of Gürsey[2] … can be used to build up the world of elementary particles, starting with a single spinor field and requiring "Pauli invariance".

Pauli's letter ends with:

> I leave the old – not iso-invariant – work of Heisenberg to your discretionary execution.

Finally, in the summer of 1958, Pauli was the Chairman of a session called "Fundamental Theoretical Ideas" at the "1958 Annual International Conference[3] on High Energy Physics", CERN (30 June-5 July). He opened the session by stating:

> "This session is called 'fundamental ideas' in field theory, but you will soon find out or have already found out that there are no new fundamental ideas. … So, you will also see that there are two kinds of ignorance; the rigorous ignorance and the more clumsy ignorance. You will also hear that many speakers will want to form new credits for the future. …"

Indeed, Heisenberg was one of the speakers in this session! Recently, Jack Steinberger who was a participant at the above conference told me (CJ) that he had found Pauli's treatment of Heisenberg very unpleasant. This is not surprising. Playing "games" among friends in seclusion is one thing but doing it in front of young people conveys a completely different message. All those, who as Weisskopf used to say, "had been spanked by Pauli long enough" got to appreciate it and love him. But how is a young outsider supposed to know that Pauli's purpose was simply to keep people scientifically completely honest and that he actually "loved" people?

---

[2] Feza Gürsey (1921–1992), who corresponded with Pauli and later on visited him in Zürich, had proposed a variation on the theme of Heisenberg-Pauli. A short account Gürsey's scientific achievements is given in the June 1992 issue of CERN Courier.

[3] The Proceedings of this Conference were published by CERN, Scientific Information Service.

Heisenberg's Disapproval of Pauli's Behavior

For once Heisenberg, who had always been very polite to Pauli and not reacted to his nasty remarks, rebelled against him and explicitly expressed his dissatisfaction. In a letter dated 13 April 1958, he wrote (translated from German):

> I believe I have to tell you that lately you have been hurting me. You have emotionally placed our article "up one minute – down the next" while it is simply a question of a long-term difficult work, where now and then difficulties must arise, which may require many weeks of drilling in hard wood or carving. …
>
> You write that one is allowed to publish a paper only when one clearly understands all its consequences. I believe, and I am sure that you will agree, had this principle been applied neither Bohr's work on hydrogen atom, or his work on the periodic table of elements, nor my work on quantum mechanics had been allowed to be published. …

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Correspondence With Paul Dirac – A Brief Introduction

The correspondence Källén-Dirac is especially interesting as an elucidation of their personalities and views on field theory.

Dirac was a much appreciated visitor in Lund, to the extent that he was elected a foreign member of the "Royal Physiographic Society"[1] in Lund – an honor that he shared with, among others, Niels Bohr and Wolfgang Pauli. When in Lund, he was relaxed, and was well taken care of by Professor Torsten Gustafson, a true master of making the distinguished guests feel at home.

Dirac was notoriously aloof and quiet, to the extent that the students in Cambridge had introduced a new unit "Dirac", one Dirac being one spoken word per year.[2] He did, however, communicate with children.[3] Once I (CJ) asked Jim Hamilton, who had been a Lecturer at Cambridge University (1950–60) about how the students perceived Dirac as compared to his contemporary at Cambridge, the famous cosmologist Fred Hoyle. Having read some of Hoyle's science fiction, I was expecting to hear that Hoyle was far more popular. According to Hamilton, however, the situation was just the opposite: The students "loved" Dirac because he taught them precisely what they needed for passing their exams, neither more nor less.

During a visit by Dirac in Lund, in 1966, he and Källén had some discussions on field theory and renormalization. Afterwards, Källén was in the process of writing a long letter to Dirac (he had finished the first 15 pages describing his views on field theory) when he received a letter from Dirac. In order to make their discussions more transparent, we start with the first part of Källén's letter (a condensed version of the first 15 pages) followed by Dirac's letter and the second part of Källén's first letter which addresses questions raised by Dirac.

All in all, each of the two men wrote three letters. As mentioned before, Dirac's letters are hand-written and Källén's are typed.

[1] See Chap. 60.

[2] This information originates from James Hamilton (1918–2000) who was the only professor of particle physics at NORDITA (Copenhagen).

[3] According to Torsten Gustafson who had witnessed Dirac's communication with children.

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Dirac-Källén Correspondence – The First Letters

In September 1966 Källén writes from Lund to Dirac at St. John's College, Cambridge, England:[1]

> "Dear Professor Dirac:
>
> As we agreed when you were here a few weeks ago I hereby send you a short summary of the way I like to look at the relation between the Heisenberg fields and the incoming fields in quantum field theory with special emphasize on the commutators of these quantities.
>
> First a few words about notation which, I am afraid, does not entirely agree with yours. However, I hope you will permit me to use my own notation as it minimizes the chance of my making trivial errors in signs, factors i and 2 etc. ..."

Källén informs Dirac that he uses Pauli metric and explains his notations in detail.

He considers a free scalar field $\varphi^{(0)}$ where the superscript (0) denotes that the field is free. Then he points out that the standard canonical quantization rules imply that the equal-time commutator [note that x_0 denotes time] of these fields vanishes, viz.,

$$[\varphi^{(0)}(x), \varphi^{(0)}(x')]_{x_0=x_0'} = 0,$$

whereby

$$[\varphi^{(0)}(x), \varphi^{(0)}(x')] = -i\Delta(x' - x, m^2).$$

[1] Notes added by me (CJ): This letter tells a lot not only about Källén's science but also about his personality. He tries to be very clear, explains via doing specific calculations, gives plenty of details and does not sweep anything "under the carpet". The Källén correspondence shows that he behaved in this manner not only when dealing with great men, such as Pauli or Dirac, but also when he answered questions by not well-known or young physicists.

We do not know whether Dirac actually read Källén's above letter carefully (probably not). But we do know that he answered it soon after receiving it.

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Källén then writes down the equation of motion of the interacting version of the field in the Heisenberg representation,

$$(\Box - m^2)\varphi(x) = -j(x)$$

where $j(x)$ stands for the generalized current, which is in general a functional of other fields. Next, he converts this differential equation into an integral equation, using the values of the field and its time derivative at a given time $x_0 = T$

$$\varphi(x) = \varphi_T^{(0)}(x) - \int_T^{x_0} dx' \, \Delta(x - x', m^2) j(x'),$$

$$\varphi_T^{(0)}(x) = - \int_{x_0'=T} d^3x' \left[\frac{\partial \Delta(x - x', m^2)}{\partial x_0} \varphi(x') + \Delta(x - x', m^2) \frac{\partial \varphi(x')}{\partial x_0'} \right],$$

$$(\Box - m^2)\varphi_T^{(0)}(x) = 0.$$

Here dx' denotes four-dimensional element of integration while the three dimensional integration has been explicitly exhibited. Källén notes further that the integral equation contains more information than the equation of motion because it also includes a statement about the boundary values at time T. He then points out that $\varphi_T^{(0)}$ is a free field which happens to have the Heisenberg field $\varphi(x)$ as boundary value for the time $x_0 = T$. As a special case he considers the limit when $T \to -\infty$ and identifies the limiting field with the incoming free field $\varphi^{(in)}(x)$. He thus obtains

$$\varphi(x) = \varphi_T^{(in)}(x) - \int dx' \, \Delta_R(x - x', m^2) j(x'),$$

$$\varphi^{(in)}(x) = - \lim_{T \to -\infty} \int_{x_0'=T} d^3x' \left[\frac{\partial \Delta(x - x', m^2)}{\partial x_0} \varphi(x') \; + \Delta(x - x', m^2) \frac{\partial \varphi(x')}{\partial x_0'} \right].$$

Källén then calculates the commutator of the two free fields at time T and finds

$$[\varphi_T^{(0)}(x), \varphi_T^{(0)}(x')] = -i\Delta(x' - x, m^2)$$

i.e., the right-hand side is the standard singular function appearing in the commutator of free fields and the result is "independent of the boundary time T" [underlined by Källén]. Consequently, it also holds in the limit when $T \to -\infty$. Therefore

$$[\varphi^{(in)}(x), \varphi^{(in)}(x')] = -i\Delta(x' - x, m^2).$$

He goes on to discuss the significance of this result, that it is independent of the specific form of interaction (the form of current $j(x)$) and notes that it can be generalized to higher spin. For the case of Dirac field ψ, after some calculations, he finds

$$\{\overline{\psi}^{(in)}(x), \psi^{(in)}(x')\} = -iS(x' - x, M).$$

He adds

> "The only assumption which really goes into the argument [derivation] is that the division of the Heisenberg field in an incoming field and a retarded potential makes sense."

Subsequently, Källén presents a specific model and does a detailed calculation, several pages long, in perturbation theory and checks the validity of the above results. He informs Dirac that:

> "As you see from the calculations above, the commutation relations for the incoming field (in this way of looking into formalism) are identical with the commutation relations for the Heisenberg fields only for equal times. For different times the commutation relations for the incoming fields are much simpler and essentially given by a c-number expression while the commutation relations for the Heisenberg fields involve extra quantities ...
>
> In the process of writing the above, I have just received your letter of September 30th. Thank you very much for it. ... "

We shall reproduce a condensed version of the second part of Källén' first letter after presenting Dirac's first letter and his questions.

Dirac's First Letter On 30 September 1966, Dirac writes to Källén:

"Dear Källén,

I have been thinking about our discussion and would like to summarize the situation as I see it now. We have two different starting points.

Mine. I assume field quantities A, ψ, $\overline{\psi}$ satisfying the usual commutation relations at one instant of time. I assume a Hamiltonian H which forces the time variation of the field quantities $i\hbar\frac{dA}{dt} = AH - HA$ etc. In order to avoid infinities in the integration of these equations I put a cut-off in H, involving a cut-off energy g. One can proceed to define retarded fields and ingoing fields in the usual way and one can express them in terms of A, ψ, $\overline{\psi}$ at some instant of time. The results will involve g, since they require the integration of the field equations. The commutation relations of the ingoing field quantities will then involve g.

Yours (as I understand it)

(i) You assume ingoing field quantities A_{in}, ψ_{in}, $\overline{\psi}_{in}$ satisfying the usual commutation/anticommutation relations.
(ii) You assume (or define) total field quantities A, ψ, $\overline{\psi}$ by $A = A_{in} + A_{ret}$ etc.
(iii) You assume the usual formula for A_{ret} in terms of ψ, $\overline{\psi}$ on the past light cone, and similarly for ψ_{ret}. You then have a complete scheme of equations and you do not need to assume a Hamiltonian. You can proceed to make calculations, expressing A and ψ in terms of A_{in} and ψ_{in}, using a perturbation method in which you expand everything in powers of e. But you will have infinities appearing, and I would like to know how you deal with them. I suppose you have to change one of your assumptions (i), (ii), (iii). I should be glad if you tell me which one, and how you change it. I do not mind your using a negative metric, provided you have a self-consistent set of equations in the Heisenberg picture.

Yours sincerely

p a m Dirac[2]"

[2] Dirac writes his initials with small letters.

30 – 9 – 66

Dear Källén

I have been thinking over our discussion and would like to summarize the situation as I see it now. We have two different starting points.

Mine. I assume field quantities $A, \psi, \bar{\psi}$ satisfying the usual commutation relations at one instant of time. I assume a Hamiltonian H which fixes the time variation of the field quantities $i\hbar \frac{dA}{dt} = AH - HA$ etc. In order to avoid infinities in the integration of these equations I put a cut-off in H, involving a cut-off energy g. One can proceed to define retarded fields and ingoing fields in the usual way and one can express them in terms of $A, \psi, \bar{\psi}$ at some instant of time. The results will involve g, since they require the integration of the field equations. The commutation relations of the ingoing field quantities will then involve g.

Yours (as I understand it)

(i) You assume ingoing field quantities $A_{in}, \psi_{in}, \bar{\psi}_{in}$ satisfying the usual ~~anti~~commutation relations

(ii) You assume (or define) total field quantities $A, \psi, \bar{\psi}$ by $A = A_{in} + A_{ret}$ etc.

(iii) You assume the usual formula for A_{ret} in terms of $\psi, \bar{\psi}$ on the past light cone, and similarly for ψ_{ret}

You then have a complete scheme of equations and you do not need to assume a Hamiltonian. You can proceed to make calculations, expressing A and ψ in terms of A_{in} and ψ_{in}, using a perturbation method in which you expand everything in powers of e. But you will have infinities appearing, and I would like to know how you deal with them. I suppose you have to change one of your assumptions (i), (ii), (iii). I should be glad if you tell me which one, and how you change it. I do not mind your using a negative metric, provided you have a self-consistent set of equations in the Heisenberg picture

Yours sincerely
P A M Dirac

Källén's First Letter – Part 2

As mentioned above, Källén received Dirac's first letter when he had already written 15 pages of his letter, explaining what he meant by the incoming fields and how one derives (not assumes) their commutation relations. On page 16 of his letter he continues (we quote his own writing):

> "I have just received your letter of September 30th. Thank you very much for it. I think I essentially agree with what you say there but would like to add the following amplifying comments.
>
> It is quite correct as you say that I do not actually need a Hamiltonian for the explicit calculations. However, as I have the standard commutation rules for equal times for the Heisenberg fields I can, in case I want it, introduce the standard form for the Hamiltonian. As I have the whole canonical formalism, I also have the usual formula that the time derivative of an arbitrary operator is essentially given by the commutator of the operator and the Hamiltonian. Therefore, I have recovered this aspect of the formalism as a consequence of my assumptions. It follows that I do not really believe one can obtain radically different formalism by starting from the Hamiltonian version of the theory. It rather appears to me that it is a question of taste which part of the formalism one wants to take as a starting point. In particular, I feel that it should be possible to start from the Hamiltonian formalism and derive the commutation relations for the incoming fields by a suitable argument. This is essentially what the calculation above is supposed to do.
>
> I certainly do agree that the specific way in which a cut off is introduced can be very important for the detailed aspects of the formalism. As you explicitly ask for my cut off procedure in your letter, I hereby add an example of a rather explicit calculation. More or less at random I pick the example of the vacuum polarization and the photon self energy as this is perhaps the part of the formalism where the cut off enters in the most critical way. The point I want to illustrate is that the cut off procedure I am using (which is really the technique of Pauli and Villars as I mentioned before) gives zero self energy for the photon as well as the standard result for the vacuum polarization. To simplify matters as much as possible I now consider a problem where I have an external electromagnetic field $A_\mu^{ext}(x)$ coupled to an electron-positron field. The equation of motion for the latter quantity is $(\gamma \frac{\partial}{\partial x} + m)\psi(x) = ie\gamma A^{ext}(x)\psi(x)$. The perturbation theory solution to this equation can be written as
>
> $$\psi(x) = \psi^{(in)}(x) - ie \int S_R(x - x', m)\gamma A^{ext}(x')\psi^{(in)}(x')dx' + \ldots$$
>
> As the electron field itself is not immediately observable, we use the expression above to calculate a quantity of more physical importance, viz. the current

operator. For this one we have (in perturbation theory)

$$
\begin{aligned}
j_\mu(x) &= \frac{ie}{2}[\overline{\psi}(x), \gamma_\mu \psi(x)] = \frac{ie}{2}[\overline{\psi}^{(in)}(x), \gamma_\mu \psi^{(in)}(x)] + \\
&\quad + e^2 \int dx' K_{\mu\nu}(x,x') A_\nu^{ext}(x') + \ldots \\
K_{\mu\nu}(x,x') &= \frac{1}{2}[\overline{\psi}^{(in)}(x),\ \gamma_\mu S_R(x-x',m)\ \psi^{(in)}(x')] + \\
&\quad + \frac{1}{2}[\overline{\psi}^{(in)}(x'),\ \gamma_\mu S_A(x'-x,m)\ \psi^{(in)}(x)]. \\
S_A\ (x,m) &= \theta(-x) S(x,m).
\end{aligned}
$$

The term of order e^2 is the correction to the current operator due to the applied external field. Therefore, this is the vacuum polarization term. The induced field or, rather, the potential for the induced field can be calculated from the vacuum polarization current using the standard Maxwell equation according to the formula

$$
\begin{aligned}
\Box A_\mu^{(ind)}(x) &= j_\mu^{(ind)} \\
&= -e^2 \int dx' K_{\mu\nu}(x,x') A_\nu^{ext}(x') + \ldots, \\
A_\mu^{(ind)}(x) &= e^2 \int dx' D_R(x-x') \int dx'' K_{\mu\nu}(x',x'') A_\nu^{ext}(x'').
\end{aligned}
$$

The calculation so far is, of course, extremely formal and especially the expression $K_{\mu\nu}(x,x')$ involves infinite quantities. To handle them we have to introduce a cut off procedure. For this purpose we use the technique originally developed by Pauli and Villars (Rev. Mod. Phys. 21, 434 (1949)) and introduce several extra degrees of freedom. We consider a large number of spin 1/2 particles with masses m_i and with charges $C_i\, e$. In a way, this sounds like a very reasonable thing to do as we know that there is not only one kind of particles around in nature. However, if this device is going to act as a cut off, it will be necessary to assume that some of the constants C_i are purely imaginary. This is certainly a very unphysical property of these particles and implies that the Hamiltonian is not Hermitian in the standard metric. However, it is possible to introduce an artificial metric where this new Hamiltonian is self adjoint. In this metric probability is conserved but probabilities are not always positive quantities. Either one prefers to work with a non-Hermitian Hamiltonian or with indefinite metric it is clear that these new particles are going to bring trouble with them. However, we are going to assume that all the masses m_i of those particles which have unphysical properties are extremely large. Mathematically, we are even going to take the limit when those masses go to infinity. On an intuitive bases it is reasonably clear that the physical effects of particles with very large masses in observable quantities are going to be negligible. This is the reason why we recover the standard formula for vacuum polarization at the end of the calculations.

To see how this idea works out somewhat in detail we consider the most singular part of the induced current above, viz. the vacuum expectation value of that current. The vacuum here means the state with no incoming particles. Of course, if the external field is suitable, there may be particles created during the process and present for any finite time as well as for $x_0 \to +\infty$. Using the standard formalism this vacuum expectation value becomes, when all the new degrees of freedom are added[3]

$$
\begin{aligned}
&< 0 \mid K_{\mu\nu}(x, x') \mid 0 > \to K_{\mu\nu}(x, x') \\
&= \sum_i C_i^2 \frac{1}{2} \{ Sp[\gamma_\mu S_R(x - x', m_i) \gamma_\nu S^{(1)}(x' - x, m_i)] \\
&\quad + Sp[\gamma_\nu S_A(x' - x, m_i) \gamma_\mu S^{(1)}(x - x', m_i)] \}
\end{aligned}
$$

The new function $S^{(1)}(x, m)$ appearing here comes from the vacuum expectation value of a commutator of two electron fields. It is given by

$$
\begin{aligned}
S^{(1)}(x, m) &=< 0 \mid [\overline{\psi}^{(in)}(0),\ \psi^{(in)}(x)] \mid 0 > \\
&= \frac{1}{(2\pi)^3} \int dq e^{iqx} \delta(q^2 + m^2)(i\gamma q - m).
\end{aligned}
$$

As earlier, this function can be computed explicitly and is given by a Neumann function. The details are not interesting for us here. The summation over the index i goes over all the particles present in our problem. We make the convention that $i = 1$ corresponds to the ordinary electron where $C_1 = 1$ and $m_1 = m =$ the electron mass. We note, in particular, that the trace corresponding to different degrees of freedom add incoherently to this order when we take the vacuum expectation value as the creation operators of one kind of particles must always be paired together with annihilation operators of the same kind of particles. Therefore, the formula for $K_{\mu\nu}(x - x')$ involves a sum of terms each of which contains only one kind of particles.

[3] Note that Källén uses Sp for trace, as was often done in the past. Sp is the short-hand for the German word Spur which means trace.

The Highlights of the Letter

Up to now we have been quoting directly from Källén's letter, where he has been pedagogically explaining his method to Dirac. Since the letter is rather long, from here on we concentrate on the major steps in his arguments. Next he computes $K_{\mu\nu}(x-x')$ by using the expressions for the Fourier transforms of the functions S_R, S_A and $S^{(1)}$. After some computations he finds that the Fourier transform of the function $K_{\mu\nu}$ decomposed into its real and imaginary parts is given by

$$K_{\mu\nu}(p) = R_{\mu\nu} + i\pi\,\epsilon(p)I_{\mu\nu}(p)$$

where the imaginary part is the familiar expression, by now found in many textbooks, viz.

$$I_{\mu\nu} = \frac{1}{12\pi^2}(p_\mu p_\nu - \delta_{\mu\nu}p^2)\sum C_i^2(1-\frac{2m_i^2}{p^2})\times \\ \times\sqrt{1+\frac{4m_i^2}{p^2}}\;\Theta(-p^2-4m_i^2).$$

Concerning this result he writes:

"We see from this expression that $I_{\mu\nu}$ vanishes as long as p is spacelike [Note that Källén is using the Pauli metric]. For time like values of p, $I_{\mu\nu}$ still gives no contribution until $-p^2$ is larger than $4\,m^2$, i.e., the threshold for the production of an electron-positron pair. This is, of course, in accordance with what one expects intuitively. We also note that if all the masses m_i for $i \neq 1$ are large, the imaginary part of the vacuum polarization kernel gives only the standard electron contribution for reasonable values of p^2. Another important feature of the result exhibited above is the appearance of the factor $p_\mu\,p_\nu - \delta_{\mu\nu}\,p^2$. This factor implies

$$p_\nu I_{\mu\nu} = 0.$$

This expression can alternatively be considered as a consequence of the invariance of the imaginary part of the vacuum polarization under gauge transformations of the external field, or as a consequence of charge conservation for this part. If the photon has no self mass, the same equation must hold not only for the $I_{\mu\nu}$ but for the total current. On this basis one expects also the real part $R_{\mu\nu}$ to fulfill this expression Actually, it is possible to construct a formal argument which appears to give this result."

Källén writes in detail the expression for $R_{\mu\nu}$ and computes $p_\nu\,R_{\mu\nu}$ and shows that the famous Pauli and Villars conditions $\sum C_i^2 = 0, \sum C_i^2 m_i^2 = 0$ make the latter expression vanish. $R_{\mu\nu}$ itself is gauge invariant, i.e., proportional to $p_\mu p_\nu - \delta_{\mu\nu} p^2$. However, it contains a logarithmically divergent contribution proportional to $\sum C_i^2 \log \frac{m_i^2}{m^2}$. He notes that "this term is the standard charge renormalization and must be subtracted. It is a term which does not depend on the momentum p and, therefore, is present also for a static field. Therefore, the observable part of the vacuum polarization kernel is finite." Källén ends his 25 pages long letter with the following paragraphs taken directly from his letter:

> I am sorry that the calculation above involves so many details. However, you asked for cut off procedure I am using and I do not really know how to explain it except by showing exactly how a calculation is done. Quite evidently, there are other procedures too, which could be used. For some purposes it is, e.g., convenient to consider besides the unphysical spin 1/2 particles a similar set of unphysical photons carrying imaginary coupling constants or negative probabilities. For the calculation of the Lamb shift one finds it suitable to use both. Mathematically, there are simpler procedures like differentiating with respect to the mass a sufficient number of times to make everything convergent and then integrating afterwards from very large masses to the physical masses of the electron or the photon. Clearly, such a procedure is equivalent to the method sketched above. However, the "physics" is perhaps clearer if one really considers the auxiliary particles.
>
> One thing which is not properly handled by the Pauli-Villars technique as used here is the convergence of some time integrations at $x_0 = -\infty$ or the limit $T \to -\infty$ in the calculation with the asymptotic fields above. However, the exponential damping factors which we discussed when you were here will take care of that problem. In a semiphysical way of talking this means that the coupling constant is not considered as an absolute constant but rather as a function which is varying very slowly (adiabatically) with time. This particular aspect of the problem does not enter in the calculation of the vacuum polarization as sketched here, but comes in the Lamb shift calculation. I do not think I will give the details of that here but refer you to Handbuch der Physik V_1 in particular pp. 290–306 and 316–323. The adiabatic variation of the coupling constant is of importance in connection with the renormalization of the mass on pp. 297–298.
>
> Of course, it is quite conceivable that a different cut off technique, in particular a non-covariant one, might lead to a different result both as far as the commutators are concerned and in the vacuum polarization calculation above. However, I would say that every cut off technique which leads to a non-vanishing photon self mass must be considered unreliable. I believe that commutators, etc. come out in the way I have sketched it above as soon as one uses a cut off technique which is both Lorentz invariant and gauge

invariant even if I admit that I have only verified this for the particular cut off studied above. I conclude from what you said when you were here both during your lecture and afterwards that the cut off technique which you have and which is, probably, non-covariant (?) actually does lead to a result different from what I have obtained here. For that reason I should be very much obliged to you if you could find the time to send me a short review of your calculation with particular emphasis on how the infinities are handled. If we could nail down the detailed difference between the two calculations, I feel that the situation has been considerably clarified.

Sincerely yours

Gunnar Källén

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Dirac-Källén Correspondence – The Second Letters

Dirac's Second Letter

In a letter dated 18 October 1966, Dirac answered Källén's first letter as follows:

Dear Källén,

Thank you for your long letter explaining your method in detail, and also the relativistic cut-off. At one time I worked a good deal with such cut-offs. As far as I know they do not apply to a logarithmic infinity. One can see the difficulty by a simple dimensional argument. One may introduce some procedure which makes $\int_0^\infty kdk$ count as zero and also $\int_0^\infty dk$ counts as zero. The corresponding result for the $log^{mic}\,\infty$ would be to count $\int_a^\infty \frac{dk}{k}$ as zero. Here we must have some lower limit $\underline{a}$, not zero, having the dimensions of frequency or energy. The result cannot be relativistic. It is just the $logar^{mic}\,\infty$ which comes into renormalization. So I believe one must resign oneself to a non-relativistic cut-off.

There is another difficulty with your treatment. Ingoing fields cannot be defined in general for electrodynamics. Consider the example of two electrons scattering one another with the Coulomb force as the main force between them. Then the waves representing the electrons do not tend to plane waves sufficiently rapidly with increasing distance between the electrons. The ingoing waves for the electrons have definite frequencies and directions, but not definite phases, so they are not completely definable. This is a difficulty that occurs only with the inverse square law of force, but it does stop one from building up a complete electrodynamics in terms of ingoing fields.

On page 18 of your letter near the bottom you say that with no incoming particles there may be particles created at $x_0 \to \infty$. Is this not ruled out by conservation of energy?

Yours sincerely

p a m Dirac

C. Jarlskog (Ed.), *Portrait of Gunnar Källén*, DOI 10.1007/978-3-319-00627-7_46,

Källén's Second Letter to Dirac

On 25 October, 1966 Källén answers Dirac's second letter (see the previous chapter). Here we reproduce this letter in its entirety (it is "only" 9 pages long and not 25 as was his previous letter). Källén treats Dirac as he did his students. He gives a lot of details to make sure that Dirac will be able to follow his arguments.

Dear Professor Dirac:

Thank you very much for your letter of October 18th. I shall try to answer your questions as well as I can.

First of all, I am completely at a loss to understand your remark that a relativistically invariant cut off technique is unable to handle logarithmic infinities. Let me, e.g., mention that in general, i.e., in a theory of particles with non-zero rest mass logarithmically divergent integrals are normally of the type

$$\int_0^\infty \frac{dk}{\sqrt{k^2+m^2}}.$$

Quite clearly, the mass of the particles acts as a low energy cut off in this connection and it is only the high energy behaviour which is really interesting. Consequently, trouble with the low energy limit comes only in electrodynamics and, possibly, in a neutrino theory. However, the low energy problem in electrodynamics is normally referred to as "the infrared problem" and is never a serious difficulty in a practical application. For instance, in the calculation of the Lamb shift it is essentially the binding energy of the electron which appears as an "effective photon mass" and makes the integral convergent at the lower limit. In other problems like the scattering of electrons by protons or, more generally, by external fields, it is the energy resolution of the detector which determines the effective low energy cut off. As a practical method of calculation it is often very convenient to consider electrodynamics as a limit of a theory with a small rest mass also for the photon. In that case, the artificial photon mass, which I normally call μ enters directly as a low energy cut off in the calculation of an integral like the one above but has to disappear in the final result. This effective vanishing of the small photon mass or, more precisely, the statement that the limit of the theory exists when the artificial photon mass goes to zero, provided all contributions to a given process are considered together, is reasonably easily verified in simple calculations like the Lamb shift etc. There is also a very large literature trying to prove a similar result more generally in n:th order perturbation theory. A standard reference in this connection is a paper by J. M. Jauch and F. Rohrlich in Helv. Phys.

Acta 27, 613 (1954). Historically, the first argument of this kind was given by F. Bloch and A. Nordsieck in the Phys. Rev. 52, 54 (1937). However, as I believe that none of us is particularly concerned about what happens in very high order perturbation theory calculations, let it be enough to mention here that the disappearance of the photon mass μ to the lowest order in the Lamb shift calculation and its replacement by the binding energy of the electron is discussed, e.g., in my Handbuch paper on pp. 318–320. The corresponding problem in the scattering of electrons by an external field is treated on pp. 307–309.

Also, your statement that the result of a cut off infinity cannot be relativistic is very difficult for me to comprehend. It is true that the low energy cut off must have the same dimension as a frequency or an energy but it could very well be, and is in the technique I am using, a mass which is an invariant quantity. Such a concept is perfectly relativistic.

Unfortunately, the paragraphs above contain many unsupported statements of a somewhat general nature. To substantiate my remarks a little bit I will illustrate once more the effect of the relativistically invariant cut off technique of Pauli and Villars on a standard problem containing a logarithmic infinity, viz. the self energy of the electron. For this purpose we consider again the equations of motion for the electron and photon fields

$$\psi(x) = \psi^{(in)}(x) - ie \int S_R(x-x')\gamma A(x')\psi(x')dx',$$
$$A_\mu(x) = A_\mu^{(in)}(x) + \frac{ie}{2}\int D_R(x-x')[\overline{\psi}(x'), \gamma_\mu\psi(x')]dx'.$$

Here I have dropped the letter m in $S_R(x-x', m)$ for simplicity and written D for a Δ-function with the photon mass μ

$$D_R(x-x') = \Delta_R(x-x', \mu^2).$$

Expanding in powers of e in the way described in my last letter we find for the electron field

$$\begin{aligned}\psi(x) = {} & \psi^{(in)}(x) - ie\int dx' S_R(x-x')\gamma_\lambda\psi^{(in)}(x')A_\lambda^{(in)}(x') - \\ & - e^2 \int\int dx'dx'' S_R(x-x')\gamma_\lambda S_R(x'-x'')\gamma_\rho\psi^{(in)}(x'')A_\rho^{(in)}(x'')A_\lambda^{(in)}(x') + \\ & + \frac{e^2}{2}\int\int dx'dx'' S_R(x-x')\gamma_\lambda\psi^{(in)}(x')D_R(x'-x'')\times \\ & \times[\overline{\psi}^{(in)}(x''), \gamma_\lambda\psi^{(in)}(x'')] + \dots\end{aligned}$$

where, I hope, the origin of the various terms is easily traced. Next, we calculate the particular matrix element of this field operator which we obtain by considering a transition from a one electron state to the vacuum. This gives

$$
\begin{aligned}
< 0 \mid \psi(x) \mid q > =& < 0 \mid \psi^{(in)}(x) \mid q > + \\
&+ \int dx' S_R(x-x') < 0 \mid \Phi(x') \mid q >, \\
< 0 \mid \Phi(x) \mid q > =& -e^2 \int dx' \{\gamma_\lambda S_R(x-x')\gamma_\rho \times \\
&\times < 0 \mid \psi^{(in)}(x') \mid q >< 0 \mid A_\rho^{(in)}(x') A_\lambda^{(in)}(x) \mid 0 > - \\
&- \gamma_\lambda < 0 \mid \psi^{(in)}(x) \overline{\psi}^{(in)}(x') \mid 0 > \gamma_\lambda \times \\
&\times < 0 \mid \psi^{(in)}(x') \mid q > D_R(x-x')\} \\
=& -e^2 \int dx' K(x-x') < 0 \mid \psi^{(in)}(x') \mid q >, \\
K(x-x') =& \gamma_\lambda S_R(x-x')\gamma_\rho < 0 \mid A_\rho^{(in)}(x') A_\lambda^{(in)}(x) \mid 0 > - \\
&- \gamma_\lambda < 0 \mid \psi^{(in)}(x) \overline{\psi}^{(in)}(x') \mid 0 > \gamma_\lambda D_R(x-x').
\end{aligned}
$$

The vacuum expectation values of products of incoming fields appearing in the kernel $K(x-x')$ can be directly evaluated. One finds

$$
\begin{aligned}
&< 0 \mid A_\rho^{(in)}(x') A_\lambda^{(in)}(x) \mid 0 > \equiv iD^{(+)}(x'-x)\delta_{\lambda\rho} = \\
&= \frac{1}{(2\pi)^3} \int dk e^{ik(x'-x)} \delta_{\lambda\rho} \delta(k^2+\mu^2)\Theta(k), \\
&< 0 \mid \psi^{(in)}(x) \overline{\psi}^{(in)}(x') \mid 0 > \equiv -iS^{(+)}(x-x') = \\
&= \frac{-1}{(2\pi)^3} \int dp e^{ip(x-x')} (i\gamma p - m)\delta(p^2+m^2)\Theta(p).
\end{aligned}
$$

In writing down the first of these formulae I have introduced the small artificial photon mass μ in the equation. Further, I have written the tensor appearing on the right hand side as $\delta_{\lambda\rho}$. This is exactly true in the Gupta-Bleuler gauge (cf., e.g., the Handbuch paper pp. 199–204, esp. Eq. (9.24)). In any other gauge which one might like to choose, there may be an extra term, essentially proportional to $k_\lambda\, k_\rho$. However, it is possible to verify that such an extra term is going to disappear in the result for the self mass obtained below. I will not discuss that point further – unless you explicitly ask me to do so in your next letter.

We can now write the kernel $K(x-x')$ as follows

$$K(x-x') = \frac{1}{(2\pi)^4}\int dQ e^{iQ(x-x')}K(Q),$$

$$K(Q) = \frac{1}{(2\pi)^3}\int dk\gamma_\lambda[i\gamma(Q+k)-m]\gamma_\lambda\times$$

$$\times\{\delta(k^2+\mu^2)\Theta(k)\{\frac{1}{[(Q+k)^2+m^2]_P}+$$

$$+ i\pi\epsilon(Q+k)\delta((Q+k)^2+m^2)\}+$$

$$+ \delta((Q+k)^2+m^2)\Theta(Q+k)\{\frac{1}{(k^2+\mu^2)_P}-$$

$$- i\pi\epsilon(k)\delta(k^2+\mu^2)\}\}.$$

The notation here is, I hope, clear. The letter P on the two denominators indicates principal value. The combination of principal values and δ-functions which appear in this expression, are the usual expressions which occur from the retarded properties of the solutions to the equations of motion. When this expression for $K(x-x')$ is substituted in the formula for the matrix element $< 0 \mid \Phi(x) \mid q >$ considerable simplifications occur. First of all, the integration over the variable x' yields a δ-function between Q and q. Further, when applied to the incoming electron field, the factor involving the γ-matrices can be simplified in the following way

$$\gamma_\lambda[i\gamma(q+k)-m]\gamma_\lambda < 0 \mid \psi^{(in)}(x) \mid q >$$

$$= -2[i\gamma(q+k)+2m] < 0 \mid \psi^{(in)}(x) \mid q >$$

$$= -2(i\gamma k+m) < 0 \mid \psi^{(in)}(x) \mid q > .$$

It follows

$$< 0 \mid \Phi(x) \mid q >$$

$$= \frac{e^2}{4\pi^3}\int dk\{\frac{\delta(k^2+\mu^2)\Theta(k)}{(k^2+2qk)_P} + \frac{\delta(k^2+2qk)\Theta(q+k)}{(k^2+\mu^2)_P}\}$$

$$\times (i\gamma k+m) < 0 \mid \psi^{(in)}(x) \mid q > .$$

In this expression I have dropped the terms containing a product of two δ-functions in momentum space. One finds very easily that the two expressions $k^2+\mu^2$ and $k^2+2\,qk$ cannot vanish simultaneously. Consequently, the prescription that the denominators should be interpreted as principle values is really unnecessary and I will drop it from now on. Next we note that the integration over the four dimensional vector k contains two characteristic expressions, viz.

$$I(q) = \int dk\{\frac{\delta(k^2+\mu^2)\Theta(k)}{(k^2+2qk)} + \frac{\delta(k^2+2qk)\Theta(q+k)}{(k^2+\mu^2)}\}$$

and

$$I_\lambda(q) = \int dkk_\lambda\{\frac{\delta(k^2+\mu^2)\Theta(k)}{(k^2+2qk)} + \frac{\delta(k^2+2qk)\Theta(q+k)}{(k^2+\mu^2)}\}.$$

The first of these integrals is a perfectly covariant expression. Consequently, it can depend only on the Lorentz square q^2 which is actually equal to $-m^2$. The second integral is a vector and, consequently, it must, for reasons of invariance, be proportional to q_λ. Therefore, we write

$$\begin{aligned} I_\lambda(q) &= I_1(q^2)q_\lambda, \\ I_1(q^2) &= \frac{1}{q^2}\int dkqk\{\ldots\} = \\ &= \frac{1}{-m^2}\int dk\frac{qk}{2qk-\mu^2}\{\delta(k^2+\mu^2)\Theta(k) - \delta(k^2+2qk)\Theta(q+k)\}. \end{aligned}$$

Introducing these notations in the expression for the quantity $\Phi(x)$ we get

$$\begin{aligned} <0 \mid \Phi(x) \mid q> &= \frac{e^2}{4\pi^3}\{I_1(-m^2)i\gamma q + mI(-m^2)\} < 0 \mid \psi^{(in)}(x) \mid q >= \\ &= \frac{me^2}{4\pi^3}[I(-m^2) - I_1(-m^2)] < 0 \mid \psi^{(in)}(x) \mid q > . \end{aligned}$$

It follows that the matrix element of the quantity $\Phi(x)$ is proportional to the corresponding matrix element of the incoming electron field. Actually, this proportionality constant is the self mass of the electron. This is perhaps most easily seen by applying the differential operator $\gamma\frac{\partial}{\partial x} + m$ from the electron equation of motion to the original expression where $\Phi(x)$ was introduced (cf. p. 3 in this letter)

$$\begin{aligned} &(\gamma\frac{\partial}{\partial x} + m) < 0 \mid \Phi(x) \mid q >= (\gamma\frac{\partial}{\partial x} + m) < 0 \mid \psi^{(in)}(x) \mid q > + \\ &\quad + \int(\gamma\frac{\partial}{\partial x} + m)S_R(x-x') < 0 \mid \Phi(x') \mid q > dx' \\ &= - < 0 \mid \Phi(x) \mid q >= \frac{-me^2}{4\pi^3}[I(-m^2) - I_1(-m^2)] < 0 \mid \psi^{(in)}(x) \mid q > \approx \\ &\quad \approx \frac{-me^2}{4\pi^3}[I(-m^2) - I_1(-m^2)] < 0 \mid \psi(x) \mid q > . \end{aligned}$$

The last approximation here takes into account the fact that in an e^2-term and to the accuracy we are working here, we are not obliged to make any distinction between the field $\psi(x)$ and the incoming field. Consequently, it follows that the real equation of motion for the vacuum to one particle matrix elements of the electron Heisenberg field can be written

$$[\gamma\frac{\partial}{\partial x} + m + \delta m] < 0 \mid \psi(x) \mid q >= 0$$
$$\delta m = \frac{e^2 m}{4\pi^3}[I(-m^2) - I_1(-m^2)].$$

If we now try a straight forward evaluation of the two integrals $I(-m^2)$ and $I_1(-m^2)$ we find, of course, the standard logarithmic divergence. Introducing here the Pauli-Villars cut off technique using, e.g., photons with heavy masses as artificial cut off particles we have

$$\delta m = \frac{e^2 m}{4\pi^3}\int dk \sum_i C_i^2[\delta(k^2+\mu_i^2)\Theta(k) - \delta(k^2+2qk)\Theta(q+k)] \times$$
$$\times [1+\frac{qk}{m^2}]\frac{1}{2qk-\mu_i^2}\,|_{q^2=-m^2}\;.$$

It is now a reasonably straight forward problem to calculate this integral and one finds

$$\delta m = m\frac{3\alpha}{2\pi}\{\sum_{i\neq 1} C_i^2 log\frac{m}{\mu_i} + \frac{1}{4}\}.$$

This is a finite result but, of course, dependent on the cut off parameters C_i^2 and μ_i. An essentially analogous situation happened in the vacuum polarization calculation with the charge renormalization which I displayed in my previous letter. On page 23 in that letter, one has a logarithmic infinity which is made into a finite expression by the cut off technique. In view of these two results I find your statement that a relativistic cut off cannot handle a logarithmic infinity completely bewildering. Consequently, I do not agree with your conclusion that one must resign oneself to a non-relativistic cut off technique.

It is, of course, quite clear that if one really wants to do electrodynamics with a photon mass which is exactly zero, one has the problem you mention in your letter about the asymptotic waves not being plane waves and, therefore, not being easily described in terms of standard free fields. However, I do not believe that this is a serious problem neither in practical calculations nor in principle. What one does is simply to consider electrodynamics as the limit of a theory with a small but finite photon mass. In such an approach the asymptotic fields evidently do exist before the limit $\mu \to 0$ is taken. The convenience of the finite photon mass is particularly noticeable in connections where one has to handle infrared divergences of the kind mentioned above on pp. 1 and 2. Further, it has been shown by many authors that it is possible to obtain normal electrodynamics including gauge invariance as the limit of a theory with a finite photon mass. As an example of papers proving this I should like to mention F. Coester, Phys. Rev. 83, 798 (1951) or R. J. Glauber, Progr. Theor. Phys. 9, 295 (1953). I am completely aware of the fact that other papers proving the same or similar results also exist. Personally, I particularly like the paper by Coester. In any case, I do not believe that the infrared problem is something to be taken seriously in this connection. I rather feel that it has very little to do with the high energy divergence problems in electrodynamics.

Finally, the answer to the last question in your letter.

The problem I treated in some detail in my last letter was concerned with the polarization of the vacuum in a given external c-number field. However, if the field is time dependent and, in particular, if it has a Fourier component with a "frequency" q fulfilling $-q^2 > 4\,m^2$ it can create real particles. This is mathematically expressed by the imaginary part of the vacuum polarization kernel I discussed in my last letter. Quite clearly, the creation of real particles by a time dependent external field does not in any way violate conservation of energy as the energy necessary to create the particles is supplied by the generator of the external field.

I sincerely believe that the way of calculating things that I have tried to explain in these two letters as well as in the encyclopedia article in Handbuch der Physik (not to mention the original papers) is quite consistent provided one is willing to accept the limiting procedures used. For details I should like to recommend the Handbuch paper for your serious attention. For clarity, I repeat the various limits here. They are

1. A relativistic cut off, e.g., à la Pauli-Villars.
2. An adiabatic exponential damping of the integrals at $x_0 = -\infty$ give a well-defined meaning to expressions which otherwise would be of the form 0:0.
3. A small mass for the photon to handle the infrared problem in a mathematically well-defined way.

As I said before, this technique does give the Lamb shift etc. in agreement with experiment and in agreement with what everybody else is doing. I am also of the opinion that to give a reasonably well-defined meaning to the very formal procedures that most authors are using in this connection, one has to make use of limiting procedures which may not be identical with but which have to be equivalent to the things I have tried to explain here. Therefore, I should very much like to find out exactly what it is you are doing in your calculation and where our procedures differ. Would it be too much trouble for you to send me a short review of the way you want to do the cut off etc.?

Sincerely yours

Gunnar Källén

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Dirac-Källén Correspondence – The Third Letters

Dirac's third letter, dated 16 November 1966 reads:

Dear Källén,

Thanks for your letter of Oct 25. I started to answer it at once and then noticed there were mistakes in what I was saying, so I have thought it over more carefully.

In the cut-off problem we are concerned with the integrals like

$$\int_0^\infty f(k)dk \qquad (1)$$

which converge for small k but diverge for large k. We may introduce some device which changes the integral into

$$\int_0^\infty f(k)cos(k\lambda)dk \qquad (2)$$

with some small parameter λ. If $f(k) \sim k$ for large k, the integral (2) converges with neglect of oscillations and goes to a definite limit as $\lambda \to 0$. If $f(k) \sim 1/k$ the integral (2) converges but does not go to a definite limit as $\lambda \to 0$. To get a value for (1) in this case one must introduce some constant with dimensions, and it is difficult to do this in a relativistic way.

I am not happy about the Pauli-Villars cut-off because of the imaginary C_i. It is necessary to have some general definition to fix which dynamical variables are real and it is necessary that the Hamiltonian should be real according to the definition. I do not see how to arrange this. I would not mind an indefinite metric in the Schrödinger picture, because Schrödinger picture is not very useful anyway, but the Heisenberg picture must be well grounded.

The other question is concerned with the use of ingoing fields, To make the ingoing fields definite one must introduce a finite rest-mass μ for the photon. μ must be extremely small and must not appear in the result of any practical

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calculation. All the important equations will not involve μ and one might as well formulate the whole theory without involving μ. This means avoiding the concept of ingoing fields. The ingoing fields require one to introduce something that is not of physical importance into the theory, namely μ, and I do not think it is worth while.

In my formulation I do not use ingoing fields and I put a cut-off in the interaction energy H_Q. This is of the form

$$H_Q = \int A^\mu j_\mu d^3x = 4\pi e \int \overline{\psi} \alpha_\mu \psi A^\mu d^3x.$$

Express $\overline{\psi}, \psi, A_\mu$ in terms of their Fourier components and let $E, E', |k|$ be the energies of the Fourier components. I then exclude the high-energy terms from H_Q. If there is a static field present (as one needs in applications to the Lamb shift and anomalous mag. moment) one uses eigenfunctions $\overline{\psi}, \psi$ in the static field instead of Fourier components, and E and E' are the corresponding eigenvalues. I considered three cut-offs.

Cut-off (A) $|k| < g$ where g is some large energy, of the order 10^9 volts.
Cut-off (B) $|E| + |E'| + |k| < 2g$.
Cut-off (C) $|E - eA_0| + |E' - eA_0| + |k| < 2g$ where A_0 is the static electric potential.

Cut-off (A) is the simplest, but the perturbation H_Q then gives an energy-change which cannot be compensated for in the absence of a static field simply by mass renormalization but cut-off (B) is satisfactory in the absence of a static field. The energy change can be compensated by mass renormalization. When there is a static field we must use cut-off (C) from considerations of gauge-invariance. This is the one used in my calculations of the Lamb shift and anomalous mag. moment. The result is a logical theory, starting from well-defined equations in agreement with the usual Heisenberg form of quantum mechanics, with the neglect only of small quantities.

Yours sincerely

p a m Dirac

Källén's Third Letter

Källén answers Dirac's above letter very quickly. In his letter dated 22 November 1966 he writes the following.

Dear Professor Dirac:

Thank you very much for your letter of November 16th. Thank you also very much for outlining your cut off procedure. However, I must confess that I do not understand the exact meaning of procedure c) which seems to be the one you are really using. The formula you give is

$$| E - eA_0 | + | E' - eA_0 | + | k | < 2g,$$

where A_0 is supposed to be the static electric potential. It is this point which I do not understand. Even if the potential is static it will, in general, depend on the space coordinate $\overline{x}$. Therefore, you must be using either the potential at a special point or some average of the potential, perhaps its Fourier transform? Could you find time to write to me an explanation of this point. Also, if I am not troubling you too much, I would like very much to see how an explicit calculation of some physical effect goes. As I believe the problem of gauge invariance is most acute in vacuum polarization, I would perhaps be most interested in that particular case. However, if that calculation is not available, anything you happen to have worked out in detail would be interesting for me.

As far as general philosophy is concerned, we seem to be back at the starting point. Perhaps you remember that at the beginning of our discussion I quite frankly said that the Pauli-Villars procedure contained an indefinite metric and, therefore, that one might object to it. At that time you said that you did not mind the indefinite metric as such. You seem to have changed your point of view now. Further, the discussion about the incoming field started off with your assertion that the commutation relations for incoming fields were necessarily complicated. I had, and still have, a different opinion on that point. You now assert in your letter that you feel that incoming fields are an extra complication and should be avoided. I presume this means that you do not want to discuss them any further and I do not know what you now think about their commutation relations. Actually, whether or not to use the incoming fields is a question of taste and my personal taste is that the incoming fields are very convenient for practical calculations and physically also extremely appealing. However, all this is philosophy and I don't know if there is any point in discussing questions of that kind. The real remaining problem is for me to understand your cut off procedure completely, and I would be grateful if you could help me with that.

Sincerely yours

Gunnar Källén

This concludes our report on the Dirac-Källén correspondence. There are no more letters exchanged between the two men in the Källén Collection. However, before closing this chapter, it is perhaps interesting to reproduce here below the opinions of a couple of the great masters in the field. After having found these letters in the Källén Collection, I sent a copy to Martinus Veltman, who gave, among other comments the following:

> "Thanks for the copies. In reading through them I once more was struck by the enormous change in field theory since those days. The 'hot' topics that were discussed in the letters have become more or less irrelevant.
>
> Other than that I noticed that there was not much real communication going on between the two. Källén largely rewrote his handbuch article (which was my start of going into field theory), while Dirac lived entirely in his own world."

Having mentioned the nature of these letters to Steven Weinberg, he immediately countered with the question:

> Did Dirac communicate with *anyone*?

Källén's Collaborators – a Preview

In the following few chapters, we introduce three of Källén's collaborators, with whom he published joint papers: Afaf Sabry, John S. Toll and Arthur S. Wightman and give some information about their interactions with him. He had a very extensive correspondence with them, especially with the last two.

Källén's other collaborators were Vladimir Glaser, Wolfgang Pauli and Hans Wilhelmsson. The relationship Källén-Pauli is described in much detail in several chapters in this book. We would have liked to write about Glaser and Wilhelmsson as well. Unfortunately, however, Glaser passed away in 1984 and Wilhelmsson was not well while I (CJ) was preparing this manuscript and passed away in 2011. Moreover, the Källén Collection does not give us any information about them.

The interested reader can find a biography of Glaser (1924–1984) in the March 1984 issue of "CERN Courier", which is available on the internet. Hans Wilhelmsson (1929–2011) [full name: Karl Hans Björner Wilhelmsson] who was a student of Källén, ended up as professor of electromagnetic field theory at Chalmers Institute of Technology in Gothenburg. Later on he went into plasma physics. In 1974 he was elected as a member of the Royal Swedish Academy of Sciences.

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Källén and Afaf Sabry

In a letter dated 9 July 1964, Källén writes to his collaborator Afaf Sabry at Department of Applied Mathematics, University of Assyout, Assyout, United Arab Republic:

> "Returning to Sweden at the beginning of this month I found your manuscript 'Quantum Mechanics and General Relativity' waiting for me. It has been a great pleasure to read it and see what kind of courses you give at Assyout. Personally, I feel that teaching at the level where these courses of yours are given is one of the most important tasks of a university. Especially, it is essential to have someone who is really interested in teaching and willing to put some time and effort into it. The enthusiasm of the lecturer is perhaps even more important than the actual material presented. Evidently, the university of Assyout is very lucky in this respect. … "

In his letter, as was his custom, Källén includes a list of specific comments on the manuscript of the above book. Källén had also earlier expressed his appreciation of Sabry explicitly in a letter to him from Bures-sur-Yvette in France on May 10, 1961. He wrote:

> "… We shall be glad to have you in Lund as long as you want to stay. … I am always glad to hear from you and am very much looking forward to seeing you when I return to Lund. Please don't hesitate to write to me any time you wish. … "

Källén's correspondence shows that he appreciated Sabry very much. Therefore, we found it appropriate to get a written account of Källén-Sabry relationship from Sabry himself. He kindly agreed to do so and his report is found in the next chapter. Here we would like to note that Källén and Sabry's joint paper "Fourth Order Vacuum Polarization", published in the journal of the Royal Danish Academy of Sciences (Mat. Fys. Medd. Dan. Vid. Selesk. 29, No. 17 (1955) 1) has become a classic work. It is used not only in particle physics but also in other areas, such as atomic physics and exotic atoms. It is a great pity that the ISI Web of Knowledge does *not* recognize this beauti-

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ful and useful work as a "publication" and does not list it. The Källén-Sabry paper appeared first as a CERN-55-17 report. It is electronically available on CERN's document server and has been reprinted in the Part 5 of this book.

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A. A. Sabry: How it Was to Work With Prof. G. Källén

I[1] first met Prof. Gunnar Källén, as he delivered a seminar in Max Planck in Göttingen, on his way back from Zürich to Lund. I then met him while he was active in Bohr's Institute in Copenhagen, while I was there during a study leave (August–Dec.1954). There, he helped me scientifically, from time to time to follow his work in Quantum Electrodynamics. During a subsequent visit to the Bohr's Institute in Copenhagen, summer 1955, Prof. Källén still helped me scientifically that ended in writing a common paper published in Math. Fys. Medd. (1955).

He was then invited to Cairo University, Egypt, 3 Jan. to 3 Feb. 1956, where he gave a series of lectures on: Selected Topics in Field Theory. During that period, accompanied with his wife (Mrs. Gunnel Källén) they visited many places in Cairo, and two days in Luxor. As I visited Lund, during the summer 1961, I continued collaborating with Prof. Källén, and he aided me in publishing a paper on Fourth order spectral functions. We were also aided in the university of Assyut, by professor Källén in raising the standard of our teaching courses in theoretical physics. During the summer 1964, after sending to him a manuscript of a whole book written by me to be published by the University on: Quantum Mechanics and General Relativity. He answered, in a long letter of seven pages (July 64) mentioning … I have great pleasure to read it and see what kind of course you give at Assyut … He gave many details and comments on the equations from p.1 to the end of the book. He was then invited to the University of Assyut. Coming back from India, he stopped in Cairo, and visited our University for three days (from 27 to 30 Dec.1965). He gave seminars on modern developments. Also he visited the Department of Physics and had different contacts with the teaching staff.

I remember that Prof. Källén told me that he acquired a small plane for two persons, and had maps of international air routes. I was a little scared, but I had confidence in his abilities. Later I knew that while flying with his wife in his small plane from Sweden to Geneve, he had an accident on landing near Hannover. I did not know whether it was a forced or intended landing. Later

[1] A. A. Sabry – current address: Ain Shams University, Cairo, Egypt

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I received a letter including an invitation to attend his funeral in S:t Olofs Kapell, Lund, on Nov.1, 1968 at 13pm. (he died on October 13, 1968). I could not assist.

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Källén and John Toll

Källén's emerges from his correspondence as a kind of scientific father to Sabry, a critical and sometimes aggressive loving scientific son of Pauli, and a tense competitive scientific brother of Wightman. With Toll, the situation was completely different. They collaborated as equals and yet Källén felt totally at ease in his company. Endowed with a great deal of social competence, Toll knew how to handle Källén.

John Sampson Toll[1] (called Johnny by Källén, and Toll's other friends) was Källén's closest scientific friend. Toll would invite Källén to visit him in USA, for as long as he wanted, and Källén would be glad to have him visit Lund.

On 28 July 1964, after having spent the spring term at Toll's department in Maryland, Källén writes to Toll:

> "Dear Johnny,
>
> First of all we all want to thank you for the wonderful time we have had in College Park this term. It has been a wonderful experience for the children to see the US on close distance this way. We hope they will keep their English alive and will do our best to help them. Gunnel[2] and I have also had great fun and by now I nearly feel that College Park is practically home!"

On Källén-Toll paper ([1960a]) in the Pauli Memorial Volume

In a letter dated 30 June 1959, Källén informs Toll that he has received a letter from Heitler[3] in Zürich asking him to contribute to a Pauli Memorial Volume,

[1] For a CV of Toll (1923–2011) see, for example, http://www.physics.umd.edu/people/faculty/cv/TollCV.pdf.
[2] Mrs Källén; for more information about her see chapter 29.
[3] Walter Heitler (1904–1981), a German physicist who was much respected and famous, among other things, for his book "The Quantum Theory of Radiation". Heitler was professor at the University of Zürich (1949–1974).

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to be published in the journal Helvetica Physica Acta. Källén continues as follows:

> "As you certainly know, I came in contact with Pauli at a rather early stage of my development. I have learned more from him than from anyone else and have always been one of his more ardent admirers. Therefore, I should like very much to have a paper in the HPA Pauli volume. However, there is the problem that I have very little to say. About the only suitable thing I can think of is our joint work on the Weil formula for the unpermutated domain. Therefore, I should like to ask what is your reaction to a joint paper of us two in that volume? I cannot possibly imagine that you should have any objection to publishing a paper in honor of Pauli (?) but there is, of course, the thing we have discussed before, viz. that this Weil formula is not very useful and does not seem to lead anywhere. However, in a volume of this kind everyone knows that you bring what you happen to have available and it is not quite the same as publishing something without external reason. Thus, I think we can be excused for this subject. What do you say? (Heitler even mentioned that the paper must not necessarily contain something new.) I think that the connection between the imaginary part of the Δ^+ function which is very similar for the two and the three point functions is very amusing and perhaps worth while mentioning."

John Toll as Described by Källén

Källén was approached by the Executive Vice Chancellor of the University of Irvine, California, to evaluate his collaborator John Toll. In a letter, dated 23 January 1965, he wrote:

> "Dear Dr. Peltason,
>
> Replying to your letter of January 18th concerning Professor John Toll at Maryland University, it is a pleasure to inform you that I have the highest regards and respects for the abilities of Professor Toll.
>
> As a scientist, he is especially well-known for having initiated much of the modern work in causality in theoretical physics. These developments have ultimately led to the whole field of dispersion relations and related subjects, which have been of the utmost importance both for theoretical and experimental physics. Much of these developments can be traced back to Professor Toll's original thesis and subsequent publications. Let me also add that during the last few years I have been in personal contact with Professor Toll and we have collaborated on a few research projects. It is not for me to evaluate the significance of this work but I can sincerely state that I have enjoyed working

with Professor Toll very much and that I personally think our efforts have not been quite futile.

Concerning Professor Toll as a teacher, I have had no real opportunity to observe him in that capacity. However, I have heard him give several seminar talks describing recent research work, both his own and reviewing what other people have done. His way of presentation is very lucid and clear and I think I can indirectly conclude that he must be a very good teacher. However, I also understand that in the position you are offering him, his actual teaching activities would be only a small portion of his total efforts.

Finally, there can be no doubt that the real merit of Professor Toll lies in his ability as an administrator. You certainly know as well as I do how he started essentially from nothing a little more than ten years ago to build up the physics department of Maryland university. Thanks to him, that department is now of a very good standard and has several first class physicists attached to it. These are external facts which everybody can observe for himself, and I may perhaps presume that this is one of your main reasons for considering him for your purposes. However, let me add a more personal comment, viz., that I have been visiting that department on several occasions during the last few years and that I have been struck by the very happy atmosphere and good spirit of collaboration which one finds in that department. This is certainly to a very large extent due to the personal ability of Professor Toll of collaborating with people and to create good working conditions for everybody. In short, I can recommend him in every respect, but especially as an administrator.

Let me only add, that I know that Professor Toll is in serious negotiations with another American university which also wants him. Therefore, if you consider him seriously, I should urge you to approach him very soon.

I hope these remarks will be of some help to you.

Sincerely yours

Gunnar Källén"

Källén's correspondence shows that he disliked exaggerations. Neither was he exaggerating in his letter quoted above. John Toll was quickly grabbed by "another American university", and became the first President of the State University of New York at Stony Brook. As president (1965–1978), he was highly successful. For more information, about John Toll see, for example, an article published about him by American Institute of Physics in their section on history (history matters).

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Källén and Arthur Wightman

Källén's correspondence shows that his relationship with Wightman[1] was somewhat "complex". Their letters, as expected, contain a large number of mathematical formulas and purely scientific discussions. But there is more in them. Källén shows a much broader spectrum, all the way from delightful friendship to bitter criticism and outburst of anger. Nonetheless, there can be no doubt that the two men respected and appreciated one another very much.

As an example, on January 27, 1964, Källén writes to Wightman who is visiting IHÉS [Institut des hautes études scientifiques; Institute for advanced scientific studies] in Paris, telling him all that he should have done but hasn't and taking for granted, in advance, that he isn't going to do anyway. The letter reads:

> "Dear Arthur:
>
> Some time ago I received a preprint of the paper by you and Lars[2] on 'Fields as Operator Value Distributions in Relativistic Quantum Theory'. Thank you very much for it.
>
> The reason why I am writing to you now is that I am particularly concerned about the part of your paper which discusses the asymptotic properties of the fields. As you know, I like the adiabatic technique and your comments about it on p. II.52 are, in my mind, somewhat unfair. One thing which is completely missing both on that page and also in the rest of your discussion is any mentioning of the reduction formulae. They are easily (and originally) obtained using adiabatic arguments. After all, from the practical point of view, these expressions are very important and, from my point of view, the real motivation for my being interested in vacuum expectation values at all. The representations we are trying to work out for these quantities using analyticity arguments and what else we can find, have as their ultimate goal (still, of course, speaking from my own point of view) the purpose of giving an insight into scattering amplitudes and off shell quantities. I think it is the great drawback of the Haag-Ruelle approach that it does not even come close to

[1] Wightman's chronology can be found on the homepage of "Array of Contemporary American Physicists". He introduces his own work and collaboration with Källén in Chap. 66 and 80.
[2] Lars alludes to Lars Gårding (born 1919), an eminent Swedish mathematician at Lund University.

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these reduction formulae! The LSZ-technique does give reduction formulae but I insist that that method is useless in one of the few cases which is of really practical importance, viz. quantum electrodynamics. (Incidentally, you never mention that either!)

I really do not know if this comment is going to influence you at all. I am essentially acting on the assumption that if I write nothing, nothing will be changed, and if I write something there is always the chance of your making small improvements. I guess you are aware of everything I have said above anyhow.

'Recently, we have been studying the Wu paper about the DANAD'. We are having great difficulties in understanding it and I am not sure that even the main result is correct. I have written a long letter to Wu about it but so recently, that I have not had time to get any response yet.

Preterea censeo[3]: You ought to have mentioned at least van Hove and possibly also Friedrich in connection with your 'Haag' theorem, e.g. in your footnote 41. However, I know that you will not change your opinion on that matter.

Lars is getting a copy of this letter.

Regards

P.S. This letter is a very tuned down version of my original draft which was so acid that even I hesitated to mail it off!"

The "adiabatic arguments" that Källén mentions above has to do with adiabatic switching on and off of the electric charge at the remote past and future respectively. He used to replace the electric charge e with $e\exp(-\epsilon|t|)$, where t denotes time and the limit $\epsilon \to 0$ is taken at the end of the computations.

Wightman described Källén's work in a talk given at a conference in Stockholm in 1980 (reproduced in Part 3, Chap. 66) as well as in connection with the 40th anniversary of IHÉS (see Chap. 80).

Giving Talks at Conferences and Schools The following chapters give an account of Källén's participation in a selection of conferences and schools. The reader will find him:

- As the only representative from the West at an exclusive meeting in Moscow 1955, where he meets Landau and other eminent Soviet theorists. He comes back to his home institution, CERN in Copenhagen, bringing along papers

[3] This comes from a famous Latin statement [Et preterea censeo Carthago delenda est]. Here it means "Moreover I think that"

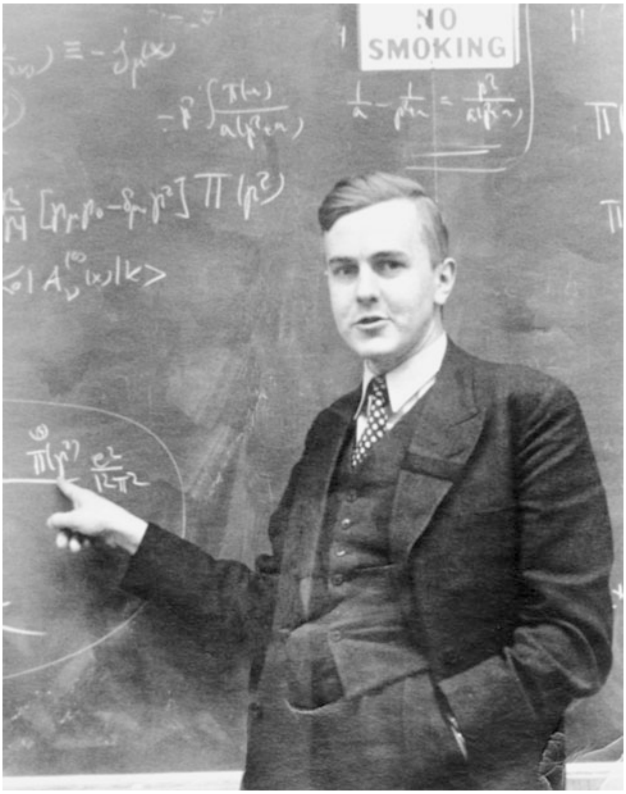

Figure 51.1 Källén lecturing and pointing at one of his beloved weight-functions

by them to be translated from Russian and thus be known to the scientists in the West.

- Giving a talk at an international conference at CERN in 1956 trying to convince his audience of his results (from 1953) that quantum electrodynamics, by itself, is an inconsistent theory and to explain to them the difference between his approach and Landau's.
- Giving lectures and "fighting" at Schaldming Schools in Austria in 1960's.
- Giving a talk as well as answering questions, from a very distinguished audience, at the 1961 Solvay Conference in Brussels.

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At 1955 Moscow Meeting

At the end of a letter[1] to Pauli, dated 18 January 1955, Källén writes (translated from German):

> Finally, I would like to ask you something very different, that has nothing to do with our work. Have you by any chance been invited to Moscow? Myself, I have received an invitation to a conference from 31 March to 6 April.[2] But I am somewhat puzzled that, for example, Møller has not been invited, in spite of the fact that the conference, according to its preliminary program, will also deal with non-local theories[3]. Under no circumstances, do I wish to participate in a communistic manifestation against "western physics" or some similar action. Do you know anything about this conference?

On the 21 January Pauli answers. His letter ends with (translated from German):

> The Devil is behind the indefinite metric[4]. Is he also behind the conference in Moscow? I hear from you, for the first time, about this conference and so far I have *not* been invited.
>
> It *could* be very nice over there, but it could *also* suddenly be decided to have an unpleasant revolution, and then you are a part of it!

It turned out that the organizers had only invited young theorists from abroad. Finally, Källén did go to the conference in Moscow and upon his return home, at the end of a long letter dated 19 April 1955, he gave the following report to Pauli (translated from German):

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[1] The main body of the letter is about the indefinite metric and about the Källén-Pauli work on the Lee Model, paper [1955b].

[2] This was about two years after the death of Joseph Stalin (1878–1953), the much feared head of state in Soviet Union.

[3] Christian Møller, the Director of CERN Theoretical Study Group, had worked in this field, together with Poul (called Pablo) Kristensen. At the time Källén belonged to this group as a staff member.

[4] Pauli, on several occasions in his correspondence with Källén, declares that he loathes the indefinite metric.

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> In Moscow I had a very nice time without any political complications. (The *usual* life in Moscow is probably not so pleasant but that is a different matter.) During the conference a very large number of talks were given (I talked about our work on the Lee Model.) Most of the talks were not so interesting, but I would like to tell you that *Landau* and his students have also the opinion that the renormalized charge in a consistent theory must be zero. They have reached this conclusion through a kind of Tamm-Dancoff approximation. Their results have been partially published – but unfortunately in Russian, whereby they are incomprehensible to me {Doklady Akademii Nauk SSSR **95**, 497 (1954); ibid **96**, 773 (1954); ibid **95**, 1177 (1954); **96**, 261 (1954)}. I will now see to it that they are translated. For the time being it seems to me however, that the most important point of the calculation is the integral equation (1) in the first paper. If one believes this equation, the rest follows automatically. I have not yet quite understood the justification for this equation. At least, in discussions, they half-admitted to me that this equation is *not* mathematically well founded but is due to "the physical feeling". How one, in an *inconsistent* theory, can argue at all, based on feeling, is difficult for me to understand, but perhaps I should wait with my final judgement until I have actually studied the papers.

Indeed Källén came back, from behind the "iron curtain" and this somewhat strange meeting. He had been the only participant from Western Europe! He imported to the West papers by Lev Landau and collaborators. These were translated into English, in Copenhagen, and thus got known to scientists in the rest of the world. Pauli mentioned several times in his letters that Landau had come upon the same result as he himself had found. Källén, as we have seen here above, remained sceptical.

A short account of the above meeting has been given by one of the local participants, D. V. Shirkov who writes (see D. V. Shirkov, 1994 Russ. Math. Surv. 49:5 (1994) 155):

> "At the above-mentioned conference at the Institute of Physics Gunnar Källén presented a paper written in collaboration with Pauli on the so-called "Lie [should read Lee] model", the exact solution of which contained an illusory pole (which, in contrast to the physical one corresponding to a bound state, had negative residuum) in the nucleon propagator. Källén and Pauli's analysis led to the conclusion that the Lie [Lee] model is physically void.
>
> In view of the result on the presence of a similar pole in the photon propagator in QED (which follows from the solution of Landau's group as well as an independent analysis by Fradkin) obtained a little later in Moscow, Källén's report resulted in a heated discussion on the possible inconsistency of QED. I remember particularly well a scene by a blackboard on which Källén

> was presenting an example of a series converging non-uniformly with respect to a parameter (the terms of the series being dependent on the parameter) to support the claim that no rigorous conclusion about the properties of an infinite sum can be drawn from the analysis of finitely many terms.
>
> The parties left without convincing one another and before long a publication by Landau and Pomeranchuk appeared presenting an argument that not just quantum electrodynamics, but also local quantum field theory is self-contradictory.
>
> Without going into details, let me remark that the analysis of this problem carried out by N.N. [stands for Nikolai Nikolaevich Bogolyubov] with the aid of the renormalization group formalism just developed by himself led to the conclusion that such a claim cannot have the status of a rigorous result, independent of perturbation theory. Nevertheless, like Källén's arguments, our work also failed to convince the opponents. It is well known that Isaak Yakovlevich Pomeranchuk even closed his quantum field theory seminar shortly after these events."

Finally, this is what Pauli had to say about the work by Landau and collaborators, in a letter ([2076] in the Pauli Collection) to Källén dated 24 April 1955 (translated from German):

> … I am glad that you have returned home [from Moscow] …
>
> In the meanwhile I have read his [Landau's] summary article "The Quantum Theory of Fields" written for the Bohr Festschrift (Attention: to be kept secret from Bohr!). In it, the four Russian articles that you brought [from Moscow] are cited (please tell me more when you have read their translations). It was for me gratifying to see that he hits upon the same conjecture as I, concerning quantum electrodynamics – (His idea/attempt to relate the cutoff scale to gravity is amusing.)
>
> From what I have read in the aforementioned article, however, it is likely that he has no more of a proof of this conjecture than I have.

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The 1956 CERN Symposium

CERN[1], currently the largest organization in the world for particle physics, was founded in 1954. Originally located in Meyrin, at the outskirts of the city of Geneva in Switzerland, it has with time extended into neighboring France.

The Theoretical Study Division of CERN, however, was created already in 1952, i.e., *before* the official inauguration of CERN. It was situated in Copenhagen. Christian Møller [1] was appointed (part-time) as the Director and there were two full time senior staff members, Gunnar Källén and Ben R. Mottelson[2].

While constructing buildings and accelerators were in progress, an international conference was organized by CERN in the city of Geneva. This "CERN Symposium on High Energy Accelerators and Pion Physics", 11–23 June 1956, attracted about 250 participants from outside CERN, among them at least 18 Nobel Laureates or future Laureates. Unfortunately, the participants from CERN are not listed in the Proceedings [2].

The conference focused on measuring devices such as bubble chambers, track chambers as well as counter techniques. These turned out to be essential for many future discoveries. Physics-wise, as the title indicated, the conference was dominated by pions.

The most exciting news of the Symposium was an announcement made by Pauli [3] stating that he had just received a telegram from Fred Reines and Clyde Cowan (Los Alamos) informing him:

> "We are happy to inform you that we have definitely detected neutrinos ..."

Pauli added:

> "I make this announcement because otherwise everybody would ask me separately."

[1] For history of CERN, see http://public.web.cern.ch/public/en/About/History-en.html. See also Chap. 6 in Part 1.
[2] Ben Mottelson was awarded a Nobel Prize in 1975. Hence his autobiography is easily accessible on internet.

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When the conference closed on 23 June the participants were, of course, not aware that something fantastic had happened the day before, not in Geneva but on the other side of the Atlantic Ocean. On 22 June, Physical Review had received a paper, later published under the title

"Question of Parity Conservation in Weak Interactions"

the authors being: T. D. Lee and C. N. Yang. This marked the beginning of a new era in physics. Already in the following year Lee and Yang were awarded the 1957 Nobel Prize in Physics [4].

Returning to the CERN Symposium, it offered a few theoretical talks. One of them [5] was given by the 30 year old Källén, from CERN in Copenhagen. The chairman of his session was W. Heitler, from University of Zürich, and the scientific secretary Bernard d'Espagnat (CERN).

Källén's talk is presented here below. The talk also addresses what we nowadays call "asymptotic slavery" and where does it set in. Källén describes the difference between his approach (vacuum polarization) and that of the Landau school (vertex function). It is a beautiful talk. However, it must have been very tough for all those experts on detectors, accelerators, and pion physics as well as almost all theorists who attended the conference, to follow his arguments. The reader is invited to judge for herself/himself.

Plenary Talk at CERN Symposium 1956

In his Plenary talk Källén addresses the question of internal consistency of quantum electrodynamics (QED) upon renormalization. He, together with Pauli, had studied what he calls "the very beautiful model of a renormalizable field theory constructed by T. D. Lee", looking for some clues that perhaps could help resolve this issue. They had found that the S-matrix of the Lee Model is non-unitary. However, that result was not necessarily applicable to QED as the structures of the two models are very different. Källén presents his arguments for why he believes that even quantum electrodynamics is an *inconsistent* theory. His arguments are physical rather than mathematical. Therefore, he notes:

> "To avoid misunderstandings we want to emphasize from the beginning that the ideas put forward below are not supposed to constitute a mathematical proof but only serve as a basis for further discussions and future investigations."

In fact this issue was on his mind as long as he lived. He gave a series of lectures about it at the 4th Schladming Winter School in Austria, 1965 (paper [1965a] on his list of publications in Part 5 of this book; see also Chap. 56). In his talk Källén also explains the difference between his approach and that of Landau and collaborators who also had found that quantum electrodynamics is inconsistent, but in a different way (see after his Eq. (17) of the article here below).

References

1. See also Christian Møller's reminiscences in Chap. 65
2. Proceedings of CERN Symposium on High Energy Accelerators and Pion Physics (CERN 56-26, edited by A. Citron et al.)
3. W. Pauli, *ibid* p. 258
4. See the Nobel Lectures by T. D. Lee and C. N. Yang. These are found online at the site http://nobelprize.org/; reprinted from Nobel Lectures, Physics 1942–1962, Elsevier Publishing Company, Amsterdam, 1964
5. G. Källén, Ref. [2] p. 187

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G. Källén: On the Mathematical Consistency of Quantum Electrodynamics

CERN, Copenhagen

In spite of the very great success of renormalized quantum electrodynamics in accounting for several observable effects with extremely high accuracy it is nevertheless more or less generally recognized that we cannot be sure of the internal consistency of the mathematical machinery used in this connection. Hereby, we are not referring to the fact that finite, observable quantities can only be obtained after a somewhat optimistic handling of infinite expressions but, rather, that even if this "wishful mathematics" is accepted, there are still some doubts left as to the consistency of the *resulting* theory. That this is a non-trivial problem has recently been emphasized by the very beautiful model of a renormalizable field theory constructed by T. D. Lee [1]–[2]. This model can be solved explicitly to a certain extent, and it turns out that even if the renormalization programme can be performed without difficulty and does eliminate all the infinite quantities in the model, the resulting theory has a non-unitary S-matrix and is, therefore, unphysical. The question whether or not a similar breakdown occurs in quantum electrodynamics or in any meson theory is very difficult to answer and no very definite argument has been presented so far either way. However, if one tries to copy the exact mathematical argument, which led to the breakdown in the Lee model, one finds that the structure of e.g. quantum electrodynamics where all fields are renormalized is no [should read so] different from the structure of the model that there is no real justification for the often expressed conjecture that quantum electrodynamics suffers from the same inconsistency as the Lee model. The analysis referred to here is given in [3]. It, therefore, does not seem to be very fruitful to tackle the consistency problem of quantum electrodynamics in this way. In what follows, we shall instead try to present a heuristic argument where we are also led to an inconsistency in quantum electrodynamics but of entirely different nature from the inconsistency in the Lee model, and where we obtain it from an entirely different starting point and after an entirely different argument. To avoid mis-

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understandings we want to emphasize from the beginning that the ideas put forward below are not supposed to constitute a mathematical proof but only to serve as a basis for further discussions and future investigations.

One question to be answered in connection with a discussion of the consistency of renormalized quantum electrodynamics is whether or not the well-known infinities obtained in perturbation theory are really inherent in the formalism or if they are only caused by an unjustified use of expansions in the coupling constant e. It has been known for some time that the first alternative is, indeed, correct [4], and because of the argument to follow below is very intimately connected with the proof of this statement, we shall begin with a very short résumé of that proof. The fundamental quantity is a certain function $\pi(p^2)$, defined by the equation

$$\pi(p^2) = \frac{V}{-3p^2} \sum_{p^{(z)}=p} < 0 \mid j_\mu \mid z >< z \mid j_\mu \mid 0 > . \tag{1}$$

Here j_μ is the renormalized current operator, V the volume of periodicity, and the summation is performed over all states $\mid z >$ where the total energy momentum vector is equal to p. The function $\pi(p^2)$ appears in two ways in the formalism. First of all the integral

$$\overline{\pi}(0) = \int_0^\infty \frac{\pi(-a)da}{a} \tag{2}$$

is, roughly speaking, the charge renormalization factor while the integrals of the form

$$\overline{\pi}(p^2) - \overline{\pi}(0) = -P \int_0^\infty \frac{\pi(-a)da}{a(1+\frac{a}{p^2})} \tag{3}$$

appear in observable expressions. If the theory is consistent and finite after renormalization, the integral (3) must be convergent, but the integral (2) might very well diverge. (Proofs of these statements are given in [5]. Similar arguments have later been given by H. Lehmann [6] and M. Gell-Mann and F. Low [7].) Further, it must be emphasized that the sum over intermediate states in (1) contains only a finite number of terms. Therefore, the function $\pi(p^2)$ is trivially finite if only the renormalized current operator exists. The only point where infinite quantities can enter into the calculation is then in the integrals (2) and (3), and the whole question of the convergence of the theory has been reduced to a discussion of the behaviour of the function $\pi(p^2)$ for large values of $-p^2$. [Here Källén adds a footnote.][1] We now get a lower limit to the function $\pi(p^2)$ by considering only a few of the terms in the definition (1) Especially, we may write[2]

$$\pi(p^2) > \frac{V}{-3p^2} \sum_{q+q'=p} < 0 \mid j_\mu \mid q, q' >< q', q \mid j_\mu \mid 0 > . \tag{4}$$

Quite generally the states with one (incoming) pair used in the estimate (4) give matrix elements of the current operator of the form

$$\begin{aligned} &< 0 \mid j_\mu \mid q, q' >= \\ &\quad = [1 + \overline{F}(Q^2) - \overline{F}(0) + i\pi F(Q^2)] < 0 \mid j_\mu^{(0)} \mid q, q' > + \\ &\qquad + [\overline{S}(Q^2) + i\pi S(Q^2)] < 0 \mid m_{\mu\nu}^{(0)} \mid q, q' > \end{aligned} \tag{5}$$

$$Q = q + q'. \tag{5a}$$

The quantities $j_\mu^{(0)}$ and $m_{\mu\nu}^{(0)}$ are respectively the current and the magnetic moment of the incoming free particles in the state $\mid q, q' >$. The expression (5) follows from very general arguments of Lorentz invariance, and the relation between the real and imaginary parts of square brackets are again given by integral transformations of the "causal" type (3). The constant $\overline{F}(0)$ in the first bracket in (5) is due to renormalization performed and can be expressed in terms of the renormalization constant of the Dirac field and the charge renormalization constant. If we now make the explicit assumption that not only the observable quantities in the formalism but also the two renormalizations just mentioned are finite, that means e.g. that the integral $\overline{F}(0)$ is assumed to be

[1] Actually, more than one function of this kind enters into the formalism, but it will not be necessary for us to discuss all of them here. The appearance of the integration over the variable a in (2) and (3) is a consequence of the causal structure of the theory which says that certain expressions must be boundary values of analytic functions regular everywhere except on the negative, real axis. The function $\overline{\pi}(p^2) - \overline{\pi}(0)$ is the real part of such an expression while the function $\pi(p^2)$ itself is the imaginary part. Arguments involving integral transforms of this kind have recently been used e.g., by M. L. Goldberger [8], and others.

[2] The statement that we get a lower limit in this way is, of course, connected with the positive definite character of the terms of the right hand side of (1). Due to antihermitian properties of the fourth component of the current operator and the indefinite metric conventionally used for the treatment of scalar and longitudinal photons, this is not an entirely trivial statement but can nevertheless be shown to hold. See [5].

convergent. Under these circumstances it is intuitively clear that

$$\lim_{-Q^2\to\infty}[\overline{F}(Q^2)-\overline{F}(0)] = -\lim_{-Q^2\to\infty} P\int_0^\infty \frac{F(-a)da}{a(1+\frac{a}{Q^2})} = \\ = -\int_0^\infty \frac{F(-a)da}{a} = -\overline{F}(0) \tag{6}$$

and therefore[3]

$$\lim_{-Q^2\to\infty} \overline{F}(Q^2) = 0. \tag{7}$$

The constant $\overline{S}(0)$ is the anomalous magnetic moment of the electron in a slowly varying external field and must therefore be finite. It follows as above (7) that,

$$\lim_{-Q^2\to\infty} \overline{S}(a^2) = 0. \tag{8}$$

We therefore have

$$\lim_{-Q^2\to\infty} < 0 \mid j_\mu \mid q, q' >= [1-\overline{F}(0)] < 0 \mid j_\mu \mid q, q' > . \tag{9}$$

In other words, for high energies the matrix element of the current operator is, apart from a constant factor, equal to the Born approximation for the corresponding transition. As stated above, the constant factor involves the renormalization constants, and a closer investigation of it shows that it exactly converts the operators in the definition of the current to the unrenormalized operators and at the same time changes the physical charge to the bare charge. We therefore find that at high energies and for the special transition studied in (5) the matrix element for the unrenormalized current is equal to its Born approximation. We can then use this estimate in (4) to show that the exact function $\pi(p^2)$ is larger than a certain constant at high energies thus contradicting the starting assumption that, among other things, the integral (2) was

[3] Actually, these arguments are not very satisfactory from the point of view of rigorous mathematics, and counter examples can easily be given. However, the important point is not that the functions $\overline{F}(Q^2)$ etc. really vanish for large values of $-Q^2$ but rather that integrals like

$$\int^\infty \frac{da}{a}[\{1+\overline{F}(-a)-\overline{F}(0)\}^2+\pi^2F^2(-a)]$$

do *not* converge. This can be shown under rather general assumptions about the function $F(Q^2)$ provided the integral $\overline{F}(0)$ is convergent, and if $1-\overline{F}(0)\neq 0$. See [2].

convergent. This proves that at least one renormalization constant must be infinite.

As we have seen, there are two main points in the argument above, viz. first the positive definiteness of the terms in the right hand side of (4) and second the proof that the matrix element for the transition (5) of the unrenormalized current operator is given by the Born approximation at high energies. The last result is in general agreement with the intuitive feeling that the Born approximation becomes good at high energies or, in other words, that at high energies there is very little interaction between the particles. The small residual interaction that is left over can be computed by perturbation theory, and if the theory is consistent this should be a small effect. If we really do this computation, we find that the effect is not small, and this is – roughly speaking – the way in which we find contradiction. However, it must be remembered that the argument above was based on the explicit assumption that the unrenormalized quantities do exist, and we first have to investigate how the result is modified if we only make the weaker assumption that the integrals like (3) but not (2) are convergent. In that case, the difference (3) will have no limit for large values of $-p^2$ and the term $\overline{\pi}(0)$ is therefore replaced by an increasing function of $-p^2$. Physically this means that if there is no bare charge inside the cloud of virtual particles surrounding the physical electron, another high energy particle approaching the first one will penetrate deeper and deeper into the cloud of the virtual particles but never find any "bare core" however high the energy might be. For our purpose, this is not very serious obstacle to the argument as we can always replace the *increasing* function we have by a constant and thereby still get a lower limit of the absolute value of the matrix element. However, there is also another factor involved in (9) which changes the Dirac field of the electron to the corresponding unrenormalized operator. This is the inverse of a factor of the same general nature as the charge renormalization factor, i.e. a quantity which must be expected to go to zero for very high energies. Therefore we write

$$\lim_{-Q^2\to\infty} < 0 \mid j_\mu \mid q, q' >= e(a^2)/e.N(Q^2) < 0 \mid j_\mu^{(0)} \mid q, q' > . \qquad (10)$$

Here, $e(Q^2)$ goes to infinity for large values of $-Q^2$ and represents the energy dependent "bare charge" while $N(Q^2)$ goes to zero for high energies. We can further make the observation that $e(Q^2)$ does not increase faster than const. $(-Q^2)$. This follows immediately from the definition of this function in (3) and the assumption that (3) does exist. If we neglect the indefinite metric in the Hilbert space, a similar expression can be given for the inverse of the function $N(Q^2)$, and we would conclude that this function does not vanish faster than $1/Q^2$. In reality the scalar and longitudinal photons make such an argument somewhat unreliable for a non-gauge invariant quantities (like the Dirac field), and the indefinite metric might possibly change the positive definiteness of the function involved. Actually, the positive definiteness of the weight function is sufficient but far from necessary to insure this behaviour at high energies for the function involved. If only some integral of the form

$$I_n = \int^{\infty} G(-a)a^n da; n = 0, 1, 2, \ldots \qquad (11)$$
$$G = \text{weight function,}$$

is different from zero, we can conclude that $N(Q^2)$ does not vanish faster than a power of the argument. The assumption that the indefinite metric should change the function $G(-a)$ to such a wildly oscillating function that all the integrals (11) would be convergent and zero is hardly realistic. In what follows we are therefore going to neglect this difficulty and assume that $N(Q^2)$ does not vanish faster than some power of Q^{-2} for high energies. This is one respect in which our argument is incomplete.

We are now ready for the main step in our analysis which is the assumption that the result with the Born approximation holds not only for the special transitions in Eq. (5) but for a transition from the vacuum to any state $| z >$. In other words, we put down as a guess

$$\lim_{-Q^2\to\infty} < 0 \mid j_\mu \mid z >= [\frac{e(Q^2)}{e}]^n N(Q^2) < 0 \mid j_\mu^{(B)} \mid z > . \qquad (12)$$

Here $j_\mu^{(B)}$ is the Born approximation i.e. the first nonvanishing expression obtained in perturbation theory for the transition studied, and n is the corresponding order of perturbation theory. A few words about the domain of validity to be expected from the estimates (12) are certainly appropriate. It is e.g. not to be expected that the asymptotic forms of the matrix elements are reached at the same energy for every transition. Rather, one must assume that the system behaves as a system of free particles when the energy of each particle is above a certain critical energy E_0. Hence, we expect

$$< 0 \mid j_\mu \mid z >\approx [\frac{e(Q^2)}{e}]^n N(Q^2) < 0 \mid j_\mu^{(B)} \mid q, q' > \qquad (12a)$$

if

$$- Q^2 = -(\sum_i^{\nu} p^{(i)})^2 >> \nu^2 E_0^2, \qquad (12b)$$

where ν is the number of (incoming) particles in the state $| z >$.

If we now accept the two assumptions (12a) and (12b), we can proceed in the following manner. A lower bound to the function $\pi(p^2)$ in (1) is obtained if we sum over all states with $3, 5, 7 \ldots \overline{N}$ (incoming) photons. (The contribution from states with an even number of photons is zero due to the charge symmetry of the theory.) Using (12a) we get for high energies

$$\pi(p^2) > N^2(p^2) \sum_{\nu=1,3,5,\ldots}^{\overline{N}} [\frac{\alpha(p^2)}{\alpha}]^\nu \frac{V}{-3p^2} \times \\ \times \sum_{k_1+\ldots+k_\nu=p} < 0 \mid j_\mu^{(B)} \mid k_1, \ldots k_\nu >< k_\nu, \ldots k_1 \mid j_\mu^{(B)} \mid 0 >, \tag{13}$$

$$\alpha(p^2) = \alpha[\{1 - \overline{\pi}(p^2) + \overline{\pi}(0)\}^2 + \overline{\pi}^2 \pi^2(p^2)]^{1/2}. \tag{13a}$$

As we are using (12a) the upper limit $\overline{N}$ in the sum must not be larger than

$$\overline{N} << \sqrt{-p^2/E^2}. \tag{13b}$$

The sum over the vectors k in (13) gives essentially the perturbation theory probability for the emission of ν photons in a system with a weak external field. This probability is a Poisson distribution, where the mean number of photons is proportional to the square of the logarithm of the total energy available [9]. (We are using his general result Eq. (101), dropping inessential factors like $(\pi/2)^\nu$ and putting $\epsilon_{\max} \approx E, \epsilon_0 \approx m.$)

Therefore

$$\frac{V}{-3p^2} \sum_{k_1+\ldots+k_\nu=p} < 0 \mid j_\mu^{(B)} \mid k_1, \ldots k_\nu >< k_\nu, \ldots k_1 \mid j_\mu^{(B)} \mid 0 >= \\ = C\frac{\alpha^\nu}{\nu!}[log\frac{-p^2}{m^2}]^{2\nu} \tag{14}$$

or

$$\pi(p^2) > CN^2(p^2) \sum_{\nu=1,3,\ldots}^{\overline{N}} \frac{\alpha^\nu(p^2)}{\nu!}[log\frac{-p^2}{m^2}]^{2\nu}. \tag{15}$$

If $\alpha(p^2)$ behaves as $(-p^2/m^2)^\epsilon$ $(0 < \epsilon < 1)$ for large values of $-p^2$, we can choose $\overline{N}$ fixed (i.e. independent of the energy $-p^2$) and sufficiently large to make the integral (3) divergent. Therefore $\alpha(p^2)$ must increase slower than any power of $-p^2$.

The remaining series in (15) is the series for

$$sinh[\alpha\, log^2(-p^2/m^2)]$$

except for the fact that the upper limit $\overline{N}$ cannot be made arbitrarily large as it would then violate (13b) and thereby make the use of the estimate (12a) impossible. However, it is well known that the main contribution to the exponentials series and therefore also to the series *sinhx* comes from terms where ν is of the same order of magnitude as x. If only $\overline{N}$ fulfills[4]

$$\overline{N} >> \alpha(p^2) log^2(\frac{-p^2}{m^2}), \tag{16}$$

a condition which is easily reconciled with (13b) for large energies, we get

$$\begin{aligned} \pi(p^2) &> CN^2(p^2) sinh[\alpha(p^2) log^2(\frac{-p^2}{m^2})] \\ &\approx \frac{C}{2} N^2(p^2) e^{\alpha(p^2) log^2(\frac{-p^2}{m^2})} = \frac{C}{2} N^2(p^2)(\frac{-p^2}{m^2})^{\alpha(p^2) log(\frac{-p^2}{m^2})}. \end{aligned} \tag{17}$$

According to our assumptions above, the function $N(p^2)$ does not vanish faster than some power of p^2, and it follows that the function $\pi(p^2)$ increases faster than any power of p^2, thereby contradicting the assumption that the integral (3) is convergent. This would be a definite inconsistency in the formalism.

The asymptotic form (17) is widely different from the results obtained by Landau and collaborators [10] for similar expressions, and few words about the way this difference comes about might be of some interest. Both arguments make a very essential use of perturbation theory results but in rather different ways. A very important point in the calculation by Landau et al. is

[4] The "epsilontic" of this statement is the following:

$$| e^x - 1 - x - \ldots - \frac{x^{\nu-1}}{(\nu-1)!} | < \frac{x^\nu}{\nu!} e^x < (\frac{xe}{\nu})^\nu e^x.$$

Let us take e.g.

$$\nu = c.x.$$

It follows that

$$(\frac{xe}{\nu})^\nu e^x = (\frac{e}{c})^{cx} e^x = e^{x[1+c(1-logc)]}.$$

If we therefore take c so large that $1 + c(1 - logc) < 0$, the absolute value of the difference above will go to zero for large values of x.

the expansion of a certain function (the so-called "vertex function") in powers of the coupling constant together with the proof that each coefficient in this expansion has a certain simple limiting form for high energies. It is then argued that the asymptotic form of the sum is equal to the sum of the asymptotic forms of the coefficients and the rest of the calculation is based on this assumption. As already said, the result obtained in this way is very different from (17). Even if our calculation is based on another unproved assumption viz. the Born approximation result (12a) and therefore cannot claim to be very rigorous, it nevertheless shows one physical feature in the formalism which is not included in the argument by Landau et al. As the energy is increased, more and more processes become energetically possible, and it is not to be expected that the corresponding terms in a perturbation theory expansion reach their asymptotic forms until the energy is well above the highest threshold allowed in that order. Therefore, if the energy is kept fixed and higher and higher orders of perturbation theory are included, one will soon get contributions from terms that are at, or only slightly above, their thresholds and for these contributions the asymptotic forms are certainly not reliable. This might also explain the somewhat startling result that one sums what is believed to be the most important part of a presumably divergent series and gets a convergent result! If our calculation is examined, it appears that it is just the feeding in of more and more processes into the formalism as the energy is increased which causes the exponential increase of the result (17). Some care has to be taken to show that those processes, which are included, are really well above their thresholds. That this is indeed possible is shown by the consistency of the two conditions (12a) and (16).

We want to remark that the result (17) is consistent with everything that is known from perturbation theory. If the functions $\alpha(p^2)$ and $N(p^2)$ are replaced by α and 1, respectively, (this would be the case in a perturbation theory approach) and the result expanded, each term of the expansion would behave as

$$\pi^n(-a) \approx \frac{\alpha^n}{n!} log^{2n} \frac{a}{m^2}.$$

In this approach each term gives a convergent result for the integral (3) but the resulting series would diverge as

$$\begin{aligned} \frac{\overline{\pi}(p^2) - \overline{\pi}(0)}{-p^2}\Big|_{p^2=0} &= \int_{m^2}^{\infty} \frac{\pi(-a)}{a^2} da \approx \sum \frac{\alpha^n}{n!} \int_{m^2}^{\infty} \frac{log^{2n}(a/m^2)}{a^2} da \\ &= \frac{1}{m^2} \sum \frac{\alpha^n (2n)!}{n!} \approx \frac{1}{m^2} \sum \frac{(4\alpha)^n}{\sqrt{\pi n}} n!. \end{aligned} \tag{18}$$

The actual behaviour of the perturbation series in quantum electrodynamics is not known but a study of some simplified models [11, ? , ? ,] have indicated that a behaviour like (18) is not improbable. If our result here is taken seriously, it would indicate that this divergence of the series is not due to unjustified expansions but reflects a deep-lying inconsistency in the theory.

The blowing up of the integral (3), so to say, starts at an energy where

$$\alpha(p^2) log(-p^2/m^2) \approx 1. \tag{19}$$

If the increasing function $\alpha(p^2)$ is replaced by its smallest value $\alpha \approx 1/137$, (19) leads to an extremely large energy, up to which the theory should, in a certain sense, be consistent. (Incidentally, the limit obtained in this way is of the same order of magnitude as the energy limit obtained by Landau et al. [10].) However, such an estimate is certainly too conservative as the increase of the "bare charge" $\alpha(p^2)$ must be a very important effect. At the present stage it is impossible to make any reliable estimate of how large the influence of this effect would be, but it will certainly considerably reduce the domain of validity of the theory. That such a reduction is not in contradiction with present experimental data has recently been emphasized by Arnous and Heitler [15]. In any case, we find it an interesting feature of the present analysis that there is at least an indication that the theory becomes inconsistent earlier than at the enormously high energies found previously.

If the analysis presented here should convince anyone except the speaker, the estimate (12a) must be put on a somewhat firmer basis. For the moment the situation is that we can prove the result for certain simple states $|z>$, and then we guess that it should hold for *all* states. The proof for the simple states with one incoming pair made use of the Hilbert transforms in (5) i.e. of the "causal" structure of the theory. It is easy to prove that we can write a matrix element of the current operator from the vacuum to a state with ν photons as[5]

$$\begin{aligned}
&< 0 \mid j_\mu(x) \mid k_1, \ldots k_\nu >= i^n \int \ldots \int (dx^1 \ldots dx^\nu)\theta(x-x')\times \\
&\times \theta(x'-x'')\ldots\theta(x^{\nu-1}-x^\nu)\times \\
&\times < 0 \mid [j_{\mu_\nu}(x^\nu), [\ldots[j_{\mu_1}(x'), j_\mu(x)]\ldots]] \mid 0 > \times \\
&\times < 0 \mid A^{(0)}_{\mu_1}(x')\ldots A^{(0)}_{\mu_\nu}(x^\nu) \mid k_1, \ldots k_\nu > .
\end{aligned} \tag{20}$$

The causal structure of this expression is described by the θ-functions and the vanishing of the symmetrized, iterated commutator for space like values

[5] Expressions of this general form were first used by H. Umezawa and F. Kamefuchi [16]. Apparently independently, similar formulae have recently been "rediscovered" by F. Low [17] and by many others.

of the corresponding distances. We therefore have to find a Lorentz-invariant generalization of the Hilbert transformation applicable to expressions of the form (20). This generalization is not known for the moment, but the task of finding it posses a definite mathematical problem that can perhaps be solved. If the expected result (12a) and (12b) should turn out as a consequence, the argument presented here would be turned from a plausibility argument to a mathematical proof, the rigour of which could perhaps be acceptable from the point of view of theoretical physics.

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Notes added by me (CJ) I have retyped the article from the Proceedings of the CERN Symposium, keeping as closely as possible its original style. I have also corrected a few misprints. Concerning the famous paper by Gell-Mann and Low (reference 7, on the above list), Källén wrote to Pauli on 30 June 1955 (letter [2123] in the Pauli Collection) that the authors had intended to disprove quantum electrodynamics, but that he had corrected some errors [formulas (2.11) and (2.12)]. The Gell-Mann-Low paper has several references to Källén's work. Unfortunately, Källén's correspondence with other scientists, before 1958, seems to have been lost. As mentioned before the disruption caused by his sudden death, followed shortly after by that of his wife, could have been the reason for this unfortunate situation.

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At Schladming Winter Schools – An Introduction

The Schladming Winter Schools[1] were founded in Austria in 1962 by Paul Urban[2] with support from Walter Thirring.[3] These Winter Schools attracted top class lecturers as well as young students from many countries. The proceedings of these Schools give valuable information about the development of physics and many of the lectures are still worth reading for anyone who cares about the history of physics. As Bjorken[4] writes, in the preface to his book "In Conclusion" [2]:

> "In the folk history, the Standard Model was created as a relatively logical and straightforward process, while in reality it was a tortured one, with many false leads. It is hard for this generation of particle physicists to visualize the rich environment of confusion, and the variety of abandoned alternatives, from which the Standard Model ideology emerged."

Källén loved to go to the Schladming Schools, whenever possible with his wife. His correspondence shows that he and his wife were present already at the first Schladming School in 1962. These Schools were organized so that there would be some time for skiing in the middle of the day. Källén said that he had been taken to a difficult downhill terrain by a young Austrian, where he had fallen and broken two fingers. Hence, his theory that "unnecessary bodily exercise is very dangerous" had been confirmed (see Chap. 11).

Källén gave six lectures (or lecture series) at the Schladming Schools 1962–1968 as follows:

- Review of Consistency Problems in Quantum Electrodynamics, *in* 1965 School on "Quantum Electrodynamics" [3];

[1] The official title of these schools was "Internationale Universitätswochen"; in English: International University Weeks.

[2] Paul Urban (1905–1995) was a theoretical physicist at the University of Graz, Austria. Urban often expressed his appreciation of Källén's support of his School.

[3] Walter Thirring (born 1927) is a distinguished Austrian theoretical physicist. Källén's correspondence with Pauli shows that Källén's relationship with Thirring was far from harmonious, to say the least. See also [1] and Chap. 37.

[4] James D. Bjorken (called Bj in the physics community) is a distinguished theoretical particle physicist. For a short chronology see Array of Contemporary American Physicists [ACAP], on the internet.

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- Radiative Corrections in Nucleon-β-Decay and Electromagnetic Form Factors, *in* 1966 School on "Elementary Particle Theories" [4];
- An attempt to Calculate Radiative Corrections to a Pure Fermi Decay, [5] as well as a summary[5] talk [6], *in* the 1967 School on "Special Problems in High Energy Physics";
- Gradient Terms in the Commutators of Currents and Fields, [7] and again a summary talk [8], *in* the 1968 School on "Particles, Currents, Symmetries".

Detailed references to the papers by Källén are found on his list of publications in Part 5 of this book.

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3. G. Källén paper [1965a]
4. G. Källén paper [1966c]
5. G. Källén paper [1967a]
6. G. Källén paper [1967b]
7. G. Källén paper [1968a]
8. G. Källén paper [1968b]

[5] Since the School lasted for two weeks, there were two summary talks, one for each week.

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The 1965 Schladming School and J. D. Bjorken's Recollections

The 1965 School became famous, at least among students, due to a fight between Källén and Kenneth Johnson[1] who presented their orthogonal views on the underlying nature of quantum electrodynamics. Johnson et al. [1] had found that "quantum electrodynamics may be regarded as a perfectly consistent theory" and were advocating [2] that the unrenormalized electron Green's functions are finite; the bare mass of the electron vanishes and the electron mass must be totally dynamical in origin. This irritated Källén enormously as he believed that quantum electrodynamics was not a consistent theory. From the very beginning, he was aware of the fact that his arguments were not *mathematically* stringent but they were *physically* so plausible that it was difficult to imagine how they would not be valid. After all, he was an engineer, a master of electromagnetism, and took intuitive physical arguments very seriously – far more so than ϵ's and δ's of mathematics.

The summary talk at the 1965 Schladming School was given by James D. Bjorken, who later wrote a letter to Källén, dated 17 March 1965, stating:

> "… I enclose the manuscript of my summary talk at Schladming for your blessing. I, of course, don't expect to get it, but will appreciate very much your criticisms."

Källén's answer to Bjorken (at Stanford Linear Accelerator Center), dated March 29, 1965, reads as follows:

> "Dear Björkén[2]:
>
> Thank you very much for your letter of March 17th and your Schladming manuscript. You say that you don't expect to get my 'blessing' for it and I see no reason why I should disappoint you in that respect. There are several of the things you say which I would like to have formulated rather differently, but as I don't expect you will change much anyhow, it would probably only be a vaste [read: waste] of time for me to go into details. Consequently, I shall

[1] Kenneth A. Johnson (1931–1999) was a well-known theorist at MIT.
[2] Källén insisted on using the Swedish spelling of Bjorken's name. Once he said, jokingly, that Björkén is the only Swede who doesn't know how to spell his own name.

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restrict myself to just one point with the hope, that I might influence you slightly there. I am referring to the bottom of p. 8 of the manuscript, where you get involved with questions of rigour. Don't you think it is rather unfair to discuss such things only in connection with one of the contributions? Even if I certainly agree with you that the standard of rigour involved here is not comparable to axiomatic field theory, I would still like to insist that there is absolutely no comparison between my argument and the rest of contributions during the conference. I really don't think I am unreasonable if I insist that you ought to modify your comment at this point.

Incidentally, I am not coming to the DESY-meeting in Hamburg.

Sincerely yours

Gunnar Källén"

Bjorken's Recollections

Bjorken has kindly provided [3] the following information about the 1965 meeting:

> "I doubt that folks outside Sweden really appreciate the magnitude of the impact that Källén made. Regarding Schladming, that was the first summary talk I ever gave. It was a last-minute request from Urban, who clearly saw my role as arbitrator. The lectures of Källén and Johnson were mostly over my head, and the exchanges between them quite sharp. Although I basically avoided taking sides in my talk (mainly out of technical incompetence), I am proud of it, because I feel that I got closer to the right answer than the protagonists. I argued that their considerations were moot, given the unsolved problem of synthesizing the Fermi theory with QED, and that the asymptotics of QED depended on the nature of the solution to that problem. Regarding what happened after Schladming, there was the program of Johnson, Baker, and Willey. Last fall, I visited Brown Univ. and encountered (retiree) Herb Fried, who put in my hands several of his latest works. He seems to have pursued the subject further and is excited about what he has done[3]. I don't think that Källén's arguments passed the test of time, but I may be wrong there. For me, the fact that Z_1 and Z_2 are gauge dependent makes the issue hinge only on the properties of Z_3. And that pushes things in the direction of the work cited above. …"

[3] Actually I (CJ) had already contacted Herb Fried, but about a different issue [4].

Bjorken also wrote about a visit he had made to Lund:

> "I was invited to Lund to give a talk (late 1960's?) and was hosted most graciously the whole time by Källén, who gave me a personal tour of the Lund facilities, along with the dinner invitation at their home. My talk was on the photon as Goldstone boson, an edgy topic to this day (but one I still entertain. My visit to Brown was motivated by a reminiscence by Guralnik[4] on the origin of his work on the Higgs mechanism. He claims to have been influenced by my Goldstone-photon work. And we are both motivated to Goldstonize the graviton.) Anyway, the Lund talk went rather smoothly, with good critical questions by Gunnar enroute. At the end, he got up and said 'Thank you, Dr. Bjorken, for this very interesting talk – but of course WE do not believe a word of it.'"

In his summary talk at the 1965 School, Bjorken writes [5]

> "Here the Källén point of view is that the theory must at small distances be modified in a profound way, and the present theory bears a relation to the modified theory something like classical to quantum physics."

Källén's Last Words?

In 1968, Källén would argue as follows:

Johnson and his collaborators base their conclusions on an iteration scheme and claim to get a finite result after a finite number of iterations. However, this isn't worth much as it doesn't say anything about the existence of solutions to the basic equations. Before the convergence of the iteration scheme is discussed, one is very far from an existence proof of any kind. After all, ordinary perturbation theory is just an iterative scheme where (after renormalization) each order is finite. This, however, does not mean that this perturbation theory proves the existence of solutions to the basic equations of quantum electrodynamics.

Källén often pointed out that a result obtained in perturbation theory does not necessarily have general validity. Källén had strong suspicion that quantum electrodynamics is not a consistent theory. However, he did not go as far as Landau who claimed that no field theory is consistent!

[4] Here Bjorken is referring to the eminent physicist Gerald S. Guralnik at Brown University, Providence, RI, USA.

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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 $\eta \to$ 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].

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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)

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Talk at 1961 Solvay Conference

Källén's talk at the this prestigious conference is published in its proceedings [1]. Here below, I (CJ) summarize the "essence" of what he had to say, i.e., his paper [1961b] so that our readers can follow the discussions ensuing his talk, reproduced in the next chapter. It is of historical interest to know what occupied the minds of the giants of theoretical physics about 50 years ago. What did they think and what baffled them?

Källén started his talk to his distinguished audience very pedagogically, as if he was lecturing schoolboys. This was often his style. The points that he called attention to were:

- We talk about fields (such as electromagnetic fields) or forces in a given space-time point but that is not what we can measure. One always measures averages over a small but finite space-time volume.
- In classical mechanics one always assumes that this averaging procedure is not very essential. But in field theory the fields are no longer given numbers but rather "operators" with somewhat intricate mathematical properties [commutation relations, appearance of delta functions]. There is, of course, the mathematically well-developed theory of distributions that one could use. But that involves heavy machinery "which does not seem to add very much to our understanding of the subject". Therefore, Källén concludes this first part of his talk by again advising against the unwarranted use of what he used to call "epsilontics" in physics:

> "If one is not mainly interested in rigour it is easier to use the somewhat sloppier way of expression which has always been used in physics."

Then Källén reminds the audience of the Lagrangian formalism for *free fields* and the quantization rules for fermions and bosons, concluding by:

> "All these subjects are so well-known that it should not be necessary for me to enter into them here."

Now comes a point that preoccupied Källén a great deal. It concerned the *interacting fields*. He took the example of quantum electrodynamics and wrote

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[in his favorite Pauli metric] the relevant equation,

$$\left(\gamma\frac{\partial}{\partial x}+m\right)\psi(x)=ie\gamma A(x)\psi(x).$$

Then he noted that on the right-hand side of this equation one has the *product* of two field operators:

> "According to what has been said above this is really a product of two distributions and such a product does not always make sense. (For an illustration, one might think of the Dirac delta function which cannot be squared!) … it is somewhat amazing that in spite of this one can get information from it which can be checked with experience. As is well-known this information is not obtained in a perfectly straight-forward way but one encounters various infinite quantities when one tries to work out in practice any quantity to sufficiently high order. It is also well-known that these infinite quantities can be removed with the aid of a renormalization technique. … "

In passing Källén notes that introduction of fields for particles such as pions has been a "bold idea" as these have no classical counterparts (which electromagnetism had). Returning to interacting fields, he mentions the Tamm-Dancoff method [to improve perturbative treatment], which had been very much in fashion about ten years before, and explains what had gone wrong. His correspondence shows that he didn't think that the method was any good. On the contrary, for example, Heisenberg was one of the leading Tamm-Dancoff promoters. Källén then turns to a discussion of beyond perturbation theory:

> "Because of the disappointing situation for perturbation theory and perhaps also because of some 'mathematical curiosity' there have been a few attempts during the last ten years or so to try to discuss the structure of quantized field theory without any resource [resort] to perturbation theory or other approximation schemes. Ironically enough, these methods were first developed for electrodynamics (here he refers to three paper [2]) but have afterwards found applications in meson theory. … "

Here the point being that the meson-nucleon coupling constant is too large to allow perturbation theory. Among new approaches, Källén talks about the axiomatic approach to field theory and quotes his friend and collaborator Wightman [3]. Then he writes:

> "In its most extreme form an axiomatic paper starts by three or four 'axioms' which are supposed to give essentially the whole physical content of a theory.

Then one tries to work out as many consequences of these assumptions as possible. ..."

Among the assumptions/requirements, Källén discusses: Lorentz invariance and reasonable mass spectrum and then pays particular attention to local commutativity. He re-examines Pauli's proof of the year 1950 "that it is impossible to quantize a scalar field according to the exclusion principle and have particles with spin zero obey Fermi-Dirac statistics". He points out that Pauli's proof considers non-interacting fields while he is presenting the generalization to interacting fields. Finally, Källén deals with his own "asymptotic condition", which was very much appreciated by Pauli. He compares it with the Lehmann, Symanzik and Zimmermann approach (the so-called LSZ formalism) and discusses some subtle differences.

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1961 Solvay Conference

Figure 58.1 Courtesy of the International Solvay Institutes[1]

The participants on the picture are, from left to right:

- front row: S. Tomonaga, W. Heitler, Y. Nambu, N. Bohr, F. Perrin, J. R. Oppenheimer, W. L. Bragg, C. Møller, C. J. Gorter, H. Yukawa, R. E. Peierls, H. A. Bethe;
- middle row: I. Prigogine, A. Pais, A. Salam, W. Heisenberg, F. J. Dyson, R. P. Feynman, L. Rosenfeld, P. A. M. Dirac, L. van Hove, O. Klein;
- standing far left between second and third rows: A. S. Wightman;
- back row: S. Mandelstam, G. Chew, M. L. Goldberger, G. C. Wick, M. Gell-Mann, G. Källén, E. P. Wigner, G. Wentzel, J. Schwinger, M. Cini.

[1] I (CJ) wish to thank the Director, Professor Marc Henneaux, for sending me this picture as well as a copy of the Proceedings of the 1961 Conference.

Four invited participants are absent in this picture: Erwin Schrödinger (1887–1961) had died in January 1961 and three participants from the Soviet Union (N. N. Bogoluibov, L. D. Landau and I. E. (*also Y.*) Tamm) were not able (allowed?) to attend.

Note that the participants are *all* men, which was a common scenario in those days.

There are 14 Nobel Laureates on this picture, but nine of them don't know it yet.

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The Discussions After Källén's Talk at the 1961 Solvay Conference

The discussion after Källén's talk is of historical interest – it touches upon some fundamental issues in theoretical physics and tells us about the "tastes" of the participants [among them two Nobel Laureates (Dirac and Heisenberg) and four yet to become: Feynman, Gell-Mann, Nambu and Wigner]. Here is a what was said:

P. A. M. Dirac. – I would like to add some remarks to what I said yesterday about quantum field theory to make it more precise. Some of my more mathematically-minded colleagues have told me that all the representations of the inhomogeneous Lorentz group are known. This would imply that they know the representation needed for quantum field theory.

However, the mathematicians assume that two representations are equivalent if they are connected by a unitary transformation, which means counting a unitary transformation as trivial. To a physicist, a unitary transformation may be very far from trivial. A good deal of atomic physics consists in trying to find the S-matrix, which is just a unitary transformation and is certainly far from being a trivial one.

We must take the physical point of view that two representations connected by a unitary transformation should not necessarily be regarded as equivalent. The number of different representations is then much greater, and we have the problem of picking out the right representation from among this greater number. In this search we should use any mathematical methods that we can think of; for example, we need not restrict ourselves to working in terms of tensors of finite rank, but may bring in tensors of infinite rank, corresponding to all possible representations of the homogeneous Lorentz group.

The next comment came from Wigner, a great master of unitary transformations and symmetries. Of course he didn't know that he was to receive the Nobel Prize two years later for "his contributions to the theory of the atomic nucleus and the elementary particles, particularly through the discovery and application of fundamental symmetry principles."

E. P. Wigner. – May I say that the significance of unitary transformation is very greatly appreciated by us, the representations themselves consist of unitary transformations and we are surely interested in them. In fact, there is no real difficulty in specifying the most general representation of the inhomogeneous

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Lorentz group in an arbitrary coordinate system. The difficulty is, rather, that if we specify the form of the representation, we also specify the coordinate system and we specify it in such a way which does not tell us what the operators for the other physical quantities are. All that is given are the operators for energy, momentum, angular momentum, etc.

There is one exception to these statements. If the representation is irreducible, or if we consider the part of Hilbert space which is spanned by the axes which belong to the discrete spectrum of the restmass. The restmass is one of the two characteristics of the irreducible representations (the other one being the intrinsic spin). In this case, or in the aforementioned part of Hilbert space in the general case, all the operators which appear significant to me can be obtained. The principal ones are the position operators which can be defined by their relations to the operators of the inhomogeneous Lorentz group. Thus the operator of the X coordinate is changed into this operator plus a constant if the operator is transformed by a displacement in the X direction by this constant.

This, however, is an exceptional case and in the really interesting case of a continuous restmass spectrum that is for collision systems, only the various momentum operators can be obtained by any known consideration. In fact, if I am quite sincere, I cannot say that it is clear to me what the physical quantities are for which one wants the operators to be defined. These may be the various fields or they may not be. This is a very difficult question and it is possible that an essential physical idea will be needed before it will be answered definitively.

It may be worth while to point out that the equation derived by Källén

$$< o \mid A(x)A(x') \mid o > = < o \mid A(x')A(x) \mid o >$$

for space like $x - x'$ does not depend on "a reasonable mass spectrum" but follows already from simple Lorentz invariance. If $x - x'$ is space-like, it is always possible to find a Lorentz transformation which carries x into x' and x' to x. In fact, this can be done by a rotation in the coordinate system in which x and x' are simultaneous. Let us denote the Lorentz transformation in question by $\mathcal{L}$. Since the vacuum is invariant under all Lorentz transformations $\mathcal{L}\, o = o$ and $\mathcal{L}^{\dagger} = \mathcal{L}^{-1}$

$$\begin{aligned} &< o \mid A(x)A(x') \mid o > = < o \mid \mathcal{L}^{\dagger} A(x)A(x')\mathcal{L} \mid o > \\ &= < o \mid \mathcal{L}^{-1} A(x)\mathcal{L}\mathcal{L}^{-1} A(x')\mathcal{L} \mid o > = < o \mid A(x')A(x) \mid o > \end{aligned}$$

This derivation does not give, of course, *all the results of Källén*, not in particular those which depend on the positive definite nature of quantities. On the other hand, one may generalize the argument to some degree. Thus if the Minkowski distance of x_0 from x and x' is the same, one can find an $\mathcal{L}$ which leaves x_0 unchanged but transforms x and x' into each other. One can then conclude in the same way as before that

$$< B(x_0)\, o \mid A(x)A(x') \mid B(x_0)o >=< B(x_0)o \mid A(x')A(x) \mid B(x_0)o >$$

where B is either the same field as A or is another field or even product of fields. This shows that if, in the theory discussed $A(x)$ and $A(x')$ (for space like $x-x'$) do not commute, their commutator must at least have many o matrix elements.

It may be of some interest also that the original form of the consideration, that leading to Källén's equation, can be used for fields which are not scalars but have spin. One then obtains the result that the vacuum expectation values of $A_\mu(x)\,A_\mu(x')$ and $A\mu(x')\,A_\mu(x)$ differ by a factor $(-)^{2\mu}$ which is one for vector, tensor, etc. components, -1 for spinor, etc. components. Thus if one assumes that either the commutator or the anticommutator of two fields is a C-number, the connection between spin and "statistics" follows. This connection originally proved by Pauli was obtained of course, by Wightman and his students under less stringent assumptions as far as the nature of the commutator or anticommutator is concerned, but by assuming a "reasonable mass spectrum".

G. Källén. – I thank Professor Wigner very much for showing us this simple and elegant derivation. If I may defend the somewhat clumsier method I used myself to show the same result, I should like to say that I really wanted the explicit representation in terms of the analytic function (the $\Delta^{\pm}$ function) for the later argument. Therefore, I should have had to do all I did anyhow at a later stage and a simpler derivation for this particular result should have increased the overall length of the discussion.

W. Heisenberg. – In connection with the so-called "theorem of Haag" I would like to point out, that its content should be well known already from conventional quantum mechanics. If one compares e.g. the state of a ferromagnet where the total magnetic moment has the direction of the Z-axis and another state with a slightly different direction of the total magnetic moment, these two states will always be completely orthogonal to each other, if we have to do with an infinite ferromagnet. If we excite an electron from one of these states, again the resulting state will be exactly orthogonal to the other. Therefore, in writing down matrix equations for such systems one must be careful not to write down relations between matrix elements in which both sides of the equation are trivially zero. Such an error might occur in perturbation theory or in the old Tamm-Dancoff method, where one starts with the "bare" vacuum; it ought however not to occur in the new Tamm-Dancoff method, where one starts from the real vacuum. Taking the theory of superconductivity (Bardeen-Bogoliubov) as an example, the new Tamm-Dancoff method gives correct results (the energy gap), while the theory of perturbation does not. This whole problem therefore has nothing to do with the real difficulties of quantum field theory, nor will it give rise to any criticism concerning the use of the new Tamm-Dancoff method in field-theory.

G. Källén. – I agree very much that the theorem discovered by Van Hove and Friedrichs and usually referred to as the "Haag theorem" is really of a very trivial nature and it does not mean that the eigenvalues of a Hamiltonian never exist or anything that fundamental. Your analogy with ferromagnetism is also very interesting. However, I do think that this theorem does show that the old fashioned Tamm-Dancoff method is essentially not better than perturbation theory and I do not believe that the new Tamm-Dancoff method is so much better. The fundamental difficulty is that a finite amount of probability (one) has to be divided between so many states that each state gets essentially zero probability. This problem remains also in the new Tamm-Dancoff method.

W. Heitler. – I find it very difficult to understand from a physical point of view that local commutativity should not always follow from Lorentz-invariance. Once a signal can travel faster than light even in a microscopic domain Lorentz-invariance is violated. One more example is the non-local theory I mentioned at the end of my report. Here local commutativity of the Hamiltonian density was violated in a microscopic domain and the consequence was that Lorentz-invariance was violated in the results. Is there a more physical way to understand that cases exist where local commutativity is not but Lorentz-invariance is fulfilled?

G. Källén. – It is possible to make formal mathematical models where Lorentz-invariance holds but where local commutativity is violated. However I do not really understand what that means and know of no simple description of the physics involved.

A. S. Wightman. – I should like to explain the "some reason or another" why the Haag theorem is so-called. What Haag found was that the phenomena of the strange representations of the commutation relations, discovered by Friedrichs and Van Hove in special models, is a general feature of any translation invariant theory in which non trivial pair production occurs.

The significance which one attaches to Haag's theorem depends on one's attitude towards model such as Heitler's. On the one hand, one can regard this model as a short hand for the investigation of the numerical effect of cut-offs in the perturbation series of a relativistic theory. Then mathematical questions about the exact spectrum of the model are quite irrelevant. On the other hand, one can try to take the Hamiltonian of the model really seriously, and try to find out what spectrum it predicts and what properties its exact eigenfunctions have. In this case, it seems to me that Haag's theorem is distinctly non-trivial. It says that to make physical sense of the Hamiltonian one must insert not the familiar representation of the annihilation and creation operators but one of the strange representations.

L. Van Hove. – I would like to make a few remarks on the question of the expansion

$$| \, n \, phys. >= \sum_{n'} C_{nn'} \mid n' math > \tag{1}$$

mentioned in Källén's talk and in various of our discussions. The formal difficulties connected with this expansion originate from the fact that all $C_{nn'}$ become zero in a realistic situation. This can be due to two completely different causes which should be sharply distinguished.

In the case of an interaction modifying the physical system over the whole of space (examples are field theories with pair creation and practically all many-particle systems) $C_{nn'}$ is zero because of the infinite extension of space: this is seen by enclosing the system in a finite volume V, calculating $C_{nn'}$ for V finite and noticing that for $V \to \infty$, $C_{nn'}$ goes to zero, usually with an exponential dependence on V. This situation holds even in a field theory with cut off, we know how to handle it and it is not connected with the real difficulties of field theory (nevertheless Haag's theorem, if I understand it correctly, refers to this situation and is therefore, I think, of little direct relevance to the basic difficulties of field theory).

The second case where one knows that all $C_{nn'} \to 0$ is the case of point particles interacting with a quantized field, the interaction giving rise to ultra-violet divergences. In this case, the interaction acts in a limited region of space only. One introduces an ultra-violet cut-off K. All $C_{nn'}$ go to zero as $K \to \infty$. This situation, which is absent in many particle problems, is connected with the divergence problem of field theory. It has been demonstrated on the simple example of a scalar field in scalar interaction with a static source (see K. O. Friedrichs, *Comm. Pure and Applied Math.*, **5**, 349 (1952) and L. Van Hove, *Physica*, **18**, 145 (1952)). Similar but less explicit conclusions have been obtained for more realistic cases in L. Van Hove,*Acad. R. de Belg.*, Bull. Cl. des Sc. 5^e S, **39**, 1055 (1951).

I would like to mention another point in connection with the expansion (1). It is natural to try to avoid the difficulty $C_{nn'} = 0$ by attempting to replace in the righthand side $| \, n', math. >$ by other states $| \, n' >$ which, although being simpler than $| \, n', phys. >$, would be better approximations to the latter and thereby give rise to a meaningful expansion with $C_{nn'} \neq 0$. A choice of $| \, n' >$ which has been considered is to take states of several dressed particles neglecting their mutual interaction; in terms of diagrams the definition of such states is quite easy (see W. Frazer and L. Van Hove, *Physica*, **24**, 137 (1958)). Such states are then linearly independent of but non orthogonal to each other. In simple non relativistic models one has been able to show that they have the following interesting properties:

(1) the metric tensor element $< n \mid n' >$ and the matrix elements $< n \mid H \mid n' >$ of the Hamiltonian H have simple, finite expressions involving only the renormalized coupling constant.

(2) iterative solution of the Schrödinger equation in the $\mid n' >$ representation leads to convergent expressions (see Th. W. Ruygrok, *Physica*, **24**, 205 (1958)). The difficulty however, is to carry out this program convariantly, although possible recent applications of dispersion techniques seem to embody the main idea of the method in a modified, manifestly covariant form.

Y. Nambu. – Regarding Professor Van Hove's remark on Haag's theorem I would like to emphasize the distinction between two different origins of the effect. One is related to the spatial volume or the size of the box which we consider and the other shares the common origin with the ultraviolet divergences. The latter is due to the fact that even in a finite volume there are an infinite number of field oscillators. In some models the divergence difficulties may be overcome. But perhaps we should keep in mind the possibility that Haag's phenomenon can arise from two different physical reasons, namely the continuous nature of space time and the practically infinite volume of the universe.

G. Källén. – I should like to remark that when we write e.g. the function $G(p^2)$ above as $G(p^2) = V \sum_{p^{(z)}=p} |< 0 \mid A \mid z >|^2$ the states $\mid z >$ that enter in this formula are the physical states including all of the interaction. When these states are classified as two-particle states, three particle states and so on this classification is made in terms of asymptotic states (e.g. incoming) particles. In principle, these states are not the same as the states indicated by Professor Van Hove. However, in many practical applications one makes approximations, sometimes to the effect that the interactions between certain particles are neglected at one stage or another. In that case, one may not be so very far from the situation described by Professor Van Hove.

G. Chew. – There is a historical question that I have never before had the chance to ask of the people involved. In the forties – at the time when I could not yet call myself a physicist – it is my impression that most of the difficulties of quantum field theory were already recognized. Discouraged by the situation, Heisenberg proposed that the S-matrix, defined a few years earlier by Wheeler, should be used as the fundamental basis for a theory. Lorentz invariance and unitarity were recognized as essential properties, as was analytic continuation in the energy, and for several years there was great enthusiasm for the S-matrix. The enthusiasm died down, I suppose, because people were not bold enough, then, to assume analyticity in all momentum variables and so found the theory lacking in dynamic content. Also the principles of renormalization were discovered and raised new hopes for field theory. During the fifties however, as Källén has told us, the difficulties of field theory have been confirmed and nothing here seems to have budged for a number of years. Ironically, the studies of field theory have suggested far broader analyticity properties of the S-matrix than were contemplated in the forties, and many of these properties by now have experimental support. As we shall hear tomorrow, it now seems likely that the S-matrix, with full analytic continuation, is dynamically as complete as field theory ever expected to be. The question is then two-fold:

(1) is my impression correct of the early history of S-matrix development?
(2) how do those people who shared the early enthusiasm feel about it now as a substitute for field theory?

W. Heisenberg. – I would like to give at least a partial answer to the questions by Chew. When I had worked on the S-matrix for a while in the years 1943 to 1948 I came away from the attempt of construction of a pure S-matrix theory for the following reason: when one constructs a unitary S-matrix from simple assumptions (like a hermitian η-matrix by assuming $S = e^{i\eta}$), such S-matrices always become non analytical at places where they ought to be analytical. But I found it very difficult to construct analytical S-matrices. The only simple way of getting (or guessing) the correct analytical behaviour seemed to be a deduction from a Hamiltonian in the old-fashioned manner. One also could argue that by allowing for an analytical continuation of the S-matrix elements, one actually went away from the energy-shell into a more "local" region. Therefore finally I had the impression that a *simple* definition of a field theory could only be found by stating something about a genuine "local" interaction.

In principle however I agree entirely with Chew's program. It should be possible to define the S-matrix by postulating some underlying groups as basis of the theory, adding the postulates of unitarity and analyticity and calculating the masses, etc. from some condition of consistency, without any use of an indefinite metric in Hilbert space. My criticism comes only from the practical point of view. I cannot see how one could overcome the enormous complications of such a program. The indefinite metric may just be a practical tool to bring these S-matrix relations (concerning their analytical behaviour) back into the form of a local field theory. In such a theory one can find simple devices for estimating mass-eigenvalues, etc. In the end this theory might just lead to that unitary S-matrix you are looking for.

G. Källén. – Not taking the historical point of view but looking at the situation today, it appears to me that the important difference between an S-matrix theory and a field theory in a broad sense is that the S-matrix theory speaks only of quantities on the energy shell, while a field theory considers also quantities off the shell. Another way of describing this situation is to say that the S-matrix considers everything as happening between $t = -\infty$ and $t = +\infty$. In many purposes this is, of course, a very good approximation but I wonder if it is always so. This would mean that one could completely eliminate time from physical theory and that appears to be a very radical idea.

Another point I should like to ask Professor Heisenberg concerns the indefinite metric. If we have an indefinite metric, the function $G(p^2) = V\sum_{p^{(z)}=p} < 0 \mid A \mid z >< z \mid A \mid 0 >$ is not a sum of positive terms any more. Therefore, it could be negative somewhere. This means that one, with the aid of a suitable test function, could get a negative value for a vacuum fluctuation like

$$< o \mid A(f)A(f) \mid o > \sim \int dp \mid f(p) \mid^2 G(p^2).$$

If $A(x)$ is a component of the electromagnetic field this seems to be a statement with physical meaning. What is your interpretation of this?

W. Heisenberg. – I certainly agree with Källén that paradoxes of this kind might occur occasionally in an indefinite metric.

But there I would like to remind you of similar paradoxes in ordinary quantum mechanics, and here I am referring to a paper by Sudarshan [Sudarshan] and some papers by Bopp. Once can – as Wigner has shown long time ago – put quantum mechanics into a mathematical form so that it resembles classical statistical mechanics. One may introduce a density function $f(p,q)$ depending upon the coordinates and momenta of the particles and may write down a kind of Boltzmann equation constructing $\frac{d}{dt}f(p,q)$ by an integral operator acting upon $f(p,q)$. In some very simple cases like the harmonic oscillator the quantum equation is even identical with the classical one. But there is one essential difference between the classical and the quantum theoretical $f(p,q)$. In classical theory the density (or probability) $f(p,q)$ must by definition always be positive, in quantum theory it is not.

This paradox of course can be understood finally by the uncertainty relations. In a similar way I would expect that there will never be measurements by which you could find negative values of $|A(f)|^2$, even if formally such values could appear.

R. P. Feynman. – I would like to give my interpretation of history (for Chew's sake). I think someone said once that the problem in theoretical physics is to prove yourself wrong as quickly as possible. The difficulty we have had for 27 years is that we haven't been able to prove Yukawa was wrong. I would like to discuss the history of attempts. The central problem at the beginning was to solve equations, figure out the consequences (that is what we used to do in physics once), make experiments and then think of another idea. The best progress is made when this can be done.

In the case of field theories, other than electrodynamics where there was essentially no difficulty in making calculations other than infinities, no one has figured out how to make the calculations. So there was an original history of Tamm-Dancoff method, various damping approximations, Salpeter equations and other tricks … One tried to solve these things and people became discouraged. A group of mathematically minded people who were not able to solve the equations tried to prove they had no solution and made no sense. This has not succeeded and absolutely demonstrates that this is essentially or nearly a blind out.

The other way to side track was to try to formulate things in another way. That is where S-matrix and your attempts to understand the π-meson without actually using field theory but getting clues from it came in. During all this time, no complete solution either of the S-matrix or of the field equations hasn't really been produced. You sit there and say: why isn't everybody doing S-matrix; another guy says: why isn't anybody doing field theory? The real problem is : *why is nobody solving anything?*

One of the reasons why you don't solve the problems is that you don't work hard enough. One of the reasons it is and has always been difficult to work hard on these problems is that nature keeps telling us that it has the quality of being much more elaborate than we thought and that any minute another resonance may come in and give another clue. There has always been a feeling that something is incomplete. But that is a side point.

I see I got some applause for the main point I tried to make. I think it would be a good idea if some people could keep trying.

M. Gell-Mann. – A good feature of the dispersion theory approach is that one works with quantities that are observable or nearly so. While the S-matrix theory is being built, we are learning to understand a great deal about the experiments. We could mention as examples the use of forward scattering and form factor dispersion relations, pology and the current study of high energy diffraction. These applications have not only helped to interpret data but have stimulated a great deal of experimental work.

F. Dyson. – In reply to the historical question posed by Chew, I would like to state my personal interpretation of the history of field theory during the last 27 years. I believe that the central problem of field theory is to define a precise notion of convergence which makes the solution of an infinite set of equations a meaningful and feasible mathematical operation. We have had four infinite sets of equations, each of which has a good claim to represent the physical content of field theory. These are: ordinary perturbation theory, the Tamm-Dancoff equations, the Lehmann-Zimmermann-Glaser equations, and the Chew-Mandelstam equations. Each in turn has occupied the attention of physicists for 5–10 years. If in any case we had found a workable definition of convergence which made the equations solvable, we would have had a well-defined field theory which could be compared with experiment. In fact no such definition of convergence has been found for any of the four sets of equations. It is justifiable to hope that the Chew-Mandelstam equation may overcome this difficulty which has stultified the three older attempts to formulate a meaningful field theory. However, the Chew-Mandelstam program is at present at least as far as the other methods from honestly facing up to this problem.

The above "Discussions after Källén's Talk ..." have been re-typed for this book by me (CJ).

Part 3

Promotion of Science in His Honor

Learned Societies and Great Scientists Pay Tribute to Källén

"I owe a great debt of gratitude to Gunnar Källén."

Steven Weinberg (2009)

Källén was a member of several learned societies, among them:

The Royal Physiographic Society in Lund
&
The Royal Swedish Academy of Sciences in Stockholm.

This Part of the book is devoted to what these societies have done for promotion of science, to honor his memory.

Steven Weinberg describes Källén's work and his own contacts with him in an article, with the title "Living with Infinities".

Källén, with his passion for the education of young people, supported the creation of the famous Erice School (Ettore Majorana Center) in Sicily, which in return honors his memory by giving scholarships to young scientists, as described in a chapter in this Part.

Källén was very interested in the Lee Model of 1954, looking for guidance and clues as to how to deal with non-perturbative renormalization in quantum electrodynamics. He and Pauli published a joint paper about it in 1955. The author of the Model, T. D. Lee, together with R. Friedberg, has written an article as a tribute to Källén. It appears in the last chapter of this Part.

This Part also includes an article about Källén by Christian Møller and some comments by N. G. van Kampen about the role played by his supervisor, the famous Dutch physicist H. A. Kramers, in connection with the issue of renormalization which was of utmost importance to Källén up until the end of his life. Finally, I (CJ) have examined the issue of a "Nobel Prize to Kramers".

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The Royal Physiographic Society in Lund and the Gunnar Källén Lectures

The Royal Physiographic Society in Lund is a learned society founded already in 1772, to promote "Physiography". However, that does not mean that the Society's main concern is physical geography. In fact it is an academy for natural sciences, medicine and technique – its task being promotion of knowledge in these areas. An account of the history of this Society is given in [1].

In spite of having been created to deal with "local affairs" related to the province of Scania, the Royal Physiographic Society early on realized the importance of international contacts and has had a large number of foreign members. Among the distinguished physicists present in this book who were foreign members of this Society we find: Niels Bohr, Paul Dirac, Christian Møller and Wolfgang Pauli. Pauli was delighted to become a member and so was also the Society – they could ask him for services which he happily delivered. After all his dear friend Niels Bohr was in Copenhagen, just across a few kilometers wide channel between Denmark and Sweden.

Källén was elected as a member on 8 April, 1959, i.e., less than a year after becoming a professor in Lund. It is interesting to note that on the very same day, the Society also elected a man called Tage Erlander (1901–1985) who had studied at Lund University. He had been the first chairman of the then newly founded Mathematical Society in Lund but later on "advanced" to becoming the longest lasting Prime Minister of Sweden (i.e., nonstop for 23 years, 1946–1969). The initiative to several such wonderful actions was taken by Torsten Gustafson who himself had been a member of the Society since 1940. He was indeed an impressively active member! He would deposit research articles in the archives of the Society, make presentations, nominate new members, etc. See also Chap. 3.

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The Gunnar Källén Lectures

After the tragic death of Gunnar Källén a fund of about 40 thousand Swedish Crowns (a considerable amount of money in those days) was raised to honor his memory as well as that of his wife Gunnel who had passed away soon after, in April 1969. The "Gunnar and Gunnel Källén Memorial Fund" thus created was placed under the auspices of the Royal Physiographic Society, on 8 October 1969. The main purpose of the fund was defined to be promotion of international contacts, for young physicists, as well as organization of small conferences, or series of lectures called "Gunnar Källén Lectures". These lectures have been an invaluable source of inspiration, in accord with what Källén would have wished. The support of young physicists' visit abroad reminds one of the young Källén's visit in Zürich in 1949 which turned out to be of utmost importance to his scientific development (see his letter in Chap. 50).

The "Gunnar Källén Lectures" have taken place in Lund since 1972. The topic of the lectures has by no means been restricted to Gunnar Källén's domain of expertise. Open mindedness has prevailed. Mini-symposia or series of talks have been organized on a variety of subjects such as:

- New Vistas in Cosmology
- The Science of Climate Change
- The Physics of Life
- Planet Earth
- New Horizons in Physics.

The Källén Lectures started in 1972, with Rudolf Haag and Harry Lehmann, both from Hamburg, as the lecturers. Fortunately, the written version of their talks is available (see below). In 2009, the Källén Lecture was given by Steven Weinberg, who has provided a written version for publication in this book (see the next chapter). These three lecturers had extensive personal contacts with Källén during his time in Copenhagen. We shall now briefly describe what Haag and Lehmann had to say in 1972.

On Lectures by Haag and Lehmann

The Källén lectures in 1972 were:

- Rudolf Haag: "The problem of particle statistics (permutation symmetry)"
- Harry Lehmann: "The development of renormalization theory".

These scientists had both known Källén very well and at times had had their tough encounters with him. Källén's correspondence shows that he was on familiar terms with Haag (he used the familiar German "du"-form in his letters to him and called him by his first name).

Haag, an axiomatic field theorist, opened his talk as follows:

> "Our meetings here today and tomorrow have a personal and a scientific aspect. The personal one is to honour the memory of our friend and colleague Gunnar Källén and that of his brave wife Gunnel. The scientific aspect: To discuss some – hopefully interesting – topics in theoretical physics. It was, I understand, Gunnel Källén's thought that the most fitting way to give a tribute to her husband's work and personality was indeed a scientific activity undertaken in his name. Of course, in choosing the topic of this lecture, I would have preferred to talk about something in direct contact with one of Gunnar Källén's mainstreams of effort. Unfortunately my work in the recent years has developed into a direction of which I am not sure that he would have approved: 'too much emphasis on highbrow mathematics and too little on physical phenomena.' This would probably have been his criticism. I remember very vividly a conversation with him during the Varenna summer school 1959[1]. One of the purposes of the organizers of that school was to acquaint physicists with ideas and results of functional analysis. He was very unhappy with this trend and snapped: 'If this kind of mathematics ever becomes a fashion in physics I am going to abandon the subject.'"

Lehmann had been treated more severely, as Källén would fiercely object whenever anyone uttered the words "Lehmann representation". For example, in a letter in 1961 he wrote [2] about Lehmann's contribution that it "is only a pedagogical summary of what others have done", and gave several references. Källén would also object when the above representation was referred to as "Källén-Lehmann" representation pointing out that "this formula had quite a long history". We are sure that Källén himself didn't think that he had ever been unfriendly to Lehmann. To have been critical all the time didn't mean unfriendliness. Actually, in 1967, when he heard that Lehmann was

[1] The date here should read 1958.

visiting Copenhagen, he invited him to come over to Lund as well. Lehmann thanked him for the invitation but wrote that he couldn't come due to other duties.

Lehmann, in the written version of his lectures, discusses the main ideas of renormalization as stipulated by Källén and Bogoluibov and finishes his talk by giving a list of some "current problems" in the renormalization theory that were under investigation. He gives no personal recollections of Källén.

In spite of Källén's "fights" he was much respected for his uncompromising scientific honesty and commitment. Both Haag and Lehmann attended Källén's funeral on November 1, 1968 and were visibly gloomy. I (CJ) met them for the first time at that sad occasion. I got to know Lehmann better afterwards, at DESY[2] in Hamburg and at CERN. He had very strong opinions and would be as enthusiastic about particles running around the accelerator at DESY as the horses racing around the loops of Trabrennbahn, Bahrenfeld, located nearby. He was a wonderful person, friendly and open to discussions. T. T. Wu, who collaborated with Lehmann for several years and knew him quite well has told me (CJ) that he never heard Lehmann complain about Källén. As Bert Schroer[3] who was a student of Lehmann points out, in those days "Streitkultur", i.e., fighting culture, was part of the everyday life of many of the great theoretical physicists, at least in Germany. They would "fight" and yet be friends. As we have seen in Chap. 15, Schwinger had hard time understanding this behavior.

[2] The acronym stands for "Deutsches Elektronen-Synchrotron". This "German Electron Synchrotron" Laboratory was founded in December 1959.
[3] Bert Schroer, private communication to CJ.

Gunnar Källén Lecturers 1972–2010

Here below we list the scientists who have given Källén lectures.[4] The list is in alphabetic order. Inside the parentheses, we have indicated the speaker's affiliation and the year of the lecture. Note also that Geneva means CERN which by itself represents an international environment.

J. Arvieux (Paris Saclay, 1994)
M. Berry (Bristol, UK, 1997)
B. Bolin (Stockholm, Sweden, 2001)
L. J. Curtis (Toledo, USA, 1990)
C. M. Dobson (Cambridge, UK, 2004)
J. Ellis (Geneva, Switzerland, 1981)
W. Freedman (Pasadena, USA, 2000)
R. Haag (Hamburg, Germany, 1972)
J. J. Hopfield (Princeton, USA, 2004)
E. Källén (Stockholm, Sweden, 2001)
L. Krauss (Cleveland, USA, 2000)
H. Lehmann (Hamburg, Germany, 1972)
A. Liddle (Brighton, UK, 2000)
R. Lundin (Kiruna, Sweden, 2005)
A. Martin (Durham, UK, 2002)
G. Mourou (LOA, Palaiseau, Paris, 2010)
G. Myatt (Oxford, UK, 2003)
G. Parisi (Rom, Italy, 1993)
I. Procaccia (Rehovot, Israel, 1987)
J. Randrup (Berkeley, USA, 1990)
H. Satz (Geneve, Switzerland, 1994)
P. Schuster (Vienna, Austria, 2004)
M. Spiro (Paris Saclay, 1998)
T. Stocker (Bern, Switzerland, 2001)
T. Torsvik (Oslo, Norway, 2005)
G. Watkins (Bethlehem, USA, 1990)
C. Will (St. Louis, USA, 2008)
J. Barrow (Cambridge, UK, 2007)
S. Björck (Lund, Sweden, 2001)
G. Charpak (Geneva, Switzerland, 1993)
B. Dingus (Las Alamos, USA, 2010)
Y. Dokshitzer (Paris, France, 2002)
R. Epstein (Copenhagen, Denmark, 1981)
G. Gabrielse (Cambridge, USA, 2007)
F. Halzen (Madison, USA, 2010)
M. Jacob (Geneva, Switzerland, 2002)
W. Kittel (Nijmegen, Netherlands, 2002)
J. Laskar (Paris, France, 2005)
I. Lindau (Lund & Stanford, 2010)
R. Lindzen (Boston, USA, 2001)
B. Mandelbrot (Yorktown Heights, USA, 1987)
P. Mohr (Gaithersburg, USA, 2007)
A. Müller (New York, USA, 1994)
J. Papaloizou (London, UK, 2005)
M. Pepper (Cambridge, UK, 1993)
E. Quercigh (Geneva, Switzerland, 1997)
Z. Rudzikas (Vilnius, Lithuania, 1993)
D. Schramm (Chicago, USA, 1981)
D. V. Shirkov (Dubna, Russia, 1990)
R. Stock (Frankfurt, Germany, 1994)
H. Svensmark (Copenhagen, Denmark, 2001)
M. Turner (Chicago, USA, 2000)
S. Weinberg (Austin, USA, 2009)
X. Zhuang (Cambridge, USA, 2004)

Here I (CJ) would like to add that the latest event in this series were two Symposi in 2011 and 2013 respectively:

1. "Carbon in the nanoworld, in space, and in humans", organized in 2011. The lecturers were Mildred Dresselhaus (Massachusetts, USA), Bengt Gustafsson (Uppsala, Sweden) and Kostas Kostarelos (London, UK). There was a subsequent panel discussion. The participants were the lecturers, together with Eleanor Campbell (Edinburgh, UK), Mats Jonson (Gothenberg, Sweden) and Christelle Prinz (Lund, Sweden).
2. "Higgs – Tiny Particles and Big Science" (2013), with lectures given by Lyn Evans (CERN), Peter Jenni (CERN), and Brian Cox (Manchester),

[4] I wish to thank the former permanent secretary of the Society, Professor Rolf Elofsson, for providing me with this list.

followed by a panel discussion on "Big Science and Society" with the participation of the lecturers as well Mikael Eriksson (Lund) and James Yeck (Lund).

Källén Lecture 2009

The acknowledgement of the Nobel Laureate Steven Weinberg's very first paper (Phys. Rev. **102** (1956) 285) reads as follows:

> **"The author wishes to express his gratitude to Dr. Gunnar Källén for suggesting this problem, and for many valuable discussions ..."**

Therefore he could give a first hand account of Källén's science and personality. Weinberg's talk was announced as follows:

> The Departments of Theoretical Physics, Physics and Astronomy of Lund University, sponsored by the Gunnar and Gunnel Källén Memorial Fund of the Royal Physiographic Society, proudly present

The Gunnar Källén Lecture 2009

"Living with Infinities - The Contributions of Gunnar Källén and Expectations for the Future"

by

Professor Steven Weinberg

Friday 13 February at 15.15, Lecture Hall B, Physics Department

The date 13 February was Källén's birthday. His four children were invited to the event and took part in the subsequent dinner. It was a memorable event.

And now we present the written version of the talk.

References

1. H. Westling, "KUNGL. FYSIOGRAFISKA SÄLLSKAPET I LUND 1772–2006", ISBN: 978-91-631-9800-7 (in Swedish)
2. Letter to J. J. Henning, 7 December, 1961 *in* the Källén Collection

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Steven Weinberg: Living With Infinities

UTTG-01-09
TCC-013-09

Living With Infinities

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Abstract This is the written version of a talk given in memory of Gunnar Källén, at the Departments of Theoretical Physics, Physics, and Astronomy of Lund University on February 13, 2009. I discuss some of Källén's work, especially regarding the problem of infinities in quantum field theory, and recount my own interactions with him. In addition, I describe for non-specialists the current status of the problem, and present my personal view on how it may be resolved in the future.

I owe a great debt of gratitude to Gunnar Källén. In the summer of 1954, having just finished my undergraduate studies at Cornell, I arrived at the Bohr Institute in Copenhagen, where Källén was a member of the Theoretical Study Division of CERN, which had not yet moved to Geneva. Richard Dalitz had advised me to go to Copenhagen partly because of the presence there of CERN. But my real reason for coming to Copenhagen with my wife was that we had just married, and thought that we could have a romantic year abroad before we returned to the U.S. for me to enter graduate school. I brought with me a bag of physics books to read, but I did not imagine that I could start original research. You see, I had the idea that before I started research on any topic, I first had to know everything that had been done in that area, and I knew that I was far from knowing everything about anything.

It wasn't long before people at the Institute let me know that everyone there was expected to be working on some sort of research. David Frisch, a visiting American nuclear physicist, kindly suggested that I do something on nuclear alpha decay, but nothing came of it.

C. Jarlskog (Ed.), *Portrait of Gunnar Källén*, DOI 10.1007/978-3-319-00627-7_61,

Early in 1955 I heard that a young theorist named Källén was doing interesting things in quantum field theory, so I knocked on his office door, and asked him to suggest a research problem. As it happened, Källén did have a problem to suggest. A year earlier, Tsung-Dao Lee at Columbia had invented a clever field-theoretic model that could be solved exactly.[1] The model had some peculiarities, which I'll come back to. These problems did not at first seem fatal to Lee, but Källén joined with the great Wolfgang Pauli to show that scattering processes in the Lee model violate the principle of unitarity – that is, the sum of the probabilities for all the things that can happen when two particles collide did not always add up to 100%.[2] Now Källén wanted me to see if there were other things wrong with the Lee model.

With a great deal of patient help from Källén, I was able to show that there were states in the Lee model whose energies were complex – that is, not ordinary real numbers. I finished the work on the Danish freighter that took my wife and me back to the U.S., and soon after I started graduate school at Princeton I had published the work as my first research paper.[3] This was a pretty unimportant paper (I recently checked, and found that it has been cited just nine times in 53 years), but it was a big thing for me – I started to feel like a physicist, not a student.

Incidentally, Källén's kindness to me went beyond starting me in research. He and his wife had my wife and me to their house for dinner, and going to the bathroom there, I learned something about Källén that probably most of you don't know – he had hand towels embroidered with the Dirac equation. Mrs. Källén told me that they were a present from Pauli. Källén also introduced me to Pauli, but I didn't get any towels.

Even though I had benefited so much from Källén's suggestion of a research problem, I felt that there was something odd about it. Lee was then not a well-known theorist – his great work with Yang on parity violation and weak interactions was a few years in the future. Also, the Lee model was not intended to be a serious model of real particles. So why did Källén take the trouble to shoot it down, even to the extent of enlisting the collaboration of his friend Pauli? The explanation, which I understood only much later, has to do with a long-standing controversy about the future of quantum field theory, in which Källén was playing an important part.

The controversy concerned the significance of infinities in quantum field theory. The problem of infinities was anticipated in the first papers on quantum field theory by Heisenberg and Pauli,[4] and then in 1930 infinite energy

[1] T.D. Lee, Phys. Rev. 95, 1329 (1954).
[2] G. Källén and W. Pauli, Dan. Mat. Fys. Medd. 30, no. 7 (1955).
[3] S. Weinberg, Phys. Rev. 102, 285 (1956).
[4] W. Heisenberg and W. Pauli, Z. f. Physik 56, 1 (1929); 59, 168 (1930).

shifts were found in calculations of the effects of emitting and reabsorbing photons by free or bound electrons, by Waller[5] and Oppenheimer.[6] In both cases you have to integrate over the momenta of the photons, and the integrals diverge. During the 1930s it was widely believed that these infinities signified a breakdown of quantum electrodynamics at energies above a few MeV. This changed after the war, when new techniques of calculation were developed that manifestly preserved the principles of special relativity at every step, and it was recognized that the infinities could be absorbed into a redefinition, called a *renormalization*, of physical constants like the charge and mass of the electron.[7] Dyson was able to show (with some technicalities cleared up later by Salam[8] and me[9]) that in quantum electrodynamics and a limited class of other theories, the renormalization of a finite number of physical parameters would actually remove infinities in every order of perturbation theory – that is, in every term when we write any physical observable as an expansion in powers of the charge of the electron, or powers of similar parameters in other theories. Theories in which infinities are removed in this way are known as *renormalizable.* They can be recognized by the property that in renormalizable theories, in natural units in which Planck's constant and the speed of light are unity, all of the constants multiplying terms in the Lagrangian are just pure numbers, like the charge of the electron, or have the units of positive powers of energy, like particle masses, but not negative powers of energy.[10]

The great success of calculations in quantum electrodynamics using the renormalization idea generated a new enthusiasm for quantum electrodynamics. After this change of mood, probably most theorists simply didn't worry about having to deal with infinite renormalizations. Some theorists thought that these infinities were just a consequence of having expanded in powers of the electric charge of the electron, and that not only observables but even quantities like the "bare" electron charge (the charge appearing in the field equations of quantum electrodynamics) would be found to be finite when we

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[5] I. Waller, Z. f. Physik 59, 168 (1930); 61, 721, 837 (1930); 62, 673 (1930)

[6] J. R. Oppenheimer, Phys. Rev. 35, 461 (1930).

[7] See articles by Bethe, Dyson, Feynman, Kramers, Lamb & Retherford, Schwinger, Tomonaga, and Weisskopf reprinted in *Quantum Electrodynamics*, ed. J. Schwinger (Dover Publications, Inc., New York, 1958).

[8] A. Salam, Phys. Rev. 82, 217 (1951).

[9] S. Weinberg, Phys. Rev. 118, 838 (1959).

[10] The units of these constants of course depend on the units we assign to the field operators. In using this criterion for renormalizability, it is essential to use units for any field operator related to the asymptotic behaviour of its propagator; if the propagator goes like k^n for large four-momentum k, then the field must be assigned the units of energy to the power $n/2 + 2$. In particular, because of $k^\mu k^\nu/(k^2 + m^2)$ terms in the propagator of a massive vector field, for these purposes the field must be given the unconventional units of energy to the power +2, and any interaction of the field would be non-renormalizable, unless the field is coupled only to conserved currents for which the terms in the propagator proportional to $k^\mu k^\nu$ may be dropped.

learned how to calculate without perturbation theory. But at least two leading theorists had their doubts about this, and thought that the appearance of infinite renormalizations in perturbation theory was a symptom of a deeper problem, a problem not with perturbation theory but with quantum field theory itself. They were Lev Landau, and Gunnar Källén.

Källén's first step in exploring this problem was in an important 1952 paper,[11] in which he showed how to define quantities like the bare charge of the electron without the use of perturbation theory. To avoid the complications that arise from the vector nature of the electromagnetic field, I'll describe the essential points here using the easier example of a real scalar field $\varphi(x)$, studied a little later by Lehmann.[12] The quantity $-i\,\Delta'(p)$ known as the propagator, that in perturbation theory would be given by the sum of all Feynman diagrams with two external lines, carrying four-momenta p^μ and $-p^\mu$, can be defined without the use of perturbation theory by

$$\Big\langle 0\Big| T\{\varphi(x)\,,\;\varphi(0)\Big\}\Big|0\Big\rangle = -i\int \frac{d^4p}{(2\pi)^4}\Delta'(p)e^{ip\cdot x}\,, \tag{1}$$

where $|0\rangle$ is the physical vacuum state, and T denotes a time-ordered product, with $\varphi(x)$ to the left or right of $\varphi(0)$ according as the time x^0 is positive or negative. By inserting a complete set of states between the fields in the time-ordered product, one finds what has come to be called the Källén–Lehmann representation

$$\Delta'(p) = \frac{|N|^2}{p^2+m^2} + \int \frac{\sigma(\mu)\,d\mu}{p^2+\mu^2}\,, \tag{2}$$

where $\sigma(\mu^2)\geq 0$ is given by a sum over multiparticle states with total energy-momentum vector P^λ satisfying $-P^2=\mu^2$, and N is defined by the matrix element of $\varphi(x)$ between the vacuum and a one-particle state of physical mass m and three-momentum $\mathbf{k}$:

$$\Big\langle 0\Big|\varphi(x)\Big|\mathbf{k}\Big\rangle = \frac{Ne^{ik\cdot x}}{(2\pi)^{3/2}\sqrt{2k^0}}\,, \tag{3}$$

with $k^0 \equiv \sqrt{\mathbf{k}^2+m^2}$. If $\varphi(x)$ is the "unrenormalized" field that appears in the quadratic part of the Lagrangian without any extra factors, then it satisfies the canonical commutation relation

$$\Big[\dot\varphi(\mathbf{x},t)\,,\;\varphi(\mathbf{y},t)\Big] = -i\delta^3(\mathbf{x}-\mathbf{y})\,. \tag{4}$$

[11] G. Källén, Helv. Phys. Acta 25, 417 (1952).
[12] H. Lehmann, Nuovo Cimento XI, 342 (1954).

By taking the time derivative of Eq. (1) and then setting the time x^0 equal to zero and using the commutation relation (4), one obtains the sum rule

$$1 = |N|^2 + \int \sigma(\mu)\, d\mu \; . \tag{5}$$

One immediate consequence is that, since $|N|^2$ is necessarily positive, Eq. (5) gives an upper limit on the coupling of the field φ to multiparticle states

$$\int \sigma(\mu)\, d\mu \leq 1 \; . \tag{6}$$

I'll mention in passing that this upper limit is reached in the case $N = 0$, which only applies if $\varphi(x)$ does not appear in the Lagrangian at all – that is, if the particle in question is not elementary. Thus, in a sense, composite particles are coupled to their constituents more strongly than any possible elementary particle.

This kind of sum rule has proved very valuable in theoretical physics. For instance, if instead of a pair of scalar fields in Eq. (1) we consider pairs of conserved symmetry currents, then by using methods similar to Källén's, one gets what are called a spectral function sum rules,[13] which have had useful applications, for instance in calculating the decays of vector mesons into electron–positron pairs.

What chiefly concerned Källén was the application of these methods to quantum electrodynamics. In his 1952 paper, Källén derived a sum rule like (5) for the electromagnetic field, with $Z_3 \equiv |N_\gamma|^2$ in place of $|N|^2$, where N_γ is the renormalization constant for the electromagnetic field. As in the scalar field theory, this sum rule (and the definition of Z_3 as an absolute value squared) shows that

$$0 \leq Z_3 < 1 \; . \tag{7}$$

This is especially important in electrodynamics, because Z_3 appears in the relation between the bare electronic charge e_B that appears in the field equations, and the physical charge e of the electron:

$$e^2 = Z_3\, e_B^2 \; . \tag{8}$$

The fact that e^2 is less than e_B^2 has a well-known interpretation: it is due to the shielding of the bare charge by virtual positrons, which are pulled out of the vacuum along with virtual electrons, and unlike the virtual electrons are attracted to the real electron whose charge is being measured.

[13] S. Weinberg, Phys. Rev. Lett. 18, 507 (1967).

Now, in lowest order perturbation theory, we have

$$Z_3 = 1 - \frac{e^2}{6\pi^2} \ln\left(\frac{\Lambda}{m_e}\right) , \tag{9}$$

where Λ is an ultraviolet cut-off, put in as a limit on the energies of the virtual photons. This is all very well if we take Λ as a reasonable multiple of the electron mass m_e, but if the cut-off is taken greater than $m_e \exp(6\pi^2/e^2) \approx 10^{280}\, m_e$ (which is more than the total mass of the observable universe) then we are in trouble: In this case Eq. (9) gives Z_3 negative, contradicting the inequality (7). As Landau pointed out,[14] this ridiculously large energy becomes much smaller if we take into account the fact that there are several species of charged elementary particles; for instance, if there are ν species of spin one-half particles with the same charge as the electron, then the factor 10^{280} is replaced with $10^{280/\nu}$. So if ν is, say, 10 or 20, the problem with the sign of Z_3 would set in at energies much closer to those with which we usually have to deal. But this is just lowest order perturbation theory – to see if there is really any problem, it is necessary to go beyond perturbation theory.

To explore this issue, Källén set out to see if the integral appearing in $1 - Z_3$, and not just its expansion in powers of e^2, actually diverges in the absence of a cut-off. Of course, he could not evaluate the integral exactly, but since every kind of multiparticle state makes a positive contribution to the integrand, he could concentrate on the contribution of the simplest states, consisting of just an electron and a positron – if the integral of this contribution diverges, then the whole integral diverges. In evaluating this contribution, he had to assume that all renormalizations including the renormalization of the electron mass and field were finite. With this assumption, and some tricky interchanges of integrations, he found that the integral for $1 - Z_3$ does diverge. In this way, he reached his famous conclusion that at least one of the renormalization constants in quantum electrodynamics has to be infinite.[15]

Not everyone was convinced. To quote the Källén memorial statement of Paul Urban in 1969,[16] "Indeed, other authors are in doubt about his famous proof that at least one of the renormalization constants has to be infinite, but so far no definite answer to this question has been found." It should be noted that at the end of his 1953 paper, Källén had explicitly disavowed any claim to mathematical rigor. As far as I know, this issue has never been settled. Of course, the important question was not whether some of the renormalization constants are infinite for infinite cut-off, but whether something happens at

[14] L. Landau, in *Niels Bohr and the Development of Physics* (Pergamon Press, New York, 1955): p. 52.
[15] G. Källén, Dan. Mat. Fys. Medd. 27, no. 12 (1953).
[16] P. Urban, Acta Physica Austriaca, Suppl. 6 (1969).

very high energies, such as $10^{280}\, m_e$, to prevent the cut-off in quantum electrodynamics from being taken to infinity. I don't know if Källén ever expressed an opinion about it, but I suspect that he thought that quantum electrodynamics does break down at very high energies, and that he wanted to be the one who proved it.

Which brings me back to the Lee model. This is a model with two heavy particles, V and N, and a lighter particle θ, all with zero spin. The only interactions in the theory are ones in which V converts to $N + \theta$, or vice versa. No antiparticles are included, and the recoil energies of the V and N are neglected, so the model is non-relativistic, though the energy ω of a θ of momentum $\mathbf{p}$ is given by the relativistic formula $\omega = \sqrt{\mathbf{p}^2 + m_\theta^2}$. The model is exactly soluble in sectors with just one or two particles. For instance, to find the complete amplitude for $V \to N + \theta$, one can sum the graphs for

$$V \to N + \theta \to V \to N + \theta \to V \to \cdots \to N + \theta \ ,$$

which is just a geometric series. One finds that, if the physical and bare V-particle states are normalized so that

$$\langle V, \text{phys}|V, \text{phys}\rangle = \langle V, \text{bare}|V, \text{bare}\rangle = 1 \ , \qquad (10)$$

then we have an exact sum rule resembling (5):

$$1 = |N|^2 + \frac{|g|^2}{4\pi^2} \int_0^\Lambda \frac{k^2\, dk}{\omega^3} \ , \qquad (11)$$

where

$$N \equiv \langle V, \text{bare}|V, \text{phys}\rangle \qquad (12)$$

Here Λ is again an ultraviolet cut-off, and g is the renormalized coupling for this vertex, related to the bare coupling g_B by the exact formula $g = N g_B$. For $\Lambda \gg m_\theta$, the integral in Eq. (11) grows as $\ln \Lambda$, so if $g \neq 0$ then Λ cannot be arbitrarily large without violating the condition that $|N|^2 \geq 0$. This is just like the problem encountered in lowest-order quantum electrodynamics, except that here there is no use of perturbation theory, and hence no hope that the difficulty will go away when perturbation theory is dispensed with.

Despite this difficulty, Lee found that his model with $\Lambda \to \infty$ gave sensible results for some simple problems, like the calculation of the energy of the V particle. In their 1955 paper, Källén and Pauli confronted the difficulty that $|N|^2$ then comes out negative, and recognized that for very large Λ this was necessarily a theory with an indefinite metric – that is, it is necessary to take all states with odd numbers of bare V particles with negative norm, while all other states with definite numbers of bare particles have positive norm. In particular, in place of (10), we must take $\langle V, \text{bare}|V, \text{bare}\rangle = -1$, while calculations show that the physical V state has positive norm, so that we can still normalize it so that $\langle V, \text{phys}|V, \text{phys}\rangle = +1$. (There is also another discrete energy eigenstate formed as a superposition of bare V and $N + \theta$ states, that has negative norm.) Then in place of (11), we have

$$1 = -|N|^2 + \frac{g^2}{4\pi^2} \int_0^{\Lambda} \frac{k^2\, dk}{\omega^3} \,, \tag{13}$$

which gives no problem for large Λ. The device of an indefinite metric had already been introduced by Dirac,[17] for reasons having nothing to do with infinities (Dirac was trying to find a physical interpretation of the negative energy solutions of the relativistic wave equations for bosons), and Pauli[18] had noticed that if we can introduce suitable negative signs into sums over states, it should be possible to avoid infinities altogether. I think that what Källén and Pauli in 1955 disliked about the indefinite metric was not that it solved the problem of infinities, but that it did so too easily, without having to worry about what really happens at very high energies and short distances, and this is why they took the trouble to show that it did lead to unphysical results in the Lee model.

Experience has justified Källén and Pauli's distrust of the indefinite metric. This device continues to appear in theoretical physics, but only where there is some symmetry principle that cancels the negative probability for producing states with negative norm by the positive probability for producing other unphysical states, so that the total probability of producing physical states still adds up to 100%. Thus, in the Lorentz-invariant quantization of the electromagnetic field by Gupta and Bleuler,[19] the state of a timelike photon has negative norm, but gauge invariance insures that the negative probability for the production of these unphysical photons with timelike polarization is canceled by the positive probability for the production of other unphysical photons, with longitudinal polarization. A similar cancelation occurs in the Lorentz invariant quantization of string theories, where the symmetry is conformal symmetry on the two-dimensional worldsheet of the string. But it

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[17] P. A. M. Dirac, Proc. Roy. Soc. A180, 1 (1942).
[18] W. Pauli, Rev. Mod. Phys. 15, 175 (1943).
[19] S. N. Gupta, Proc. Phys. Soc. 58, 681 (1950); K. Bleuler, Helv. Phys. Acta 28, 567 (1950).

seems that without any such symmetry, as in the Lee model, the indefinite metric does not work.[20]

I should say a word about where we stand today regarding the survival of quantum electrodynamics and other field theories in the limit of very high cut-off. The appropriate formalism for addressing this question is the renormalization group formalism presented by Wilson[21] in 1971. When we calculate the logarithmic derivative of the bare electron charge $e_{B\Lambda}$ with respect to the cut-off Λ at a fixed renormalized charge, then the result for $\Lambda \gg m_e$ can only depend on $e_{B\Lambda}$, since there is no relevant quantity with the units of energy with which Λ can be compared. That is, $e_{B\Lambda}$ satisfies a differential equation of the form

$$\Lambda \frac{de_{B\Lambda}}{d\Lambda} = \beta(e_{B\Lambda}) \; . \tag{14}$$

The whole question then reduces to the behavior of the function $\beta(e)$. If it is positive and increases fast enough so that $\int^{\infty} de/\beta(e)$ converges, then the cut-off in quantum electrodynamics cannot be extended to a value greater than a finite energy E_∞, given by

$$E_\infty = \mu \exp\left(\int_{e_{B\mu}}^{\infty} \frac{de}{\beta(e)} \right) \, , \tag{15}$$

with μ arbitrary. On the basis of an approximation in which in each order of perturbation theory one keeps only terms with the maximum number of large logarithms, Landau concluded in ref. 14 that quantum electrodynamics does break down at very high energy. In effect, he was arguing on the basis of the lowest-order term, $\beta(e) \simeq e^3/12\pi^2$, for which $\int^{\infty} de/\beta(e)$ does converge.

No one today knows whether this is the case. It is equally possible that higher-order effects will make $\beta(e)$ increase more slowly or even decrease for very large e, in which case $\int^{\infty} de/\beta(e)$ will diverge and $e_{B\Lambda}$ will just continue to grow smoothly with Λ. One might imagine that $\beta(e)$ could instead drop to zero at some finite value e_*, in which case $e_{B\Lambda}$ would approach e_* as $\Lambda \to \infty$, though there are arguments against this.[22] Lattice calculations (in which space-time is replaced by a lattice of separate points, providing an ultraviolet cut-off

[20] It has been argued that the PT symmetry of the Lee model allows the definition of a scalar product for which the theory is unitary; see C. M. Bender, S. F. Brandt, J-H Chen, and Q. Wang, Phys. Rev. D 71, 025014 (2005); C. M. Bender and P. D. Mannheim, Phys. Rev. D 78, 025022 (2008).

[21] K. G. Wilson, Phys. Rev. B4, 3174, 3184 (1971); Rev. Mod. Phys. 47, 773 (1975).

[22] S. L. Adler, C. G. Callan, D. J. Gross, and R. Jackiw, Phys. Rev. D6, 2982 (1972); M. Baker and K. Johnson, Physica 96A, 120 (1979); P. C. Argyres, M. Ronen, N. Seiberg, and E. Witten, Nucl. Phys. B461, 71 (1996).

equal to the inverse lattice spacing) indicate that the beta function for a scalar field theory with interaction $g_B \varphi^4$ increases for large g_B fast enough so that $\int dg_B/\beta(g_B)$ converges and the theory therefore does not have a continuum limit for zero lattice spacing.[23] And in the Lee model without an indefinite metric, Eq. (11) together with the relation $g_B = g/N$ gives

$$\beta(g_{B\Lambda}) \equiv \Lambda \frac{dg_{B\Lambda}}{d\Lambda} = \frac{g_{B\Lambda}^3}{8\pi^2}$$

for $\Lambda \gg m_\theta$, so $\int^\infty dg/\beta(g)$ converges, and as we have seen, the cut-off cannot be taken to infinity.

If limited to quantum electrodynamics, the problem of high energy behavior has become academic, since electromagnetism merges with the weak interactions at energies above 100 GeV, and we really should be asking about the high energy behavior of the $SU(2)$ and $U(1)$ couplings of the electroweak theory. Even that is somewhat academic, because gravitation becomes important at an energy of order 10^{19} GeV, well below the energy at which the $SU(2)$ and $U(1)$ couplings would become infinite. And there is no theory of gravitation that is renormalizable in the Dyson sense – the Newton constant appearing in General Relativity has the units of an energy to the power -2.

Källén's concern with the problems of quantum field theory at very high energy did not keep him from appreciating the great success of quantum electrodynamics. In a contribution to the 1953 Kamerlingh Onnes Conference,[24] he remarked that "there is little doubt that the mathematical framework of quantum electrodynamics contains something which corresponds closely to physical reality." He did practical calculations using perturbation theory in quantum electrodynamics, on problems such as the vacuum polarization in fourth order[25] and the radiative corrections to decay processes.[26] He wrote a book about quantum electrodynamics,[27] leaving for the very end of the book his concern about the infinite value of renormalization constants.

Källén's interests were not limited to quantum electrodynamics. In 1954 he showed that the renormalizable meson theory with pseudoscalar coupling could not be used to account for both pion scattering and pion photoproduction, because different values of the pion-nucleon coupling constant are

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[23] For a discussion and references, see J. Glimm and A. Jaffe, *Quantum Physics – A Functional Integral Point of View*, 2nd ed. (Springer-Verlag, New York, 1987), Sec. 21.6; R. Fernandez, J. Fröhlich, and A. D. Sokal, *Random Walks, Critical Phenomena, and Triality in Quantum Field Theory* (Springer-Verlag, Berlin, 1992), Chapter 15.
[24] G. Källén, Physica XIX, 850 (1953).
[25] G. Källén and A. Sabry, Dan. Mat. Fys. Medd. 29, no. 7 (1955).
[26] G. Källén, Nucl. Phys. B 1, 225 (1967).
[27] G. Källén, *Quantum Electrodynamics*, transl. C. K. Iddings and M. Mizushima (Springer-Verlag, 1972).

needed in the two cases.[28] Again, this result relied on lowest-order perturbation theory, so Källén acknowledged that it did not conclusively kill this meson theory. He remarked that "It would certainly be felt as a great relief by many theoretical physicists – among them the present author – if a definite argument against meson theory in its present form or a definite mathematical inconsistency in it could be found. This feeling together with wishful thinking must not tempt us to accept as conclusive evidence an argument that is still somewhat incomplete."

Of course, Källén was right in his dislike of this particular meson theory. A decade or so later the development of chiral Lagrangians showed that low energy pions are in fact well described by a theory with *pseudovector* coupling of single pions to nucleons, plus terms with two or more pions interacting with a nucleon at a single vertex, as dictated by a symmetry principle, chiral symmetry.[29] This theory is not renormalizable in the Dyson sense, but we have learned how to live with that. It is an effective field theory, which can be used to generate a series expansion for soft pion scattering amplitudes in powers of the pion energy. The Lagrangian for the theory contains every possible interaction that is allowed by the symmetries of the theory, but the non-renormalizable interactions whose coupling constants are negative powers of some characteristic energy (which is about 1 GeV in this theory) make a small contribution for pion energies that are much less than the characteristic energy. To any given order in pion energy, all infinities can be absorbed in the renormalization of a finite number of coupling parameters, but we need more and more of these parameters to absorb infinities as we go to higher and higher powers of pion energy.

My own view is that all of the successful field theories of which we are so proud – electrodynamics, the electroweak theory, quantum chromodynamics, and even General Relativity – are in truth effective field theories, only with a much larger characteristic energy, something like the Planck energy, 10^{19} GeV. It is somewhat of an accident that the simplest versions of electrodynamics, the electroweak theory, and quantum chromodynamics are renormalizable in the Dyson sense, though it is very important from a practical point of view, because the renormalizable interactions dominate at ordinary accessible energies. An effect of one of the non-renormalizable terms has recently been detected: An interaction involving two lepton doublets and two scalar field doublets generates neutrino masses when the scalar fields acquire expectation values.[30]

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[28] G. Källén, Nuovo Cimento XII, 217 (1954).

[29] For a discussion with references to the original literature, see Sec. 19.5 of S. Weinberg, *The Quantum Theory of Fields*, Vol. II (Cambridge Univ. Press, 1996.)

[30] S. Weinberg, Phys. Rev. Lett. 43, 1566 (1979).

None of the renormalizable versions of these theories really describes nature at very high energy, where the non-renormalizable terms in the theory are not suppressed. From this point of view, the fact that General Relativity is not renormalizable in the Dyson sense is no more (or less) of a fundamental problem than the fact that non-renormalizable terms are present along with the usual renormalizable terms of the Standard Model. All of these theories lose their predictive power at a sufficiently high energy. The challenge for the future is to find the final underlying theory, to which the effective field theories of the standard model and General Relativity are low-energy approximations.

It is possible and perhaps likely that the ingredients of the underlying theory are not the quark and lepton and gauge boson fields of the Standard Model, but something quite different, such as strings. After all, as it has turned out, the ingredients of our modern theory of strong interactions are not the nucleon and pion fields of Källén's time, but quark and gluon fields, with an effective field theory of nucleon and pion fields useful only as a low-energy approximation.

But there is another possibility. The underlying theory may be an ordinary quantum field theory, including fields for gravitation and the ingredients of the Standard Model. Of course, it could not be renormalizable in the Dyson sense, so to deal with infinities every possible interaction allowed by symmetry principles would have to be present, just as in effective field theories like the chiral theory of pions and nucleons. But it need not lose its predictive power at high energies, if the bare coupling constants $g_n(\Lambda)$ for an ultraviolet cut-off Λ (multiplied by whatever positive or negative powers of Λ are needed to make the g_n dimensionless) approach a fixed point g_{n^*} as $\Lambda \to \infty$.[31] This is what happens in quantum chromodynamics, where $g_* = 0$, and in that case is known as asymptotic freedom.[32] In theories involving gravitation it is not possible for all the g_{n^*} to vanish. In this more general case where g_{n^*} is not necessarily zero, the approach to a fixed point is known as "asymptotic safety," because the theory is safe from the danger that dimensionless couplings like $g_{\text{grav}} = G\,\Lambda^2$ (where G is Newton's constant) might run off to infinity as Λ goes to infinity.

For asymptotic safety to be possible, it is necessary that $\beta_n(g_*) = 0$, where $\beta_n(g(\Lambda)) \equiv \Lambda\, dg_n(\Lambda)/d\,\Lambda$. It is also necessary that the coupling constants $g_n(\Lambda)$ at any finite cut-off lie on a trajectory in coupling constant space that is attracted rather than repelled by this fixed point. There are reasons to ex-

[31] S. Weinberg, in *Understanding the Fundamental Constituents of Matter – 1976 Erice Lectures*, ed. A. Zichichi (Plenum Press); and in *General Relativity*, ed. S. W. Hawking and W. Israel (Cambridge University Press, 1979) 790.

[32] D. J. Gross and F. Wilczek, Phys. Rev. Lett. 40, 1343 (1973); H. D. Politzer, Phys. Rev. Lett. 30, 1346 (1973).

pect that, even with an infinite number of coupling parameters, the surfaces spanned by such trajectories have finite dimensionality, so such a theory would involve just a finite number of free parameters, just as for ordinary renormalizable theories. The trouble, of course, is that there is no reason to expect the g_{n^*} to be small, so that ordinary perturbation theory can't be relied on for calculations in asymptotically safe theories. Other techniques such as dimensional continuation,[33] $1/N$ expansions,[34] lattice quantization,[35] and the truncated "exact" renormalization group equations,[36] have provided increasing evidence that gravitation may be part of an asymptotically safe theory.[37] So it is just possible that we may be closer to the final underlying theory than is usually thought.

Källén continued his interest in general elementary particle physics, and wrote a book about it, published in 1964.[38] Arthur Wightman quoted a typical remark about this book: "That is the book on elementary particles that experimentalists find really helpful." But Källén's timing was unlucky – the development not only of chiral dynamics but also of the electroweak theory were then just a few years in the future, and they were to put many of the problems he worried about in a new perspective.

It was a tragic loss not only to his friends and family but also to all theoretical physics that Källén died in an airplane accident just 42 years ago. For me, this was specially poignant, because he had been so kind to me in Copenhagen, and yet we had become estranged. Some time in 1957, just before I finished my graduate work, Källén visited Princeton, and left a note in my mail box.

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[33] S. Weinberg, ref. 31 (1979); H. Kawai, Y. Kitazawa, & M. Ninomiya, Nucl. Phys. B 404, 684 (1993); Nucl. Phys. B 467, 313 (1996); T. Aida & Y. Kitazawa, Nucl. Phys. B 401, 427 (1997); M. Niedermaier, Nucl. Phys. B 673, 131 (2003) .

[34] L. Smolin, Nucl. Phys. B208, 439 (1982); R. Percacci, Phys. Rev. D 73, 041501 (2006).

[35] J. Ambjørn, J. Jurkewicz, & R. Loll, Phys. Rev. Lett. 93, 131301 (2004); Phys. Rev. Lett. 95, 171301 (2005); Phys. Rev. D72, 064014 (2005); Phys. Rev. D78, 063544 (2008); and in *Approaches to Quantum Gravity*, ed. D. Oríti (Cambridge University Press).

[36] M. Reuter, Phys. Rev. D 57, 971 (1998); D. Dou & R. Percacci, Class. Quant. Grav. 15, 3449 (1998); W. Souma, Prog. Theor. Phys. 102, 181 (1999); O. Lauscher & M. Reuter, Phys. Rev. D 65, 025013 (2001); Class. Quant. Grav. 19. 483 (2002); M. Reuter & F. Saueressig, Phys Rev. D 65, 065016 (2002); O. Lauscher & M. Reuter, Int. J. Mod. Phys. A 17, 993 (2002); Phys. Rev. D 66, 025026 (2002); M. Reuter and F. Saueressig, Phys Rev. D 66, 125001 (2002); R. Percacci & D. Perini, Phys. Rev. D 67, 081503 (2002); Phys. Rev. D 68, 044018 (2003); D. Perini, Nucl. Phys. Proc. Suppl. C 127, 185 (2004); D. F. Litim, Phys. Rev. Lett. **92**, 201301 (2004); A. Codello & R. Percacci, Phys. Rev. Lett. 97, 221301 (2006); A. Codello, R. Percacci, & C. Rahmede, Int. J. Mod. Phys. A23, 143 (2008); M. Reuter & F. Saueressig, 0708.1317; P. F. Machado and F. Saueressig, Phys. Rev. D77, 124045 (2008); A. Codello, R. Percacci, & C. Rahmede, 0805.2909; A. Codello & R. Percacci, 0810.0715; D. F. Litim 0810.3675; H. Gies & M. M. Scherer, 0901.2459; D. Benedetti, P. F. Machado, & F. Saueressig, 0901.2984, 0902.4630; M. Reuter & H. Weyer, 0903.2971.

[37] For reviews see M. Niedermaier & M. Reuther, Living Rev. Relativity 9, 5 (2006); M. Niedermaier, Class. Quant. Grav. 24, R171 (2007); M. Reuter and F. Saueressig, 0708.1317; R. Percacci, in *Approaches to Quantum Gravity*, ed. D. Oríti (Cambridge University Press).

[38] G. Källén, *Elementary Particle Physics* (Addison-Wesley, Reading, MA. 1964).

Apparently he had seen a draft of my Ph. D. thesis, which was about the use of renormalization theory to deal with strong interaction effects in weak decay processes. His note seemed angry, and said that my work showed all the misconceptions about quantum field theory that were then common. Well, my thesis was no great accomplishment, but I didn't see why he was angry about it. Maybe he was annoyed that I was following the common practice, of not worrying about the fact that the renormalization constants I encountered were infinite. Torsten Gustafson[39] has said of Källén that "Like Pauli he often expressed his opinion in a provocative fashion – especially to well-known physicists." I certainly was not a well-known physicist, but maybe Källén was paying me a compliment by treating me like one.

I did not meet Källén again after this, and I never replied to his note. I regret that very much, because I think that if I had replied we could have understood each other, and been friends again. Perhaps this talk can substitute for the reply to Källén that I should have made half a century ago.

I am grateful to C. Jarlskog and the Källén Lecture Committee for inviting me to Lund to give this talk, and to the Gunnar and Gunnel Källén Memorial Fund of the Royal Physiographic Society for sponsoring it. This material is based in part upon work supported by the National Science Foundation under Grant No. PHY-0455649 and with support from The Robert A. Welch Foundation, Grant No. F-0014.

[39] T. Gustafson, Nucl. Phys. A140, 1 (1970).

62

The Royal Swedish Academy of Sciences and Nobel Guest Professor

The Royal Swedish Academy of Sciences (hereafter referred to as the Academy) was founded in 1739 and on 25 May 1966 Källén was elected as the member number 1036 of this, by then 227 years old, body. In fact, the Academy started off by being a rather exclusive and slowly changing society. For example, its members of Nobel Committees (for physics and chemistry) had life tenure. Nowadays, the situation is completely different and the number of newly elected members per unit time has increased dramatically. In 1968, Källén became an auxiliary member of the Nobel Committee for Physics (hereafter referred to as the Nobel Committee). To become an ordinary member required having an open slot which didn't happen so often. When I (CJ) was a member of this committee (1989–2000) the mandate period had been shortened to 12 years. Since then it has been further reduced to nine years.

The archives of the Academy show that Källén took his duties at the Academy very seriously and was often present at the meetings. These meeting, that took place in Stockholm, would give him the additional pleasure of piloting a small plane from Malmö in the South of Sweden to Stockholm, back and forth. The last meeting that Källén attended took place on 11 October, 1968, i.e., only two days before his death.

One of the five members of the Nobel Committee at that time was Ivar Waller (1898–1991). Due to his breath and depth of knowledge, supplemented by his domineering personality, he was considered as a great authority in physics, on the Swedish scene. He had been elected into the Academy in 1945 and had immediately become a member of the Nobel Committee, a position which he held for 28 years (1945–1972). In his Nobel work, he had hesitation as his trade mark and wanted to check everything and make sure that no wrong decisions were ever made. I (CJ) remember vividly a visit he made to CERN where he cross-examined many scientists. Rudolf Mössbauer gave once another example of "Waller in action" in a story that he told me. He and Waller were participating in a conference. Waller would join Mössbauer all the time asking him questions about his discovery (the Mössbauer effect). After one evening meal, the then 32 years old Mössbauer turned to Waller and said: Professor Waller, tomorrow, at the breakfast table, I don't want to see you! Mössbauer received the Nobel Prize later that year.

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Källén performance on the Nobel Committee must have been very impressive. Arne Magnéli (1914–1996), who had been the secretary of both Nobel committees for physics and chemistry for several years, summarized it [1] by : Källén got Waller to shut up. He knew more than Waller on every topic which was discussed (see also Chap. 12).

Immediately after Källén's death, the Academy took action to honor his memory. On the 11 November 1968, i.e., less than a month after his death, the Nobel Committee suggested that a memorial fellowship fund be created to honor him. Only two of the five members (the Chairman Erik Rudberg and Ivar Waller) were present during the meeting. However, instead another proposal was implemented as follows. Källén's unexpected death had created a scientific vacuum in Lund. In order to remedy the damage as much as possible, Torsten Gustafson proposed (on the 27 December 1968) that the Academy use her contacts with her sister Academy in Moscow and invite distinguished theorists from the Soviet Union to come to Lund as "Nobel guest professors" for one year at a time. They were to be financed by the Nobel Institute. This proposal was very quickly supported by the physics class of the Academy and approved by the Academy in plenum. Already on the 22 January 1969, i.e., less than a month after the proposal was made, it was decided that the Academy should ask her sister Academy in Moscow to allow one of the following three theoretical physicists L. D. Faddeev, V. N. Gribov and L. B. Okun, to come to Sweden, as the Nobel Guest Professor.

A clear "njet" answer from Moscow came very quickly – none of the suggested scientists were acceptable to the USSR Academy. Ludwig Faddeev, asked years later by me (CJ) whether he had known Källén, recalled [2]:

> "As about Källén, I knew his name, of course, because of Källén – Lehmann and also his fight with Landau, but I never met him. Incidentally, after his tragic death, Swedish govt. asked Soviet govt. to send a Russian professor to Lund. They asked for Gribov or me. But it was Shirkov, who got the place … I was not known at home.
>
> With best wishes, Ludwig"

Indeed Professor Dimitry V. Shirkov did come to Lund as Nobel guest professor (1970–1971). What impressed me (CJ) most was that he was wearing training suit at work! To see a professor in such outfit was well beyond what one expected in the then formal academic environment in Sweden where professors wore ties, suits and often vests. Shirkov was ahead of his time! I also remember the very first time I came to work wearing trousers – it was 1966 – Källén who happened to see me in the corridor looked utterly surprised and exclaimed: are you going horseback riding?

Times have changed!

References

1. Private communication to CJ at about 1990
2. L. D. Faddeev, email communication to CJ, April 2009

63

Källén and the Ettore Majorana Centre in Erice

Gunnar Källén was genuinely interested in the education of young people. He took his teaching duties very seriously and seemed to enjoy them. His lecture notes were typed and made available to students. Phrases such as "it can be shown", "it has been found that" did not exist in his frame of reference. He would start from the "fundamentals" and successively build up the required theoretical framework. The students were expected to have a solid theoretical ground to stand on. He gave courses on classical mechanics, electricity and magnetism, quantum mechanics, etc., never giving the same course twice. Therefore, the students often had to learn the material by themselves, consulting his lecture notes and those few books that were available, such as Dirac's book on quantum mechanics. Källén wrote a large number of recommendation letters for his students to attend "summer schools", to acquire further knowledge. Therefore, he was very supportive of the establishment of the School of physics at Erice, Italy, that later on developed into the "Ettore Majorana Centre in Erice" [1].

Professor Antonino Zichichi[1], the initiator of the Centre, has this to say about Källén [2]:

> "He was also very helpful in his support for my project for the Ettore Majorana Centre in Erice. As you probably know he was supposed to sign the basic act of establishing the Centre. Unfortunately though, last minute problems arose, and since he could not be at CERN when Rabi, Blackett and Weisskopf signed the paper, the other two young fellows John Bell and myself, signed. Your late Professor should have been one of the six signatories. For this reason I established a Scholarship in his honour in 1969, the first year of the International School of Subnuclear Physics, following his demise. As Enrico Fermi said:
>
> 'Without memory there is neither science nor civilization.'"

[1] For biographical information see the homepage of "Ettore Majorana Foundation and Centre for Scientific Culture" on the internet. I indebted to Professor Antonino Zichichi for conveying to me the information about Erice and Källén, presented in this chapter.

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Professor Zichichi has kindly provided me (CJ) with the list of recipients of the Gunnar Källén Scholarship since its creation in 1969. The list is presented in Table 63.1.

References

1. For the history of the Center see http://www.ccsem.infn.it/em/history/index.html
2. A. Zichichi, private communication

Table 63.1 The recipients of Gunnar Källéns Scholarships and Diplomas at the International School of Subnuclear Physics

1969	C. A. **NELSON**	The City College of New York, NY, USA
1970	Jan **HLADKY**	Cekoslovenska Akademie Ved Praha, Czechoslovakia
1971	P. **MANNHEIM**	Universitïé Libre de Bruxelles, Belgium
1972	G. **BERLAD**	Technion Israel Inst. of Technology, Haifa, Israel
1974	Ferenc **NIEDERMAYER**	Eotvos University, Budapest, Hungary
1975	Laurence **JACOBS**	MIT, Cambridge, MA, USA
1976	Barbara **YOON**	MIT, Cambridge, MA, USA
1977	Hans P. **PAAR**	CERN, Geneva, Switzerland
1978	Guy **ANASTAZE**	Centre de Recherches Nucléaires, Strasbourg, France
1979	Giora **MIKENBERG**	DESY, Hamburg, Germany
1980	Yasu Nari **TOSA**	University of Rochester, NY, USA
1981	Hidenaga **YAMAGISHI**	Princeton University, NJ, USA
1982	Sunil **MUKHI**	ICTP, Trieste, Italy
1983	Sandra P. **KLEVANSKY**	University of Frankfurt, Germany
1984	Dieter **ISSLER**	LPThPE, Paris, France
1985	Hai-bin **ZHENG**	Niels Bohr Inst., Copenhagen, Denmark
1986	Steven **CARLIP**	University of Texas, Austin, TX, USA
1987	Elias **KIRITSIS**	California Inst. of Technology, Pasadena, CA, USA
1988	Raymond **VOLKAS**	University of Melbourne, Australia
1989	Bruno **ROSTAND**	École Normale Supérieure, Paris, France
1991	Anwarl **HASAN**	World Laboratory and CERN, Geneva, Switzerland
1992	Karim **BENAKLI**	École Politechnique, Paris, France
1993	Jeffrey **FORSHAW**	Rutherford Appleton Laboratory, Didcot, UK
1994	Grigori T. **GABADADZE**	Moscow State University, Russia
1995	Salvatore **ESPOSITO**	INFN, Napoli, Italia
1996	Bernd-Jochem **SCHAEFER**	Universität Heidelberg, Germany
1997	Sergei **DUBOVSKY**	INR-Inst. for Nuclear Research, Moscow, Russia
1998	Dirk **HOLTMANNSPOTTER**	Ludwig-Maximillians-Universität, Munich, Germany
1999	Daniel **BOER**	RIKEN-BNL, Upton, NY, USA
2000	Krzysztof **TURZYNSKI**	Warsaw University, Poland
2001	Richard **HILL**	Cornell University, Ithaca, NY, USA
2002	Axel **MAAS**	Technische Universität Darmstadt, Germany
2003	David **GALLETLY**	The University of Edinburgh, UK
2004	Angelo Raffaele **FAZIO**	JINR, Dubna, Russia
2005	Shoaib **MUNIR**	University of Southampton, UK
2006	Grigory **VARTANOV**	JINR, Dubna, Russia
2007	Alexander **ROTHKOPF**	Technische Universität Darmstadt, Germany
2008	Henrik **JOHANSSON**	UCLA, Los Angeles, CA, USA

Table 63.1 *Continued*

2009	Oleksandr **GAMAYUN**	Bogolyubov Inst. for Theoretical Physics, Kiev, Ukraine
2010	Vasyl **ALBA**	ITEP, Moscow, Russia
2011	Philipp **BURDA**	ITEP, Moscow, Russia
2012	Fabio **Colamaria**	INFN and University of Bari, Italy

Memorial Conferences, Møller's Talk and Comments about H. A. Kramers – a Preview

> "In the light of history Gunnar Källén's appearance in the world of physics was like a shooting star."
>
> Christian Møller

> "Without memory there is neither science nor civilization."
>
> Enrico Fermi (as quoted by A. Zichichi)

The following two international conferences were dedicated to the memory of Gunnar Källén:

- **Lund International Conference on Elementary Particles, June 25–July 1, 1969**;
- **Perspectives in Modern Field Theories, September 23–26, 1980**.

In the following chapters, we give a brief description of the first Conference, the purpose being to give our young readers (who weren't even born then) a flavor of what particle physics was like at the time of Källén. We reproduce a talk given by Christian Møller, a distinguished Danish physicist who worked in Copenhagen and knew Källén quite well. Källén's correspondence shows that he respected Møller very much.

The second conference was more technical. One of its most interesting features was a talk by Arthur S. Wightman about Källén's work. It is reproduced in chapter 66.

While writing this book, I (CJ) became curious about the well-known Dutch theorist Hendrik A. Kramers, whose name is often mentioned as a pioneer in renormalization theory. We have been fortunate to get a first-hand information about him from his student N. G van Kampen. After all the issue of renormalization was a central one for Källén. I have also examined the issue of Kramers as a Nobel Prize candidate.

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The 1969 Lund Conference on Particle Physics

In 1969, the biggest conference of the year on particle physics was held in Lund [1]. There were about 600 participants, from more than 30 different countries as well as several international organizations.

A special feature of the conference was that the rapporteurs were required by the organizers to produce the written version of their talks before leaving Lund. They each had an office and a scientific secretary to help them. This turned out to be hard work for some rapporteurs and their secretaries but at the end of the conference several rapporteurs were very happy. For example, Lucien Montanet from CERN, after having finished his written version, threw all the papers, preprints, etc., that he had been reporting on into the waste-paper basket and looked exceptionally pleased.

I (CJ) was the scientific secretary of two sessions on current algebra, a plenary session in which Bruno Renner was the rapporteur and a parallel session led by Bruno Zumino. Renner was working hard on his talk, with a box of sharpened pencils and a pile of blank sheets of paper in front of him. And a large pile of articles. I had to help him mostly by running to the library and finding references for him. During the conference a visit was organized, for all participants, to the company SAAB [2] in Linköping, about 400 kilometers from Lund – the purpose being to visit SAAB's production plant for scanning tables for bubble chamber pictures. I was eager to take part in the visit and therefore asked Bruno Renner if I may do so. Renner asked me: "Are you a potential buyer?" I had to admit that I was not. Renner then explained to me: "You must understand that these visits are intended for potential buyers." Indeed, Renner was a very special person, with strong principles, as I discovered later. I came to appreciate his opinions and honesty. Sadly, in 1973 he was killed in an Alpine accident, only 32 years old.

It may interest the younger generation to know what occupied the minds of the particle physicists in those days. The Proceedings of the conference give an incredible story: there were no gluons, no W or Z, no charm, beauty or top either, and of course none of the rapporteurs talked about Higgs. Some rapporteurs, however, mentioned the quark model, for example J. David Jackson [3]:

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> "One of the most peculiar phenomena in high energy physics is the continuing success of the 'realistic' quark model. The use of quarks as a mnemonic has widespread acceptance, but the idea of dynamic or even kinematic considerations with 'real' quarks leaves some segments of our community cold. Nonetheless, intrepid theorists push the model further and further."

The insignificant role played by quarks at the Lund Conference could be understood by taking a look at the Proceeding of the 1968 "Rochester Conference" in Vienna [4], where a rapporteur talk [5] had been devoted to the quark model. Quarks had been looked for but not found. However, in 1969, the "quark revolution" was just around the corner. For a historical account see, for example, the Nobel lectures by the 1990 Physics Laureates Friedman, Kendall and Taylor, who received the Prize for

> "their pioneering investigations concerning deep inelastic scattering of electrons on protons and bound neutrons, which have been of essential importance for the development of the quark model in particle physics".

It should also be mentioned that at the Lund Conference, no one mentioned the Standard Model of electroweak interactions, in spite of the fact that the relevant articles were published some years before. The conference was dominated by data on resonances and models to describe them.

Källén, who had been a leading member of the organizing committee of the conference, had passed away in October of the previous year. He as theorist, and the experimentalist Guy von Dardel (chairman of the conference) had been the major "poles of attraction" to Lund. The tragic absence of Källén was strongly felt during the conference. It was decided to dedicate the Conference to his memory.

The Lund Conference was opened by Christian Møller who spoke about Källén's science and personality (see the next chapter).

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1. Lund International Conference on Elementary Particles, June 25–July 1, 1969. Proceedings edited by G. von Dardel was printed by Berlingska Boktryckeriet (Lund, 1969)
2. SAAB stands for "Swedish Aeroplane Limited" (in Swedish: Svenska Aeroplan AB)
3. J. D. Jackson, Rapporteur talk at the Lund International Conference, *ibid* p. 67
4. Proceedings of the "14th International Conference on High-Energy Physics, Vienna, 28 August – 5 September, 1968", edited by J. Prentki and J. Steinberger
5. G. Morpurgo, *ibid*, p. 225

65
Christian Møller on Gunnar Källén

The Lund Conference [1] was opened by Christian Møller who gave the following illuminating talk [2] about Källén's science and personality:

> "This International Conference on Elementary Particles has been dedicated to the memory of Gunnar Källén. It seems appropriate that we should start the conference by recalling his important contributions to the development of many of the subjects of the conference.
>
> Gunnar Källén was born February 13, 1926, so he was only 42 years old when he died in the fatal airplane crash on October 13 last year. In spite of the short span of time in which he was active in physics he left behind him a large number (about 60) of original papers, conference reports, lecture notes, and monographs on many different subjects of modern physics, in particular in the domains of quantum electrodynamics, quantum field theory in general, and elementary particle physics. It will not be possible, and in this circle also not necessary, to mention all these papers here today, but I shall try to give an outline of his main contributions to our science in the different stages of his nineteen years of activity in physics.
>
> As so many other physicists he started his career as an engineer. Twenty-two years old he came to Lund to pursue his studies of theoretical physics at the University. With amazing speed he caught up with the problems and soon he was working at the front line of our knowledge at that time. The main subject of interest among the theoretical physicists in Lund and elsewhere at that time was the new method in quantum electrodynamics which was initiated by Kramers in 1947, and which seemed to make it possible to evade the disturbing divergence difficulties, inherent in the formalism of quantum electrodynamics, by a renormalization procedure. In the following years this program was successfully carried through by Tomonaga, Schwinger and Feynman by making use of the so-called interaction picture. Källén was fascinated by this difficult subject and by the challenge it represented. His first paper appeared in the Helv. Phys. Acta in 1949. It contained a treatment of the higher approximations in the vacuum polarization. This problem was suggested to him, during a short visit in Zurich, by Wolfgang Pauli who was much impressed by the young student's quick and independent mind. Källén, on the other side, admired Pauli immensely and took him as a model for his future

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work. The relations between the older and the young physicist developed into a life-long warm friendship, which also led to a fruitful collaboration between them in the later years.

After his return to Lund, Källén set himself the task to carry through the renormalization program without the use of the interaction representation which he regarded as an unnecessary mathematical complication. In a series of papers leading up to his Inaugural Dissertation in 1950, he was able to show that the ideas of renormalization can easily be formulated in the original Heisenberg picture, and that many of the calculations are simpler and their physical interpretation more transparent in this picture. In these papers the notions of free 'in'- and 'out'-fields were defined clearly for the first time, and a method was developed which in the literature often has been called the Yang-Feldman method. The reason for this is probably that Yang's and Feldman's paper appeared in the Phys. Rev., while Källén's first paper on the subject was published in Ark. f. Fysik. Since these papers appeared nearly simultaneously and were produced independently, there is no room for any priority claims (and Gunnar would have been the last to make such claims), but one thing is certain: Källén made much more extensive use of his method for practical calculations, and soon he was also recognized by his colleagues everywhere as a master in his field. His brilliant appearance at international conferences, starting with the Paris Conference in the spring of 1950, contributed much to this. His elegant way of presenting his points of view and his sharp and witty dialogue in the discussions made him an excellent advocate for his ideas, which evoked the admiration of his older and younger colleagues. One of the latter was A. S. Wightman who later wrote about the early work of Källén: 'At that time I was trying to puzzle out the grammar of the language of quantum field theory, and here was Källén already writing poetry in the language'.

Gunnar Källén's connection with CERN dates back to the very first years of this organization. Already in 1952, when the site in Meyrin still consisted of a collection of deep holes in the ground and a few shacks, Källén became a Fellow of CERN's Theoretical Study Group, which at that time was placed at the Niels Bohr Institute in Copenhagen. I remember vividly his appearance there, which brought exciting new life to our group. He gave a series of admirable lectures on Quantum Electrodynamics, which clearly showed his superior mastery of the field and his exceptional gifts as a lecturer. Simultaneously, he pursued with characteristic energy a plan which he had conceived after the completion of his Doctor's thesis.

The current renormalization theory was based on a series expansion in powers of the fine structure constant and, although each term in this expansion was finite and showed a surprisingly good agreement with the experimental results, the convergence of the series had not been proved. Thus, it was still an open question whether Renormalized Quantum Electrodynamics could be regarded as a consistent physical theory or whether it only represented a handy cookery-book prescription for getting useful results. The answer to this question was of great principle importance, but also so difficult to obtain that it required all the courage and tenacity of a Källén to attack and finally solve the problem. By means of the formulation of the renormalization theory he had given in his thesis, he was able to define the renormalization constants without making use of perturbation theory. In a series of papers in Helv. Phys. Acta and in Physica he showed how this can be done and, in a final paper in the Proceedings of the Danish Academy (which later was reprinted in special collections both in Japan and U.S.A.) he proved that at least one of the renormalization constants had to be infinite. Thus, he had come to the conclusion that Renormalized Quantum Electrodynamics could not be regarded as a completely satisfactory physical theory, in spite of the success of the perturbation theory version of the theory in accounting for the experimental results. On the other hand, the latter circumstance gives good reason for believing that the present formalism may be regarded as a limiting case of a future more complete theory.

Källén was a fellow at CERN's theoretical Study Group from October 1st, 1952 to June 15th, 1953. During this period, his professional ability and his personality had impressed us so much that we naturally tried to get him on the permanent staff of the Study Group. After he had finished a second longer stay in Zurich he joined our staff in October 1954 where he remained until CERN's Theoretical Study Group finally moved to Geneva in September 1957. Thereafter, he accepted a chair as professor at the simultaneously established NORDITA in Copenhagen, where he stayed until a personal professorship was created for him at the University of Lund at the end of 1958.

Thus, we had the privilege of having Gunnar with us as collaborator in Copenhagen during more than five of his perhaps most productive years. It is impossible in a few words to describe how much we owe him as a constant source of inspiration, as a teacher, and last but not least as an always alert critic. The ruthless honesty and objectivity of his criticism, which soon became legendary, recalled that of the young Pauli. It has even been said that Gunnar modeled his style on Pauli, but this was only partly true. I rather think that the similarity in their reactions was due to an inherent kinship of these two original personalities. In Zurich Källén and Pauli had started a fruitful collaboration which was continued after Gunnar's arrival in Copenhagen. It resulted in a paper 'On the mathematical structure of T. D. Lee's model of a renormalizable field theory' which was published in the Proc. of the Dan. Academy in 1955. Although the Lee-model is non-relativistic, it is of great interest as an illustration of what might be hidden in the more complicated formalism of Quantum Electrodynamics. The advantage of the model is that it contains a renormalization of both the coupling constant and the mass, and still is so simple that it allows exact solutions. The main result in the just mentioned paper was the surprising discovery that the renormalized Lee model contains an unphysical state – the 'ghost'-state – which has a negative probability. It is quite possible that the formalism of Renormalized Quantum Electrodynamics also contains such unphysical states which further supports the view that Quantum Electrodynamics, when taken literally, does not represent a consistent physical theory. A fortiori this holds for the current meson theories which, for obvious reasons, do not lend themselves to a perturbation treatment.

Therefore, in the following years and in particular after his return to Lund, Källén joined in the trend of research, which has been called the axiomatic way, and which was being pursued at several places in Europe and America. Instead of investigating the properties of a definite formalism, the idea was to see how far one can go by starting from a few general physically necessary requirements, such as relativistic invariance, causality and positive energy. With his usual energy Källén threw himself on this seemingly infinite problem, which consisted in investigating the mathematical properties of the vacuum expectation values of the product of an arbitrary number n of field operators. In collaboration with Wightman, Toll and several of his students in Lund he obtained many important results in particular for $n = 3$ and $n = 4$. However, the general n-point function turned out to be so complicated that the problem could only be partially solved. This is probably the only problem, attacked by Gunnar, which he had to leave without having obtained a complete solution. In parallel with these more mathematical investigations he also worked on problems which had more immediate physical applications. I am thinking of his calculations of the radiative corrections to weak interactions, in particular to the nucleon decay and the electromagnetic and nucleon form factors, a work which inspired his pupils in Lund to interesting investigations.

After Källén's appointment as a professor at the University of Lund, much of his time was occupied by teaching and guiding students, and his natural gifts as an educationalist were brought into full play. His review articles and monographs, in particular his article on Quantum Electrodynamics in the Handbuch der Physik (1958) and his book on Elementary Particle Physics (1964), are lasting witnesses of his pedagogical faculties. He gathered around him a large number of Swedish and foreign pupils, who benefited immensely by his profound knowledge, his lucid lectures and his objective criticism which fortunately was linked with a deep sense of humour. The latter most essential characteristic allowed him in the course of the years to develop a totally harmonious and well-balanced personality. A contributing factor in this respect was undoubtedly his happy family life with his charming children and his lovable wife Gunnel. She was an intelligent woman with a strong personality.

The bravery and courage, which she showed when her husband suddenly was taken away, aroused the admiration and compassion of all her friends. On the day of the funeral she said to my wife: 'After this I am not afraid of anything in the world, my only ardent wish is that I may keep my health'. As you know this wish was not fulfilled, only half a year later she followed her husband into the grave.

In the light of history Gunnar Källén's appearance in the world of physics was like a shooting star. It was short, but so ardent, so shining that his name will be remembered, not only by those who knew him personally or even had the good fortune to become his friends, but also by the coming generations of physicists. At this conference, where he would have played a central role, he will be sorely missed.

Let us rise and stand quietly a moment in the memory of Gunnar Källén."

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1. Lund International Conference on Elementary Particles, June 25–July 1, 1969. Proceedings edited by G. von Dardel (Berlingska Boktryckeriet, Lund, 1969)
2. Christian Møller, *ibid*, p. 7

Note added In the Källén Collection there is a letter from Møller to Källén dated 1 April 1966. Møller, in his capacity as the President of the Royal Danish Academy of Sciences, informs Källén that he has just been elected as a foreign member of that body.

66

A. S. Wightman: Looking Back at Quantum Field Theory

In 1980 an international symposium, dedicated to the memory of Gunnar Källén, was organized in Stockholm. The theme of the symposium was

PERSPECTIVES IN MODERN FIELD THEORIES

In the Preface to the Proceedings, the editors Bengt Nagel and Håkan Snellman write:

> "In the last decade, field theory has come to play a much more central role in fundamental physics than it did at the untimely death of Gunnar Källén in 1968. Gunnar Källén might not have fully approved of all the new branches and sometimes daring speculations in present day field theory. We think, however, that his penetrating mind and search for clarity would have continued to exert a healthy influence on its development. The Symposium is dedicated to his memory."

At the above symposium, Lamek Hulthén gave a short opening talk which he ended by stating:

> "The symposium on 'Perspectives in Modern Field Theories' is now opened. We are very fortunate that an old friend and collaborator of Gunnar Källén, Arthur Wightman, will give the memorial lecture. Dr Wightman!"

Here comes Dr Wightman's presentation!

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Looking Back at Quantum Field Theory

The Gunnar Källén Memorial Lecture

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"The people who live in a Golden Age usually go around complaining how yellow everything looks."

Randall Jarrell

Abstract

Some high-lights in the development of quantum field theory are presented.

An invitation to give a Memorial Lecture for Gunnar Källén provides a certain license for reminiscence. Since I was a friend and co-worker of Källén's, it is a pleasure for me to indulge myself in this respect. Nevertheless, I suspect that Gunnar himself would have been rather impatient with this sort of thing. "Get on with the physics", he would have said. It is clear from the rest of the program that we will. In the same spirit, after telling a few old tales, I will try to restate some of the questions that motivated Källén's work and describe what we have and haven't learned about the answers to them since. I will concentrate on general problems of quantum field theory because that was the area in which Källén made most of his original contributions.

1. Field theory in the fifties

To evoke the atmosphere in quantum field theory in the 1950's, let me compare it with the aftermath of a great football match (the triumph of renormalization theory in quantum electrodynamics – the achievement of Tomonaga, Schwinger, Feynman, Dyson et al.). A crowd of supporters, flushed with victory, piles boisterously onto a bus. Their names could be Salam, Källén, Gell-Mann, Low, Landau, Pomeranchuk, Lehmann, Symanzik, Zimmermann . . .) Nobody knows quite where the bus is going, but each passenger has a strong opinion about where it ought to be going. The bus driver isn't talking very much. It was in this rollicking atmosphere that Källén started work on quantum field theory in general and quantum electrodynamics in particular.

I am not overstating in using the adjective "boisterous" to describe the atmosphere. For most of those working on quantum field theory at the time, the boisterousness was intellectual. For Källén, it was also, on occasion, physical. I recall a seminar in Copenhagen given by Bernard Jouvet with Källén in the audience. Jouvet wrote on the blackboard an expression which provoked comment from Källén. Gunnar proceeded directly from his seat, over chairs and tables to reach the offending expression on the board.

Källén earned his spurs by calculating, at Pauli's instigation, the higher order corrections to vacuum polarization in an external field [1]. In so doing he found that by working directly in the Heisenberg picture, he could obtain the same results somewhat more easily than with Feynman-Dyson techniques. In the process, he introduced and used systematically what nowadays would be called the in-field of the electron [2]. Independently, Yang and Feldman obtained similar results [3]. They went farther, however, introducing the out-field and proving that the S-matrix may be defined by the equation $\psi^{\text{out}} = S^{-1}\,\psi^{\text{in}}\,S$. One of the virtues of this work by Källén, Yang and Feldman was that it is, in principle, independent of perturbation theory. Of course, to convince oneself that it really works one expands in the charge and compares the resulting perturbation series with those obtained by other methods.

In two important papers, Källén went farther by formulating the equations of quantum electrodynamics, including the definition of the renormalization constants, independently of perturbation theory [4, 5]. These papers are full of nuggets. For example, Källén introduced and exploited the spectral representations of the electron and photon propagators, using them to express the renormalization constants. By now we take these things so much for granted that it is difficult to recall how striking they appeared at the time. However, workers in the field were far from unanimous about the importance of a non-perturbative treatment. M. Gell-Mann was heard to remark that there is nothing worth knowing about quantum electrodynamics that cannot be learned from its perturbation series. That could be dismissed (and was) as an outrageous statement calculated to raise the hackles of the mathematically inclined. (How could you verify its truth or falsity without studying quantum electrodynamics non-perturbatively?) Nevertheless, it does give some flavor of the diversity of opinion. There was impatient dissatisfaction with the then existing understanding of the foundations of quantum electrodynamics, but the impatience was especially acute when it came to other people's understanding of the foundations. Probably one should not pay too much attention to such programmatic pronouncements on methods – having delivered himself of this one, Murray proceeded to write (with Francis Low) one of the most important papers of the 1950's on the non-perturbative behaviour of quantum electrodynamics [6].

Källén regarded his non-perturbative formulation as the first step toward an analysis of the consistency of the theory. He suspected, in fact, that the theory might be inconsistent. His joint work with W. Pauli on the Lee Model [7] was an effort to understand how such inconsistencies come about in a simple model. He expected to find analogous troubles in quantum electrodynamics. His paper on the infinity of the renormalization constants may be regarded as a preliminary to a general investigation of consistency [8]. I will return later to comment on his conclusions in more detail.

When I arrived in Copenhagen in the Fall of 1956, Källén was nearing the end of an investigation of a representation formula for the three-point function which he hoped to use in studying the consistency problem. I came with a copy of the Princeton Ph.D. thesis of D. Hall in hand. Hall had obtained a description of the boundary of the analyticity domain of the three-point function that follows from general principles of field theory. The region that followed from Källén's representation was much larger than that found by Hall. Now Hall and I knew that this was not necessarily a contradiction, because the holomorphy envelope of Hall's domain was definitely larger than the domain itself. Conceivable, the holomorphy envelope was just Källén's domain. That was settled in the negative by Jost and Lehmann, who produced a counter-example showing that the holomorphy envelope was *not* as large as Källén's domain. [9] (Jost had seen Hall's thesis in Princeton and had learned of Källén's representation from Pauli.) After some initial confusion caused by a different convention for the Minkowski metric in Zürich and Copenhagen, all parties agreed. Källén's representation could not follow from general principles; so he abandoned it. Meanwhile, I had considerably simplified Hall's formulae for the boundary and in January 1957 he and I sat down to try to compute the holomorphy envelope at Hall's domain. We were gluttons for punishment – it's not so easy to compute holomorphy envelopes for domains in three complex variables – but by August we had the result [10]. There was an interlude in the Spring during which Källén went to the Rochester High Energy Conference while I went on a trip to Naples. I mention this because who should show up in Rochester with the abandoned integral representation but Schwinger. I have been told that Källén pointed out the inadequacy of the representation with considerable vehemence. Poor Schwinger – he probably didn't realize that he was the victim of our frustration – at that time we still did not have the holomorphy envelope.

The computation of the domain of the holomorphy of the three-point function was only the first step in the representation problem. One had then to learn how to incorporate the restrictions arising from spectral conditions, and to get a usable representation formula for general functions satisfying the spectral condition and analytic in the domain. The former turned out to be quite difficult; there have been many contributions to a solution over the past twenty years. I will mention only [11] and [12]. A representation formula for functions analytic in the holomorphy domain was obtained by Källén and Toll [13]. It is a sum of terms all but one of which can be interpreted as weighted sums of contributions arising in the perturbation theory of some Lagrangian field theory models. The remaining term seems strictly non-perturbative. The obvious questions are: Does the non-perturbative term occur in the non-perturbative three-point function of a Lagrangian field theory? If not, does its vanishing follow from general non-linear conditions (structure analysis)? So far as I know these questions are open. If we are ever to know the precise status of multiple dispersion relations for the three-point function they will have to be answered. It seems however that they are not regarded as burning issues today.

After the holomorphy domain of the three-point function, what next? Clearly, the holomorphy envelope of the four-point function. By the time Källén tackled this, he was firmly established in Lund with a school of coworkers. A computer had replaced the slide rule that we had used for the three-point function. I will not try to summarize this work, but instead will give some representative references [14–16] and ask the question: Whatever came of all of this work on the holomorphy envelopes and representation formulae for the three- and four-point function? In my opinion, the answer is: Not much. The further important developments leading to the so-called axiomatic analyticity domain in S-matrix theory that one associates with such names as Bros, Epstein, Glaser, Stora, Martin, and others were not based on these results [17, 18]. Furthermore, in the work on the S-matrix important positivity restrictions arising from unitarity were introduced and exploited. To my knowledge, Källén never succeeded in using these representations to analyze the consistency of quantum electrodynamics. His last word on the subject appears to be his Schladming lectures of 1965 [19].

He was not much impressed by the developments in field theory in the early 1960's. In particular, Reggeology and Analytic S-Matrix Theory left him cold. My colleague Sam Trieman recalls meeting him at the High Energy Physics Conference sitting in a parallel session on experimental weak interactions. Sam said: "What are you doing here? Why aren't you in the field theory session?" Gunnar said: "I can't stand that stuff."

Let me complete these historical remarks by retelling an anecdote about Landau which bears on this subject. At the Kiev version of the High Energy Conference in 1959, I made it clear to my hosts that I regarded Landau along with the Kremlin as a National Monument, and eventually I was invited to converse with him. Landau immediately asked whether I believed that any non-trivial quantum field theories exist. I said that I reserved judgment on the question since the evidence was not convincing either way. Landau said that one could use the arguments of Landau and Pomeranchuk to conclude that only trivial theories exist but he thought that there was a much simpler and equally convincing argument as follows: Did I believe that the crux of the matter was in the large momentum behaviour of the Green's functions? Yes, I agreed. Did I think that anyone had conjectured a consistent large momentum behaviour? No, I didn't. Well then there was no non-trivial consistent large momentum behaviour because physicists are smart, and if there were a consistent behaviour they would have found it. Källén did not accept either of these arguments. He felt that they did not respect the possible subtlety of a theory which had up to then shown itself more clever than we. Are we better off today in this respect?

2. What have we learned

In the last two decades we have had a series of conceptual developments which have changed our perspective on quantum field theory. A partial list could include:

(1) Broken symmetry – Goldstone bosons;
(2) Higgs phenomenon in gauge theories;
(3) Confinement;
(4) Asymptotic freedom;
(5) Euclidean field theory and its connection with statistical mechanics;
(6) Constructive field theory;
(7) Renormalization group.

How could we have been so naive as to think we really understood something about quantum field theory in the 1950's? In

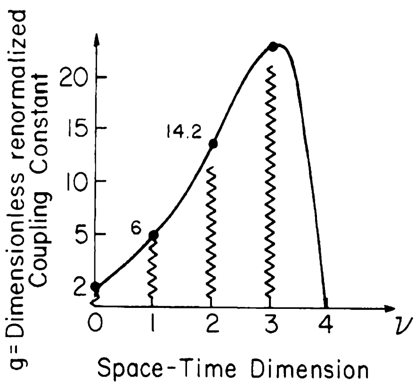

Fig. 1. Diagram representing the possible values of the renormalized coupling constant as a function of space-time dimensions in the $\lambda\phi^4$ theory.

the light of all this, what can we say about Källén's question of consistency? For simplicity, I want to discuss this question in the $\lambda\phi^4$ theory rather than in quantum electrodynamics and in space-time dimension ν varying continuously in the range $0 \leqslant \nu \leqslant 4$.

Figure 1 is a diagram representing the possible values of the renormalized coupling constant

$$g = -\frac{m^\nu \int dx_1\, dx_2\, dx_3 \langle 0 x_1 x_2\, x_3\rangle_T}{[\int dx_1 \langle 0 x_1\rangle]^2}$$

If the curve is denoted $g_w(\nu)$ then g takes the values $0 \leqslant g \leqslant g_w(\nu)$.

The zig-zag lines Fig. 1 represent the values for which constructive quantum field theory has obtained a solution. According to the conventional wisdom the curve $g_w(\nu)$ gives the value of the coupling constant in the scaling limit of the Ising model. (The subscript W is intended to record the contributions of K. Wilson on this subject.) Approximations to the curve from a theory of the strong coupling limit have been derived by Bender, Cooper et al. [20] A gap has been left between the constructive field theory values and the Ising values at $\nu = 2, 3$ because it still has not been proved for those dimensions that the upper bound of the coupling constant in the constructive field theory solutions coincides with that from the scaling limit Ising model. The values $g = 2$ at $\nu = 0$ and $g = 6$ at $\nu = 1$ have been independently derived by Bender et al. [20] and C. Newman [21]. I would like to repeat the arguments of C. Newman because they are so instructive.

In dimension $\nu = 0$, the Schwinger functions are just numbers, the moments of a positive measure $d\mu$ on the real line

$$S_n = \int x^n\, d\mu(x)$$

the x being the possible values of the Euclidean field. One may assume $d\mu$ normalized

$$\int d\mu(x) = 1$$

Furthermore, I want to consider the case in which the odd moments vanish (no symmetry breaking)

$$S_{2n+1} = 0, \quad n = 0, 1, 2, \ldots$$

Then the formula for g reduces to

$$g = -\frac{S_4 - 3[S_2]^2}{[S_2]^2} = 3 - \frac{S_4}{[S_2]^2}$$

and a lower bound on $S_4/[S_2]^2$ will yield an upper bound on g. Schwarz's inequality does just that

$$\left[\int x^2 \cdot 1\, d\mu(x)\right]^2 \leqslant \int x^4\, d\mu(x) \cdot \int 1^2\, d\mu(x)$$

i.e.,

$$[S_2]^2 \leqslant S_4$$

with equality if and only if x^2 and 1 are proportional almost everywhere with respect to $d\mu$. But that means, if we use the fact that odd moments vanish

$$d\mu(x) = \tfrac{1}{2}[\delta(x-a) + \delta(x+a)]$$

for some constant a, just the measure associated with the Ising model. For it and it alone g takes its largest possible value, 2.

In dimension $\nu = 1$, things are almost as simple. The symmetry of $\langle 0 x_1, x_2, x_3\rangle_T$ under permutations permits one to reduce the integration to a sector in which $\{0, x_1, x_2, x_3\}$ are in time order times a factor 4! for the number of sectors. The integrals over the time differences then convert operators $e^{-\tau H}$ into $1/H$. The result is this expression

$$g = 6m\, \frac{[(\Omega, \phi\, 1/H^2\, \phi\Omega)(\Omega, \phi\, 1/H\, \phi\Omega) - (\Omega, \phi\, 1/H\, \phi\, E_{>0}/H\, \phi\, 1/H\, \phi\Omega)]}{[(\Omega, \phi\, 1/H\, \phi\Omega)]^2}$$

Here the 6 is $4!/(2!)^2$, Ω is the ground state of H, $E_{>0}$ is the projection onto the orthogonal complement of the ground state, and $(\Omega, \phi\Omega) = 0$ has been assumed. Since $E_{>0}/H$ is non-negative dropping the second term in the numerator gives an upper bound for g

$$g \leqslant 6\, \frac{(1/\sqrt{H}\, \phi\Omega, m/H\, 1/\sqrt{H}\, \phi\Omega)}{\| 1/\sqrt{H}\, \phi\Omega \|^2}$$

and since m/H is an operator of norm $\leqslant 1$, $g \leqslant 6$. Equality holds in the first step when the negative discarded term is actually zero. Then

$$\phi\, 1/H\, \phi\Omega = a\Omega$$

for some constant a. For equality in the second step

$$m/H\, 1/\sqrt{H}\, \phi\Omega = 1/\sqrt{H}\, \phi\Omega$$

i.e.,

$$H\phi\Omega = m\phi\Omega$$

but then by virtue of the two conditions together Ω and $\phi\Omega$ span a two-dimensional space invariant under ϕ and H and we are again dealing with the Ising model, with $\phi^2 = am$. Thus $g = 6$ if and only if the theory is that of an Ising model.

These are the results and arguments of C. Newman. It is educational to try to extend them to $\nu = 2$. It turns out that it is possible to derive an analogue of the formula that worked for $\nu = 1$. It is

$$g = 6m^2\, \frac{[\iiint dx_1\, dx_2\, dx_3\, [(\Omega, \phi(0,0)\, 1/H^2\, \phi(0,x_2)\Omega)(\Omega, \phi(0,x_1)\, 1/H\, \phi(0,x_2)\Omega) - (\Omega, \phi(0,0)\, 1/H\, \phi(0,x_1) E_{>0}/H\, \phi(0,x_2)\, 1/H\, \phi(0,x_3)\Omega)]}{[\int dx_1 (\Omega, \phi(0,0)\, 1/H\, \phi(0,x_1)\Omega)]^2}$$

Here the x integrals run over the real line.

Because of the x integrations, this formula presents us with an entirely new problem. Before we could throw away the term with the minus sign and estimate the rest. Here each of the two terms in the numerator has a divergent integral and it is only after cancelling them that one comes to a finite result. So far no one has succeeded in finding an explicit expression for the difference of the two terms which is visibly integrable. If such an expression can be found and the conventional wisdom is correct, it should be possible to bound it by the result for the planar Ising model obtained by integrating the formulae of Wu and co-workers [22], and to see how, in the limiting case, the Hilbert space of states and the Hamiltonian are squeezed until they turn into those associated with the transfer matrix of the scaling limit planar Ising model. The problem is open both here and for $\nu = 3$, where recent developments have buttressed the view that the situation is similar after years in which there was considerable doubt [23].

It is a striking feature of the above arguments for $\nu = 0, 1$, that the results $g = 2$ and 6, respectively, are independent of the degree of the interaction; the range of possible values of the four-point coupling constant is completely independent of the presence of higher degree terms ϕ^{2n}, $n > 2$. Whether this holds for all higher dimensions and, if not, for which dimension the differences make themselves felt, is an interesting open problem.

Whether or not one can find a simple direct proof that the scaling limit Ising model yields the least upper bound of $g(\nu)$, the extraordinary fact remains that none of the solutions whose existence is suggested by statistical mechanics or constructed in constructive field theory to this date seems to lie above the curve $g_w(\nu)$, $0 \leqslant \nu \leqslant 4$, and all evidence points to $g_w(4) = 0$. Does this mean that the only consistent $\lambda\phi^4$ theory has $g = 0$ and therefore is a trivial free field theory? It appears that at the moment, most people would be pleased if the answer were yes, and, more generally, if the only theories with non-trivial solutions were asymptotically free. That would mean in particular that the quantum electrodynamics of spin 1/2 would also have no non-trivial solutions. This position seeks to put ignorance to work for us: we don't know much about such solutions, so they don't exist. I belong to the little band of those who are made queasy by arguments that proceed from the existing ignorance about renormalization theory to physical conclusions. (Usually this is stated succinctly: non-renormalizable means non-physical, but the terminology is tendentious. "Non-renormalizable" means in this connection that we don't know how to renormalize with standard methods and new methods are still under development.) I am old enough to remember that the same line of argument was used to eliminate pseudo-scalar meson theory with pseudo-vector coupling from consideration in pion-nucleon problems – a dubious ploy to say the least. If theories which are not asymptotically free have no solutions, let us understand exactly why.

There is one obvious possibility for theories above and to the right of the curve $g_w(\nu)$, $0 \leqslant \nu \leqslant 4$. Perhaps solutions exist which are more singular than those (tempered distribution solutions) heretofore considered. To point this out amounts only to spelling out Wilson's vision – the curve $g_w(\nu)$, $0 \leqslant \nu \leqslant 4$ defines an *ultraviolet phase transition*: the ultraviolet asymptotic behaviour of a theory changes as one crosses the curve. In the past few years there has been a series of developments in the general theory of quantized fields which has prepared the ground for an investigation of this possibility. I would like to call attention in particular to two classes of theories: strictly localizable theories in which tempered distributions are replaced by a family of ultra-distributions and hyperfunction theories where a class of hyperfunctions is used [24, 25]. In both cases, the general machinery of quantum field theory has been established: reconstruction theorem, relation between the Minkowski and Euclidean theory, spectral representation of the two-point function, Haag–Ruelle scattering theory.

If hyperfunction solutions of ϕ_ν^4 exist with $g > g_w(\nu)$, it seems that Newman's argument requires $\nu > 1$. Do such solutions first appear at $\nu = 1 + \epsilon$ or at $\nu = 2 + \epsilon$? It is tempting to try to link the appearance of singular solutions to the dimensional poles in Schwinger functions in perturbation theory arising from ϕ^{2n}, $n = 2, 3, 4, \ldots$ interactions. They have $\nu = 2$ as a limit point.

Until we have clear answers to such questions, it would be prudent to take the view that quantum field theories are smarter than we are – at any rate, most of us.

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On H. A. Kramers, Renormalization, and Nobel Prize

"Everyone" seems to mention the name of the distinguished Dutch physicist H. A. Kramers (1894–1952) [1] in connection with renormalization. This subject having been of utmost importance to Källén, it seems appropriate to take advantage of this occasion and if possible include some information about Kramers in this book. Indeed, time flies, and eventually those who had first hand knowledge of what happened will no longer be around to witness. In Chap. 65, Christian Møller (1904–1980), who knew Kramers well, reported:

> "… the new method [renormalization] in quantum electrodynamics which was initiated by Kramers in 1947, and which seemed to make it possible to evade the disturbing divergence difficulties, inherent in the formalism of quantum electrodynamics, by a renormalization procedure. In the following years this program was successfully carried through by Tomonaga, Schwinger and Feynman. …"

Källén also mentions Kramers's name in passing in his popular article on the 1965 Nobel Prize (see Chap. 14) and Weinberg refers to him as well (see Chap. 61).

What did Kramers actually do? I (CJ) contacted Professor Martinus Veltman who kindly put me in contact with Professor Nico G. van Kampen, a doctoral student of Kramers, who gave the following report:[1]

N. G. van Kampen

In classical physics the electron was regarded as a small sphere with an electric charge. This was worked out by H. A. Lorentz and he found that the electric field created by the charge contained a large energy when the radius of the sphere is small. To a first approximation this energy could be taken into account by adjusting the mass in the mechanical energy term $m c^2$. This is 'renormalization', but it was of course classical theory. The aim of H. A. Kramers was to extend this idea to quantum mechanics. He did some prelim-

[1] I wish to thank Professor van Kampen for sharing his knowledge with us. I am also indebted to Martinus (Tini) Veltman for his valuable suggestion and helping me to get in contact with Professor van Kampen.

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inary calculations in that direction, but then put them on my desk as a subject for my PhD thesis. This I finished conscientiously in 1951, but in the meantime Kramers had also talked about his idea at the Shelter Island conference in 1947, where it was picked up by H. A. Bethe and others. They applied the more modern methods of field theory, culminating in the computation of the Lamb shift. This is described by the other contributions to this volume.

Kramers and the Nobel Prize

I (CJ) vividly remember that Torsten Gustafson[2], on several occasions, expressed his great admiration of Kramers. Both Kramers and Gustafson had been at "Niels Bohr's Court" in Copenhagen, the former as Bohr's assistant and "ambassador" and Gustafson as a friend and helping hand. It is said [2] that Kramers spoke such good Danish that the Swedes believed that he was a Dane!

Gustafson loved to tell stories about great scientists and CJ liked to listen to him. On one occasion he said that the Swedish Academy of Sciences was about to give Kramers the Nobel Prize but, unfortunately, he died that year. However, while Kramers was alive Gustafson was not yet a member of the Academy – he was elected as a member in 1958 – so his information must have been based on his conversations with some members (indeed he knew "everybody"). The question is what did actually happen?

In order to be considered for a Nobel Prize, the first necessary condition, in addition to being alive, is that the candidate must have been nominated for the year in question. One single nomination is enough and, in fact, the Nobel archives show that several Laureates in physics had only one single nomination. Indeed, Gustafson believed that Kramers passed this hurdle as he himself had nominated him.

The full story of the presumed Nobel Prize to Kramers, as told by the archives of the Academy, [3] is the following. Kramers was nominated to Nobel Prize in Physics as follows:

1949 by Heisenberg,
1950 by Bohr; Gustafson; Møller,
1952 by Bohr; Møller.

In his nomination (in German), Werner Heisenberg, emphasized that he wished to draw the attention of the Nobel Committee to the totality of

[2] Torsten Gustafson was introduced in Chap. 3.

contributions by Kramers as they had played an important role in the development of physics. He urged the committee to investigate the matter and see if a good case could be made for honoring him. Heisenberg emphasized in particular Kramers' dispersion theory from 1924 which had played an important role in his own formulation of quantum mechanics. He also argued that Kramers' work on magneto-optical properties of crystals as well as his more recent results on ferromagnetism were well worth considering.

The Committee took Heisenberg's suggestion very seriously and two independent evaluations of Kramers' contributions were made in 1949, by respectively the theorist Ivar Waller and the experimentalist Gudmund Borelius[3]. The major conclusion of these investigations was that Kramers' most significant contribution was his dispersion theory. But did it merit a Prize? Waller wrote (translated from Swedish):

> Kramers' formulation of his dispersion theory was undoubtedly a very significant achievement, that had an ineradicable value and became of cardinal importance to Heisenberg in his laying the foundation of quantum mechanics. This is further emphasized in Heisenberg's nomination. However, the decisive step to [formulation of] quantum mechanics was taken by Heisenberg alone and that discovery has already been honored by a prize in 1933[4]. It seems to me that under existing conditions it would be difficult to recommend that he [Kramers] be honored for this contribution of his.

In 1950, Bohr, Gustafson and Møller, in their (separate, but no doubt correlated) nominations suggested that the Prize be awarded to Max Born and Kramers, for their decisive contributions to the development that had taken place in the realm of atomic physics. Born was to be honored for his statistical interpretation of wave mechanics. In the case of Kramers, again, the nominators put forward his dispersion theory that had served as the starting point of the formulation of quantum mechanics. Møller had in addition the following interesting comment in his nomination (translated from Danish):

> Both in the domains of the original and modern quantum mechanics, he [Kramers] has shown an extraordinary faculty to deeply penetrate into applications of the fundamental theories and thus elucidate their strength and limitations. … such as his incisive analysis of the formidable difficulties in quantum electrodynamics.

[3] Carl Olof Gudmund Borelius (1889–1985) was a much appreciated professor of physics at the Royal Institute of Technology in Stockholm.
[4] Heisenberg received in 1933, the Prize for 1932 which had not yet been awarded.

In 1950, the Nobel Committee was not prepared to recommend to the Academy that the Prize be given to Kramers. In 1951, Kramers was not nominated (and therefore not eligible to be considered for the Prize) and in 1952 the Committee, in its annual report to the Academy, mentioned him but did not discuss his case on the grounds that he had passed away.

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3. Nobel Archives at "Center for History of Science", Royal Swedish Academy of Sciences, Stockholm[5]

[5] I (CJ) am indebted to the members of this Center for their hospitality, while I was visiting them to study the case of Kramers.

Källén and T. D. Lee – a Preview

In the spring of 1954, a young man by the name of T. D. Lee gives a seminar at the Institute for Advanced Study, Princeton. This event is attended by a distinguished audience, including Freeman Dyson, Res Jost, Gunnar Källén and Wolfgang Pauli. The talk is followed by a heated debate between the two young stars Källén and Lee (both born 1926). Pauli gets highly excited by this intellectual battle between the two opponents, both of whom he holds at high esteem, as well as interventions by Dyson, whom he distrusts and refers to as self-appointed referee[6].

This event marks the start of a collaboration that eventually leads to a joint paper by Källén and Pauli (paper [1955b]) dedicated to Niels Bohr on the occasion of his 70th birthday.

When I (CJ) invited T. D. Lee to contribute to this volume, he responded immediately:

> "Your letter of July 5th brought back fond memories of Gunnar and Pauli. I will be happy to contribute a short article on the subject of the soluble model."

We now present his article, written together with his colleague R. Friedberg.

[6] However, later on, he changed his mind and in a letter ([2247] in the Pauli Collection) dated 28 February 1956, he let his assistant in Zürich, Armin Thellung, know that he appreciates Dyson. Pauli's nickname for Dyson was "Geheimrat" (privy councillor).

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R. Friedberg and T. D. Lee: A Soluble Model of "Higgs boson" as a Composite

December 1, 2009[1]

Higgs boson may turn out to be a composite. The theoretical description of such a composite is illustrated by an example of a soluble model.

1. Introduction

The σ-model [1] is a successful phenomenological theory of low energy particle physics. Yet, the 0^+ σ-particle itself has never been identified experimentally [2]. One of the possible reasons for this failure might be that the spin-parity transformation of σ is the same as that of

$$\sigma^2, \sigma^3, \cdots, \sigma^n, \cdots . \tag{1.1}$$

Thus, what is a σ-field in the idealized theoretical model may appear experimentally as a "composite" due to the possible mixture of (1.1). Today, a major focus of high energy physics is to search for Higgs boson. It might be that for similar and other reasons, Higgs boson [3] could also turn out to be a "composite" [4–8]. Neither the experimental identification of σ nor that of Higgs boson would correspond to the usual simple theoretical description of a single pole in the complex energy plane. In this paper, we explore the theoretical description of such a composite from a more elementary perspective, by examining the generalization of a soluble model [9]. The structure of the model is given in Section 2, and its solution in Sections 3 and 4.

This paper is dedicated to the memory of Gunnar Källen, who (besides others) has made important contributions [10–15] to the original soluble model.

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2. A Generalized Soluble Model

We generalize the original $V \rightleftarrows N\theta$ model by retaining the same fixed Fermion states V and N, but replacing the single $\theta(r)$ field by three boson fields $A(r), B(r)$ and $C(r)$. The Hamiltonian H in the new model is given by

$$H = H_0 + H_1 + H_2 \tag{2.1}$$

where

$$H_0 = m_0 V^\dagger V + \sum_k (\lambda_k a_k^\dagger a_k + \mu_k b_k^\dagger b_k + \nu_k c_k^\dagger c_k). \tag{2.2}$$

For convenience, the entire system is enclosed within a sphere of large radius R. The s-wave part of the annihilation field operators $A(r), B(r)$ and $C(r)$ are given in terms of their annihilation operators a_k, b_k and c_k by

$$\begin{aligned} A(r) &= \textstyle\sum_k (4\pi R\lambda_k)^{-\frac{1}{2}} u_k r^{-1} (\sin kr) a_k, \\ B(r) &= \textstyle\sum_k (4\pi R\mu_k)^{-\frac{1}{2}} v_k r^{-1} (\sin kr) b_k \end{aligned} \tag{2.3}$$

and

$$C(r) = \textstyle\sum_k (4\pi R\nu_k)^{-\frac{1}{2}} w_k r^{-1} (\sin kr) c_k$$

with

$$\begin{aligned} \lambda_k &= (k^2 + \alpha^2)^{\frac{1}{2}}, \\ \mu_k &= (k^2 + \beta^2)^{\frac{1}{2}}, \\ \nu_k &= (k^2 + \gamma^2)^{\frac{1}{2}} \end{aligned} \tag{2.4}$$

and α, β, γ the masses of bosons a, b and c. The functions u_k, v_k, w_k are convergence factors, which may all be chosen to be 1 for $k < k_{max}$ and 0 otherwise. In (2.3) all summations extend over

$$k = n\pi/R \tag{2.5}$$

with $n = 1,2,3,\cdots$. At equal time, we have the anti-commutation relations

$$\{V, V^\dagger\} = \{N, N^\dagger\} = 1 \tag{2.6}$$

and the commutation relations

$$[a_k, a_{k'}^\dagger] = [b_k, b_{k'}^\dagger] = [c_k, c_{k'}^\dagger] = \delta_{kk'}. \tag{2.7}$$

If one wishes, (2.6) can also be changed into commutation relations, and V and N would then be bosons.

In (2.2), we set the mass of N to be zero, and the "bare" mass of V to be m_0. The interaction Hamiltonians H_1 and H_2 are given by

$$H_1 = g(V^\dagger NC(0) + N^\dagger VC^\dagger(0)) \tag{2.8}$$

and

$$H_2 = f(V^\dagger NB^\dagger(0)A(0) + N^\dagger VA^\dagger(0)B(0)). \tag{2.9}$$

The g-coupling governs the transition

$$V \rightleftarrows Nc. \tag{2.10}$$

and the f-coupling gives rise to the scattering

$$Na \rightleftarrows Vb. \tag{2.11}$$

Thus, when $f = 0$ the Hamiltonian is identical to that of the original $V \rightleftarrows N\theta$ model, with θ replaced by c.

Throughout the paper, we assume V to be unstable through $V \to Nc$ when $R \to \infty$. Since the mass of N is set to be zero in the model, V is unstable if its physical mass m is larger than γ, the mass of c; i.e.,

$$m > \gamma. \tag{2.12}$$

Thus, in a collision of Na, beside the elastic scattering

$$Na \to Na, \tag{2.13}$$

we also have the inelastic process

$$Na \to Vb \to Nbc \tag{2.14}$$

provided that the total energy E satisfies

$$E > \beta + \gamma, \tag{2.15}$$

the threshold energy of the channel Nbc.

We shall assume

$$\alpha < \beta + \gamma. \tag{2.16}$$

Hence, in the Na channel at low energy when

$$\alpha < E < \beta + \gamma, \tag{2.17}$$

there is only the elastic scattering (2.13); at higher energy when $E > \beta + \gamma$, we have both (2.13) and the inelastic process (2.14).

Consider first the process

$$Nc \rightleftarrows V \rightleftarrows Nc. \tag{2.18}$$

Denote the corresponding state vector by

$$|Nc\rangle \propto \left[V^\dagger + g(4\pi R)^{-\frac{1}{2}} \sum_k \nu_k^{-\frac{1}{2}} k w_k (E-\nu_k)^{-1} N^\dagger c_k^\dagger \right] |vac\rangle. \tag{2.19}$$

One can readily verify that it satisfies

$$H|Nc\rangle = E|Nc\rangle. \tag{2.20}$$

At a finite R, E satisfies the eigenvalue equation

$$h_R(E) \equiv E - m_0 - g^2 \sum_k \frac{k^2 w_k^2}{4\pi \nu_k R} \left(\frac{1}{E-\nu_k} \right) = 0, \tag{2.21}$$

with its derivative

$$h_R'(E) = 1 + g^2 \sum_k \frac{k^2 w_k^2}{4\pi \nu_k R} \left(\frac{1}{E-\nu_k} \right)^2 \tag{2.22}$$

always positive.

When $R \to \infty$, $h_R(E)$ becomes

$$h_\infty(E) = E - m_0 - g^2 \int_0^\infty \frac{k^2 w_k^2}{4\pi^2 \nu_k} \frac{dk}{(E-\nu_k)}. \tag{2.23}$$

The condition for V being unstable is that when $E = \gamma$,

$$h_\infty(\gamma) < 0. \tag{2.24}$$

In this case, we introduce a cut along the real axis from

$$E = \gamma \text{ to } \infty \tag{2.25}$$

where γ is the mass of the c-meson. The derivative of $h_\infty(E)$ is

$$h'_\infty(E) = 1 + g^2 \int_0^\infty \frac{k^2 w_k^2}{4\pi^2 \nu_k} \frac{dk}{(E-\nu_k)^2}. \tag{2.26}$$

which is positive ≥ 1, when E is real $< \gamma$. For $E = \nu_k > \gamma$ but just above the cut along the real axis, we have from (2.23)

$$\mathrm{Im} h_\infty(\nu_k + io+) = i\left(\frac{g^2}{4\pi}\right) k w_k^2. \tag{2.27}$$

Thus, on the second sheet near and below the cut (2.25), there is a zero of $h_\infty(E)$, which corresponds to the V-resonance. It can be shown that the phase shift δ for Nc scattering (2.18) is related to $h_\infty(\nu_k + io+)$ and its complex conjugate by

$$e^{-2i\delta} = \frac{h_\infty(\nu_k + io+)}{[h_\infty(\nu_k + io+)]^*} \tag{2.28}$$

3. *Na* Sector (General Discussion)

As discussed in the Introduction, assume the idealized case that there does exist a fundamental spin 0 field ϕ which is the origin of masses of spin nonzero particles. In any physical process, there are bound to be effective couplings between ϕ and some of its higher power products, such as

$$\phi^2, \phi^3, \cdots, \phi^n, \cdots.$$

Thus, the physical Higgs channel becomes connected to not only a complex pole, but also to a cut in the complex energy plane, or other more complicated analytical structure.

In this section, the Na channel that we shall analyze represents a highly idealized model of "Higgs" as a composite. From reactions (2.10) and (2.11), we see that a state vector $|\rangle$ in the Na sector must also have components in Vb and Nbc channels as well. Thus for R finite, we may write

$$|\rangle = \left[\sum_k \psi(k) a_k^\dagger N^\dagger + \sum_p \phi(p) b_p^\dagger V^\dagger + \sum_{p,q} \chi(p,q) b_p^\dagger c_q^\dagger N^\dagger\right] |0\rangle. \tag{3.1}$$

From

$$H|\rangle = E|\rangle, \tag{3.2}$$

we find

$$(E - \lambda_k)\psi(k) = fU_k \sum_p V_p\phi(p), \tag{3.3}$$

$$(E - m_0 - \mu_p)\phi(p) = g\sum_q W_q\chi(p,q) + fV_p \sum_k U_k\psi(k) \tag{3.4}$$

and

$$(E - \mu_p - \nu_q)\chi(p,q) = gW_q\phi(q) \tag{3.5}$$

where k, p, q are all given by (2.5) and U_k, V_p, W_q are related to the u_k, v_p and w_q of (2.3) by

$$\begin{aligned} U_k &= (4\pi R\lambda_k)^{-\frac{1}{2}} ku_k \\ V_p &= (4\pi R\mu_p)^{-\frac{1}{2}} pv_p \end{aligned} \tag{3.6}$$

and

$$W_q = (4\pi R\nu_q)^{-\frac{1}{2}} qw_q$$

Substituting (3.5) into (3.4), we have

$$(E - m_0 - \mu_p)\phi(p) = g^2\sum_q W_q^2(E - \mu_p - \nu_q)^{-1}\phi(p) + fV_p\sum_k U_k\psi(k) \tag{3.7}$$

and therefore

$$\phi(p) = [D_p(E)]^{-1} fV_p \sum_k U_k\psi(k) \tag{3.8}$$

where

$$D_p(E) = E - m_0 - \mu_p - g^2\sum_q W_q^2(E - \mu_p - \nu_q)^{-1}. \tag{3.9}$$

From (3.8), we also have

$$\sum_p V_p\phi(p) = f\left[\sum_p \frac{V_p^2}{D_p(E)}\right]\sum_k U_k\psi(k).$$

Thus, (3.3) becomes

$$(E - \lambda_k)\psi(k) = f^2 U_k\left[\sum_p \frac{V_p^2}{D_p(E)}\right]\sum_{k'} U_{k'}\psi(k') \tag{3.10}$$

Multiplying both sides by $U_k/(E-\lambda_k)$ and summing over k, we find that the eigenvalue E satisfies

$$1 = f^2 F(E) \sum_p \frac{V_p^2}{D_p(E)} \tag{3.11}$$

in which

$$F(E) = \sum_k U_k^2 (E-\lambda_k)^{-1}. \tag{3.12}$$

Next, we study the continuum limit. When $R \to \infty$, the sum

$$U_k \sum_p V_p = (4\pi R)^{-1} \sum_p (\lambda_k \mu_p)^{-\frac{1}{2}} k u_k p v_p \tag{3.13}$$

becomes

$$(4\pi^2)^{-1} \int (\lambda_k \mu_p)^{-\frac{1}{2}} k u_k p v_p dp. \tag{3.14}$$

Thus, from (3.3) we have

$$(E-\lambda_k)\psi(k) = (4\pi^2)^{-1} f k u_k \int (\lambda_k \mu_p)^{-\frac{1}{2}} p v_p \phi(p) dp. \tag{3.15}$$

Likewise, (3.7) leads to

$$\begin{aligned}(E - m_0 - \mu_p)\phi(p) &= (4\pi^2)^{-1} g^2 \phi(p) \int \nu_q^{-1} (E-\mu_p-\nu_q)^{-1} q^2 w_q^2 dq \\ &+ (4\pi^2)^{-1} f p v_p \int (\mu_p \lambda_k)^{-\frac{1}{2}} k u_k \psi(k) dk,\end{aligned} \tag{3.16}$$

which gives

$$\phi(p) = (4\pi^2 \mathfrak{D}_p(E))^{-1} f p v_p \int (\mu_p \lambda_k)^{-\frac{1}{2}} k u_k \psi(k) dk \tag{3.17}$$

where

$$\mathfrak{D}_p(E) = E - m_0 - \mu_p - (4\pi^2)^{-1} g^2 \int [\nu_q (E-\mu_p-\nu_q)]^{-1} q^2 w_q^2 dq \tag{3.18}$$

In a collision of Na, in order to describe reactions (2.13) and (2.14), we write $\psi(k)$ and $\phi(p)$ as

$$\psi(k) = \delta(k-k_0) + \widetilde{\psi}(k) \tag{3.19}$$

and

$$\phi(p) = \widetilde{\phi}(p) \tag{3.20}$$

in which $\widetilde{\psi}(k)$ and $\widetilde{\phi}(p)$ denote the scattered wave amplitudes. Thus, (3.15) remains valid if we replace ψ, ϕ simply by $\widetilde{\psi}$ and $\widetilde{\phi}$. Hence

$$(E - \lambda_k)\widetilde{\psi}(k) = (4\pi^2)^{-1} fku_k \int (\lambda_k \mu_p)^{-\frac{1}{2}} pv_p \widetilde{\phi}(p)dp. \tag{3.21}$$

On the other hand, (3.17) yields

$$\widetilde{\phi}(p) = (4\pi^2 \mathfrak{D}_p(E))^{-1} fpv_p \left[(\mu_p \lambda_0)^{-\frac{1}{2}} k_0 u_0 + \int (\mu_p \lambda_k)^{-\frac{1}{2}} ku_k \widetilde{\psi}(k)dk \right] \tag{3.22}$$

with

$$\lambda_0 = \lambda_k \quad \text{and} \quad u_0 = u_k \quad \text{at} \quad k = k_0. \tag{3.23}$$

Define

$$\mathfrak{A} = (4\pi^2)^{-1} \int (v_p p/\mu_p^{\frac{1}{2}}) \widetilde{\phi}(p)dp, \tag{3.24}$$

$$\mathfrak{B} = (4\pi^2)^{-1} \int (u_k k/\lambda_k^{\frac{1}{2}}) \widetilde{\psi}(k)dk \tag{3.25}$$

and

$$\mathfrak{C} = (4\pi^2)^{-1}(u_0 k_0/\lambda_0^{\frac{1}{2}}). \tag{3.26}$$

Hence, (3.21) and (3.22) can be written as

$$\widetilde{\psi}(k) = f\mathfrak{A} u_k k/\lambda_k^{\frac{1}{2}}(E - \lambda_k) \tag{3.27}$$

and

$$\widetilde{\phi}(p) = f(\mathfrak{B} + \mathfrak{C}) v_p p/\mu_p^{\frac{1}{2}} \mathfrak{D}_p(E) \tag{3.28}$$

with $\mathfrak{D}_p(E)$ given by (3.18). In above expressions, all integrations over p and k extend from 0 to ∞.

Substituting (3.28) into (3.24), we find

$$I \equiv \mathfrak{A}/(\mathfrak{B} + \mathfrak{C}) \tag{3.29}$$

is given by

$$I = \frac{f}{4\pi^2} \int_0^\infty \frac{(v_p^2 p^2/\mu_p)dp}{E - m_0 - \mu_p - \frac{g^2}{4\pi^2} \int_0^\infty \frac{q^2 w_q^2 dq}{\nu_q(E - \mu_p - \nu_q)}}. \tag{3.30}$$

Likewise,

$$J \equiv \mathfrak{B}/\mathfrak{A} \tag{3.31}$$

becomes

$$J = \frac{f}{4\pi^2} \int_0^\infty \frac{(u_k^2 k^2/\lambda_k)dk}{E - \lambda_k}. \tag{3.32}$$

Thus

$$\mathfrak{A} = \frac{I}{1 - IJ}\mathfrak{C} \tag{3.33}$$

and

$$\mathfrak{B} + \mathfrak{C} = \frac{1}{1 - IJ}\mathfrak{C} \tag{3.34}$$

From (3.26), (3.30) and (3.32), we have the explicit expressions for $\mathfrak{C}$, I and J. Hence $\mathfrak{A}$ and $\mathfrak{B}$ are also known. Equation (3.27) and (3.28) then give scattering amplitudes $\widetilde{\psi}(k)$ and $\widetilde{\phi}(p)$.

4. *Na* Sector (Critical f^2)

We shall show that when $R \to \infty$ and f^2 greater than a critical strength f_c^2, there exists a bound state in the Na sector. Write the $R \to \infty$ limit of (3.11) as

$$1 = f^2 \mathfrak{F}(E)\mathfrak{G}(E) \tag{4.1}$$

in which

$$\begin{aligned} \mathfrak{F}(E) &= \lim_{R\to\infty} F(E) \\ &= (4\pi^2)^{-1} \int_0^\infty [k^2 u_k^2/\lambda_k(E - \lambda_k)]dk \end{aligned} \tag{4.2}$$

with u_k, λ_k given by (2.3) and (2.4). The function $\mathfrak{G}(E)$ is similarly related to the last summation in (3.11) by

$$\begin{aligned} \mathfrak{G}(E) &= \lim_{R\to\infty} \sum_p \frac{V_p^2}{D_p(E)} \\ &= (4\pi^2)^{-1} \int_0^\infty [p^2 v_p^2/\mu_p \mathfrak{D}_p(E)]dp \end{aligned} \tag{4.3}$$

where $\mathfrak{D}_p(E)$ is given by (3.18), Since $\mathfrak{D}_p(E)$ is related to $h_\infty(E)$ of (2.23) by

$$\mathfrak{D}_p(E) = h_\infty(E - \mu_p), \tag{4.4}$$

we have from (2.24)

$$\mathfrak{D}_p(\mu_p + \gamma) = h_\infty(\gamma) < 0. \tag{4.5}$$

From (2.4),

$$\nu_q = (q^2 + \gamma^2) > \gamma. \tag{4.6}$$

Thus, (3.18) and (4.4) – (4.5) imply that $\mathfrak{D}_p(E)$ and its derivative

$$\mathfrak{D}'_p(E) = \frac{\partial}{\partial E}\mathfrak{D}_p(E) \tag{4.7}$$

are continuous and satisfy

$$\mathfrak{D}_p(E) < 0 \quad \text{and} \quad \mathfrak{D}'_p(E) > 0 \tag{4.8}$$

over the range

$$E < \mu_p + \gamma, \tag{4.9}$$

which includes the range

$$E < \alpha \tag{4.10}$$

in accordance with (2.4) and (2.16). Thus, both $\mathfrak{F}(E)$ and $\mathfrak{G}(E)$ are negative, with negative derivatives; their product is positive and varies from 0 to a finite value as E increases from $-\infty$ to α, the mass of a-meson.

Define a critical f^2-coupling by

$$f_c^2 = [\mathfrak{F}(\alpha)\mathfrak{G}(\alpha)]^{-1} \tag{4.11}$$

It then follows that there exists a bound state energy E_0 in the Na sector with

$$1 = f^2\mathfrak{F}(E_0)\mathfrak{G}(E_0) \tag{4.12}$$

provided

$$f^2 > f_c^2 \tag{4.13}$$

For $f^2 < f_c^2$, the state turns into a resonance with a complex E_0. In this case the scattering amplitudes $\widetilde{\psi}(k)$ and $\widetilde{\phi}(k)$ have besides the cuts given by (3.30) and (3.32), also a complex pole at $E = E_0$.

It is of interest to note the difference between this pole in the Na sector and the V-pole in the Nc sector. The V-pole becomes stable in the weak coupling limit when $g^2 \to 0$, whereas in the Na sector the boundstate E_0 becomes stable only in the strong coupling region when $f^2 > f_c^2$.

We note that when $g^2 = 0$, $\mathfrak{G}(E)$ of (4.3) becomes

$$\mathfrak{G}_0(E) = (4\pi^2)^{-1}\int_0^\infty \left[p^2 v_p^2/\mu_p(E - m_0 - \mu_p)\right] dp. \tag{4.14}$$

Correspondingly, (4.12) becomes

$$1 = f^2 \mathfrak{F}(E_0)\mathfrak{G}_0(E_0) \tag{4.15}$$

with the same $\mathfrak{F}(E)$ of (4.2). Thus, the existence of the pole at $E = E_0$ does not depend sensitively on g^2; instead, it is closely related to the second (and higher) order attractive potential between Na due to the f-coupling transitions

$$Na \rightleftarrows Vb \rightleftarrows Na. \tag{4.16}$$

Its physical origin is quite different from the V-pole in the Nc channel of (2.18).

5. Remarks

Consider the case

$$f^2 < f_c^2. \tag{5.1}$$

The composite Vb is unstable, and may serve as a highly simplified model of either the σ-meson or the Higgs boson. Besides the elastic process (4.16), there is also the inelastic reaction

$$Na \rightleftarrows Vb \rightleftarrows Nbc. \tag{5.2}$$

In order to detect the composite Vb as a resonance, we require in (4.12) the corresponding pole at

$$E = E_0 \tag{5.3}$$

to be not too far from the real axis; hence f^2 cannot be too small. The amplitude for the continuum background must then also be relatively large.

In any composite model, we may regard the amplitudes Na and Vb as the idealized representations of its low and high frequency components of the same composite state vector. A second order transition between these two components would always depress Na and elevate Vb, as in (4.16). A resonance thus formed would require a strong coupling, and therefore also a large continuum background as in the model. This could be the reason why the σ-meson does not appear as a sharp resonance, and it might also be difficult to isolate the Higgs boson resonance.

We wish to thank N. Christ and E. Ponton for discussions.

Appendix

In the special case when

$$w_k^2 = k/\nu_k^3, \tag{A.1}$$

the integral

$$F_\gamma(E) = \int_0^\infty \frac{k^2 w_k^2}{4\pi^2 \nu_k} \frac{dk}{(E - \nu_k)} \tag{A.2}$$

in (2.23) is given by

$$F_\gamma(E) = \frac{1}{4\pi^2\gamma}\left[\frac{1}{z^3}(1-z^2)\ln\frac{1}{1-z} - \frac{1}{2z^2}(z+2)\right] \tag{A.3}$$

with

$$z = E/\gamma. \tag{A.4}$$

At $E = \nu_k + i\,0+$, we have

$$\text{Im}F_\gamma(E) = \frac{-i}{4\pi\gamma}\left[\frac{1}{z^3}(z^2-1)\right] \tag{A.5}$$

in agreement with (2.27).

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Part 4

On Källén's Scientific Work

Preface

This Part deals primarily with Källén's view on his own work. He registers as a graduate student in Lund in 1948, at the age of 22. In the following year he is sent to Zürich to attend Pauli's lectures April–July. Upon returning home, he proudly sends his first paper to Pauli. This remarkable paper is followed by several other astonishing pieces of work. Non-perturbative approach to quantum electrodynamics becomes his passion. He is writing "poetry" in field theory. Schwinger places him in his "Hall of Fame of Quantum Electrodynamics".

Elucidating the non-perturbative features of quantum electrodynamics with rigor is too difficult, even for him. He turns to the much simpler Lee Model, hoping for guidance, but finds none. He writes a paper together with Wolfgang Pauli, announcing the existence of ghosts in the Model. He is disappointed.

He turns to the study of some general properties of the building blocks of scalar field theories (the n-point functions), hoping to learn about the structure of quantum electrodynamics. This new field of research is "close to his heart" and he has wonderful collaborators. He enjoys himself in the wonderland of functions of several complex variables. He claims being against too much emphasize on mathematics but loves the subject. He even writes a paper, "for physicists and not for publication", on an alternative derivation of the Bergman-Weil integral. It gives him plenty of joy and he has found wonderful mathematical results. Again, he is disappointed by not having made progress in *physics*.

He chooses a new field of research: particle physics phenomenology. He has learned the subject quickly by writing a unique textbook about it. He spends a great deal of effort on doing radiative corrections to beta decay of the neutron. But he is a latecomer in the fast moving field of particle physics. At the very end, he becomes interested in current algebra but then death strikes.

The following chapters follow him, on his scientific path in the landscape of theoretical physics, primarily through his correspondence concerning his publications.

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A Brief Background to Källén's Scientific Publications

Källén was sent to "Pauli's Court", just like a number of other talented young men those days, to attend his summer term lectures for about three months during April–July 1949. After returning home, he finished his first paper, on a work that he had started during his stay in Zürich. Pauli wanted Källén to publish his work in the Swiss journal Helvetica Physica Acta. Källén didn't know whether it was allowed for a Swede, funded by Sweden, to publish in a foreign journal. The formal rules were important to him and he asked for permission to do so. It was granted. His paper [1] "Higher Approximations in the External Field for the Problem of Vacuum Polarization" was received by Helv. Phys. Acta on 13 August 1949. His affiliation is given as Swiss Federal Institute of Technology, Zürich, as well as at leave from: Department of Mechanics and Mathematical Physics, University of Lund, Sweden.

In fact, it was unfortunate that, after the above work, a series of Källén's remarkable papers were published in the Swedish journal Arkiv för Fysik. Of course, this journal of the Swedish Academy of Sciences was of high quality but there was almost nothing in it in Källén's area of research. As a proud Swede, he felt the obligation to publish in his local journal but paid the high price of not being read. As Christian Møller noted, the reason the physicists use the heading "Yang-Feldman method" and neglect Källén's contribution is because the former authors published in Physical Review while Källén's work appeared in Arkiv f. Fysik. (Actually, Källén's paper was submitted several months before that of Yang and Feldman.) We are sure that Källén later regretted this. After having written my first paper and approval by Källén, I (CJ) asked him: shall I send it to the Arkiv? Källén responded immediately: why, do you want to bury it?

It is also unfortunate that Källén's, as well as many other authors', excellent articles published in the journal of the Royal Danish Academy of Sciences don't seem to count as serious publications and are not easily available. It is difficult to understand why such crown jewels of science are being thrown into the waste basket of history. For example, many excellent articles are not even listed by ISI Web of Knowledge! Upon contacting them, one is informed that the reason is because these papers were not "refereed". Many papers in our by now remote past were "communicated" by a scientist whose name was made

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known by the journal. This means that the scientist in question was openly responsible for having accepted the paper. Compare this with the case of an anonymous referee. In the worst case he/she may actually have had too much to do and no time for doing the assigned referee work. He/she gets reminders from the journal and finally accepts the paper without having read it carefully. He/she knows that, in principle, nobody will know that he/she hasn't done his/her duty. After all it takes much less time to accept a paper than to reject it! Hopefully, the serious shortcoming of not treating the jewels of the past as publications will soon be rectified. It is sad to see that even some highly respected journals of Källén's time, are nowadays relegated to the less accessible storage areas in the libraries. One has to make an appointment well in advance to be able to consult them! Neither are such articles electronically accessible.

To sum up, Källén did not get the credit he deserved partially because he didn't publish in fashionable and widely read journals.

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Källén's Debut Article with Early Trace of Supersymmetry

On the origin of his first published work, Källén states in his paper [1]:

> "I want to express my respectful gratitude to professor W. Pauli, Zürich, who has suggested this investigation to me, and to thank him and Dr. R. Jost and Dr. J. Luttinger for many helpful discussions."

Källén considers the vacuum polarization, to orders e^2 and e^4, in a model containing N scalar and n spinor (Dirac) fields, all with charge e and interacting with an external field. He discusses the gauge invariance and convergence properties of his results. The e^2 case was previously treated by J. Schwinger [2], and by Jost and Rayski [3].

Källén informs the reader that Jost and Rayski[1] have shown that to the order e^2 the "non-gauge invariant (and divergent) terms cancel each other if one uses a suitable mixture of spinor and scalar fields." The mixture being

$$N = 2n$$

$$\sum_{i=1}^{N} M_i^2 = 2 \sum_{i=1}^{n} m_i^2$$

where M_i (m_i) denote the masses of the scalars (spinors). The first relation, that there be as many bosonic as fermionic degrees of freedom, is exactly the basic ingredient of supersymmetry. The second relation includes the degeneracy condition of supersymmetry $M_i = m_i$ but it is more general. Källén studies what happens at the order e^4, which is, of course, much more difficult to investigate. After impressive calculations, he finds that the first relation alone is

[1] Jerzy Rayski (1917–1993) from Krakow, Poland, had spent one year during the period 1949–1950 in Zürich. He relates some of his recollections in [4]. Actually, Pauli and Villars, in their famous paper [5] on regularization in field theory, recognize the pioneering contribution of Rayski in this area – the idea of introducing auxiliary fictitious particles. The authors state: "Rayski made this proposal in the summer of 1948 during his investigation of the photon self-energy of *Bosons* (see reference 6). With his friendly consent we later resumed his work and generalized the method for arbitrary external fields (not necessarily light waves)". Note, however, that Pauli and Villars consistently refer to J. Rayski as G. Rayski. This could be due to the fact that Rayski, when he had been in the USA, had translated his first name, Jerzy, into English and had published under the name George Rayski.

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enough for removal of divergences in the fourth order. (There are no divergences in higher than the fourth order; an overall charge renormalization is required.)

Pauli was very pleased with Källén's work. On 15 July 1949 he wrote to Gustafson in Lund [6]:

> "Dear Prof. Gustafson!
>
> I wish to thank you very much that you have send me Dr. Källén, who turned out to have great skill and talent. He is working both very quickly and very reliable.
>
> About a paper on application of Schwingers formalism to higher approximations of the vacuum-polarisation which he is just going to finish he will write to you soon himself. We shall publish it in the Helvetica Physica Acta.
>
> I would be very glad if we could have him here again the summer term 1950 (which starts end of April). (During the winter I shall go to Princeton.) ..."

However, Källén could not go to Zürich in 1950, as Pauli had wished, because he had to do his compulsory military service.

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Källén-Yang-Feldman Formalism

Källén Proudly Informing Pauli of His Approach to Quantum Electrodynamics

Already on 12 December 1949, Källén, a second year graduate student, writes a long letter to Pauli which begins with "Sehr verehrter Herr Professor", as if Källén is addressing an "Excellency". Källén proudly informs Pauli that he has found a way to simplify certain calculations in quantum electrodynamics by going to the Heisenberg representation[1] instead of the interaction representation which was commonly used, especially by Schwinger. His letter is like a neat preprint, with many equations and explanatory texts in between.

He also informs Pauli that he will not be able to go to Zürich in the summer of 1950 as he has to do his military service, starting in June 1950 and lasting at least nine months. He concludes his letter by (translated from German):

> I am still reminiscing about last summer, that was very important for my education and gave me a great deal of pleasure. I wish to thank you very much indeed for that wonderful time.

Pauli, who was in Princeton, answers on 22 December (translated from German):

> Thank you for your extensive letter. Here [i.e., in Princeton], there is a very talented young Chinese named Yang who has also been working along a similar path as you. Therefore, I gave him your letter to read and I am enclosing a letter by him.[2]

---

[1] Källén made very extensive use of his work on Heisenberg representation later on. His tour de force calculations, concerning the renormalization constants in quantum electrodynamics, are done in this representation.

[2] Unfortunately, this letter is not found in the Källén Collection which, except for his correspondence with Pauli, contains material only from after late 1958. However, the trace of it can be found in Källén's published paper which in its footnote 3 states "A similar formalism has been developed independently by C. N. Yang, Princeton, and I should like to take this opportunity of thanking him for sending me his unpublished results".

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On the 25 January 1950 Källén sends the manuscript of his new paper to Pauli and in a letter, after a short greeting, informs him that he intends to publish his work in Arkiv[3] [the physics journal “Arkiv f. Fysik” of the Royal Swedish Academy of Sciences]. Then he adds (translated from German):

> Actually, there isn't more [in the current manuscript] than what I have already written to you. I have only added a short calculation on bosons in external field. To me it looks very satisfactory that the space-like surfaces do not appear at all. Earlier, these were removed in the final result only after doing a painstaking calculation. I have also sent a copy of the manuscript to Yang and I hope that my ideas are not going to disturb his.
>
> In my letter to Yang I have also shown that the commutator formula of Schwinger and Dyson's “P-formula” are in accord with the equations in Heisenberg representation. This fact is indeed more or less evident, but I want to show that the explicit calculation in this case is at least as simple as the original proof of Dyson. In case you may be interested, here I briefly describe my calculation.

The calculation sent by Källén to Pauli is one page long (letter [1076] in the Pauli Collection). We shall not reproduce it here, but note that it is straightforward and elegant.

This is a typical Källén-to-Pauli letter, from the first years of their correspondence: it is more like a preprint, sandwiched between short opening and closing courtesy phrases.

On February 16 Pauli answers (translated from German):

> I thank you very much for your letter and manuscript (from 25.1.) that has been very useful for many people here. Yang would like to wait until he has further results before he answers your letter. (His calculations concerning this matter are not yet quite finished).

In addition, Pauli informs Källén that his work has already been quoted in a paper by Pais and Uhlenbeck. For historical reasons, it is interesting to take a look at this paper, as we do now.

The paper by A. Pais and G. E. Uhlenbeck was received on 14 February 1950 and published in July 1950 [Physical Review Volume 79 (1950) 145]. The authors write, in a section on exponential modifications of quantum electrodynamics (a topic that Källén would not have appreciated), page 163:

[3] Källén's article [1] was received by the journal on 8 February 1950, i.e., two weeks after the date of his letter to Pauli.

"In fact, this procedure can be justified by means of the S-matrix methods recently developed by Källén and by Yang and Feldman (Ref. 47). Their idea is to stay in the Heisenberg representation throughout the calculations and in this representation to solve by iteration the equations describing the behavior of sources and fields."

The Ref. 47 of the paper reads:

G. Källén, "Mass and charge renormalizations in quantum electrodynamics without use of the interaction representation", Arkiv. f. Mat. Astr. o. Fys. (to be published);

C. N. Yang and D. Feldman (to be published). We are indebted to Professor Pauli for making Dr. Källén's manuscript available to us as well as for instructive discussions on this point.

The Yang-Feldman paper was received by Physical Review, about three months later, on 17 May 1950. This is what Pauli, now at Zürich, had to say about it in a letter to Yang dated 13 June 1950 (letter [1124] in the Pauli Collection):

"Dear Yang!

I thank you very much for your and Feldman's paper. At the same time I received a paper[4] from Källén which also tries to develop the consequences of the use of the Heisenberg representation. Although he renounces the derivation of the S-matrix, restricting himself to the field operators themselves. I prefer his method in comparison with yours, as he does not go back to Dyson's P-symbol and the surfaces.

He also received your paper and says in a letter to me about it 'that Glauber's calculations make the main part of the paper'. I can confirm this strange impression from my own reading and think that the way Glauber is quoted in your footnote 14 does not go far enough. It is not only 'the generalization of the incoming and outgoing fields' which is due to him but most of the calculations of Section II A, which establishes the connection of your other results with the method of the interaction-representation that uses the surfaces.

[4] Here Pauli refers to a second paper [2] from Källén that is about 40 pages long. This paper (reproduced in Part 5) was received by the journal on 7 June 1950. In it Källén states that: "As has been pointed out by Yang, both the incoming and the outgoing field operators will fulfill the usual commutation relations (equations (57) and (58) below) and are thus related through a canonical transformation [There is a footnote here, referring to Dirac's book on quantum mechanics]

$$\psi^{(out)}(x) = S^{-1}\psi^{(in)}(x)S, \quad A_\mu^{(out)}(x) = S^{-1}A_\mu^{(in)}(x)S.$$

The matrix S above is identical with the S-matrix of Heisenberg."

As I am quoted at the end of your paper, I am entitled to say that I hope that you will find a better way to quote Glauber before the paper is printed.

With best regards, also to Feldman,

Sincerely yours, W. Pauli"

On the 27 June 1950, Yang replies to Pauli that his letter had taken him completely by surprise and

"As I have committed no other crime than not being able to obtain neat results useful for practical calculations from the Heisenberg representation, I do not feel that I should be ashamed of the original idea, unhappy though I am about the whole situation. Whether using the operators themselves instead of the S matrix is to be preferred, is perhaps a personal opinion and depends largely on the view-point.

We had absolutely no intention of belittling Glauber's contribution. It was the idea of the generalization of the incoming and outgoing fields that we learned from Glauber and so this was where we made the acknowledgement. However, I do think now that our quotation did not go far enough, and it will be duly changed when the proof comes. ..."

In their published paper [3] Yang and Feldman acknowledge Glauber, in their footnote 14, by stating:

"We are indebted to Dr. R. J. Glauber for pointing out this generalization of the incoming and outgoing fields and their relation to the Hamiltonian in the interaction representation."

However, under the same footnote they also add three other references. We have not examined what the role of the latter publications is. Pauli is now satisfied and writes to Yang that "I still believe that your original idea, which you already had before my arrival in Princeton is very interesting ...".

This shows how provocative Pauli could be and how quickly he could change his opinion. This could scare some physicists. There are stories about the "harm" he caused by his disbelief of the concept of spin and parity violation. However, by reading his letters (he wrote many) one finds out that he cared much more about *physics* than issues of reputation or prestige. Not believing in what others had done was for him a platform for creating suspense and excitement as well as incertitude, for the purpose of getting the young to think deeper and work harder. Indeed many people were intimidated by Pauli's attacks. However, one thing is certain: his method didn't work on Källén. Whenever Källén thought that Pauli had made an unjustified statement he would not let go of it but would retaliate.

According to all evidence given by those who were close to him, for example his assistants Weisskopf and Peierls as well as Telegdi, Pauli was *not* vicious, but on the contrary kind and wonderful! Concerning women, however, Pauli's "theorem" was that when a woman goes to physics her real purpose is to find a husband. It would have been interesting to ask him why he thought that theoretical physicists made good husbands. Even in this respect, after having met Nina Byers, Pauli was no longer certain that his "theorem" was generally valid [4].

Non-Perturbative Renormalization After the initial phase described in the previous chapters, Pauli was much interested in the scientific development of his "discovery" and was particularly impressed by Källén's subsequent work on the renormalization in quantum electrodynamics beyond perturbation theory, published during 1952–53. He liked to use the expression "Looking behind the veil[5] of Dyson's power series". He wrote about this subject to several people. For example, here below is what he wrote to Homi Bhabha, on 31 December 1951 (letter [1332] in the Pauli Collection) who had invited him to India:

> "… Until then [the time of his visit] I hope to know a little more about physics than now, although the progress of theoretical physics is rather low and slow at these times. Here in Zürich Källén (a Swede) is working on the rather difficult question to look behind the veil of Dyson's power series. He has already a way to *define* the renormalization, without using explicitly these series and is now attacking the more difficult question whether his equations have at all solutions. It is in no way excluded that Dyson's power series (though every single term is finite) never converge (never means for *no* value of the electric charge *e* different from zero) and it is even so, that during last summer (when Dyson stayed for a while in Zürich) Dyson himself guessed that it is really so. (Now he is in Cornell and I did not hear anything from him since then.) I have some hope that this question can be decided. At least Källén is trying it very eagerly. We shall see. …"

In the following year, in a letter ([1458] in the Pauli Collection) to Rudolf Peierls dated 29 August 1952, Pauli writes (translated from German):

> Concerning the renormalization theories, Mr. G. Källén here in Zürich has made a pioneering attempt to move away from power expansion [perturbative

[5] In German: hinter den Schleier.

approach]. His work has just appeared in Helvetica Physica Acta [**25**, 417–434 (1952)][6]. Your opinion [on this work] would interest me.

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[6] Paper [1952a] in this book. See Part 5.

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On the Renormalization Constants in Quantum Electrodynamics

The Young Källén's Intellectual Quantum Jump

On 16 January 1953, Källén in a letter to Pauli announces a most incredible result. The letter[1] is, as usual, in German and starts quite formally as usual, with "Sehr verehrter Herr Professor:" and then continues (translated from German):

> Enclosed I am sending you the manuscript of a work, that I have just finished. I would be grateful, if you would have time to read the manuscript and inform me of your opinion. I am very well aware that the result by itself is not so interesting - presumably every one believes that the self-energy, etc. is actually infinite. Since, at the present, I can't achieve anything better, I have written down the proof and, nonetheless, I believe I would like to publish it. Personally, I am quite happy, that I have succeeded to move a step forward – albeit a small one. Now I know, that the integrals
>
> $$\int^{\infty} \frac{\pi(x)}{x}\,dx, \quad \int^{\infty} \frac{\Sigma_i(x)}{x}\,dx$$
>
> cannot converge. Perhaps it is now not impossible to draw some conclusion about the integrals
>
> $$\int^{\infty} \frac{\pi(x)}{x^2}\,dx, \quad \int^{\infty} \frac{\Sigma_i(x)}{x^2}\,dx.$$

[1] This letter, found in the Källén Collection, is missing in the Pauli Collection.

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> The method [that I have] used is not immediately applicable to these quantities, but perhaps the estimations can be somewhat improved – I don't know yet.
>
> …
>
> Mit vielen Grüssen [with many greetings]
>
> Ihr sehr ergebener [your most respectful]
>
> Gunnar Källén

What Källén has shown, without claiming absolute rigor, is that at least one of the renormalization constants in quantum electrodynamics must be infinite. Pauli, who had greatly appreciated Källén's previous work on renormalization (paper [1952a]) answered quickly, on 21 January 1953 (translated from German):

> Thank you very much for your paper. In these days, that are certainly no blooming period for theoretical physics, I have become very modest and am already glad when I get a paper in which at least one *little* question is answered …

Pauli considers Källén's article to belong to this category. But he has problems with Källén's asymptotic conditions, where at $t = -\infty$ there are free electron, positron and photon states. He is concerned with the absence of the bound states in the formalism. Källén takes matrix elements of the type $< z|j_\mu|0 >$, where j stands for the electromagnetic current, which connects the vacuum state $|0>$ to all possible states $|z>$, to be summed over. As the leading term Källén takes z to be an electron-positron pair. Why aren't the positronium states taken into account? Pauli asks.[2] Are they present at $t = -\infty$?

Källén used to respond to Pauli's questions very quickly. It is a pity that we don't know what he said, on this occasion, because the above two letters are all that there is in the Källén Collection concerning their correspondence in 1953. This is a little "mystery", especially for the period February–May 1953. After that, Pauli and Källén must have met a couple of times in June-July, when Pauli visited Copenhagen and Lund and also at the Lorentz – Kamerlingh Onnes Centenary Conference in Leiden. In September 1953 Källén went to Princeton but Pauli was mostly in Zürich until the end of the year. Moreover, the Pauli Collection shows that in 1953 Pauli makes no comments about this work by Källén in his correspondence with other physicists, which is somewhat

[2] The situation here is somewhat similar to the case of the contribution of bound states of quark-antiquark to the cross section $e^+e^- \to$ hadrons.

unusual. He was mostly writing review articles and working on nonlocal field theory.

Returning to Källén's work, his result was later challenged, as described in and after Chap. 74. However, this work demonstrated Källén's exceptional intellectual power and placed him in "Schwinger's Hall of Fame" (see the next chapter).

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Källén in Schwinger's "Hall of Fame of Quantum Electrodynamics"

In a book [1], published in 1958 and its preface written already in 1956, the editor Julian Schwinger, gives his view on the most important historical steps in the development of quantum electrodynamics up to then. He takes 34 articles and thus promotes their 26 authors into his "Hall of Fame of Quantum Electrodynamics".

The most prominent "statue" in this Hall of Fame is, of course, that of Schwinger himself, whose contribution is seven single-authored papers. This is no surprise. Schwinger lived to a large extent in a world of his own. His Nobel lecture in 1965 has 12 references – 11 of them to himself! Returning to the Hall of Fame, the next prominent statue is that of Feynman with four articles. Among other selected theoretical celebrities one finds Dirac, Heisenberg, Pauli, Weisskopf, Dyson and Tomonaga. There are also a few experimentalists, among them two Nobel Laureates, Lamb and Kusch, for the measurements of the Lamb shift and the anomalous magnetic moment of the electron respectively. The theoretical value of the latter quantity had been computed by Schwinger and earned him a Nobel Prize in 1965 (see Chap. 14).

Also Källén made it into this Hall of Fame, as a recognition of his work "On Renormalization Constants in Quantum Electrodynamics" [2]. Schwinger had intended to place Källén's work as the very last entry in the book, i.e., as the latest discovery in quantum electrodynamics. However, for reasons beyond his editorial control, as he puts it, there is an additional paper after Källén's article. This was long before the age of modern computers and the current fantastic capabilities to easily permute the chapters in a book in any desired order.

Källén's article, which is about non-perturbative renormalization in quantum electrodynamics, represents a veritable tour de force. Källén, without resorting to extreme mathematical rigor, shows that **not** all of the renormalization constants in quantum electrodynamics can be finite quantities.

Källén returned to this work over and over again. For example, in his Handbook Article [3] he devoted the last nine sections to the general theory of renormalization and gave a more detailed account of his proof. See also Chap. 56.

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This work was also the reason for Källén's interest in the Lee Model; see also Steven Weinberg's article (Chap. 61).

References

1. Julian Schwinger (editor), "Quantum Electrodynamics" (Dover Publications, 1958)
2. G. Källén, Paper [1953c]
3. Källén's Handbook Article is presented in Chap. 19

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Controversies, Letter to Phys. Rev. Lett. and Recollections of S. G. Gasiorowicz

For some years, the conclusion that "at least one of the renormalization constants in quantum electrodynamics is infinite" was generally accepted. In the last paragraph of his [1953c] paper, Källén had, however, a disclaimer:

> "The proof presented here makes no pretence at being satisfactory from a rigorous, mathematical point of view. It contains, for example, a large number of interchanges of orders of integrations, limiting processes and so on. From a strictly logical point of view we cannot exclude the possibility that a more singular solution exists where such formal operations are not allowed. It would, however, be rather hard to understand how the excellent agreement between experimental results and lowest order perturbation theory calculations could be explained on the basis of such a solution."

In 1959, Källén's work was criticized for lack of rigor, in a paper by Gasiorowicz et al. in Physical Review Letters [Vol. 2 (1959) 513]. Källén wrote a rebuttal (Paper [1959b]) which, however, was rejected by that journal, but is reproduced in this book (see Chap. 76). Källén also returned to this issue later on in his lectures "Review of Consistency Problems in Quantum Electrodynamics" at the 1965 Schladming School (paper [1965a]) where he stated:

> "We feel very strongly that this criticism is largely based on a misunderstanding both of the mathematical rigour intended by the original work as well as of the actual calculations. In particular, we feel that the specific complaint put forward by Gasiorowicz et al. is irrelevant, as the particular limit which these authors are concerned with is actually discussed in the original paper.
>
> However, and quite independently of this published criticism, we freely admit that the actual mathematical rigour of the argument is not very high. Therefore, it is not logically excluded that a rather singular solution of the equations with finite renormalization constants could exist where certain formal interchanges of orders of integration etc, would not be allowed."

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Here below we give further details about this issue. See also Chap. 56.

Källén's Letter to the Editor of the Physical Review Letters

Källén felt that he got an unfair treatment from Physical Review Letters. The journal published the criticism of his work by Gasiorowicz et al. [Physical Review Letters 2 (1959) 513] but rejected his rebuttal. He expressed his dissatisfaction in a letter (September 1959) to the editor of the journal, S. A. Goudsmit:

> "Dear Dr. Goudsmit,
>
> Thank you very much for your letter of August 28th. I was very interested to read your explanations about the policy of the Phy. Rev. Letters to try to avoid unfruitful exchanges of disagreements – a policy that I very much appreciate. I hope you do not think it too impertinent of me to mention, in passing, that perhaps some of the unpleasant features of the present situation could have been avoided if this policy had been followed also at an earlier stage and if you had given me an opportunity to comment on the Gasiorowicz et al. letter already before its publication. Their paper was really a criticism of my earlier work. As it is, I have had a rather intense correspondence with Gasiorowicz during June and July and at the end of July I was under the impression that we had agreed that my original paper was essentially correct. I thus sent my short remark to you and hoped that that would be the end of the matter. It now appears that either the communication between the three authors of the paper in question is not what I had expected it to be, or that they have changed their minds again. I do not think that the points raised by Yennie in his comments are relevant at all, and I believe that my original proof is correct (within the limits of rigour usually used in theoretical physics). I thus do not think that the differences that exist are related to physics of the problem but that they are either misunderstandings or, at most, questions of 'epsilontic'. Under the circumstances I think I shall try to make a new effort to achieve agreement between us and in the meantime I ask you to keep my letter to you in reserve for future use. I still have not quite abandoned hope that I shall later be able to ask you to publish my short remark in roughly its present form without risking a 'counter-rebuttal'. Speed of publication is not too important in a discussion about a paper published more than six years ago.
>
> Sincerely yours (Gunnar Källén)"

Källén mentions the above unfortunate state of affairs in a letter to F. Dyson in October 1959:

> "As you know I am in the middle of a fight about the old paper in the Danish Academy about renormalization constants. I usually enjoy a fight but when the Phys. Rev. Letters merrily print what Gasiorowicz and Yennie have to say but are reluctant to print my answer the joke has gone a little too far, I feel."

Even afterwards on several occasions he expresses his dissatisfaction with the above journal, in his characteristic manner. To its mighty editor, Samuel Goudsmit, who has asked him to referee a paper by Fried he writes on Dec. 5, 1959:

> "… I must say that my conscience makes it rather difficult for me to answer question 1 [on novelty] on your form. If this discussion had been about something, where I had not been so much personally involved, I think I should have answered with 'no'. As the situation is now, I am afraid that I might be prejudiced against the author and have therefore answered with 'yes'. To this decision of mine has also contributed the fact that you did publish the paper by Schwinger, mentioned at the end of my report. The Schwinger paper contains even less (=absolutely nothing) which is new. However, the fact that the Schwinger paper was accepted makes it impossible for me to oppose the publication of Fried's paper, provided his remaining mistake is corrected.[1]
>
> Sincerely yours,
>
> Gunnar Källén"

In fact the Källén Collection contains a rather extensive and detailed correspondence between Gasiorowicz and Källén, which reveals a cordial relationship between them. Gasiorowicz had taken notes at lectures given by Källén and Källén had appreciated his two years younger colleague. When the controversy arose Källén tried to explain to Gasiorowicz his objections about the paper that he and his colleagues intended to publish. However, none of his objections made it into the Gasiorowicz et al. paper. Therefore, I (CJ) contacted Gasiorowicz to find out what had actually happened. The result is presented here below.

[1] This remaining mistake was described in a footnote in Chap. 16.

Recollections of Stephen G. Gasiorowicz

I had some interaction with Gunnar Källén in the period 1957–1960. During this period I was on the staff of the Lawrence Radiation Laboratory in Berkeley. Starting in 1955 my interests turned towards "dispersion relations" and their basis in quantum field theory. I worked with Chew, Karplus and Zachariasen on the spectral representation of the electromagnetic (isovector) form factor in 1957 [1]. That year I was awarded a National Science Foundation Fellowship. I decided to go to the Niels Bohr Institute, mainly because of Källén's presence there. His work at the time was directed towards a thorough understanding of the analytic structure of the three-point function, something that we, in Berkeley had assumed for the form factor. He and Arthur Wightman utilized the theory of several complex variables with a long term goal of checking whether quantum electrodynamics field theory, as then formulated, really existed [2]. Gunnar and I became quite friendly, and he kept me abreast of his ongoing work with Pauli on the Lee model and on the indefinite metric needed to represent the ghost states. At the end of my fellowship in Europe, I attended a summer school in Varenna, at which Gunnar lectured on field theory. He asked me to be the note-taker for his course, and we worked together on getting them into shape.[2]

On the way back to Berkeley, I stopped at Minnesota for a few weeks. During that time we looked at Källén's proof that at least one of the renormalization constants in QED had to be infinite. There had been some discussion of the role of gauge invariance in the proof [3, 4], which was an area of interest to Don Yennie and Hiroshi Suura. I was interested in the question of whether there was a conflict between Källén's work on the renormalization constants and dispersion theory. There was a paper by Drell and Zachariasen [5] in which they considered the possibility of a no-subtraction dispersion relation for the form factor. We looked into these matters and concluded that (a) gauge invariance did not affect the validity of Källén's proof, but (b) that the possibility of a no-subtraction dispersion relation for the form factor did not seem to be ruled out by Källén, but that such a result would invalidate his proof. We wrote a paper outlining our arguments [6]. Gunnar strongly disagreed. He submitted a rebuttal to Phys. Rev. Letters, which consisted of one sentence that (to my recollection) just stated that his proof had indeed covered that possibility. PRL rejected the one-line paper.

It turned out that, quite unexpectedly, I was invited to spend the year 1959–60 at the Max Planck Institute in Munich. At the end of my stay there, Gunnar invited me to spend a few days in Lund. He had, in the meantime, re-

[2] I (CJ) wish to thank Professor Stephen Gasiorowicz for this information.

formulated his proof (without any change of assumptions), and we discussed it at length. I could not find anything to criticize, and withdrew my criticism. My co-authors had other problems with the proof, so that we never rewrote the paper. I now believe that my criticism dealt with the possibility of a finite charge renormalization, whereas in its final form, Källén's arguments applied to the electron wave function renormalization.

A very detailed discussion of the final version of the proof appeared a few years later as part of a Winter School in Schladming[3]

I spent 1968–9 at DESY in Hamburg. I had hoped to meet with Gunnar and discuss recent developments in strong interaction physics with him. His untimely death made that impossible. I was very much saddened by his accident, and feel that physics lost a Pauli-like critic at a time when he was really needed.

I hope that this rather incomplete summary provides you with the information that you were seeking. With best regards, Stephen Gasiorowicz.

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6. S. G. Gasiorowicz, D. R. Yennie and H. Suura, Phys. Rev. Lett. 2 (1959) 513

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[3] Note added by me (CJ): here Gasiorowicz is referring to G. Källén, "Review of Consistency Problems in Quantum Electrodynamics", (Lectures at the 4th Schladming Winter School 1965), Acta Phys. Austriaca Suppl. II, (1966) 133; paper [1965a] in this volume. See also Chap. 56.

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Letter to Gasiorowicz

The paper by Gasiorowicz et al. was received by the Physical Review Letters on 11 May, and was published on 15 June (1959). Two weeks later, on 30 June, Källén wrote the following letter to Gasiorowicz:

"Dear Stephen:

Thank you very much for your last letter. After reading it I feel rather confused. You begin by admitting that my discussion of the Λ_μ is O.K. and then you start talking about that the sum of all terms does possibly not go to zero even if every term separately does. To this I can only remark that what really goes to zero is the sum of all absolute squares of the matrix elements. This was, I think rather clearly stated in the Dan. Acad. paper. The sum of the matrix elements added with the phase factors appropriate to the vertex function is clearly smaller than the sum of all absolute squares. Therefore, if the sum of the absolute squares goes to zero so does the vertex function. What makes me particularly confused in this connection is a comparison between your letters of June 15th and June 25th. On June 15th you said: 'For all renormalization constants to be finite, conditions of the type

$$\sum_{\{p^{(n)2}=-M^2\}} |<0|j_\mu|n>|^2 \to 0 \quad \text{as} \quad M^2 \to \infty$$

must hold, from which we are willing to deduce that all matrix elements[1] of j, f and $\overline{f}$ connecting states which differ very greatly in their masses must go to zero in a very drastic way'. Because of this sentence in your letter I did not elaborate this point in my previous letter but assumed that we agreed on that point. What is the reason for your change of opinion? Adding the things you have said you agree with on different occasions (and assuming that you really mean what you say) I find that there by now ought to be complete agreement between us. I do not understand why that is not so? Have you really thought enough about these things to know your own mind?

You say that your note has already appeared. I strongly insist that something must be done about it. For my own sake I should prefer that you yourself

[1] Here, Källén is hinting at the state $|n>=|f\overline{f}>$, in other words an electron-positron pair.

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publish a new paper saying that it has been pointed out to you that the thing you were complaining about in the proof was discussed on pp. 10 and 11 in the Dan. Acad. paper and that you now want to withdraw your statement that the proof was inconclusive and, consequently, also that other nonsense about an extra polynomial. However, from your letter of June 25th I realize that it is not a realistic possibility to expect you to do so (at least not with wholeheartedness that I should be satisfied with). Therefore I shall immediately start making a draft of a reply to your paper. The general outline of it will be something like the following. I agree that some of the technical points of the discussion were not given in the Handbuch paper but refer to the Dan. Acad. paper esp. pp. 10 and 11 for a discussion of the point you raised. To make myself as clear as possible I shall presumably also give at least some of the details we have talked about recently (the denominators in the formula for the vertex; the center of mass coordinate system and the sum of the absolute squares of the matrix elements). To make the paper contain something new I shall also say that with the aid of more recent developments the discussion can be simplified and then give the argument with the $\Delta_3^{(+)}$ more or less as I wrote to you some time ago. This should be enough as far as you are concerned. The Johnson part will mainly contain the point that if you make a connection between the various cut offs used by him with the aid of Ward's identity, you will find exact agreement between perturbation theory and the general asymptotic form for the matrix elements in question, but that part will not concern you personally.

I shall send you a copy of my remarks – possibly even before they are in their final form.

Regards,

Gunnar

P.S. In reading what I have written above I find that I have not been able to control my temper quite as much as I wanted! I am only human and I hope you will excuse it."

The above letter, as well as a number of his other correspondence, exhibits Källén's general attitude towards those who are younger than himself, even when the age difference is not so big. As a caring father, he tries to save the young from committing what he considers to be scientific sins, explaining to them their "crimes" and threatening them a little, if needed. But he is neither malicious nor rancorous. In fact, shortly after his above letter, on 4 August 1959, he wrote again to Gasiorowicz:

> "I am glad to hear that you are possibly coming to Europe again. If you are interested, perhaps we could arrange a stay in Lund for you? Do you know anything about your time schedule? …"

And on 6 September 1960 he informs Gasiorowicz that:

> "the financing of your trip to Lund is O.K. by now. … I expect that within a few days you will get a letter from them [NORDITA] describing the details and presumably with many forms to sign!
>
> We are looking forward to see you here soon. …"

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Källén's Rejected Rebuttal

Here below, we reproduce the text of Källén's rebuttal that was rejected by the Physical Review Letters:

The Renormalization Constants in Quantum Electrodynamics

G. Källén

Department of Theoretical Physics, University of Lund, Lund, Sweden

In a recent letter to this journal Gasiorowicz, Yennie and Suura [1] have expressed the opinion that the proof published a few years ago that at least one renormalization constant in quantum electrodynamics [2] is infinite is not conclusive. Gasiorowicz et al. base their criticism on the treatment of a particular integral, viz. [3]

$$\int_{-\infty}^{\infty}\int_{-\infty}^{\infty} dxdy \frac{F_\mu(\mathbf{p}, x; \mathbf{p}', y)}{[x + y - (p_0 + p_0') - i\epsilon)][y - p_0' + i\epsilon]}$$

The result one wants established is the vanishing of the integral in the limit when $p_0 + p'_0$ goes to infinity and, simultaneously, the space parts $\mathbf{p}$ and $\mathbf{p}'$ vary in such a way that $p^2 = p'^2 = -m^2$. Gasiorowicz et al. remark: "The integral would vanish if the function $F(\mathbf{p}, \mathbf{p}')$ approached zero as $\mathbf{p}, \mathbf{p}' \to \infty$ but in general there is no way of establishing this." We agree with the first half of this statement but want to point out that the necessary limiting property of the weight function in the above integral is established in the argument leading up to Eq. (44) in the first paper of Ref. [2].

References

1. S. G. Gasiorowicz, D. R. Yennie and H. Suura, Phys. Rev. Letters 2, 513 (1959)
2. G. Källén, Dan. Mat. Fys. Medd.27, no.12 (1953). A short summary is given in G. Källén, Quantenelektrodynamik, Handbuch der Physik 5:1 (1958)
3. For notation and general background we refer to the two here cited original articles

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Why the Lee Model

T. D. Lee's paper of 1954 [1] captured the attention of many of the great field theorists of the time and created a lot of excitement among them. Here was a simple toy model that perhaps could give invaluable insight into the issue of renormalization in quantum electrodynamics, beyond perturbation theory. The Lee Model has been described in this book by Steven Weinberg [2], who had worked on it when he was a PhD student. In order to give our readers a taste of the general atmosphere of that time, here below we quote a couple of excerpts from the literature.

In the book "T. D. Lee, Selected Papers" [3] the editor, G. Feinberg, makes the following comment about the Lee Model (Paper 13 in the book):

> "Paper [13] introduced the 'Lee Model', a soluble model in quantum field theory, involving the interaction of a neutral spin zero field with two states of a fermion. It is shown that renormalization can be carried out exactly in this model, using precise definitions of the renormalization constants, and that this exact renormalization procedure, when expanded in powers of a coupling constant, agrees with the usual perturbative renormalization scheme. This paper was very influential in subsequent work on field theory and renormalization."

The topic was studiously discussed at meetings and conferences. In his roundabout of the Rochester Conferences [4], John Polkinghorne gives the following historical account of a Källén performance on the scene of the 1956 Meeting:

> "A toy field theory, not realistic but possibly instructive, had been invented by T. D. Lee. The Lee Model suggested the ominous possibility of 'ghosts' in quantum field theory, that is to say, the occurrence of states carrying negative probability, a disaster which would destroy any possibility of a consistent interpretation of the theory. Was this just an artefact of the model or an indication of real trouble? Landau's group in Leningrad thought they had arguments to demonstrate the latter. At Rochester 6 these questions were discussed by the sharp-tongued Swedish physicist, Gunnar Källén. He said of the claim to have discovered ghosts in realistic field theory that, as in other spooky stories, 'even though people who tell them sincerely believe in them, they are not

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> necessarily true'. Unfortunately, Landau was too independently minded to have been let out of the Soviet Union to defend his ideas at Rochester. Those compatriots of his who were present (who probably included a few bulky individuals whose names were not well known in the physics community) declined to risk speaking on his behalf."

Several years later, Källén compared the Lee Model with an example in a textbook by Dirac [5] which gives a treatment of the natural linewidth. This in turn could be traced back to the famous paper in 1930 by Weisskopf and Wigner [6]. His point was to make the students aware of similarities, as well as differences, of various models. For him there was seldom "Anything New Under the Sun".

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Källén-Pauli Correspondence on the Lee Model

It is the spring of 1954. Pauli is visiting the Institute at Princeton for about three months during the period January to April. Källén is already there, since September 1953. Pauli's correspondence during this period, especially with Heisenberg, shows that he often asks Källén to scrutinize Heisenberg's latest calculations and provide written reports that Pauli himself forwards to Heisenberg. In addition, both Källén and Pauli become interested in a model by T. D. Lee, generally referred to as the Lee Model. In April 1954 both Källén and Pauli return to respectively Copenhagen and Zürich.

A few months later, in a letter dated 27 September 1954 (letter [1880] in the Pauli Collection), Pauli informs Källén that he will give a course on "Problems of quantum electrodynamics" and (translated from German):

> The work of the Chinese T. D. Lee about his non-relativistic model for renormalization has now appeared in the September issue of Physical Review (p. 1329, Volume 95). This is particularly suitable for a lecture, for illustration. Unfortunately, I can't find my notes on what you said about it at Princeton. … Could you write down for me once more your formulas (those that are not in Lee's article)? …

This becomes the start of the most intense period in Källén-Pauli correspondence. They "bombard" each other with at times long letters with many equations. Their correspondence is primarily on the "Lee Model" (T. D. Lee, Phys. Rev. 95 (1954) 1329), described in this volume by Steven Weinberg (see chapter 61). The letters in question are generally very technical. Moreover, the final result of their deliberations (paper [1955b]) is printed in this book. Therefore, here below, we wish to briefly transmit to our readers only some historical aspects of biographical interest.

Already on 22 October, Pauli writes a letter to Abraham (Bram) Pais (letter [1897] in the Pauli Collection):

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"Dear $\pi\alpha\iota\varsigma$!

... In the moment I am busy with the Lee-model, especially with the controversy Lee-Källén. It is not clear to me yet.

Thirring made good progress with the divergence of the Dyson-series[1] in the case of the pseudoscalar theory with coupling $g(i\overline{\psi}\gamma_5\psi)\phi$. He will find out now whether his proof is 'Källén-proof'[2]. ..."

What is this Lee-Källén controversy that Pauli is talking about? As it is evident from Pauli's later correspondence with several people, he had really enjoyed an "intellectual battle" between the two young men Lee and Källén, both then about 28 years old. He who had complained about boredom – nothing was happening in physics – was finally experiencing some excitement. Here we give two examples.

In a letter, from Zürich, dated 12 December 1954 (letter [1943] in the Pauli Collection) and addressed to Markus Fierz[3], he relates what happened after Lee, in the spring of 1954, gave a talk at Princeton about his work (translated from German):

Källén was also in Princeton when Lee gave the talk ... A heated discussion took place between Källén and Lee. Lee made an excellent impression on me, quite young, somewhat inexperienced, but an unusually talented man. Källén insisted that certain quantities, that according to his [Källén's] formulas must be positive (he didn't explain this dogma any further) are not so in the case of Lee. ... Anyhow, the oral polemics between Lee and Källén, both of whom I esteem highly, woke in me the desire to achieve a better understanding of the matter. ...

Pauli's letter ends with a general remark, which is indeed a great compliment to Källén:

but I don't expect any "miracle". First I must (hopefully with the help of Källén) still learn a lot of mathematics.

[1] Here Pauli is referring to series expansion in powers of the coupling constant.
[2] Since Källén was a master of finding inconsistencies, Källén-proof for Pauli was synonymous to a strictly correct proof.
[3] Markus Fierz (1912–2006) had been one of Pauli's assistants. Pauli appreciated him very much indeed and had a very extensive correspondence with him. He would even tell Fierz about his dreams. Källén, was too young to qualify for such intimate communication. A short biography of Fierz can be found in November 1959 issue of CERN Courier, on the internet.

To Oskar Klein, in Stockholm, he writes on the Christmas day, 1954 (letter [1954] in the Pauli Collection), about the same event (translated from German):

> … This thing started in a seminar in *Princeton*. In the beginning of April this year (shortly before my return to Zürich and Källén's to Lund). The young, very talented *Chinese* T.D. Lee gave a talk about his – by all of us immediately conceived as a very instructive – example of renormalizable fields. … After Lee's talk, in the discussions, *Källén* contradicted him in a very impetuous juvenile manner. To me it seemed that *Lee* responded *well* to him (very Chinese-calm). I felt sympathy for *both* and immediately got the impression that there still remained something to be clarified. From Källén's arguments, I instantly got the idea that perhaps in the Lee Model, in general, S-Matrix is not unitary …
>
> I think very highly of this compatriot [meaning Källén] of yours (whom I once virtually "discovered"). As compared to us, he has ahead of him the advantage (N. B. but naturally also the disadvantage) of *youthfulness* and I believe that he has *now* (in continuation of his work in Helvetica Physica Acta 1952)[4] a significant chance of making *discoveries*.

Pauli then goes on to describe to Klein what Källén and he have discovered about the structure of the Lee Model.

Evidently, Pauli is very pleased with himself for having worked hard on a specific problem. After all, in those years, he used to spend most of his time acting as a "central node" in a network, keeping in touch with what was going on in several fields and connecting researchers to one another. He would give suggestions to others, correct errors in their manuscripts, write comprehensive review articles, etc. He expresses his joy in doing research himself in several of his other letters as well. As an example, he writes (letter [1949] in the Pauli Collection) in December 1954, to his former assistant Schafroth[5] who is wondering why Pauli has not responded to his letter (translated from German):

> Naturally, your letter of 29 September lies on my table. Naturally, I have read it. It is not at all the question of "being quiet to death". It was something quite different. "The line was busy" [This last sentence is written in English] - every week I send about two letters to Källén in Copenhagen (on the average 5 pages long) – about a Model by Lee.

[4] Here Pauli is referring to Källén's paper [1952a]. See Part 5.

[5] Max Robert Schafroth (1923–1959) was professor at the University of Sydney in Australia, and worked on superconductivity. He would send his work to Pauli for comments. Unfortunately, he was killed in a plane accident in May 1959.

It is Pauli who suggests to Källén that they should publish a joint article on the Lee Model. In a long letter, dated 4 December 1954 (letter [1938] in the Pauli Collection), he writes (translated from German):

> We must do something about explaining [our work] to others. Private letters are not quite sufficient, as there are many interested people {Lee himself, the Princeton-seminar, Heisenberg and his school which can be denoted with the symbol "The Gött(ing)er"[6]}. . . .

Källén answers (letter [1941] in the Pauli Collection) on 8 December 1954 (translated from German):

> Dear Professor Pauli,
>
> Thank you very much for your letter. Naturally, I would be very proud to publish with you. Personally, I would prefer to publish the work in the [journal of] Danish Academy, and I understand that you have at least nothing against it. . . .

The idea to publish the article in the above journal had come from Christian Møller who was collecting articles for a special volume to be presented to Niels Bohr on the occasion of his 70th birthday in 1955. Källén wrote the manuscript and sent it to Pauli on 10 January 1955. He also informed Pauli that he will no longer be working on the Lee Model but intends to continue his work on quantum electrodynamics.[7] Pauli quickly made some suggestions on improving the manuscript and at one point, concerning an appendix, used the term "I dislike this style of third class mathematicians". Källén, who would not let Pauli get away with any, in his opinion, unjustified remarks, responded on 18 January 1955 (translated from German):

> . . . However I am not so impressed by your criticism of the so-called "third class mathematicians". For example, I actually don't see the difference between when you write "performing the limiting process $\epsilon \to 0$, . . . " instead of my formulation "the integral over the semi circle . . . " and so on, in several other places. Not that I have anything *against* your formulation, but I don't understand wherein the significant improvement lies? . . .

---

[6] Here Pauli is making a joke. At the time Heisenberg was in Göttingen, and Göttinger here refers to scientists working with him in that city. Removing (ing) gives the German word Götter, which means "Gods".

[7] This may have been his intention but is not what happened. Two years later he published a second paper on the Lee Model, this time in collaboration with V. Glaser (paper [1957a]: A Model of an Unstable Particle, Nucl. Phys. 2 (1957) 706).

About one month earlier, Pauli had in a letter informed Lee of the results obtained by Källén and himself and had signed the letter:

> W. Pauli
>
> The society of ghost hunters
>
> The president

Here, ghost means a particle/state with negative probability, i.e., an unphysical state. Källén and Pauli had found that the Lee Model, when renormalized, encounters such a problem, and therefore, Pauli considers himself as a ghost hunter.

Actually, even after the publication of their joint paper, Pauli would keep on consulting Källén on the Lee Model, because his friend Heisenberg was working on it.

In the following, we exhibit first a copy of a page from a letter by Pauli, where he suggests improvements on the manuscript that he has received from Källén. Then we present copies of two other pages from letters exchanged between the two men, on the Lee Model. As usual, Pauli's letters are handwritten and difficult to read. Källén's are typed (on a poor typewriter, and certainly not by a professional typist) but have neatly handwritten equations.

Note 7) also: Feynman, Phys. Rev. — Titel: Abgesehen wir nur bei der alpha behandelten Ordnung! (um die physical soll auch vorausgesehen!)

and my paper Progr. Theor. Phys.

Appendix II

Explicite expression for $h(z)$ in the limiting case $f(k)=1$
Position of the root. Power series development prevented.

p. 12 Sum (59) + (60) does vanish for $\omega_0 < 2\mu$

p. 14 instead of "is physically admissible only":
"is only in accordance with the physical probability concept"

Appendix p. 15 forgotten to say, that use is made of the fact, that ~~[illegible]~~ all roots of $h(z)$ are real.

$$\operatorname{Im} h(z) = \frac{g^2}{2V}\, y \sum_k \frac{f^2(k)}{\omega^2}\,\frac{1}{(\omega-x)^2+y^2}$$

$$1+\frac{g^2}{2V}\sum \frac{f^2(k)}{\omega^3}\,\frac{x+iy}{\omega-x-iy} = 0 \qquad \omega_0 = x+iy$$

$$\sim 1+\frac{\omega}{\omega-\omega_0}$$

$$1+\frac{g^2}{2V}\sum \frac{f^2(k)}{\omega^3}\,\frac{(x+iy)(\omega-x+iy)}{(\omega-x)^2+y^2} = 0$$

$$1+\frac{g^2}{2V}\sum \frac{f^2(k)}{\omega^3}\,\frac{x(\omega-x)-y^2}{(\omega-x)^2+y^2} = 0$$

$$\sum f^2(k)\; iy \cdot \omega \ldots = 0$$

P.S. Aus viel früheren Briefen W. P.s geht wohl hervor, daß ich allgemeine Gründe für $\vartheta_1 + \vartheta_2 = 0$, $\vartheta_3 = 1$ den halben Phasensprung im Grenzfall des Übergangs von vollen zu komplexen Energiewerten. Es scheint diese Entscheidung bei indefiniter Metrik für diesen ganz besonders zu halten!

Physikalisches Institut
der Eidg. Technischen Hochschule
Zürich

ZÜRICH 7/6
Gloriastrasse 35

4. I. 1957

Lieber Herr Källén,

Haben Sie vielen Dank für Ihren Brief vom 2. d. M. Er kam zugleich auf meinen Tisch mit einem Brief von Heisenberg. Ich fühle mich zwischen zwei Feuern und fühle mich dabei ganz wohl: Habe den Eindruck, daß das Neue Jahr begonnen hat. Schließlich ist Mathematik und Physik objektiv und Psychologie habe ich etwas gelernt im Lauf eines längeren Lebens, viel in Praxis und ein wenig auch in Theorie.

Mein Eindruck ist, Sie haben die Heisenberg'schen Ideen ganz richtig verstanden: die Metastabilität des Dipolzustandes ist aufgegeben: Es handelt sich genau um den Fall Ihrer Figur, der dort mit A_0 bezeichnet ist. (Aposteriori!).

An Stelle der verlassenen Metastabilität des Dipolzustandes hat Heisenberg nun ein neues Axiom:

Im Falle A_0 sollte es unmöglich sein, diesen Dipolzustand zu erzeugen (herzustellen), wenn er im Anfang nicht vorhanden war. (Hier sagte H. explizite in seinen Briefen an mich!) (A_0)

Das muß doch heißen, daß nach H. in diesem Sonderfall (A_0) für die Prozesse

$$N + \theta + \theta' \to V_+ + \theta_1 \quad \nu_1 > 0 \qquad \text{(S. 2 Ihres Briefes).}$$
$$\to V_- + \theta_2 \quad \nu_2 < 0$$
$$\to N + \theta + \theta' \quad \text{Streuphase } \vartheta_3 > 0$$
$$(\vartheta_1 + \vartheta_2 + \vartheta_3 = 1)$$

$\vartheta_1 + \vartheta_2 = 0$, $\vartheta_3 = 1$ gilt. Das ist meine Interpretation von H. ($\vartheta_1 = +\infty$, $\vartheta_2 = -\infty$, ist ausgenommen; darüber hinaus

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Das Lee-Modell, so wie Lee selber es nicht haben will.

Hamiltonfunktion:

$$H = H_0 + H_w + \delta H$$

$$H_0 = mN^2 \sum_{\bar p} \psi_V^*(\bar p)\,\psi_V(\bar p) + m \sum_{\bar p} \psi_N^*(\bar p)\,\psi_N(\bar p) + \sum_{\bar k} \omega(\bar k)\,a^*(\bar k)\,a(\bar k)$$

$$H_w = -\frac{g}{\sqrt{V}} \sum_{\substack{\bar k = \bar p - \bar p' \\ \bar p}} \left(\psi_V^*(\bar p)\,\psi_N(\bar p')\,a(\bar k) + \psi_N^*(\bar p')\,a^*(\bar k)\,\psi_V(\bar p)\right) \frac{f(\bar k)}{\sqrt{2\omega}}$$

$$\delta H = -\delta m \cdot N^2 \sum_{\bar p} \psi_V^*(\bar p)\,\psi_V(\bar p)$$

Hier sind alle Grössen (wie $m, g, \psi(\bar p), a(\bar k)$ usw.) renormalisierten Konstanten oder Operatoren. Die Kommutatoren sind

$$\{\psi_V^*(\bar p), \psi_V(\bar p')\} = \frac{1}{N^2}\,\delta_{\bar p \bar p'} \qquad (5)$$

$$\{\psi_N^*(\bar p), \psi_N(\bar p')\} = \delta_{\bar p \bar p'} \qquad (6)$$

$$[a(\bar k), a^*(\bar k')] = \delta_{\bar k \bar k'} \quad \text{usw.} \qquad (7)$$

Die Funktion $f(\bar k)$ ist der Formfaktor (,der Lee nicht hat) und die Selbstmasse. In den Bezeichnungen von Lee ist weiter $N^2 = Z_2$. Weiter habe ich die beiden Massen m_V und m_N beidegleich m gesetzt. Hierdurch erhalten wir einige formalen Vereinfachungen, ohne dass die interessanten Zügen verloren gehen. Diese Gleichungen lassen sich also explizit lösen, und wir können die Lösung in der folgenden Form schreiben:

$$\langle 0|\psi_N(\bar p)|N\rangle = 1 \qquad (8)$$

$$\langle 0|\psi_V(\bar p)|V\rangle = 1 \qquad (9)$$

$$\langle 0|a(\bar k)|\bar k\rangle = 1 \qquad (10)$$

$$\langle \bar k|\psi_N(\bar p)|V\rangle = \frac{g}{\sqrt{2V}}\,\frac{f(\bar k)}{[\omega(\bar k)]^{1/2}} = \langle N|a(\bar k)|V\rangle \qquad (11)$$

$$\langle 0|\psi_V|\text{Streu}\rangle = \beta(\bar k) \qquad (12)$$

$$\langle \bar k'|\psi_N(\bar p)|\text{Streu}\rangle = \delta_{\bar k', \bar k} + \alpha(\bar k', \bar k) = \langle N|a(\bar k')|\text{Streu}\rangle \qquad (13)$$

$$\alpha(\bar k, \bar k') = \frac{g}{\sqrt{2V}}\,\frac{\beta(\bar k)\,f(\bar k')}{\sqrt{\omega'}} \left\{ P\frac{1}{\omega' - \omega} + i\pi\,\delta(\omega' - \omega) \right\} \qquad (14)$$

$$\beta(\bar k) = -g\,\frac{f(\bar k)}{\omega\sqrt{2\omega V}} \left[1 + \frac{g^2\omega}{2V} \sum_{\bar k'} \frac{f^2(\bar k')}{\omega'^3} \left\{ P\frac{1}{\omega' - \omega} + i\pi\,\delta(\omega' - \omega) \right\} \right]^{-1} \qquad (15)$$

$$\delta m = -\frac{g^2}{2V} \sum \frac{f^2(\bar k)}{\omega^2} \times \left[1 - \frac{g^2}{2V} \sum \frac{f^2(\bar k)}{\omega^3} \right]^{-1} \qquad (16)$$

$$N^2 = 1 - \frac{g^2}{2V} \sum_{\bar k} \frac{f^2(\bar k)}{\omega^3} = 1 - \left(\frac{g}{g_{krit}}\right)^2 \qquad (17)$$

$$g_{krit}^{-2} = \frac{1}{2V} \sum \frac{f^2(\bar k)}{\omega^3} \qquad (18)$$

In the Landscape of Point-Functions – a Preview

> … but I don't expect any "miracle". First I must (hopefully with the help of Källén) still learn a lot of mathematics (translated from German).
>
> Pauli to Markus Fierz (1954)

Källén firmly believed that mathematical rigor, which he sometimes called "epsilontics" does not lead to new knowledge in physics. Yet, he really loved applied mathematics and was very good in doing long and difficult calculations. As Pauli expressed it in a letter to Heisenberg:

> Källén could *not* resolve this problem (despite strong efforts) and as he is mathematically very skillful, I suppose that even I will not make any progress in this respect.

Källén's interest in doing mathematical work was described earlier in this book. The chapters here below give further insight into his work in this area. We first give some historical recollections by leading scientists, and conclude by presenting an article by Arthur Wightman, written in 1998, on the occasion of celebration of the 50th anniversary of the French Institute IHÉS (Institut des hautes études scientifiques; Institute for advanced studies). He was not asked to talk about his friend and collaborator Gunnar Källén and yet that is what he did. For more information, see the "General Commentary on the Vacuum Expectation Value Program" in Part 5 of this book.

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Early Work on Vertex Function and Rochester Conference 1957

The early phase of Källén's work in this field is summarized in his invited talk, "Structure of vacuum expectation value of three field operators" that he presented at the 7th Annual Rochester Conference [1].

A leading scientist who was present at the above Rochester Conference was John Polkinghorne. He describes Källén at the Conference as follows [2]:

> "The strong-interaction theorists continued their exploration of the complex plane. Scattering amplitudes might be well behaved there but they could not be totally so without degenerating into triviality. An analytic function without any singularities (points of bad behavior) can only be a boring constant. Since that would not do to describe the complexities of physics, it became necessary to identify what singularities were actually present. The first to be noted were called normal thresholds, points at which sufficient energy became available to permit the possibility of creating a further extra particle in the final state. The 'hiccup' represented by this new option produced the kind of singularity known to the mathematicians as a branch point. Normal thresholds occurred at real values of the energy and they enjoyed an immediate physical interpretation, it would have been very satisfactory if they and the single-particle poles had been the only singularities one had to reckon with. Schwinger presented a paper at Rochester 7 in which, in his rather high-flown style, he essentially made that claim for a particular set of amplitudes. In the Proceedings we are told concerning the aftermath of that particular talk that part of the discussion was lost. I suspect that was a diplomatic move. I recall that as Schwinger's ringing tones died away, Källén rose to his feet. He said he didn't know very much about the problem but he knew enough to be able to say that the previous speaker was totally wrong. An instant chill descended on the meeting at this stinging rebuff delivered to a great physicist. Källén was right, all the same. The singularity structure of scattering amplitudes was to prove to be very rich and subtle, beyond naïve expectation."

In their scientific biography of Schwinger, Jagdish Mehra and Kimball Milton [3] have given the following account of Schwinger's lecture at the 1957 Rochester Conference (pp. 380–381):

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"An excursion into dispersion relations:

In 1957 Schwinger became interested in the analytic structure of the Green's functions of quantum field theory, in particular in obtaining spectral forms or dispersion relations for two- and three-point functions, that is, for propagation functions and for vertex amplitudes. He presented his results at a Rochester conference, in April 1957, in a session chaired by Marvin Goldberger. His presentation, was followed by a response by Gunnar Källén who 'most violently disagree[d] that the formula written down is the most general representation of the three-point function.' After the meeting Schwinger submitted a supplement to his presentation for the conference proceedings to one of the editors, his former student Roger Newton. The issue is clarified by a letter from Stanley Deser sent to Schwinger from Copenhagen [where Källén was employed], dated 26 [April] 1957: 'The form you wrote down is not the most general under the usual assumptions, but it is in fact equivalent to the form Källén had, and to which the Lehmann-Jost counter example applies.'

The Green's function is given by a time-ordered product of fields, or, in terms of momenta p_i and spectral masses K_{ij} [Note that the equations below are taken from Schwinger's talk at Rochester 7, as reported and labeled in [3]]

$$\mathcal{G} =< (\Phi_1\Phi_2\Phi_3)_+ >\sim \int e^{i\Sigma p_j x_j}\delta(\Sigma p_i)\delta(\Sigma z_i - 1)f(K..)dpdzdK\times$$
$$\times\,[p_1^2 z_2 z_3 + p_2^2 z_1 z_3 + p_3^2 z_1 z_2 + K_{12}^2 z_3 + K_{13}^2 z_2 + K_{23}^2 z_1 - i\epsilon]^{-3} \tag{11.1}$$
$$= \int \Delta_+(x_{12}^2, K_{12}^2)\Delta_+(x_{13}^2, K_{13}^2)\Delta_+(x_{23}^2, K_{23}^2)f(K's)(dK's) \tag{11.2}$$

Δ_+ being the scalar propagation function,

$$\Delta_+(x^2, K^2) = \int (dp)e^{ipx}/[(2^4)(p^2 + K^2 - i\epsilon)]. \tag{11.3}$$

'So, the consensus is that while assumption of the form (11.1) or (11.2) for the Green's function does lead to the dispersion relation, it is as yet insufficient, since the theory may admit of more singularities, though of course perturbation theory always satisfies (11.1) or (11.2).'

After receiving this letter, Schwinger sent a telegram to Roger Newton dated 10 May 1957:

'Promised supplementary remarks are being sent. JS.'

The Proceedings of the Conference contain both the original manuscript of Schwinger's lecture , Källén's toned-down response, and a five page supplement by Schwinger, followed by another Källén rebuttal."

Mehra and Milton conclude [4]

> "Evidently, in view of the hostile response, Schwinger never wrote a journal article on this subject."

Before leaving the Rochester 1957 Conference, it is perhaps of some interest to record a short dialogue between Marvin L. Goldberger and Källén at this meeting, which gives more information about Källén's objection to Schwinger's work:

> Goldberger: You said that the naive formula which Schwinger wrote down was, in fact, a possible representation of the three-fold vacuum expectation value. To the extent that it represents, for example, what comes out of lowest order perturbation theory, it is indeed a possible formula. Nonetheless, you made a number of cryptic remarks that the formula was in itself self-contradictory. How can it be both possible and self-contradictory?
>
> Källén: What is self-contradictory is the statement that the formula is the most general representation with the required regularity. The particular formula you are referring to is supposed to represent the analytic function in x-space, regular in the cut plane. The corresponding function in p-space has a more complicated domain of regularity. I think I said it twice, maybe three times, even if I didn't prove it, that it follows from very general arguments that the domain of analyticity for the function in x-space and the function in p-space must be exactly the same. Therefore, as the domains here are different, it simply can't be the most general function. Is that clear?
>
> Goldberger: Yes. Thank you.

References

1. G. Källén, Proc. of 7th Annual Rochester Conference (1957), IV 17–27; See the Commentary on Paper [1957d]: "Structure of the Vacuum Expectation Values of Three Field Operators" in Part 5 of this volume
2. John Polkinghorne, "Rochester Roundabout, the Story of High Energy Physics" , W.H. Freeman, New York, (1989)
3. J. Mehra and K. Milton, "Climbing the Mountain, the Scientific Biography of Julian Schwinger", Oxford U. Press (2000)
4. *ibid* p. 381

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A. S. Wightman: Theoretical Physics at the IHÉS – Some Retrospective Remarks

In March of 1959, mathematics was firmly established at the IHÉS by the appointment of Jean Dieudonné and Alexander Grothendieck as professors. Their seminars attracted mathematicians from abroad as well as from the region of Paris. In physics, things developed more slowly; no permanent appointments were made in 1959. However, Res Jost, Léon van Hove, Murray Gell-Mann and Louis Michel were named permanent invités. The first visiting member appears to have been Eduardo Caianiello in April 1959. As far as I know the next two visitors were Gunnar Källén and I in May and June 1960. … The director of the IHÉS [was] Léon Motchane. Källén and I located our operations in the garden. Fortunately, the weather was outstandingly good that summer. We had plenty to talk about, as I will now relate in some detail, since our preoccupations had a connection with Motchane's idea that the IHÉS should be a place where mathematicians and physicists interacted.[1]

The discussions that Källén and I had at the IHÉS was a sequel to a period of joint work in Copenhagen (1956–58) in which we computed the holomorphy envelope of a certain domain in C^3 of which the definition is determined by the properties of a class of quantum field theories [1]. This concrete mathematical problem was arrived at by the confluence of two quite different streams of thought which we had separately developed in the early 1950's.

Källén had studied the problem of generalizing the perturbative theory of renormalization in quantum electrodynamics to a non-perturbative theory. He had arrived at what seemed to him to be a convincing argument that at least one of the so-called "renormalization constants" has to be infinite [2] He had used in his work the so-called spectral representation of the electron and photon propagators. These are functions of a single complex variable analytic in the complex plane cut along the positive real axis and the spectral repre-

[1] Note added: This article was published *in* "Festschrift for the 40th anniversary of the IHÉS (1998), pp. 191–194". Here it has been retyped by me (CJ) and the references have been re-labeled, following notations used in this book. Moreover, I have added footnotes for further clarification. I wish to thank the IHÉS Secretariat for allowing the re-use of their publication in this book.

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sentations display the function in a standard form in which the distinctions between distinct theories appear in measures on the mass spectrum of the theories. Källén suspected that he would be able to refine his argument and sharpen his result if he had a spectral representation for vertex functions analogous to the known one for propagators [3]. However, the vertex functions are functions of three complex variables and such representation was not known for them. During roughly the same period that Källén was working on these ideas, I was trying to answer the questions: what should be the mathematical definition of a quantized field?, of a quantum theory of fields? I had spent a year (1951–52) in Copenhagen on a National Research Council postdoctoral fellowship where I took advantage of an easy commute to Lund to work with Lars Gårding.[2] From our discussions it became obvious that, in a very slight generalization of what was already codified in Laurent Schwartz's book on distribution theory [4], quantized fields ought to be (in general unbounded) operator-valued distributions. I soon realized that under quite general assumptions the content of a quantum field theory could be expressed in terms of the vacuum expectation values of products of fields [5]. These are distributions, $F(n), n = 0,1,2,\ldots$ defined for the special case of a scalar field, ϕ, by

$$F^{(n)}(x_2 - x_1, x_3 - x_2, \ldots, x_n - x_{n-1}) =< \psi_0, \phi(x_1)\ldots\phi(x_n)\psi_0 >$$

where ψ_0 is the vacuum state. The Lorentz invariance of the theory under a Lorentz transformation, Λ, is simply the invariance of the $F^{(n)}$

$$F^{(n)}(\Lambda\xi_1, \ldots, \Lambda\xi_{n-1}) = F^{(n)}(\xi_1, \ldots, \xi_{n-1})$$

The assumption that the physical states of a quantum field theory satisfy the spectral condition (all energy-momentum vectors, p, lie in the future cone $V_+ : (p^2 = (p^2{}_0 - \mathbf{p}^2 \geq 0, p_0 \geq 0)$ implies that $F^{(n)}$ is the Fourier transform of a distribution $G^{(n)}(p_1, \ldots, p_{n-1})$ whose support is contained in the product of the cones $p_j \epsilon V_+, j = 1, \ldots, n-1$. This in turn implies that the $F^{(n)}$ are boundary values for $\eta_j \to 0$ of a function of $n-1$ complex vector variables $z_1 = \xi_1 + i\eta_1, \ldots z_{n-1} = \xi_{n-1} + i\eta_{n-1}$ holomorphic for $\eta_1, \ldots \eta_{n-1} \epsilon V_+, j = 1, \ldots, n-1$, a domain which will be called the tube. If for brevity this analytic function is also denoted $F^{(n)}$, the condition of Lorentz invariance continues to be expressed:

$$F^{(n)}(\Lambda z_1, \ldots, \Lambda z_{n-1}) = F^{(n)}(z_1, \ldots, z_{n-1})$$

[2] Gårding, born 1919, is a distinguished Swedish mathematician at Lund University. He has played a very important role in the development of mathematics in Sweden.

According to [6] this equation, valid for Λ a real Lorentz transformation and $z_1, \ldots z_{n-1}$ in the tube, can be continued analytically to a complex Lorentz transformation and used to continue $F^{(n)}$ as a singled-valued analytic function to all points $\Lambda z_1, \ldots \Lambda z_{n-1}$ that can be reached with complex Λ from a point $z_1, \ldots z_{n-1}$ of the tube; this domain will be called the extended tube.

A further analytic continuation of $F^{(n)}$ can be achieved if the quantized field satisfies the condition of local commutativity

$$[\Phi(x), \Phi(y)] = 0 \quad \text{if} \quad (x-y)^2 < 0$$

This implies

$$\begin{aligned} &F^{(n)}(z_1, \ldots, z_{j-1}, z_j, \ldots z_{n-1}) \\ &\quad = F^{(n)}(z_1, \ldots, z_{j-1} + z_j, -z_j, z_j + z_{j+1}, \ldots, z_{n-1}) \end{aligned}$$

When $z_1, \ldots z_{n-1}$ runs over the extended tube, $z_1, \ldots, z_{j-1} + z_j, -z_j, z_j + z_{j+1}, \ldots, z_{n-1}$ moves over a permuted extended tube, so $F^{(n)}$ turns out to be analytic and single valued in the union of the extended tube and permuted extended tube.

When I arrived in Copenhagen in September of 1956, Källén informed me that he had a representation formula for vertex function from which he could read off the analyticity domains. The result was that in the three appropriate complex variables, they were analytic in the product of three complex planes cut along the positive real axis. Källén wrote to Pauli in Zurich about this result. The response was a letter from Harry Lehmann and Res Jost which presented an example of a function of three complex variables that satisfied the physical requirements that Källén had imposed but had a singularity where his integral representation said it could not. In the first week in January of 1957 Källén and I discussed the situation and concluded that we ought to try to compute the holomorphy envelope of the domain that Douglas Hall and I had determined. The holomorphy envelope would presumably not include the point where there was a singularity in the example of Lehmann and Jost.

Källén and I worked steadily on the holomorphy envelope for several months but with only partial success. Then our ways parted. I went on a tour that involved a visit with Eduardo Caianiello at the old Physics Institute in Naples as well as brief stops in Paris and Münster to consult mathematicians who knew a great deal more than Källén and I did about holomorphy domains in several complex variables. In Paris, it was Henri Cartan and Pierre Lelong; in Münster Heinrich Behnke, Hans Grauert, Reinhold Remmert and

Friedrich Sommer.[3] All listened politely and tried to be helpful. I believe that they were somewhat astonished to see theorems of the theory of analytic functions of several complex variables, a branch of pure mathematics that they had cultivated for its own sake, used in physics; it was reassuring to realize that we had not overlooked some basic techniques and the use of what Behnke and Thullen had called the Kontinuitätsatz was regarded by the expert as a sensible way to proceed.

Meanwhile, Källén had a rather different experience. He attended the 1957 Rochester Conference on High energy Physics. To his consternation, he found from the scheduled talk of Julian Schwinger that Schwinger had independently arrived at the very same integral representation of the vertex function that had been dispatched in the fall of 1956 by the example of Lehmann and Jost. A spirited discussion ensued in which Källén was somewhat at a disadvantage since he (and I) did not know the domain of analyticity. Reports reaching me indicated that the audience (except for R. P. Feynman) was firmly on the side of Schwinger. In any case, when we got back to Copenhagen, we settled down to work and, by the middle of the summer had computed the boundary of the holomorphy envelope [1]. This lengthy digression makes it possible for me to describe in a few words what Källén and I were talking about in the garden of the Fondation Thiers in 1960. It was the progress in a grand program of research on the structure of quantum field theory (often referred to as the linear program). There were three steps

1) Compute the holomorphy envelope of the union of the permuted extended tubes.
2) Find an integral representation for the most general function analytic in the resulting domain.
3) Exploit the integral representations obtained in 2) to investigate the possible forms of quantum field theories.

There were some important positive results. Using the analyticity domain for $F^{3\vartheta}$ determined in [1]. Källén and Toll [7] found an integral representation for $F^{(3)}$, thus carrying out 2) for that case. Unfortunately, that integral representation turned out to be less useful than the optimists had hoped, much less useful than the spectral representation for $F^{(2)}$. The next obvious problem was step 1) for $n = 4$. Despite heroic efforts by Källén and a number of coworkers that problem turned out to be too hard. In fact, I think it is fair to say the same thing about the program as described by 1) 2) 3) as a whole; it was all very grand but it turned out to be too hard.

[3] According to Källén, Friedrich Sommer was the only mathematician who ever helped him. Unfortunately, since this happened before 1958 the relevant correspondence is not found in the Källén Collection.

There was important progress in our understanding of quantum field theory in the 1960's and activity at the IHÉS played a significant role, but different approaches were involved. I will not try to survey them, but only mention one development. When I returned to the IHÉS for the year 1963–64, two Princeton graduate students came along with me, Arthur Jaffe and Oscar Lanford. Their theses were among the opening salvos in what later came to be called "constructive quantum field theory."

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Pauli's Complaints about Källén's Work on the n-Point Functions

Already by the end of 1956, Källén expressed his dissatisfaction to Pauli that his work on the three-point function was not advancing as quickly as he had hoped. Pauli wrote back (letter [2413] in the Pauli Collection) on 14 December 1956 (translated from German):

> … It is, no doubt, painful for you but also instructive, to have pursued a red herring for such a long time. …

A few months later, Pauli expressed his dissatisfaction that, in his opinion, Källén had abandoned physics and had gone into futile mathematics. However, Källén disagreed that he was doing useless research. On the 10 July 1957 (letter [2664] in the Pauli Collection), Källén wrote to Pauli (translated from German):

> I don't understand why you rail so much against Wightman axioms. That is in fact for him a great honor. Generally, it is the case that whenever you in such a manner rail against something, you become soon interested in it. I hope, however, that this is not going to be the case this time. My objection against these axioms is the following. It seems to me that the locality properties of the *interaction* are not taken into account. In fact the commutativity of the field operators at space-like distances is there, but it seems to me that in the conventional theories the presence of two field operators in the *same space-time point* in the interaction is independent of that. For example in the Dirac equation of quantum electrodynamics one has, on the right-hand side the term $i\,e\,A(x)\,\psi(x)$. Even when $A_\mu(x)$ and $\psi(x)$ are *renormalized* fields and in addition each of them by itself exists and obeys the correct commutation relations, the product is in general not finite, but must be defined with the help of compensation terms [counter terms]. In this way the infinities enter into the theory and I hardly believe that one has taken into account all the important traits of the theory, as one doesn't even allude to the presence of such terms, already in the fundamental concepts of the theory.

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Källén goes on to explain in more detail what the problems are. The letter ends with Källén throwing a swipe at Schwinger, which he most probably enjoyed doing:

> I don't know if Jost has told you about Schwinger's new attempt to give a general representation for the three-point function [vacuum expectation value of the product of three field operators]. In Lille, he gave a talk about it. But his representation is completely wrong, as Wightman and I have verified. The function presented [by Schwinger] has singularities, even where these are not allowed to appear.

However, Pauli remained sceptical of the relevance of this program to *physics* up until his death. In a letter ([2997] in the Pauli Collection) addressed to Jauch on 17 May 1958 he wrote (translated from German):

> Mathematical virtuosity, theory of functions (of one or several complex variables) etc seem to me indeed good for derivation of the consequences of logically closed mathematical theories. However, when it concerns finding new laws of nature with new underlying concepts, a different kind of intuition and feeling is required. I don't believe that the researchers of the type Lehmann, Wightman, Källén, etc are going to be helpful in that respect.

Indeed, the researchers of the type Lehmann, as Pauli put it, were enjoying themselves, as long as they were making *mathematical* progress. They were *not* digging in the mathematically muddy terrain where the new physics was hidden.

Move to Particle Physics – a Preview

After having written his book "Elementary Particle Physics", published in 1964, Källén decided to move into this for him new field of research. Being a virtuoso in doing difficult calculations in field theory, it was natural for him to attack the problem of higher order corrections in weak interactions where there were many terms to keep track of. This was well before the era of high-speed personal computers that are very helpful for performing such tasks.

Källén's working hypothesis was that the divergences in the radiative corrections to neutron beta decay were cured by strong interaction formfactors. However, he was not quite sure as to how to implement this idea. There could be several such formfactors and their analytic structures were not known. He had to make simplifying assumptions in his formfactor approach while James D. Bjorken and others were pursuing a different path and getting results that Källén didn't approve of and was upset about.

The following chapters are devoted to the above issues, including a chapter in which Alberto Sirlin, a true pioneer and expert in the field, gives his verdict.

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Work on Radiative Corrections

Källén considered the case of neutron beta decay, $n \rightarrow p + e + \overline{\nu}$, where the radiative corrections were believed to be logarithmically divergent. In the framework of local current-current interactions, which was the standard theoretical model those days, the heavy particles are locally coupled in a common vertex, $n \rightarrow p$ that should include the desired formfactor. However, Källén realized that he had to make simplifying assumptions such as to take just one effective formfactor.

One of my (CJ) duties as a Ph.D student was to file articles that Källén had received and wished to keep. He would receive a large number of preprints every week and would neatly put them on his desk. Usually, I would take the whole lot and file them. On one occasion, however, he gave me an article by J. D. Bjorken [1] to file separately. It was obvious that he was puzzled and upset about it. The author, using current algebra, was claiming that the logarithmically divergent part of the electromagnetic correction to the process $\pi^- \rightarrow \pi^0 + e + \overline{\nu}$ is nonvanishing. A similar result was later reported by Abers et al. [2]. In other words, the logarithmic divergence, in radiative corrections was *not* removed by strong interactions. This was indeed a surprising result that Källén, to begin with, considered to be incorrect. He expressed his disbelief most clearly in a letter to Harry Lam [3] in connection with a Summer School at McGill University in 1967:

> "Concerning my lectures I should, after all, suggest that I talk about radiative corrections in weak interactions. I do think I have a few non-trivial remarks to make there even if I will mainly make a pedagogical summary of the subject. Among other things I shall also discuss (critically) some recent attempts (Bjorken, Norton, et al.) to use current algebra for this purpose. This will, at least vaguely, connect up with the main theme of the Summer School."

Here I wish to add an aside on Bjorken-Källén relationship. As noted here above Källén was irritated by Bjorken's work on radiative corrections. We have also seen, in Chap. 56, that he was upset by Bjorken's summary at Schladming 1965. Nonetheless, Källén's correspondence shows that he was a decent scien-

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tist who realized the value of Bjorken's contributions, in spite of not agreeing with him, as follows.

Near the end of May 1968, a "Nobel Symposium on Elementary Particle Theory: Relativistic Groups and Analyticity" took place in Sweden. Källén was on its advisory committee and corresponded with the Chairman of the organizing committee, Nils Svartholm. He was not satisfied with the choice of invitees and suggested to Svartholm to replace one of the speakers on the list by Bjorken. Finally, Källén himself didn't go to the Symposium, in spite of the fact that it was gathering some of the greatest physicists of the time and was taking place in his backyard". Nonetheless he supported the participation of five young people from Lund – I was one of them.

Källén's Final Words on Radiative Corrections

In his 1968 summary talk [4] at Schladming School, Källén presented his final views on the question of divergences in radiative corrections to β-decay. Both he and Alberto Sirlin (see the next chapter) lectured, at this School, on radiative corrections. Here below are some excerpts from what Källén had to say:

> "… it is perhaps appropriate to start with the contribution by Sirlin about radiative corrections to weak interactions. Sirlin described some recent developments which essentially did not exist a year ago – or at least existed only in a very primitive form."

Then he mentioned the relevant work within the framework of current algebra:

> "However, if one believes in this technique one ends up with the conclusion that it is impossible to calculate the radiative corrections to a weak decay in a reliable way inside present day theory [Källén means due to inherent divergences]. Today, I rather believe that, in contradistinction to the situation in the summer of 1967, everybody agrees that the fact that you get a divergence in the answer cannot be the final word. After all, we do have an effect which is more or less observable. It is, quite important to calculate the radiative corrections to ordinary β-decay if one wants to determine the Cabibbo angle from this process. There have been various suggestions in the literature this fall on how to remove the divergence."

Among the suggestions that had been made in the literature, Källén quoted:

- abandoning the ordinary quark model in favor of a model with three basic triplets;
- introducing the intermediate vector bosons $W^{\pm}$, where one finds that the mass of W should be about 10 GeV.

Finally, Källén closed this issue by stating:

> "Everybody seems to agree that the final radiative corrections should be finite. However, the exact way in which they do become finite has been discussed very seriously this year but was essentially bypassed previously. In this respect, the discussion of this year has been more fundamental but perhaps also more dangerous than the earlier calculations."

Of course, in those days strong interactions were not yet granted asymptotic freedom. Källén can be forgiven for not knowing that the formfactors are irrelevant at short distances and high energies, as was being indicated by the work of Bjorken and others. Furthermore, the new-born Electroweak Theory hardly got any attention until the Amsterdam International Conference on Elementary Particles, 1971, i.e., more than two years after Källén's death. The situation was indeed confused!

Correspondence on Radiative Corrections

It is interesting to note that in the Källén Collection there are only few letters on the topic of radiative corrections. There is, however, a letter that brings out the typical Källénian irritation when he believes that he has not been properly quoted. On October 19, 1966, Källén writes to Nicola Cabibbo at Harvard University, Cambridge, Mass. USA:

> "Dear Dr. Cabibbo:[1]
>
> Thank you for your letter and the preprint of your paper about the weak interaction. I am not terribly happy with the way you presented my results

[1] Nicola Cabibbo (1935–2010) who sadly passed away recently, was a distinguished theoretical physicist who played a leading role in the development of particle physics. I (CJ) was his scientific secretary at the "Nobel Symposium on Elementary Particle Theory: Relativistic Groups and Analyticity" (see Chap. 1) and got to know him better later on primarily from the meetings of the Scientific Policy Committee of CERN. Cabibbo was a deep thinker and a quiet man. Whenever he made a statement everyone listened carefully – he had something important to say. The reader may find a great deal of information about him on the internet.

> and think this is the last time I will let a rapporteur give a description of my work. Next time I shall absolutely refuse to be put into such a position again.
>
> I do not think it is worth while to enter into a detailed discussion of your remarks and I will just mention that I still feel your heavy emphasize on Foldy's view is quite out of proportion to its significance. Also, it is not clear from your comment in its written form that contributions from his diagram as well as from others are included in my calculation through the form factors. However, one explicit mistake in your review could perhaps be corrected. You say on the bottom of p. 12 that I am using a gauge 'where the divergence of the electron self energy is made to disappear'. This statement is incorrect as the self energy is a gauge independent quantity. What actually is gauge dependent is the field operator renormalization of the electron field. In a standard calculation, this quantity appears as a contribution from the self energy diagram but is really very different from the self energy itself.
>
> Finally, I can perhaps mention that a very preliminary account of the calculation has recently appeared in the Proceedings of the Schladming meeting in Austria this year. However, I should explicitly like to mention that several of the things said in that paper have been modified and the calculation is carried much further now that [read: than] what is indicated there.
>
> Sincerely yours,
>
> Gunnar Källén"

Going through the proceedings of conferences at about the time when the above angry letter was written one actually does find Cabibbo as a rapporteur at the XIII International Conference on High-Energy Physics held at Berkeley, California, 31 August-7 September 1966. He is reporting on "Weak interactions". Källén is a participant. No discussion session has been recorded after Cabibbo's plenary talk. Here is what Cabibbo had to say about Källén's work that made him angry:

> "It has been noted by Källén [here Cabibbo refers to G. Källén, oral presentation at this Conference. He organizes the computation so as to be able to use on-shell form factors only.] that, although the overall corrections must clearly be gauge invariant, the contribution of each particular graph depends on the choice of gauge for the electromagnetic field. In particular one can choose a gauge such that the divergence of the electron self-energy diagram is made to disappear. In this particular gauge the overall radiative corrections may turn out to be finite if the nucleon form factors vanish fast enough at high momentum transfer. A first evaluation by Källén yields corrections which are finite and agree with the ones computed by Kinoshita and Sirlin with a cut-

off[2] M_P. The situation is not very clear, still, because one would expect that if the corrections are finite in the gauge chosen by Källén they should be finite in any gauge. It would probably be worthwhile to go over the problem again in the normal gauge. Foldy noted in a discussion at the Seattle Summer Institute of Theoretical Physics, 1966, that at the weak vertex one has a flow of charge from a pointlike particle (the electron) to a diffused one (the proton). This could force the introduction of a n p e ν γ vertex and of diagrams like diagram f in Fig. 2-4, and could perhaps remove the divergence of the electron self-energy. The suggestion is interesting and should be looked into."

Before leaving this Conference, we note that its opening talk, given by Murray Gell-Mann, is wonderful as it clearly exhibits the conceptual difficulties encountered by physicists in those days. As an example, Gell-Mann reports:

"... Now what is going on? What are these quarks? It is possible that real quarks exist, but if so they have a high threshold for copious production, many BeV if this threshold comes from their rest mass, they must be very heavy and it is hard to see how deeply bound states of such heavy real quarks could look like $q\bar{q}$, say, rather than a terrible mixture of ... I would guess that they [meaning quarks] are mathematical entities that arise when we construct representations of current algebra, which we shall discuss later on. ..."

Indeed, in those days, concepts such as color and confinement were still waiting to make their triumphant entry into the realm of physics. Returning to Källén and radiative corrections, in the next chapter Alberto Sirlin, a true pioneer and expert in the field, gives his views on the subject as well as on Källén's contribution.

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[2] M_P stands for the proton mass (and not the Planck mass). In those days, one GeV was considered to correspond to a very high energy in beta decay and was taken to be a reasonable cut-off for that process!

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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: $3(\alpha/2\pi)\,ln(M_Z/2E_m) + (\alpha/2\pi)\,ln(M_Z/M_N)$, 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."
>
> 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.

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Love for Mathematics

Källén used to do long and complicated computations with great ease. He was pleased whenever his “conjecture” that epsilontics are irrelevant turned out to be correct. For example, in a letter to Wightman in 1960, he writes that a student of his (Göran Eriksson) has shown that a result by Wightman is correct provided a function called $\lambda(m)$ is negative but *not otherwise*. He continues

> “... Personally, I feel that this solution of the mystery is very satisfactory as it shows how irrelevant high brow epsilontics (like the existence or non-existence of Hankel transforms or inversion of orders of integration etc.) is in physical applications and how much more important it is to keep track of elementary mathematics like the sign of $\lambda(m)$!
>
> Yours happy,
>
> Gunnar Källén”

Perhaps it is appropriate to also say a few words about Pauli’s attitude in this respect. Oskar Klein, and several others were asked to write articles for a festschrift to honor Bohr on the occasion of his 70th birthday. Klein wrote to Pauli asking him if he thought it would be a good idea to avoid equations to some extent and say it in words. On 1 March 1955 (letter [2032] in the Pauli Collection), Pauli thundered back (translated from German):

> There is no better method to make it [the article] completely incomprehensible for the reader than this! (I see the Devil himself in front of me, who has puffed you up with pomposity!). Please, don’t overestimate your words. Formulas are much more easier to understand!

Källén shared Pauli’s opinion. He was interested in results expressed clearly, which is only possible using equations. He was suspicious of high-flown presentations. For him they were signaling that the speakers didn’t understand what they were talking about. They were just piling up ambiguous phrases and pretending that they had done something. He would insist that they write down the corresponding equations. Sometimes, on such occasions, an interest-

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ing battle (at least for the students) would ensue. Once, at a seminar a speaker who had claimed to have no Hamiltonian had to admit, after Källén's interventions, that indeed he did have one, but had simply not been aware of it.

Indeed, physics is not mathematics and vice versa. But (applied) mathematics has been and is the language of theoretical physics. There is no evidence that Källén cared about pure mathematics.

Prelude to Källén's Work on the Generalization of the Cauchy Integral

Complex numbers constitute a great gift to the mankind and the complex plane is a wonderful environment to wander around in, looking for poles, cuts, singularities, etc. It is difficult to imagine modern theoretical physics without complex numbers. How would we have described quantum mechanics, or quantum field theory without them?

One of the most profound findings in the theory of functions of a single complex variable is the Cauchy theorem. A question of interest is: does it have a generalization in a theory with several complex variables?

Källén was attracted to the mathematical world of several complex variables because of his work on the vacuum expectation value of the product of several field operators, what he used to call the "n-point functions" – n being the number of such field operators. He was not alone. The subject captured the attention of a whole community of theoretical physicists, among them Freeman Dyson, Res Jost, Harry Lehmann, John Toll and Arthur Wightman, with all of whom he was in contact. The mathematicians, as expected, had already done a great deal of work in the field of functions of several complex variables but the physicists found that they had not quite dealt with those aspects that were of interest to them. Källén and his colleagues did not need the most general results, so general that they didn't give any information, but were looking for results that satisfied a certain set of physical requirements.

In spite of making sarcastic comments about mathematics, now and then, there can be no doubt that Källén loved the subject. He was particularly interested in doing "intuitive" useful mathematics, forgetting about what he called the "epsilontics". He once said that his mathematician colleague Lars Gårding worked exactly like a physicist. He would also do "back of envelope" calculations, would interchange the orders in doing sums and integrals, etc. All these intermediate steps were easy to grasp but when he published his work, one couldn't recognize anything, he said. All the "dirty" intermediate steps had become clean, polished and consequently incomprehensible to physicists.

Källén spent the spring term of 1964 at the University of Maryland, his host being his collaborator John Toll. During this visit he gave a course on advanced field theory. Back in Lund, in the fall of 1964, he wrote the article "A Connection Between the Bergman-Weil Integral and the Cauchy Integral", reproduced in the next chapter of this book. In a letter dated 21 October 1964, addressed to Professor Claude Kacser, he explains how this work was "born":

"Dear Kacser:

I send you today by separate mail a manuscript called 'A connection between the Bergmann-Weil [should read Bergman-Weil] Integral and the Cauchy Integral'. Probably, it will take some time to arrive because the manuscript goes by surface mail and this letter by air mail. The content of the manuscript is essentially the derivation of the Bergmann-Weil integral which I precepted [presented?] last spring in College Park. At the end of the paper I have an acknowledgement where several people are mentioned, among them yourself. In case you should feel surprised about this, let me explain that it was really your insistence that a simpler derivation of the Bergmann-Weil integral than the Sommer derivation must exist, which maid [made] me start thinking along the lines presented in the manuscript. Therefore, I feel it is justified that you are being thanked for 'helpful discussions'. In case you don't agree, please let me know, and I will delete your name in the final version of the manuscript. The one which is sent to a selected few for the moment is a very preliminary version. I am not even quite sure yet that I am going to make a formal publication out of the whole thing.

Best regards, also to your wife.

Sincerely yours

Gunnar Källén"

Letter to Raymond Stora

Källén's appreciated Raymond Stora[1] and vice versa. On December 7, 1964 he wrote the following letter to him:

"Dear Ray,

Thank you very much for your letter which arrived on Saturday. A copy of my little note on the Bergmann-Weil integral is sent to you by separate mail.

[1] Raymond Stora, is among the world's experts on quantum field theory.

> Thank you very much for mentioning the paper by Norguet. I was not aware of it and am very grateful to you for pointing it out. Actually, I have been asking a few mathematicians (including Gårding and Sommer) if this simple discussion of the Bergmann-Weil determinant as a Jacobian could be found somewhere in the literature. Sommer has not answered yet and Gårding said he didn't know about it. Of course, the argument is so simple that someone nearly must have done it before. Again, thank you very much for the reference.
>
> Let me warn you explicitly that the note about the Bergmann-Weil integral is not intended to be a formal paper, but only a kind of private entertainment. This is also stated at the beginning of the paper. Concerning the embedding of the polyhedron in a bigger space, Gårding has pointed out one thing to me which probably is rather serious. Perhaps you note that on the bottom of p. 15 and on the top of page 16 I make certain rather strong assumptions about what happens in the limit when the number ϵ goes to zero. Gårding has been able to give explicit counterexamples where these assumptions are not fulfilled. Consequently, my attitude on p. 16 that the details of the limit are probably only a question of epsilontics and not to be taken too seriously is not justified any more. In the particular example Gårding was able to give, it also turned out that only the original Bergmann-Weil formula was valid and not the modified one with the q replaced by the Q. Therefore, this embedding technique, however amusing, must be handled with extreme care for the moment. I don't know yet if it is possible to improve it.
>
> You are quite right that we had very definite plans of coming to Bures during the Christmas vacation. However, my wife has been in the hospital for some time. She has had a serious operation and is now too weak to travel. Therefore, it has been necessary for us to cancel our plans for the moment. However, I hope we will be in Bures again in a not too distant future. …

The manuscript, of the article on the Bergman-Weil integral, found in the Källén Collection has two interesting non-scientific features:

(1) Throughout the manuscript, as also in the letter above, the name Bergman has been spelled with two n's as Bergmann. It occurs 31 times! However, a red pen has systematically crossed out the last n – in all the 31 cases. Källén had a very competent secretary, Mrs. Margareta Bergsten. The letter was dictated to her and the manuscript was typed by her. It could be that she, or even Källén himself, thought Bergman in question, not being a Swede, must be a German and therefore his name should be spelled with two n's while the Swedish version of the name would normally be written with only one n. It turns out that this gentleman was actually Polish, with a Germanic-looking name but with a Swedish-like spelling.

(2) The second feature is that the article has an abstract-like paragraph at the very beginning, also crossed out with the same red pen! However, it is easy to see that it reads:

- This note is intended for private circulation only and is not to be published in its present form. Copies will be sent at request as long as the supply lasts. The author very much appreciates comments on the content of the paper.

Källén was fascinated by the Bergman-Weil integral! Among other things, his work led him into correspondence with mathematicians. On 21 October 1964, he wrote to the mathematician Friedrich Sommer[2] in Würzburg, (translated from German):

> Dear Professor Sommer:
>
> Today, I am sending you, in a separate envelope, the manuscript of a little paper of mine with the title "A connection between the Bergman-Weil Integral and the Cauchy Integral". As I have stated in the paper, I have tried to find a derivation of the Bergman-Weil integral that is easier for physicists to understand than, for example, the one you have given[3]. It is rather a pedagogical piece of work than a scientific paper. I am perfectly aware that the presented arguments are not mathematically stringent. In particular, details of going to certain limits have been treated quite intuitively. If you have time, I would be very grateful to you, if you could take the trouble of reading critically through the paper and send me comments that you may have. I would especially like to know, whether one finds similar arguments somewhere in the mathematical literature, that I don't know so well. I would not exclude it, because it seems to me that my arguments are so commonplace that they are perhaps already known to the experts.
>
> Hoping that this matter will not burden you too much, I remain
>
> respectfully your
>
> Gunnar Källén

Unfortunately, however, no answer from Sommer is found in the Källén Collection. Upon contacting one of his students, Professor Herbert Abels, the reason became clear. Abels informed me (CJ) that [1]:

> "At that time [1964] Sommer was still in Würzburg, me too, as his student. But Sommer was very busy with planning to establish the Mathematics De-

[2] Friedrich Sommer (1912–1998) was according to Källén the only mathematician who ever helped him.
[3] Here Källén is referring to the article F. Sommer, Math. Ann. 125, 172 (1952).

partment of the newly founded University of Bochum, where he (and me, following him) moved to in late 1965–beginning of 1966. Sommer did a lot for the newly founded University of Bochum and in particular for the Math Department. …"

Indeed, lack of time seems to be a universal disease that afflicts not only theoretical physicists but also mathematicians. Another mathematician whom Källén contacted was the Bergman in the title of his article – Stefan Bergman.[4] In a letter, dated Dec. 7, 1965, to Sidney Drell, who had invited him to Stanford, Källén mentioned that he had the pleasure of listening to a talk by Bergman at Maryland in the spring of 1964 and added that Bergman probably doesn't know it. He informed Drell that he would like to visit Stanford and expressed his wish to also meet with Stefan Bergman at the Mathematics Department of Stanford University. However, the latter visit couldn't take place because Bergman was away in Europe when Källén was at Stanford.

Bergman was interested in Källén's work, as he expressed it, for example, in a letter dated 29 March 1966:

"Dear Professor Källén:

During the last two years you were kind enough to send me manuscripts of your papers in which you considered the integral representation of functions of several complex variables in terms of their value on the distinguished boundary. There is a considerable interest here in Stanford in this direction, and I am wondering whether you could be kind enough to send for me and my coworkers additional copies of the manuscripts or reprints of them. …"

A month later, on 27 April 1966, Bergman wrote again to Källén, thanking him for the articles he has received, and continued as follows:

"In my opinion your work and research is of great interest and I can perceive the value although lack of training in physics prevents me from following into details. In this context, I think, if you could find the time to include in your future work a survey of the possible applications of the theory of the distinguished boundary surface and of the integral formula in modern physics, it would be greatly appreciated by mathematicians.

Though we could not make it in the fall of 1966. I do hope we will have the chance to meet personally in the not too distant future!

[4] Stefan Bergman (1895–1977), unlike most mathematicians of his time, published a large number of papers. The interested reader can easily find information about his tortuous life by consulting the internet. See, for example, http://www-history.mcs.st-and.ac.uk/Biographies/Bergman.html

Thanking you again for the trouble you took on my behalf and with kindest regards,

Sincerely yours

Stefan Bergman"

Källén and Bergman continued their exchange of articles and letters. As expected, Källén sent a long list of references to Bergman. In his last letter to Källén, dated 30 June 1967, Bergman writes:

"Dear Professor Källén:

I am taking the liberty of sending you under separate cover a copy of the final draft of the last two chapters of the new edition of my book: The Kernel Function and Conformal Mapping. I mentioned there only the possibility of application of the theory of domains with a distinguished boundary in physical applications. As I know from your work, you are interested in the theory of functions of two and several complex variables, in particular, in the domains with a distinguished boundary surface. If you have some comments, I would be very much obliged to hearing from you. …"

The second edition of Bergman's book [2] was published in 1970, quite a while after Källén had passed away. Indeed there is a chapter in it, in which Bergman quotes the references that were delivered to him by Källén. He simply lists them without making any comments.

This lack of *real* communication between theoretical physicists and mathematicians is a sad *complex* problem which seems to have no simple general solution.

References

1. H. Abels, private communication to CJ. We wish to thank Professor Abels for the information that he has provided.
2. Stefan Bergman, "The Kernel Function and Conformal Mapping", second revised edition, ISBN 0-8218-1405-2, Providence, R.I. (1970). See section 2 in Chapter XI of this book.

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G. Källén: A Connection Between the Bergman-Weil Integral and the Cauchy Integral

Summary. We show how the Bergman-Weil integral for an analytic function of several complex variables can be derived from the ordinary Cauchy integral for the particular case when the regularity domain of the analytic function is a very special analytic polyhedron. Our argument applies to the case when the number of faces in the polyhedron is the same as the number of complex variables in the function. In the more general case when the number of faces is larger than the number of complex variables, a limiting procedure is indicated which allows one to arrive at the Bergman-Weil integral also for this case. However, this part of the discussion is much less rigorous than the first part.

A. Introduction

In recent years it has appeared that the generalisation of the Cauchy integral given essentially simultaneously by S. Bergman and A. Weil [1] is a useful tool in theoretical physics. In particular, it has turned out that the systematic analysis of vacuum expectation values of field operators which has attracted some attention recently leads to analytic functions of several complex variables regular in certain domains [2].[1]

For practical applications, one is interested in having a parameterization of the most general function analytic and regular in a given domain. As is well-known, the ordinary Cauchy integral is an excellent tool for such purposes for functions of only one complex variable. However, no generally applicable generalization of the Cauchy integral useful for functions of several complex variables exists. For the particular case that the regularity domain under investigation is bounded by pieces of "analytic hypersurfaces", the Bergman-Weil integral is applicable. Fortunately, some of the domains encountered in physics

[1] Gunnar Källén – Department of Theoretical Physics, University of Lund, Lund, Sweden

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are of this kind. As an example, we mention the representation of the general vertex function as well as other slightly less systematic applications in connection with Feynman diagrams [2], [3].

Very few derivations of the basic Bergman-Weil integral formula which are intended for physicists are available in the literature. Essentially, only one paper of this kind exists [4]. The discussion in this reference is based on a paper by Sommer [5] and is rigorous. However, it is also somewhat complicated in spite of the fact that the derivation is given explicitly only for the case of three complex variables. The intention of this note is to present an alternative "derivation" of the Bergman-Weil integral intended for physicists who are mainly interested in applications of the theory. We are very little concerned about mathematical rigour, and the argument given below is admittedly defect[ive] in this respect.

We first prove a particular case where the number of analytic hypersurfaces making up the boundary is the same as the number of complex variables in our function. This part of the argument can be made reasonably rigorous, even if it is here presented in the language of a theoretical physicist. However, this special case is not enough for applications to physics. Rather, one is interested in a more general situation, where the number of analytic hypersurfaces which make up the boundary is larger than the number of complex variables. In the argument below, we reduce this general case to the previous one by introducing artificial complex variables and by considering our problem in such a large space that the number of analytic hypersurfaces entering into the boundary is equal to the number of complex variables available. With the aid of the techniques first developed, we can then write down the Bergman-Weil integral for our function. Using a limiting procedure and taking advantage of the fact that our function is independent of the dummy complex variables, we then obtain the Bergman-Weil integral in the general case. It is this limiting procedure which is less rigorous than the exact mathematical arguments referred to in footnote [reference] [5]. We hope, however, that by sacrificing the mathematical rigour we obtain a certain amount of insight into the Bergman-Weil integral. In particular, we want to show that the connection between the standard Cauchy integral and the Bergman-Weil integral is more intimate than what is indicated in the literature. As a byproduct, we also obtain the result that there is an even larger amount of freedom in the choice of the integral kernel which generalizes the Cauchy denominators than what has so far been stated in the literature. This extra freedom appears to be useful in practical applications.

B. A Special Case of the Bergman-Weil Integral

We first consider an extremely simple case, viz. an analytic function $F(z_1,\ldots, z_n)$ or, in short hand notation, $F(z)$ which depends on n complex variables $z_j, j=1,\ldots,n$. The function $F(z)$ is supposed to be regular analytic when the variables z_j vary independently, e.g., in their upper half planes where $Imz_j > 0$. We are going to refer to this domain as the "product of the upper half planes". Assuming further that the function $F(z)$ vanishes so rapidly when one or more of the variables z_j goes to infinity in the upper half plane that the integral over a large circle can be neglected, we can use an iterated Cauchy integral and represent the function $F(z)$ in the following standard way

$$F(z) = \frac{1}{(2\pi i)^n} \int_{-\infty}^{+\infty} \cdots \int_{-\infty}^{+\infty} \frac{d\varsigma_1 \ldots d\varsigma_n F(\varsigma)}{\prod_{j=1}^{n} (\varsigma_j - z_j)}. \tag{1}$$

A formally slightly more general case can, by simple transformation of variables, be reduced to Eq. (1) above. If we assume that the function $F(z)$ is regular analytic, not in the product of the upper half planes but in an analytic polyhedron bounded by "analytic hypersurfaces" and if the number of faces of the polyhedron is exactly the same as the number n of complex variables, we can map the interior of this polyhedron on the product of the upper half planes by using the parameters of the analytic hypersurfaces as new complex variables. To do this, we remember that an analytic hypersurface is defined as a manifold of $2\,n-1$ real dimensions in the $2\,n$ dimensional space of the n complex variables z_j. It is expressed with the aid of an equation of the following form

$$f(z_1,\ldots,z_n) = f(z) = r, \tag{2}$$

where r is a real parameter and $f(z)$ is an analytic function of the n complex variables z_j. Further, it is supposed that the analytic hypersurface in Eq. (2) is such that it divides the entire $2\,n$-dimensional space in two parts, characterized by the sign of the imaginary part of r. An analytic polyhedron is now defined in the following way. Consider a number m of analytic hypersurfaces of the form (2) or

$$f_k(z) = r_k, \quad k = 1,\ldots,m. \tag{2a}$$

The analytic polyhedron $P_n^m(z)$ is defined as the intersection of all the domains where the imaginary parts of the variables r_k are all positive

$$P_n^m(z) = \{z_j, j = 1,\ldots,n; \quad Im f_k(z) = Im\, r_k > 0, k = 1,\ldots,m\}. \tag{3}$$

In this section we only consider the case $m = n$. Under these circumstances, we assume that we can solve Eqs. (2a) for the variables z_k and that this solution is unique inside the domain $P_n^n(z)$. We can then consider Eqs. (2a) as a one-to-one mapping of the domain $P_n^n(z)$ on the product of the upper half planes in the space of the variables r_k. Under this mapping $F(z)$ becomes a function of the variables r_k regular in the product of the upper half planes. Consequently, Eq. (1) above is applicable and we have

$$F_1(r) = \frac{1}{(2\pi i)^n} \int_{-\infty}^{+\infty} \cdots \int_{-\infty}^{+\infty} \frac{ds_1 \ldots ds_n F_1(s)}{\prod_{j=1}^{n} (s_j - r_j)}, \tag{4}$$

$$F_1(r) = F(z(r)) \tag{4a}$$

$$f_k(z) = r_k \tag{4b}$$

We now use Eq. (2a) to introduce new variables of integration ς_k instead of the quantities s_k

$$f_k(\varsigma) = s_k. \tag{5}$$

In this way, Eq. (4) becomes

$$F_1(r) = \frac{1}{(2\pi i)^n} \int_{\partial P_n^n} \cdots \int \frac{d\varsigma_1 \ldots d\varsigma_n F_1(s(\varsigma))}{\prod_{j=1}^{n} (f_j(\varsigma) - r_j)} J(\varsigma). \tag{6}$$

The domain of integration in Eq. (6) is the domain in the space of the variables ς_k which is mapped on the product of all the real axes of the variables s_k. Because of the definition of the analytic polyhedron, this corresponds to the common intersection of all the faces of the polyhedron $P_n^n(z)$. The ordinary terminology is that this n-dimensional part of the boundary of the polyhedron is called the "distinguished boundary". In Eq. (6) we have denoted this distinguished boundary by the symbol ∂P_n^n. The notation $J(\varsigma)$ stands for the Jacobian of the transformation (5).

$$J(\varsigma) = Det[\frac{\partial f_i(\varsigma)}{\partial \varsigma_j}]. \tag{7}$$

Returning to the original variables z_k and changing the notation in Eq. (6) slightly, we obtain the integral representation

$$F(z) = \frac{1}{(2\pi i)^n} \underset{\partial P_n^n}{\int \ldots \int} d\varsigma_1 \ldots d\varsigma_n F(\varsigma) D(z,\varsigma), \tag{8}$$

$$D(z,\varsigma) = Det[Q_j^i(z,\varsigma)], \tag{8a}$$

$$Q_j^i(z,\varsigma) = \frac{\partial f_i(\varsigma)}{\partial \varsigma_j} \frac{1}{f_i(\varsigma) - f_i(z)}. \tag{8b}$$

The conventional Bergman-Weil integral for the special case which we are considering here is written

$$F(z) = \frac{1}{(2\pi i)^n} \underset{\partial P_n^n}{\int \ldots \int} d\varsigma_1 \ldots d\varsigma_n F(\varsigma) \mathfrak{D}(z,\varsigma), \tag{9}$$

$$\mathfrak{D}(z,\varsigma) = Det[q_j^i(z,\varsigma)]. \tag{9a}$$

In the general case when $m > n$ we have formally the same expression (9) except that the distinguished boundary ∂P_n^m is now made up of several distinct parts corresponding to the common intersection of n out of m faces of the polyhedron. The integration over the distinguished boundary in Eq. (9) is then to be understood as a sum of all the integrals over the different parts of the distinguished boundary.

The similarity between the two equations (8) and (9) is obvious. In both cases we integrate over the same n-dimensional manifold, the distinguished boundary of the polyhedron, and the kernel contains the boundary value of the analytic function $F(z)$ multiplied by a certain determinant. However, the functions ${q^i}_j(z,\varsigma)$ are normally not identical with the functions ${Q^i}_j(z,\varsigma)$. Rather, the quantities ${q^i}_j(z,\varsigma)$ are constructed in the following way. We first introduce functions ${p^i}_j(z,\varsigma)$ by

$$f_i(\varsigma) - f_i(z) = \sum_j p_j^i(z,\varsigma)(\varsigma_j - z_j), \tag{10}$$

where ${p^i}_j(z,\varsigma)$ are functions which are regular analytic when z lies inside the polyhedron $P_n^n(z)$ and when ς belongs to the face defined by Eq. (5) with $k = i$ [6]. The quantities q_j^i are constructed from the p_j^i by the expression

$$q_j^i(z,\varsigma) = \frac{p_j^i(z,\varsigma)}{f_i(\varsigma) - f_i(z)}. \tag{11}$$

Clearly, these quantities q_j^i fulfill the normalization condition

$$\sum_{j=1}^{n} q_j^i(z,\varsigma)(\varsigma_j - z_j) = 1, \quad \text{for} \quad i = 1,\ldots,n. \tag{12}$$

Except for very special cases, the functions $p_j^i(z,\varsigma)$ in Eq. (10) are not unique. Therefore, there is a certain, well-known arbitrariness in the functions $q_j^i(z,\varsigma)$. However, as the functions $Q_j^i(z,\varsigma)$ in Eq. (8) are, in general, not expected to fulfill the normalization condition (12), none of the functions $q_j^i(z,\varsigma)$ obtained from any set $p_j^i(z,\varsigma)$ are identical with $Q_j^i(z,\varsigma)$. Nevertheless, the integral representations (8) and (9) are formally identical except for the replacement of $Q_j^i(z,\varsigma)$ by $q_j^i(z,\varsigma)$. This shows that the arbitrariness in the functions making up the determinant $\mathfrak{D}(z,\varsigma)$ in the Bergman-Weil representation (9) is even larger than the arbitrariness implied by the non-uniqueness of the functions $p_j^i(z,\varsigma)$.

It is comparatively easy to verify by an explicit calculation that the two integral representations (8) and (9) do, indeed, give the same result. For this purpose, we introduce functions $X_j^i(z,\varsigma)$ defined by

$$X_j^i(z,\varsigma) = Q_j^i(z,\varsigma) - q_j^i(z,\varsigma) = [\frac{\partial f_i(\varsigma)}{\partial \varsigma_j} - p_j^i(z,\varsigma)]\frac{1}{f_i(\varsigma) - f_i(z)}. \tag{13}$$

From the definition (10) follows immediately

$$p_j^i(\varsigma,\varsigma) = \frac{\partial f_i(\varsigma)}{\partial \varsigma_j}. \tag{14}$$

and we conclude that the functions $X_j^i(z,\varsigma)$ are regular analytic not only when z lies inside the analytic polyhedron and ς is a point on the boundary surface $f_i(\varsigma) = s_i$ but also when the point z is on the boundary surface $f_i(z) = r$ including the point $z = \varsigma$.

Introducing the definitions (13) in the determinant (8a) we find

$$D(z,\varsigma) = \mathfrak{D}(z,\varsigma) + \sum_{i,j} X_j^i(z,\varsigma) D_j^i(z,\varsigma) + \ldots, \tag{15}$$

where $D_j^i(z,\varsigma)$ is the cofactor of the element $Q_j^i(z,\varsigma)$ in the determinant (8a). The terms not written out explicitly in Eq. (15) contain at least two factors

$X^i_j(z,\varsigma)$. The difference between the two integrals (8) and (9) can now be written as a sum of terms, one of which is given by

$$I^i_j(z) = \frac{1}{(2\pi i)^n} \int\limits_{\partial P^n_n} \dots \int d\varsigma_1 \dots d\varsigma_n F(\varsigma) X^i_j(z,\varsigma) D^i_j(z,\varsigma). \tag{16}$$

Returning to the variables r_k and s_k we find

$$I^i_j(z(r)) = \frac{1}{(2\pi i)^n} \int\limits_{-\infty}^{+\infty} \dots \int\limits_{-\infty}^{+\infty} \frac{ds_1 \dots ds_n F_1(s)}{\prod\limits_{k \neq i} (s_k - r_k)} Y^i_j(r,s), \tag{17}$$

$$Y^i_j(r,s) = X^i_j(z(r),\varsigma(s)) \frac{J^i_j(\varsigma(s))}{J(\varsigma(s))}, \tag{17a}$$

where $J^i_j(\varsigma(s))$ is the cofactor of the Jacobian $J(\varsigma(s))$ belonging to the element $\frac{\partial f_i(\varsigma)}{\partial \varsigma_j}$. Because of the assumptions we have made about the boundary surfaces (2a), the Jacobian $J(\varsigma)$ is an analytic function of ς which has neither zeros nor infinities as long as ς lies either inside the polyhedron or on its boundary. The same is also true for the cofactors $J^i_j(\varsigma)$. Consequently, the functions $Y^i_j(r,s)$ in Eq. (17a) are regular analytic as functions of r and s when these numbers vary independently in their upper half planes and on the real axes. Therefore, the only singularities in the upper half planes in Eq. (17) come from the Cauchy denominators exhibited explicitly. We note that the particular denominator with the factor $s_i - r_i$ is missing. This comes about because the functions $X^i_j(z,\varsigma)$ in Eq. (13) are regular also for $s = \varsigma$. Consequently, when we close the path of integration in the variable s_i in the upper half plane in Eq. (17), we have no singularities of the integrand inside the path of integration and the whole integral is equal to zero. One may note that the important point in this argument is the absence of one of the Cauchy denominators in Eq. (17). If we consider the higher terms in the expansion (15) involving more than one factor $X^i_j(z,\varsigma)$, more than one of the Cauchy denominators are going to disappear. Consequently, all these higher terms also give zero and we find, indeed, that the two integrals (8) and (9) give the same result. Therefore, we can consider our argument as a derivation of the conventional Bergman-Weil integral (9) from our formula (8). However, it should be pointed out that for practical applications there is no real advantage in (9) as compared to (8).

Even if the explicit construction of the functions $p_j^i(z,\varsigma)$ in Eq. (10) usually is not too cumbersome, it can still imply rather involved algebraic manipulations. The partial derivatives appearing in Eq. (8b) are, however, calculable by straight forward techniques and quite as useful in practice. Therefore, the integral representations (8) may actually be more advantageous in applications.

C. The General Bergman-Weil Integral

The argument presented in section 2 is essentially restricted by the specialization $m=n$ in Eq. (3). However, the more general case $m>n$ is of physical interest and we here want to indicate, in a non rigorous way, how the results derived in section 2 can be generalized to cover this case. As already mentioned in the introduction, we handle this situation by introducing dummy variables $z_k, k=n+1,\ldots,m$ in such a way that our space has as many dimensions as there are faces in the analytic polyhedron $P_n^m(z)$. Evidently, this can be done in a very large number of ways. To illustrate the procedure we want [to] adopt here, we first consider the case of $n=2$ and $m=3$. Under these circumstances, the problem is to introduce a third variable z_3 in a convenient way. We do it by introducing three functions $\varphi_i(z_1,z_2,z_3,\epsilon)$ and write

$$r_i=\varphi_i(z_1,z_2,z_3,\epsilon);\, i=1,2,3. \tag{18}$$

The functions $\varphi_i(z_1,z_2,z_3,\epsilon)$ are supposed to have the following properties. When the number ϵ is different from zero, Eqs. (18) define an analytic polyhedron $P_3^3(z,\epsilon)$. In the limit when ϵ goes to zero, each of the functions $\varphi_i(z_1,z_2,z_3,\epsilon)$ degenerates into one of the faces of the original polyhedron $P_2^3(z)$,

$$\varphi_i(z_1,z_2,z_3,0)=f_i(z_1,z_2). \tag{19}$$

Note that the right hand side of (19) is independent of z_3. We further require that all points (z_1,z_2) such that (z_1,z_2,z_3) lies inside the polyhedron $P_3^3(z,\epsilon)$ lie inside the polyhedron $P_2^3(z)$.

The existence of functions $\varphi_i(z_1,z_2,z_3,\epsilon)$ in Eq. (18) is most easily demonstrated with the aid of an explicit example. We write

$$\varphi_j(z_1,z_2,z_3,\epsilon)=f_j(z_1,z_2)+\frac{\epsilon}{z_3-f_{j+1}(z_1,z_2)-f_{j-1}(z_1,z_2)}, \tag{19a}$$

where we have used the convention $f_{j+3}(z_1,z_2)\equiv f_j(z_1,z_2)$. In the limit when ϵ goes to zero, the solutions of the three Eqs. (18) separate into three distinct parts determined by the following relations

$$f_j(z_1, z_2) = r_j; j = i \pm 1, \tag{19b}$$

$$z_3 = f_{j+1}(z_1, z_2) + f_{j-1}(z_1, z_2) = r_{i+1} + r_{i-1}. \tag{19c}$$

When the numbers r_j vary in the upper half planes the complex number z_3 computed from Eq. (19c) also lies in its upper half plane. We now consider the case when ϵ is small but different from zero. If we tried to write down the Bergman-Weil integral or the alternative formula given in Eq. (8) for a function $F(z_1, z_2)$ in the space of the three complex variables z_1, z_2 and z_3 our function would be independent of z_3 and, therefore, not sufficiently damped at infinity. For this reason we consider instead a modified function $\tilde{F}$ defined by

$$\tilde{F}(z) = F(z_1, z_2, z_3) = F(z_1, z_2)G(z_3), \tag{20}$$

where $G(z_3)$ is an analytic function of z_3 regular in the upper half plane and vanishing sufficiently fast at infinity to make the following argument consistent. The Bergman-Weil like formula (8) now becomes

$$\tilde{F}(z) = F(z)G(z_3) = \frac{1}{(2\pi i)^3} \int_{\partial P_3^3(z)} \int d\varsigma_1 d\varsigma_2 d\varsigma_3 \tilde{F}(\varsigma) D(z, \varsigma), \tag{21}$$

$$D(z, \varsigma) = Det[Q_j^i] = \sum_{j=1}^{3} \frac{\partial \varphi_j(\varsigma, \epsilon)}{\partial \varsigma_3} \frac{Q_1^{j+1} Q_2^{j-1} - Q_2^{j+1} Q_1^{j-1}}{\varphi_j(\varsigma, \epsilon) - \varphi_j(z, \epsilon)}. \tag{21a}$$

In analogy with Eq. (19a) we have here used the convention $Q_i^{j+3} = Q_i^j$. We next take advantage of the factorization of the function $\tilde{F}(z)$ in Eq. (20) and rearrange Eq. (21) in the following way

$$F(z)G(z_3) = \frac{1}{(2\pi i)^2} \int \int d\varsigma_1 d\varsigma_2 F(\varsigma) \times$$

$$\times \sum_{j=1}^{3} \frac{1}{2\pi i} \int d\varsigma_3 G(\varsigma_3) \frac{\partial \varphi_j(\varsigma, \epsilon)}{\partial \varsigma_3} \frac{Q_1^{j+1} Q_2^{j-1} - Q_2^{j+1} Q_1^{j-1}}{\varphi_j(\varsigma, \epsilon) - \varphi_j(z, \epsilon)}. \tag{22}$$

In order not to be completely lost in generalities, we next introduce the explicit expression for the function $\varphi_i(z_1, z_2, z_3, \epsilon)$ given in Eq. (19a). In that case we can compute the derivatives appearing in the functions Q_i^j explicitly. We exhibit only the derivatives with respect to ς_3 in detail and rewrite Eq. (22) as follows

$$F(z)G(z_3) = \frac{1}{(2\pi i)^2} \int\int d\varsigma_1 d\varsigma_2 F(\varsigma)\times$$
$$\times \sum_{j=1}^{3} \frac{-\epsilon}{2\pi i} \int \frac{d\varsigma_3 G(\varsigma_3)}{[\varsigma_3 - f_{j+1}(\varsigma) - f_{j-1}(\varsigma)]^2} \frac{Q_1^{j+1} Q_2^{j-1} - Q_2^{j+1} Q_1^{j-1}}{\varphi_j(\varsigma,\epsilon) - \varphi_j(z,\epsilon)}. \quad (23)$$

We note the appearance of the explicit factor ϵ in front of each of the terms in the sum in Eq. (23). The domain of integration in Eq. (23) is the distinguished boundary of $P_3^3(z)$. In the limit when ϵ goes to zero, this distinguished boundary degenerates, according to what has been said above, in three pieces. On one of these pieces the functions $f_{j+1}(\varsigma)$ and $f_{j-1}(\varsigma)$ are equal to real numbers s_{j+1} and s_{j-1}, respectively. At the same time ς_3 is equal to the sum $s_{j+1} + s_{j-1}$ except for terms of order of magnitude ϵ. From this remark we conclude that the explicit factor ϵ in front of each term in the representation (23) makes the corresponding contribution vanishingly small for all terms except one, viz.

$$\begin{aligned} I = \frac{-\epsilon}{2\pi i} \int_{-\infty}^{+\infty} \frac{d\varsigma_3 G(\varsigma_3)}{(\varsigma_3 - s_{j+1} - s_{j-1})^2} \frac{Q_1^{j+1} Q_2^{j-1} - Q_2^{j+1} Q_1^{j-1}}{\varphi_j(\varsigma,\epsilon) - \varphi_j(z,\epsilon)} \approx \\ \approx \frac{-\epsilon}{2\pi i} [Q_1^{j+1} Q_2^{j-1} - Q_2^{j+1} Q_1^{j-1}] \times \\ \times \int_{-\infty}^{+\infty} \frac{d\varsigma_3 G(\varsigma_3)}{(\varsigma_3 - s_{j+1} - s_{j-1})^2} \frac{1}{\varphi_j(\varsigma,\epsilon) - \varphi_j(z,\epsilon)}. \end{aligned} \quad (24)$$

As the denominator $\varsigma_3 - s_{j+1} - s_{j-1}$ can be of the order of magnitude ϵ during the integration in Eq. (24) we cannot conclude that the integral I vanishes when ϵ becomes very small. To estimate the integral I we introduce a variable s_j as a new independent variable of integration by the definition

$$\begin{aligned} s_j = \varphi_j(\varsigma,\epsilon) = f_j(\varsigma) + \epsilon[\varsigma_3 - f_{j+1}(\varsigma) - f_{j-1}(\varsigma)]^{-1} \\ = f_j(\varsigma) + \frac{\epsilon}{\varsigma_3 - s_{j+1} - s_{j-1}}, \end{aligned} \quad (25)$$

$$\frac{ds_j}{d\varsigma_3} = \frac{-\epsilon}{(\varsigma_3 - s_{j+1} - s_{j-1})^2}. \quad (25a)$$

In this way we find

$$
\begin{aligned}
I &= [Q_1^{j+1} Q_2^{j-1} - Q_2^{j+1} Q_1^{j-1}] \frac{1}{2\pi i} \int_{-\infty}^{+\infty} \frac{ds_j G(\varsigma_3(s_j))}{s_j - \varphi_j(z, \epsilon)} = \\
&= [Q_1^{j+1} Q_2^{j-1} - Q_2^{j+1} Q_1^{j-1}] G(\varsigma_3(\varphi_j(z, \epsilon))) = \\
&= G(z_3)[Q_1^{j+1} Q_2^{j-1} - Q_2^{j+1} Q_1^{j-1}].
\end{aligned}
\tag{26}
$$

Consequently, the factor $G(z_3)$ can be divided out in Eq. (22) and we find

$$
F(z) = \frac{1}{(2\pi i)^2} \int\int d\varsigma_1 d\varsigma_2 F(\varsigma) \sum_{j=1}^{3} D_j(z, \varsigma), \tag{27}
$$

$$
\begin{aligned}
D_j(z, \varsigma) = &\frac{1}{f_{j+1}(\varsigma) - f_{j+1}(z)} \frac{1}{f_{j-1}(\varsigma) - f_{j-1}(z)} \times \\
&\times [\frac{\partial f_{j+1}(\varsigma)}{\partial \varsigma_1} \frac{\partial f_{j-1}(\varsigma)}{\partial \varsigma_2} - \frac{\partial f_{j+1}(\varsigma)}{\partial \varsigma_2} \frac{\partial f_{j-1}(\varsigma)}{\partial \varsigma_1}].
\end{aligned}
\tag{27a}
$$

One question which has not been discussed explicitly in this argument is the range of variation of the variables ς_1, ς_2, and s_j after the transformation (25). On the distinguished boundary of the polyhedron $P_3^3(z, \epsilon)$ all the variables s_k vary from minus infinity to plus infinity. After the transformation (25) we assume that the variables ς_1 and ς_2 vary over a certain manifold which, in the limit when ϵ goes to zero, becomes that part of the distinguished boundary $P_2^3(z)$ which is given by

$$
f_i(\varsigma_1, \varsigma_2) = s_i; j \neq i; \infty < s_i < +\infty. \tag{27b}
$$

At the same time the range of variation of s_j is supposed to be the whole real axis. It is mainly at this point where our argument is lacking in mathematical rigour as an exact proof that this happens in the limit when ϵ goes to zero is missing. Evidently, both this assumption and also the evaluation of the integral I in the limit when ϵ goes to zero as given in Eq. (25) assume a certain smoothness in all the functions appearing. As we are not here concerned with mathematical rigour, we do not want to discuss this point further. However, once the argument indicated above is accepted, Eq. (27) gives a representation of the analytic function $F(z)$ which is analogous to the Bergman-Weil integral in the same way as Eq. (8) above is analogous to the Bergman-Weil integral (9).

For the case considered here, the distinguished boundary of the polyhedron $P_2^3(z)$ consists of three parts. Each part is made up of the intersection of two analytic hypersurfaces $f_j(\varsigma) = s_j$ where the quantities s_j as usual are real numbers. Evidently, each term in Eq. (27) corresponds to an integration over one of these parts of the distinguished boundary. Consequently, we can write the whole representation (27) as follows

$$F(z) = \frac{1}{(2\pi i)^2} \int\limits_{\partial P_2^3}\int d\varsigma_1 d\varsigma_2 F(\varsigma) D(z,\varsigma), \tag{28}$$

$$D(z,\varsigma) = Det[Q_j^i], \tag{28a}$$

$$Q_j^i = \frac{\partial f_i(\varsigma)}{\partial \varsigma_j} \frac{1}{f_i(\varsigma) - f_i(z)}. \tag{28b}$$

This expression differs from the corresponding Bergman-Weil representation only in the fact that the functions Q_j^i do not fulfill the normalization condition given in Eq. (12). Clearly, this defect – if it is a defect – can be cured by a construction analogous to the discussion at the end of section 2.

The argument given above can be generalized to the case when $m - n$ is arbitrarily large. For this purpose we have to generalize Eq. (18) as follows

$$r_i = \varphi_i(z_1, \ldots, z_n, z_{n+1}, \ldots, z_m, \epsilon), \tag{29}$$

with

$$\varphi_i(z_1, \ldots, z_n, z_{n+1}, \ldots, z_m, 0) = f_i(z_1, \ldots, z_n). \tag{29a}$$

When expanding the big determinant corresponding to the expression in Eq. (21a) we write

$$D(z,\varsigma) = \frac{1}{\prod\limits_k [\varphi_k(\varsigma,\epsilon) - \varphi_k(z,\epsilon)]} \times$$

$$\times \sum_{\{i_j\}} (-1)^P \begin{vmatrix} \frac{\partial \varphi_{i_1}}{\partial \varsigma_1} \cdots \frac{\partial \varphi_{i_n}}{\partial \varsigma_1} \\ \frac{\partial \varphi_{i_1}}{\partial \varsigma_n} \cdots \frac{\partial \varphi_{i_n}}{\partial \varsigma_n} \end{vmatrix} \begin{vmatrix} \frac{\partial \varphi_{i_{n+1}}}{\partial \varsigma_{n+1}} \cdots \frac{\partial \varphi_{i_m}}{\partial \varsigma_{n+1}} \\ \\ \frac{\partial \varphi_{i_{n+1}}}{\partial \varsigma_m} \cdots \frac{\partial \varphi_{i_m}}{\partial \varsigma_m} \end{vmatrix}, \tag{30}$$

where $i_1, \ldots, i_m$ is some permutation of the numbers $1, \ldots, m$ and $(-1)^P$ is the sign factor. The summation in Eq. (30) goes over all possible permutations

$\{i_j\}$. The first factor after the summation sign in Eq. (30) corresponds exactly to the determinant we want in the final kernel in the Bergman-Weil representation for the domain $P_n^m(z)$. The second factor in the expansion corresponds to a Jacobian which allows us to transform the variables $\varsigma_{n+1}, \ldots, \varsigma_m$ to the variables $s_{i_{n+1}}, \ldots, s_{i_m}$. In the limit when ϵ goes to zero we can then perform the integration over the variables s_{i_ν}, $n+1 \leq \nu \leq m$, in essentially the same way as we performed the integration over the variable s_j in Eq. (26). As a result of these manipulations we obtain a representation of the function $F(z)$ of n complex variables z_k which is an n-dimensional integral over the distinguished boundary of the polyhedron $P_n^m(z)$. The representation contains several terms corresponding to the various parts of the distinguished boundary. As before, the integral representation we obtain in this way is not exactly the Bergman-Weil integral because of the lack of the normalization condition (12). However, it can be transformed to a Bergman-Weil integral using the same technique as before. For practical applications, such a transformation may, or may not, be advantageous.

Summarizing and using the same notation as in Eqs. (8) and (9) we have

$$\begin{aligned} F(z) &= \frac{1}{(2\pi i)^n} \int\limits_{\partial P_n^m} \cdots \int d\varsigma_1 \ldots d\varsigma_n F(\varsigma) D(z, \varsigma) = \\ &= \frac{1}{(2\pi i)^n} \int\limits_{\partial P_n^m} \cdots \int d\varsigma_1 \ldots d\varsigma_n F(\varsigma) \mathfrak{D}(z, \varsigma). \end{aligned} \tag{31}$$

As was remarked after Eq. (9), the integration over the distinguished boundary in Eq. (31) consists of a sum of the integrals over the different parts of the distinguished boundary.

D. Discussion

To avoid misunderstandings, we want to repeat here once more what has been said several times above, viz. that the discussion presented here is not supposed to be a rigorous mathematical argument intended to replace those derivations of the Bergman-Weil integral which are already available in the mathematical literature. Rather, our intention has been to discuss the Bergman-Weil integral on a level where mathematical rigour is sacrificed in favour of more intuitive arguments. In particular, we have tried to show that there is a very intimate connection between the elementary iterated Cauchy integral for a function regular in the product of the upper half planes and the Bergman-Weil integral for a function regular in an analytic polyhedron. The determinant which appears in the Bergman-Weil kernel is intimately related to the Jacobian of a transformation from the original variables z_k to variables r_k which are essentially the parameters of the analytic hypersurfaces which constitute the boundary of the analytic polyhedron. We hope that this remark may remove some of the feeling of mystery which many physicists have with respect to the Bergman-Weil integral. A technical complication arises because we normally have more analytic hypersurfaces in the boundary of our polyhedron than we have complex variables. A non rigorous way of handling this situation is discussed above.

The considerations given above were worked out by the author in connection with a course in advanced field theory given at the University of Maryland in the spring of 1964. Helpful discussions with many members of the Physics Department of Maryland University, in particular Drs. O.W. Greenberg, J.N. Islam, C. Kacser and J. Toll are gratefully acknowledged.

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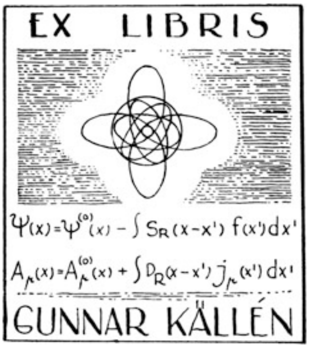
Note added by me (CJ) This article, found in the Källén Collection, has been retyped by me for reproduction in this book.

[2] Here Källén is referring to the 1960 Summer School in Les Houches, France. The topic of the School was dispersion relations and elementary particles. The article referred to is paper [1960b], on Källén's list of publication, in Part 5 of this book.

Part 5

Publications, Selected Papers and Commentaries



The Contents

This Part contains:

- **A list of Källén's publications**
- **Reprints of a selection of his papers, with commentaries**
- **Additional commentaries on some of his other papers**

We take for granted that those of our readers who are interested in this Part have easy internet access to a wealth of scientific publications. Therefore, we have selected just a few papers to give our readers a flavor of Källén's scientific style and way of thinking. Actually, these happen to be among Källén's most important publications and are not easily available, at least not yet!

After presenting the selected papers some "Additional Commentaries" have been added for the purpose of informing our readers about the contents of some of the papers that have not been included in this book.

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List of Publications

Källén's scientific work includes the articles listed below as well as his books listed further down. In addition he reviewed a large number of interesting articles for the journal "Zentralblatt für Mathematik", as is described in Appendix I-A in Part 1 of this book.

Articles

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Paper [1949]: Higher Approximations in the external field for the Problem of Vacuum Polarization

Helv. Phys. Acta 22 (1949) 637

In view of current status of theoretical particle physics, this paper is especially interesting as it "smells" **SUPERSYMMETRY**. Källén shows the cancellation of divergences in the fourth-order in the coupling constant when the number of bosonic and fermionic degrees of freedom are equal.

The origin of this paper was discussed in chapter "Källén's Debut Article – Early Trace of Supersymmetry" in Part 4.

This is what Pauli had to say about this paper in a long letter dated 10 July 1949 (letter [1039] in the Pauli Collection) to Rudolf Peierls in Birmingham (translated from German):

> 4. A Swede from Lund named Källén, who is now in Zürich, has treated with great success the problem of *higher orders* (i.e., not linear in the external field) *of vacuum polarization* that you brought up during my visit with you at Birmingham. …

C. Jarlskog (Ed.), *Portrait of Gunnar Källén*, DOI 10.1007/978-3-319-00627-7_87,

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Higher Approximations in the external field for the Problem of Vacuum Polarization

by **G. Källén.**
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(13. VIII. 1949.)

Summary. The convergence and gauge invariance of the integrals appearing in the higher approximations of vacuum polarization are here discussed for the cases of spinor and scalar fields. Only the comparatively simple case of an external electromagnetic field is treated. The more complicated problem of a quantizated field is not discussed.

As a direct result of the charge symmetry the vacuum expectation values of the current commutators vanish if an odd number of current operators are commuted. Hence the terms proportional to e^{2n+1} are identically zero, and in this form the statement is true for both the electrons and the bosons. Of the remaining terms it is shown that only the e^2 and e^4 approximations diverge, but that the still higher terms are both convergent and gauge invariant. It further appears that, apart from a numerical factor, which is the same in both approximations (and equal to $-\frac{1}{2}$), the strongest divergences are the same for the spinor and the scalar fields. The e^2 approximation has previously been treated by Jost and Rayski, who have shown that the non-gauge invariant (and divergent) terms compensate each other if one uses a suitable mixture of spinor and scalar fields. In this approximation, however, the logaritmically divergent charge renormalisation remains. The conditions of Jost and Rayski are

$$N = 2n \tag{I}$$

$$\sum_{i=1}^{N} M_i^2 = 2 \sum_{i=1}^{n} m_i^2 \tag{II}$$

where N and n are the number of scalar and spinor fields respectively and M_i and m_i are the corresponding masses. In the e^4 approximation the condition (I) alone is sufficient to secure a convergent and gauge invariante result.

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Introduction.

The e^2 approximation of the vacuum polarization has already been treated by several authors[1]) with the aid of the explicitely relativistically invariant quantum dynamics developed by SCHWINGER[2]) and others. It has been shown by WENTZEL[1]) that it is possible to obtain different results for the photon self energy (which can be considered as a special case of the more general problem of the vacuum polarization) if one uses different methods of computing the integrals which appear in the formulae for the vacuum polarization. This question of uniqueness has been further discussed by PAULI and VILLARS[1]). In their paper the latter authors have given a method of invariant regularization of the different Δ-functions with the help of auxiliary masses. These masses, however, are only regarded as a mathematical aid for the computation and in the final result they are allowed to tend to infinity. It has been observed independently by RAYSKI and UMEZAWA[3]) that it is also possible to regard these masses as observable if one assumes that the corresponding particles obey Bose-statistics. Detailed calculations by JOST and RAYSKI[1]) in the e^2 approximation have given as a result that the necessary assumptions in these realistic theories show a remarkable analogy with the more formalistical conditions of PAULI and VILLARS. We wish to extend the work of JOST and RAYSKI to the higher approximations in the fine-structure-constant and have as general equations

$$i\,\frac{\delta\,\psi[\sigma]}{\delta\,\sigma(x)} = H(x)\,\psi[\sigma] \tag{1}$$

$$H(x) = H_F(x) + H_B(x) \tag{1a}$$

$$H_F(x) = -\sum j_{\mu_F}(x)\,A_\mu(x) \tag{2}$$

$$j_{\mu_F}(x) = \frac{i\,e}{2}\left[\overline{\psi}(x)\,\gamma_\mu\,\psi(x) - \psi(x)\,\gamma_\mu^T\,\overline{\psi}(x)\right] \tag{2a}$$

$$H_B(x) = -\sum t_\mu(x)\,A_\mu(x) \tag{3}$$

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[1]) E. g. J. SCHWINGER, Phys. Rev. **75**, 651 (1949); G. WENTZEL, Phys. Rev. **74**, 1070 (1948); W. PAULI, F. VILLARS, Rev. Mod. Phys. **21**, 434 (1949); R. JOST, J. RAYSKI, Helv. Phys. Acta **22**, 457 (1949). For earlier work without use of invariant formalism a summary has been given by V. WEISSKOPF, Det. Kgl. Danske Vid. Selskab, XIV 6 (1936).

[2]) TOMONAGA, Progr. Theor. Phys. **1**, 27 (1946); J. SCHWINGER, Phys. Rev. **74**, 1439 (1948); ibid. **75**, 651 (1949); ibid. **76**, 790 (1949); F. J. DYSON: Phys. Rev. **75**, 486 (1949).

[3]) J. RAYSKI, Acta Physica Polonica **9**, 129 (1948); H. UMEZAWA, J. YUKAWA, E. YAMADA, Progr. Theor. Phys. **3**, 317 (1948).

$$t_\mu(x) = i e s_\mu(x) + \xi_{\mu\nu} A_\nu(x) \cdot e^2 \cdot \varphi^*(x) \varphi(x) \tag{3a}$$

$$s_\mu(x) = \frac{\partial \varphi^*(x)}{\partial x_\mu} \varphi(x) - \frac{\partial \varphi(x)}{\partial x_\mu} \varphi^*(x) \tag{3b}$$

$$\xi_{\mu\nu} = \delta_{4\mu} \cdot \delta_{4\nu} - \delta_{\mu\nu} \tag{3c}$$

Here[4]) $\psi(x)$ and $\bar{\psi}(x) = \psi^*(x)\gamma_4$ are the spinor fields and $\varphi(x)$ and $\varphi^*(x)$ the scalar fields[5]). The symbols $t_\mu(x)$, $s_\mu(x)$ and $\xi_{\mu\nu}$ are defined by equations (3a) (3b) and (3c). The current operator for the scalar field is given by[6])

$$j_{\mu_B}(x) = i e s_\mu(x) + 2\,\xi_{\mu\nu} A_\nu(x) \cdot e^2 \varphi^*(x) \varphi(x) \tag{4}$$

The total current is

$$j_\mu(x) = \sum j_{\mu_F}(x) + \sum j_{\mu_B}(x) \tag{4a}$$

It may be observed that none of the expressions (3a), (3b) or (3c) are tensors and that only the current $j_\mu(x)$ is a vector. The $A_\mu(x)$ are the four-dimensional vector potentials for the external electromagnetic field. They are here considered as given functions of space and time and not as operators. Hence we neglect the modification of the electromagnetic field due to polarization phenomena. The summations in equations (2) (3) and (4a) are to be extended over the spinor and scalar fields present.

The operators $\psi(x)$ and $\varphi(x)$ satisfy the following relations[4])

$$\{\psi_\alpha(x);\ \bar{\psi}_\beta(x')\} = -i S_{\alpha\beta}(x-x') \tag{5}$$

$$\{\psi_\alpha(x);\ \psi_\beta(x')\} = \{\bar{\psi}_\alpha(x);\ \bar{\psi}_\beta(x')\} = 0 \tag{5a}$$

$$[\varphi^*(x);\ \varphi(x')] = i\Delta(x-x') \tag{6}$$

$$[\varphi^*(x);\ \varphi^*(x')] = [\varphi(x);\ \varphi(x')] = 0 \tag{6a}$$

Different kinds of fields always commute.

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[4]) The notations are essentially the same as those used by SCHWINGER but with natural units ($\hbar = c = 1$).

[5]) Computations by FELDMANN have shown that it is not possible to compensate also the divergent charge renormalisation term by including fields with spin 1 (Unpublished letter to professor PAULI). The same result has also been gotten by H. UMEZAWA, R. KAWABE, Prog. Theor. Phys. (in press).

[6]) G. WENTZEL: Quantentheorie der Wellenfelder.

We want to study the modified current operator, the expectation value of which may be written as[2])

$$\langle j_\mu(x)\rangle = \sum_{n=0}^{\infty}(i)^n \int_{-\infty}^{\sigma} d\,x' \int_{-\infty}^{\sigma'} d\,x'' \cdots \int_{-\infty}^{\sigma^{(n-1)}} d\,x^n \times$$
$$\times \langle \left[H(x^n)\left[\cdots\left[H(x');\; j_\mu(x)\right]\cdots\right]\right]\rangle_0 \tag{7}$$

If the expressions (1a)—(4a) are substituted in equation (7) one gets a sum of commutators, some of which contains only one kind of fields (one of the $\psi(x)$ or one of the $\varphi(x)$ fields). The other terms contain at least one commutator between two different fields and are hence zero. This fact makes it possible to carry through the calculation for each field separately and in the end simply add the results together.

The Spinor fields.

We consider now only one of the spinor fields and write the corresponding parts of the current operator and of the hamiltonian as

$$j_\mu(x) = \frac{i\,e}{2}\left[\overline{\psi}(x);\; \gamma_\mu\,\psi(x)\right] \tag{8}$$

$$H(x) = -j_\mu(x)\,A_\mu(x) \tag{9}$$

In this case equation (7) gives

$$\langle j_\mu(x)\rangle = \sum_{n=0}^{\infty} i^{-n}\int_{-\infty}^{\sigma} d\,x' \cdots \int_{-\infty}^{\sigma^{(n-1)}} d\,x^n A_{\nu_1}(x')\cdots A_{\nu_n}(x^n)\times$$
$$\times \langle\left[j_{\nu_n}(x^n)\left[\cdots\left[j_{\nu_1}(x');\; j_\mu(x)\right]\cdots\right]\right]\rangle_0 \tag{10}$$

and our first task is to evaluate the iterated current commutators in this expression. If n is an even integer we have a commutator of an odd number of current operators, that is an expression of the following form

$$\left(\frac{i\,e}{2}\right)^{2n+1}\langle\left[\left[\overline{\psi}(x^{2n});\; \gamma_{\nu_{2n}}\psi(x^{2n})\right]\left[\cdots\left[\left[\overline{\psi}(x');\; \gamma_{\nu_1}\psi(x')\right];\right.\right.\right.$$
$$\left.\left.\left.\left[\overline{\psi}(x);\; \gamma_\mu\,\psi(x)\right]\right]\cdots\right]\right]\rangle_0 \tag{11}$$

If we use the charge conjugate spinor $\psi'(x)$ equation (8) might as well be written as

$$j_\mu(x) = -\frac{i\,e}{2}\left[\overline{\psi}'(x);\; \gamma_\mu\,\psi'(x)\right] \tag{12}$$

and equation (11) as

$$-\left(\frac{ie}{2}\right)^{2n+1}\left\langle\left[[\bar\psi'(x^{2n});\ \gamma_{\nu_{2n}}\psi'(x^{2n})]\left[\cdots[[\bar\psi'(x');\ \gamma_{\nu_1}\psi'(x')];\right.\right.\right.$$
$$\left.\left.\left.[\bar\psi'(x);\ \gamma_\mu\psi'(x)]]\cdots\right]\right]\right\rangle_0 \tag{13}$$

As the two vaccum expectation values in (11) and (13) are equal we conclude that they are both zero and that equation (10) may be simplified to

$$\langle j_\mu(x)\rangle = \sum_{n=0}^{\infty} i^{2n-1}\int_{-\infty}^{\sigma} dx'\cdots\int_{-\infty}^{\sigma^{2n}} dx^{2n+1} A_{\nu_1}(x')\cdots A_{\nu_{2n+1}}(x^{2n+1})\times$$
$$\times\left\langle\left[j_{\nu_{2n+1}}(x^{2n+1})\left[\cdots[j_{\nu_1}(x');\ j_\mu(x)]\cdots\right]\right]\right\rangle_0 \tag{14}$$

If Ω is an arbitrary operator we have

$$[[\bar\psi(x);\ \Omega\psi(x')]\bar\psi_\alpha(x'')] = \left(\bar\psi(x)\,\Omega\right)_\beta\{\psi_\beta(x');\ \bar\psi_\alpha(x'')\} +$$
$$+\{\bar\psi_\alpha(x'');\ \psi_\beta(x')\}\left(\Omega^T\bar\psi(x)\right)_\beta = -2i\left(\bar\psi(x)\,\Omega S(x'-x'')\right)_\alpha \tag{15}$$

In a similar way we get

$$[[\bar\psi(x);\ \Omega\psi(x')]\psi_\alpha(x'')] = 2i\left(S(x''-x)\,\Omega\,\psi(x')\right)_\alpha \tag{16}$$

and hence

$$[[\bar\psi(x);\ \Omega\psi(x')]j_\mu(x'')] = \frac{ie}{2}\Big([[\bar\psi(x);\ \Omega\psi(x')]\bar\psi_\alpha(x'')]\left(\gamma_\mu\psi(x'')\right)_\alpha +$$
$$+\left(\bar\psi(x'')\gamma_\mu\right)_\alpha[[\bar\psi(x);\ \Omega\psi(x')]\psi_\alpha(x'')] - [[\bar\psi(x);\ \Omega\psi(x')]\psi_\alpha(x'')]\times$$
$$\times\left(\gamma_\mu^T\bar\psi(x'')\right)_\alpha - \left(\psi(x'')\gamma_\mu^T\right)_\alpha[[\bar\psi(x);\ \Omega\psi(x')]\bar\psi_\alpha(x'')]\Big) =$$
$$= e\Big([\bar\psi(x);\ \Omega S(x'-x'')\gamma_\mu\psi(x'')] -$$
$$-[\bar\psi(x'');\ \gamma_\mu S(x''-x)\,\Omega\,\psi(x')]\Big). \tag{17}$$

From equation (17) it is imidiately seen that the commutator

$$\left[j_{\nu_n}(x^n)\left[\cdots[j_{\nu_1}(x')\,j_\mu(x)]\cdots\right]\right]$$

consists of 2^{n+1} terms of the form

$$\pm\frac{ie^{n+1}}{2}[\bar\psi(x^i);\ \gamma_{\nu_i}S(x^i-x^j)\cdots\gamma_{\nu_r}S(x^r-x^s)\gamma_{\nu_s}\psi(x^s)] \tag{18}$$

where i; ;... r; s is some permutation of the numbers 0; 1;... n. With the aid of the formula

$$\langle[\bar\psi_\alpha(x);\ \psi_\beta(x')]\rangle_0 = S^{(1)}_{\beta\alpha}(x'-x)$$

the vaccum expectation value of (18) is given by

$$\pm \frac{i\,e^{n+1}}{2}\, Sp[S^{(1)}(x^s - x^i)\,\gamma_{\nu_i} S(x^i - x^j)\cdots\gamma_{\nu_s} S(x^r - x^s)\,\gamma_{\nu_s}] \tag{19}$$

Hence each term in the series (14) may be written as a sum of 2^{2n+2} terms of the form

$$(-1)^n \left(\pm \frac{e^{2n+2}}{2}\right) \int\limits_{-\infty}^{\sigma} d\,x' \cdots \int\limits_{-\infty}^{\sigma^{2n}} d\,x^{2n+1}\, Sp[S^{(1)}(x^s - x^i)\,\gamma A(x^i)\, S(x^i - x^j) \cdots \gamma A(x^r)\, S(x^r - x^s)\,\gamma A(x^s)] \tag{20}$$

If we suppose that the external field does not allow any real pairs to be created we can transform the expression (20) into

$$(-1)^n \left(\pm \left(\frac{e}{2}\right)^{2n+2}\right) \int\limits_{-\infty}^{+\infty} \cdots \int\limits_{-\infty}^{+\infty} d\,x' \cdots d\,x^{2n+1}\ \varepsilon(01)\,\varepsilon(12)\cdots \cdots \varepsilon(2n;2n+1)\ Sp[S^{(1)}(s\,i)\,\gamma A(i)\cdots S(r\,s)\,\gamma A(s)]\,. \tag{21}$$

After changing the notations and rearrenging the terms in the traces it is always possible to write each term in equation (14) as

$$\begin{aligned}(-1)^n \left(\frac{e}{2}\right)^{2n+2} \int\limits_{-\infty}^{+\infty} \cdots \int\limits_{-\infty}^{+\infty} d\,x' \cdots d\,x^{2n+1} \Big(& Sp[\gamma_\mu S^{(1)}(01)\,\gamma A(1)\, S(12) \cdots \\ & \cdots S(2n+1,0)]\, E_1^{(2n+1)} + Sp[\gamma_\mu S(01)\,\gamma A(1)\, S^{(1)}(12) \cdots \\ & \cdots S(2n+1,0)]\, E_2^{(2n+1)} + \cdots + Sp[\gamma_\mu S(01)\,\gamma A(1)\, S(12) \cdots \\ & \cdots S^{(1)}(2n+1,0)]\, E_{2n+2}^{(2n+1)}\Big)\end{aligned} \tag{22}$$

where $E_1^{(2n+1)};\ E_2^{(2n+1)};\ \cdots\ E_{2n+2}^{(2n+1)}$ are some functions of the $\varepsilon\,(i,\,j)$. It will be proven in the appendix that the expression (22) may be written as

$$\begin{aligned}(-1)^n \frac{e^{2n+2}}{2} \int\limits_{-\infty}^{+\infty} \cdots \int\limits_{-\infty}^{+\infty} d\,x' \cdots dx^{2n+1} \Big(& (Sp[\gamma_\mu S^{(1)}(01)\,\gamma A(1)\,\overline{S}(12) \cdots \\ & \cdots \overline{S}(2n+1,0)] + Sp[\gamma_\mu \overline{S}(01)\,\gamma A(1)\, S^{(1)}(12) \cdots \overline{S}(2n+1,0)] \cdots + \\ & \cdots + Sp[\gamma_\mu \overline{S}(01)\,\gamma A(1)\,\overline{S}(12) \cdots S^{(1)}(2n+1,0)]\Big)\end{aligned} \tag{23}$$

and hence

$$\begin{aligned}\langle j_\mu(x) \rangle = \frac{1}{2} \sum_{n=0}^{\infty} (-1)^n e^{2n+2} \int\limits_{-\infty}^{+\infty} \cdots \int\limits_{-\infty}^{+\infty} d\,x' \cdots d\,x^{2n+1} \Big(& Sp[\gamma_\mu S^{(1)}(01) \times \\ \times \gamma A(1) \cdots \overline{S}(2n+1,0)] + \cdots + Sp[\gamma_\mu \overline{S}(01)\,\gamma A(1) \cdots S^{(1)}(2n+1,0)] &\end{aligned} \tag{24}$$

In momentum space this formula reads

$$\langle j_\mu(x) \rangle = \frac{1}{2} \sum_{n=0}^{\infty} (-1)^n e^{2n+2} \left(\frac{1}{2\pi}\right)^{8n+7} \int \cdots \int dp\, dp' \cdots dp^{2n+1} \times$$
$$\times e^{ix(p-p^{2n+1})} Sp[\gamma_\mu (i\gamma p - m)\gamma A(p'-p)(i\gamma p' - m) \cdots$$
$$\cdots \gamma A(p^{2n+1} - p^{2n})(i\gamma p^{2n+1} - m)] \left(\frac{\delta(p^2+m^2)}{(p'^2+m^2)\cdots(p^{2n+1^2}+m^2)} + \right.$$
$$\left. + \frac{\delta(p'^2+m^2)}{(p^2+m^2)(p''^2+m^2)\cdots(p^{2n+1^2}+m^2)} + \cdots + \frac{\delta(p^{2n+1^2}+m^2)}{(p^2+m^2)\cdots(p^{2n^2}+m^2)} \right) \tag{25}$$

where

$$A\nu(p) = \int A\nu(x)\, e^{ipx}\, dx \tag{26}$$

The equations (24) and (25) give a formal expression for the expectation value of the current operator, but the questions of convergence and gauge invariance are still open.

We first want to give a formal proof of the gauge invariance in momentum space. It is true that this proof is only valid if the integrals converge but we will leave this question unsettled for the moment.

For this calculation we need the following formula

$$Sp[\Omega(i\gamma a - m)\gamma q(i\gamma(a+q) - m)] = i[(a+q)^2 + m^2] Sp[\Omega(i\gamma a - m)] -$$
$$- i[a^2+m^2] Sp[\Omega(i\gamma(a+q) - m)] \tag{27}$$

In equation (27) a and q are two arbitrary fourdimensional vectors, m a number and Ω an arbitrary operator. The formula (27) may be proven by an explicite calculation e. g. in the following way

$$Sp[\Omega(i\gamma a - m)\gamma q(i\gamma(a+q) - m)] = -Sp[\Omega\cdot\gamma a\cdot\gamma q\cdot\gamma(a+q)] -$$
$$- im(Sp[\Omega\cdot\gamma q\cdot\gamma(a+q)] + Sp[\Omega\cdot\gamma a\cdot\gamma q]) + m^2 Sp[\Omega] \tag{28}$$

Using the equation

$$\gamma_\mu\gamma_\nu + \gamma_\nu\gamma_\mu = 2\,\delta_{\mu\nu}$$

we get from (28)

$$-2qa\, Sp[\Omega\cdot\gamma a] + a^2 Sp[\Omega\cdot\gamma q] - q^2 Sp[\Omega\cdot\gamma a] -$$
$$- mi(q^2 + 2aq) Sp[\Omega] + m^2 Sp[\Omega] = i[(a+q)^2 + m^2] \times$$
$$\times Sp[\Omega(i\gamma a - m)] - i(a^2+m^2) Sp[\Omega(i\gamma(a+q) - m)]$$

which is the right hand side of (27).

One typical term in (25) may be written as

$$\int \cdots \int d\,p\,d\,p' \cdots d\,p^n\, e^{i\,x(p-p^n)}\, Sp\,[\gamma_\mu (i\gamma\,p - m)\,\gamma\,A\,(p'-p)\cdots$$
$$\cdots (i\gamma\,p^n - m)]\,\frac{\delta\,(p^{i^2}+m^2)}{(p^2+m^2)\cdots(p^{i-1^2}+m^2)\,(p^{i+1^2}+m^2)\cdots(p^{n^2}+m^2)} \quad (29)$$

(In (29) n is supposed to be odd but for our present purpose we do not need this fact.)

If we make an infinitesimal gauge transformation

$$\left.\begin{aligned} A\,\nu\,(x) &\to A\,\nu\,(x) + \frac{\partial\,\Lambda\,(x)}{\partial\,x_\nu} \\ \varepsilon\,(p) &= \int e^{i\,x\,p}\,\Lambda\,(x)\,d\,x \end{aligned}\right\} \quad (30)$$

the integral (29) will be changed by the following amount

$$\frac{1}{i}\int \cdots \int d\,p \cdots d\,p^n\, e^{i\,x\,(p-p^n)}\,\frac{\delta\,(p^{i^2}+m^2)}{(p^2+m^2)\cdots(p^{i-1^2}+m^2)\,(p^{i+1^2}+m^2)\cdots(p^{n^2}+m^2)}\times$$
$$\times\big(Sp\,[\gamma_\mu (i\,\gamma\,p - m)\,\gamma\,(p'-p)\,(i\,\gamma\,p' - m)\,\gamma\,A\,(p''-p')\cdots]\times$$
$$\times\,\varepsilon\,(p'-p) + Sp\,[\gamma_\mu (i\,\gamma\,p - m)\,\gamma\,A\,(p'-p)\,(i\,\gamma\,p' - m)\times$$
$$\times\,\gamma\,(p''-p')\cdots]\,\varepsilon\,(p''-p') + \cdots\big) \quad (31)$$

Incerting equation (27) in formula (31) we get

$$\int \cdots \int d\,p \cdots d\,p^n\, e^{i\,x\,(p-p^n)}\,\frac{\delta\,(p^{i^2}+m^2)}{(p^2+m^2)\cdots(p^{i-1^2}+m^2)\,(p^{i+1^2}+m^2)\cdots(p^{n^2}+m^2)}\times$$
$$\times\,[(p'^2+m^2)\,Sp\,[\gamma_\mu (i\,\gamma\,p - m)\,\gamma\,A\,(p''-p')\cdots]\,\varepsilon\,(p'-p) -$$
$$-(p^2+m^2)\,Sp\,[\gamma_\mu (i\,\gamma\,p' - m)\,\gamma\,A\,(p'-p)\cdots]\,\varepsilon\,(p'-p) +$$
$$+\,(p''^2+m^2)\,Sp\,[\gamma_\mu (i\,\gamma\,p - m)\,\gamma\,A\,(p'-p)\cdots]\times\varepsilon\,(p''-p') -$$
$$-(p'^2+m^2)\,Sp\,[\gamma_\mu (i\,\gamma\,p - m)\,\gamma\,A(p'-p)\cdots]\,\varepsilon\,(p''-p') + \cdots] \quad (32)$$

With the notation

$$\int d\,p'\,A_\nu (p'-p)\,\varepsilon\,(p'-p'') = \int d\,q\,\varepsilon\,(q)\,A_\nu\,(p''-p-q) = E_\nu (p''-p) \quad (33)$$

and the formula

$$\int d\,p\,\varepsilon\,(p'-p)\,e^{i\,x\,p} = \left(\frac{1}{2\,\pi}\right)^4\,\Lambda\,(x)\,e^{i\,x\,p'} \quad (34)$$

the expression (32) may be written (the terms containing $p^{i2} + m^2$ give zero due to the factor $\delta\,(p^{i2} + m^2)$).

$$\int \cdots \int d\,p' \cdots d\,p^n e^{i\,x\,(p'-p'')} \frac{\delta\,(p^{i2}+m^2)}{(p'^2+m^2)\cdots(p^{i-1\,2}+m^2)\,(p^{i+1\,2}+m^2)\cdots(p^{n2}+m^2)} \times$$
$$\times \left[Sp[\gamma_\mu(i\gamma\,p'-m)\,\gamma\,E(p''-p')\,(i\gamma\,p''-m)\cdots]-\left(\frac{1}{2\pi}\right)^4 A\,(x) \times\right.$$
$$\times\, Sp[\gamma_\mu\,(i\gamma\,p'-m)\,\gamma\,A\,(p''-p')\,(i\gamma p''-m)\cdots]+$$
$$+\,Sp[\gamma_\mu(i\gamma p'-m)\,\gamma\,A\,(p''-p')\,(i\gamma\,p''-m)\,\gamma\,E\,(p'''-p'')\cdots]-$$
$$-\,Sp[\gamma_\mu(i\gamma p'-m)\,\gamma\,E\,(p''-p')\,(i\gamma p''-m)\cdots]+\cdots$$
$$\left.\cdots+\left(\frac{1}{2\pi}\right)^4 A\,(x)\,Sp[\gamma_\mu(i\gamma p'-m)\,\gamma\,A\,(p''-p')\cdots]\right]=0\,. \tag{35}$$

From (35) we conclude that each term in (25) if convergent is also gauge invariant.

To discuss the convergence properties of (25) we write the bracket as

$$\frac{\delta\,(p^2+m^2)}{(p'^2-p^2)\cdots(p^{n2}-p^2)}+\frac{\delta\,(p'^2+m^2)}{(p^2-p'^2)\,(p''^2-p'^2)\cdots(p^{n2}-p'^2)}+\cdots$$
$$\cdots+\frac{\delta\,(p^{n2}+m^2)}{(p^2-p^{n2})\cdots(p^{n-1\,2}-p^{n2})} \tag{36}$$

We now use the formula

$$\frac{1}{(p'^2-p^2)\cdots(p^{n2}-p^2)}+\frac{1}{(p^2-p'^2)\cdots(p^{n2}-p'^2)}+\cdots$$
$$\cdots+\frac{1}{(p^2-p^{n2})\cdots(p^{n-1\,2}-p^{n2})}=0 \tag{37}$$

to write (36) as

$$\frac{\delta\,(p'^2+m^2)-\delta\,(p^2+m^2)}{(p^2-p'^2)\,(p''^2-p'^2)\cdots(p^{n2}-p'^2)}+\cdots+\frac{\delta\,(p^{n2}+m^2)-\delta\,(p^2+m^2)}{(p^2-p^{n2})\cdots(p^{n-1\,2}-p^{n2})} \tag{38}$$

Following SCHWINGER this can also be written

$$-\frac{1}{2}\int_{-1}^{+1} d\,u \left[\frac{\delta'\left(\frac{1}{2}\,(p^2+p'^2)+m^2+\frac{1}{2}\,u\,(p^2-p'^2)\right)}{(p''^2-p'^2)\cdots(p^{n2}-p'^2)}+\cdots\right.$$
$$\left.\cdots+\frac{\delta'\left(\frac{1}{2}\,(p^2+p^{n2})+m^2+\frac{1}{2}\,u\,(p^2-p^{n2})\right)}{(p'^2-p^{n2})\cdots(p^{n-1\,2}-p^{n2})}\right] \tag{39}$$

By repeating this, process it is obviously possible to write (36) in the following form

$$\left(-\frac{1}{2}\right)^{\frac{n(n+1)}{2}} \int_{-1}^{+1} d u_1 (1-u_1)^{n-1} \int_{-1}^{+1} d u_2 (1-u_2)^{n-2} \cdots \int_{-1}^{+1} d u_n \, \delta^{(n)} \Big(m^2 +$$
$$+ \frac{p^2}{2} + \frac{p'^2}{4} \cdots + \frac{p^{n-1\,2}}{2^n} + \frac{p^{n\,2}}{2^n} - u_1 \left(-\frac{p^2}{2} + \frac{p'^2}{4} + \frac{p''^2}{8} + \cdots + \right.$$
$$\left. + \frac{p^{n-1\,2}}{2^n} + \frac{p^{n\,2}}{2^n} \right) - u_2 (1-u_1) \times \left(-\frac{p'^2}{4} + \frac{p''^2}{8} + \cdots + \frac{p^{n-1\,2}}{2^n} + \frac{p^{n\,2}}{2^n} \right) -$$
$$- \cdots - u_n (1-u_{n-1}) \cdots (1-u_1) \left(-\frac{p^{n-1\,2}}{2^n} + \frac{p^{n\,2}}{2^n} \right) \Big) \tag{40}$$

It is now convenient to make a translation of the origins and write (25) in the following way

$$\langle j_\mu (x) \rangle = \frac{1}{2} \sum_{n=0}^{\infty} e^{2n+2} (-1)^n \left(\frac{1}{2\pi} \right)^{8n+7} \int \cdots \int d q' \cdots d q^{2n+1} \times$$
$$\times e^{-i x (q' + \cdots + q^{2n+1})} K^{(2n+1)}_{\mu \nu_1 \cdots \nu_{2n+1}} (q' \cdots q^{2n+1}) \, A_{\nu_1} (q') \cdots$$
$$\cdots A_{\nu_{2n+1}} (q^{2n+1}) \tag{41}$$

where the kernel $K^{(n)}_{\mu \cdots \nu_n}$ according to (40) is given by

$$K^{(n)}_{\mu \nu_1 \cdots \nu_n} (q' \cdots q^n) = \left(-\frac{1}{2} \right)^{\frac{n(n+1)}{2}} \int d p \int_{-1}^{+1} d u_1 (1-u_1)^{n-1} \int_{-1}^{+1} d u_2 \times$$
$$\times (1-u_2)^{n-2} \cdots \int_{-1}^{+1} d u_n \, \delta^{(n)} (m^2 + p^2 + 2 p Q + Q^2 + \varphi) \, Sp \, [\gamma_\mu \times$$
$$\times (i \gamma p - m) \gamma_{\nu_1} (i \gamma (p + q') - m) \gamma_{\nu_2} \times (i \gamma (p + q' + q'') - m) \cdots$$
$$\cdots \gamma_{\nu_n} (i \gamma (p + q' + \cdots + q^n) - m)] \tag{42}$$

Here

$$Q = (1-u_1) \frac{q'}{2} + (1-u_1)(1-u_2) \frac{q''}{4} + \cdots + (1-u_1) \cdots (1-u_n) \frac{q^n}{2^n} \tag{43}$$

and φ is a bilinear expression in $q' \ldots q^n$.

Another translation of the origin transforms (42) into

$$K^{(n)}_{\mu \nu_1 \cdots \nu_n} (q' \cdots q^n) = \left(-\frac{1}{2} \right)^{\frac{n(n+1)}{2}} \int_{-1}^{+1} d u_1 (1-u_1)^{n-1} \cdots \int_{-1}^{+1} d u_n \int d q \times$$
$$\times \delta^{(n)} (q^2 + m^2 + \varphi) \, Sp \, [\gamma_\mu (i \gamma (q + a_1) - m) \gamma_{\nu_1} (i \gamma (q + a_2) - m) \cdots$$
$$\cdots \gamma_{\nu_n} (i \gamma (q + a_{n+1}) - m)] \tag{44}$$

where $a_1 \ldots a_{n+1}$ are linear combinations of $q' \ldots q^n$ with coefficients which depend on $u_1 \ldots u_n$.

We now suppose that the potentials $A_\nu(q)$ vanish for large momenta so that the integrations over $q' \ldots q^{2n+1}$ in (41) converge and are thus left with only the q integration in (44). The strongest divergence in this integral is of the form

$$\int dq\, \delta^{(n)}(q^2 + \varkappa^2)\, q_\mu q_{\nu_1} \cdots q_{\nu_n} \tag{45}$$

With the well-known representation of the δ-function (45) may be written

$$\frac{1}{2\pi} \cdot i^n \int\limits_{-\infty}^{+\infty} x^n\, dx \int dq\, e^{ix(q^2+\varkappa^2)}\, q_\mu q_{\nu_1} \cdots q_{\nu_n} \tag{46}$$

The q-space integration in (46) can be performed without difficulty and gives some constant factor λ multiplied by $|x| \cdot x^{-\frac{n+1}{2}-3}$. The integral (46) can therefore be written as

$$\frac{\lambda}{2\pi} \cdot i^n \int\limits_{-\infty}^{+\infty} |x| \cdot x^{\frac{n-7}{2}}\, e^{i\varkappa^2 x}\, dx \tag{47}$$

The expression (47) and the kernel (44) converge for $n \geqq 4$ but diverge for $n \leqq 3$. As we are only interested in odd integers n, the only divergent integrals are

$$(n = 1) \int\limits_{-\infty}^{+\infty} \frac{e^{i\varkappa^2 x}}{x \cdot |x|}\, dx \tag{48a}$$

and

$$(n = 3) \int\limits_{-\infty}^{+\infty} \frac{e^{i\varkappa^2 x}}{|x|}\, dx \tag{48b}$$

The first one appears in the e^2 approximation and as this case has already been treated by several authors we limit ourselves to the e^4 approximation, where the integral (48b) is the only divergent one. The kernel is here

$$\begin{aligned} K^{(3)}_{\mu\nu_1\nu_2\nu_3}(q' q'' q''') = \frac{1}{64} \int\limits_{-1}^{+1} du_1 (1-u_1)^2 \int\limits_{-1}^{+1} du_2 (1-u_2) \int\limits_{-1}^{+1} du_3 \times \\ \times \int\limits_{-\infty}^{+\infty} x^3\, dx \int dq\, e^{ix(q^2+m^2+\varphi)}\, Sp\left[\gamma_\mu (i\gamma(q+a_1) - m) \cdots \right. \\ \left. \cdots \gamma_{\nu_3} (i\gamma(q+a_4) - m)\right] \end{aligned} \tag{49}$$

and the divergent term

$$\frac{1}{64}\int_{-1}^{+1} du_1 (1-u_1)^2 \int_{-1}^{+1} du_2 (1-u_2) \int_{-1}^{+1} du_3 \int_{-\infty}^{-\infty} x^3 dx \int dq\, e^{ix(q^2+m^2+\varphi)} \times$$
$$\times Sp[\gamma_\mu \gamma q \gamma_{\nu_1} \gamma q \gamma_{\nu_2} \gamma q \gamma_{\nu_3} \gamma q] \qquad (50)$$

The trace in (50) is easily evaluated and is equal to

$$4\{8 q_\mu q_{\nu_1} q_{\nu_2} q_{\nu_3} - 2q^2 (q_\mu q_{\nu_1} \delta_{\nu_2 \nu_3} + q_\mu q_{\nu_3} \delta_{\nu_1 \nu_2} + q_{\nu_1} q_{\nu_3} \delta_{\mu \nu_3} +$$
$$+ q_{\nu_2} q_{\nu_2} \delta_{\mu \nu_1}) + q^4 (\delta_{\mu \nu_1} \delta_{\nu_2 \nu_3} - \delta_{\mu \nu_2} \delta_{\nu_1 \nu_3} + \delta_{\mu \nu_3} \delta_{\nu_1 \nu_2})\}. \qquad (51)$$

The scalar fields.

In this case equation (7) reads

$$\langle j_\mu(x)\rangle = \sum_{n=0}^{\infty} i^{-n} \int_{-\infty}^{\sigma} dx' \int_{-\infty}^{\sigma'} dx'' \cdots \int_{-\infty}^{\sigma^{(n-1)}} dx^n A_{\nu_1}(x') \cdots A_{\nu_n}(x^n) \times$$
$$\times \langle [t_{\nu_n}(x^n) [\cdots [t_{\nu_1}(x') j_\mu(x)] \cdots]]\rangle_0 . \qquad (52)$$

As both $t_\nu(x)$ and $j_\mu(x)$ here contain two different powers of e, formula (52) is not an expansion in powers of the charge. It is, however, always possible to rearrenge the terms so as to get such an expansion. For this purpose we write

$$\langle j_\mu(x)\rangle = \sum_{n=0}^{\infty} \sum_{k=0}^{n+1} e^{n+k+1} \langle j_\mu(x)\rangle^{(n)(k)} = \sum_{n=0}^{\infty} e^{n+1} \langle j_\mu(x)\rangle^{(n)} \qquad (53)$$

where

$$\langle j_\mu(x)\rangle^{(n)} = \sum_{k=0}^{\frac{n+1}{2}} \langle j_\mu(x)\rangle^{(n-k)(k)} \qquad (53a)$$

for odd n, and

$$\langle j_\mu(x)\rangle^{(n)} = \sum_{k=0}^{\frac{n}{2}} \langle j_\mu(x)\rangle^{(n-k)(k)} \qquad (53b)$$

when n is even. From equations (52) and (53) we also get

$$\langle j_\mu(x)\rangle^{(n)(k)} = \left(\frac{1}{2}\right)^n i^{1-k} \int_{-\infty}^{+\infty} \cdots \int_{-\infty}^{+\infty} dx' \cdots dx^n\, \varepsilon(01)\, \varepsilon(12) \cdots \varepsilon(n-1, n) \times$$
$$\times A_{\nu_1}(x') \cdots A_{\nu_n}(x^n) \{\langle[\varphi^*(x^n)\, \varphi(x^n) [\cdots [\varphi^*(x^{n-k+1})\, \varphi(x^{n-k+1}) \times$$
$$\times [s_{\nu_{n-k}}(x^{n-k}) [\cdots [s_{\nu_1}(x')\, s_\mu(x)] \cdots]] \cdots]]\rangle_0\, \xi(n-k+1, n+1) \cdots$$
$$\cdots \xi(n, n+k)\, A_{\nu_{n+1}}(x^{n-k+1}) \cdots A_{\nu_{n+k}}(x^n) + \cdots\} \qquad (54)$$

The bracket in (54) contains one term for each way in which it is possible to pick out $n-k+1$ indices $\nu_{i_1} \ldots \nu_{i_{n-k+1}}$ from the series $\mu\nu_1 \ldots \nu_n$. If all $\nu_i \neq \mu$ the corresponding term is multiplied by a factor 2.

From the charge symmetry we see here too that the series in (53) will contain only even powers of e and hence that n in equation (53a) is an odd integer.

By an argument analogous to that leading from equation (14) to equation (23) but with differential operators instead of γ-matrices it is seen that (for n odd)

$$\left(\frac{1}{2}\right)^n \int_{-\infty}^{+\infty} \cdots \int_{-\infty}^{+\infty} dx' \cdots dx^n\, \varepsilon(01) \cdots \varepsilon(n-1, n)\, A_{\nu_1}(x') \cdots A_{\nu_n}(x^n)$$
$$\langle [s_{\nu_n}(x^n)[\cdots[s_{\nu_1}(x')\, s_\mu(x)]\cdots]]\rangle_0 = \frac{i}{2}\left(\frac{1}{2\pi}\right)^{4n+3} \int \cdots$$
$$\cdots \int dp\, dp' \cdots dp^n\, e^{ix(p-p^n)} A_{\nu_1}(p'-p) \cdots A_{\nu_n}(p^n - p^{n-1}) \times$$
$$\times P^{(n)} \times \left\{ \frac{\delta(p^2+m^2)}{(p'^2-p^2)\cdots(p^{n^2}-p^2)} + \frac{\delta(p'^2+m^2)}{(p^2-p'^2)\cdots(p^{n^2}-p'^2)} + \cdots \right.$$
$$\left. \cdots + \frac{\delta(p^{n^2}+m^2)}{(p^2-p^{n^2})\cdots(p^{n-1^2}-p^{n^2})} \right\} \tag{55}$$

where

$$P^{(n)} = \pi^{(n)} - \sum_j \delta_{4\nu_i}\, \delta_{4\nu_{i-1}} (p^{i-1^2} + m^2) \left[\pi^{(n)}_{i,i-1} - \frac{1}{2} \sum_j \delta_{4\nu_j}\, \delta_{4\nu_{j-1}} \times \right.$$
$$\left. \times (p^{j-1^2} + m^2) [\pi^{(n)}_{i,i-1,j,j-1} - \cdots] \right] \tag{56}$$

and

$$\pi^{(n)} = (p'_{\nu_1} + p_{\nu_1})(p''_{\nu_2} + p'_{\nu_2}) \cdots (p^n_{\nu_n} + p^{n-1}_{\nu_n})(p_\mu + p^n_\mu) \tag{57}$$

$$\pi^{(n)}_{i,j,\ldots} = \frac{\pi^{(n)}}{(p^i_{\nu_i} + p^{i-1}_{\nu_i})(p^j_{\nu_j} + p^{j-1}_{\nu_j})\cdots} \tag{58}$$

$$\pi^{(n)}_{i,j,\ldots} = 0 \text{ if two indices are equal.}$$

We here have also used the following formula given by Jost and Rayski

$$\varepsilon(x) \frac{\partial^2 \Delta(x)}{\partial x_\mu\, \partial x_\nu} = \frac{\partial^2 (\varepsilon(x)\, \Delta(x))}{\partial x_\mu\, \partial x_\nu} - 2\,\delta(x)\, \delta_{4\mu}\, \delta_{4\nu} \tag{59}$$

The more general commutators in (54) can be evaluated in the same way. Putting

$$A_{\nu_{n+i}}(n-k+i)\, A_{\nu_{n-k+i}}(n-k+i) =$$
$$= \int \delta(n-k+i, n+i)\, A_{\nu_{n-k+i}}(n-k+i)\, A_{\nu_{n+i}}(n+i)\, dx^{n+i} \tag{60}$$

we get a formula, which is similar to (56) but with $\xi\,(ij)$ substituted for $\delta_{4\,\nu_i}\,\delta_{4\,\nu_{i-1}}$ in some of the terms. The expression (53a) may now be written

$$\langle j_\mu(x)\rangle^{(2n+1)} = \frac{1}{2}\left(\frac{1}{2\pi}\right)^{8n+7}(-1)^n\int\cdots$$
$$\cdots\int dp\,dp'\cdots dp^{2n+1}\,e^{ix(p-p^{2n+1})}$$
$$\times\,\overline{P}^{(2n+1)}_{\mu\,\nu_1\cdots\nu_{2n+1}}\left\{\frac{\delta\,(p^2+m^2)}{(p'^2-p^2)\cdots(p^{n2}-p^2)}+\cdots+\frac{\delta\,(p^{n2}+m^2)}{(p^2-p^{n2})\cdots(p^{n-1\,2}-p^{n2})}\right\}\times$$
$$\times\,A_{\nu_1}(p'-p)\cdots A_{\nu_n}(p^n-p^{n-1}) \tag{61}$$

where

$$\overline{P}^{(n)}_{\mu\,\nu_1\cdots\nu_n} = \pi^{(n)} - \sum_i f^{(1)}_{\nu_i\,\nu_{i-1}}\,(p^{i-1\,2}+m^2)\,\pi^{(n)}_{i,\,i-1} + \frac{1}{2}\sum_{i,j} f^{(2)}_{\nu_i\,\nu_{i-1}\,\nu_j\,\nu_{j-1}}\times$$
$$\times\,(p^{i-1\,2}+m^2)\,(p^{j-1\,2}+m^2)\,\pi^{(n)}_{i,\,i-1,\,j,\,j-1} - \frac{1}{6}\sum\cdots \tag{62}$$

and $f^{(k)}_{\nu_i\,\nu_{i-1}\,\nu_j\,\nu_{j-1}}\cdots$ is some function of the $\delta_{4\,\nu_i}$ and $\delta_{\nu_i\,\nu_{i-1}}$. As will be shown in the appendix, f is actually a tensor and equal to

$$f^{(k)}_{\nu_i\,\nu_{i-1}\,\nu_j\,\nu_{j-1}\,\cdots} = \delta_{\nu_i\,\nu_{i-1}}\,\delta_{\nu_i\,\nu_{j-1}}\cdots \tag{63}$$

The formulae (62) and (63) express the lorentz invariance of equation (61).

To prove the gauge invariance (supposing the integrals in (61) to converge) we need an identity of the same type as equation (27). We have

$$\overline{P}^{(n)}_{\mu\,\nu_1\cdots\nu_n}(pp'\cdots p^n)\,(p'_{\nu_1}-p_{\nu_1}) = (p'^2-p^2)\times$$
$$\times\left[\pi^{(n)}_1 - \sum_i \delta_{\nu_i\,\nu_{i-1}}(p^{i-1\,2}+m^2)\,\pi^{(n)}_{1,\,i,\,i-1}+\cdots\right]-(p^2+m^2)\times$$
$$\times\,(p'_\mu-p_\mu)\left[\pi^{(n)}_{1,0}-\sum_i \delta_{\nu_i\,\nu_{i-1}}(p^{i-1\,2}+m^2)\,\pi^{(n)}_{1,\,0,\,i,\,i-1}+\cdots\right]-$$
$$-(p'^2+m^2)\,(p'_{\nu_2}-p_{\nu_2})\left[\pi^{(n)}_{1,2}-\sum_i \delta_{\nu_i\,\nu_{i-1}}(p^{i-1\,2}+m^2)\times\right.$$
$$\left.\times\,\pi^{(n)}_{1,\,2,\,i,\,i-1}+\cdots\right] = (p'^2+m^2)\,\overline{P}^{(n-1)}_{\mu\nu_2\cdots\nu_n}(p\,p''\cdots p^n)-$$
$$-(p^2+m^2)\,\overline{P}^{(n-1)}_{\mu\,\nu_2\cdots\nu_n}(p'\,p''\cdots p^n)\,. \tag{64}$$

We can now repeat the calculation from equation (29) to equation (35) but start from (61) instead of (25) and use (64) instaed of (27). The result is obviously that, from this formal point of view, (61) is gauge invariant.

Writing (61) as (n odd)

$$\langle j_\mu(x)^{(n)} \rangle = \frac{1}{2}(-1)^{\frac{n+1}{2}} \left(\frac{1}{2\pi}\right)^{4n+3} \int \cdots \int dq' \cdots dq^n \, e^{-ix(q'+\cdots+q^n)} \times \times \overline{K}^{(n)}_{\mu\nu_1\cdots\nu_n}(q'\cdots q^n) A_{\nu_1}(q) \cdots A_{\nu_n}(q^n) \quad (65)$$

we have

$$\overline{K}^{(n)}_{\mu\nu_1\cdots\nu_n}(q'\cdots q^n) = \left(-\frac{1}{2}\right)^{\frac{n(n+1)}{2}} \int_{-1}^{+1} du_1 (1-u_1)^{n-1} \cdots \int_{-1}^{+1} du_n \times \times \int dq \, \delta^{(n)}(q^2+m^2+\varphi) \, \overline{P}^{(n)}_{\mu\nu_1\cdots\nu_n}(q+a_1, q+a_2, \cdots) \quad (66)$$

(compare equation (44)).

Formula (66) converges or diverges as

$$\int dq \, \delta^{(n)}(q^2+\alpha^2) \, q_\mu q_{\nu_1} \cdots q_{\nu_n} \quad (67)$$

which is the same equation as (45). We thus get exactly the same convergent cases as for the spinor field.

The divergent term of (66) in the case $n=3$ may according to (62) and (46) be written

$$\frac{1}{64}\int_{-1}^{+1} du_1 (1-u_1)^2 \int_{-1}^{+1} du_2 (1-u_2) \int_{-1}^{+1} du_3 \int_{-\infty}^{+\infty} x^3 \, dx \int dq \, e^{ix(q^2+m^2+\varphi)} \times \times \{16\, q_\mu q_{\nu_1} q_{\nu_2} q_{\nu_3} - 4q^2(\delta_{\mu\nu_3} q_{\nu_1} q_{\nu_2} + \delta_{\nu_1\mu} q_{\nu_2} q_{\nu_3} + \delta_{\nu_2\nu_1} q_{\nu_3} q_\mu + + \delta_{\nu_3\nu_2} q_{\nu_1} q_\mu) + q^4(\delta_{\mu\nu_3}\delta_{\nu_1\nu_2} + \delta_{\nu_3\nu_2}\delta_{\mu\nu_1})\}. \quad (68)$$

Let us now consider a mixture of N scalar fields with masses M_i and n spinor fields with masses m_i. From equations (50), (51) and (68) we get the divergent term in the kernel of the e^4 approximation (after a suitable symmetrization of (51))

$$\frac{1}{64}\int_{-1}^{+1} du_1 (1-u_1)^2 \int_{-1}^{+1} du_2 (1-u_2) \int_{-1}^{+1} du_3 \int_{-\infty}^{+\infty} x^3 \, dx \left[\left\{2\sum_{i=1}^{n} e^{ixm_i^2} - \sum_{i=1}^{N} e^{ixM_i^2}\right\} \times \times \int dq \, e^{ixq^2}\{16\, q_\mu q_{\nu_1} q_{\nu_2} q_{\nu_3} - 4q^2(\delta_{\mu\nu_3} q_{\nu_1} q_{\nu_2} + \delta_{\nu_1\mu} q_{\nu_2} q_{\nu_3} + + \delta_{\nu_2\nu_1} q_\mu q_{\nu_3} + \delta_{\nu_3\nu_2} q_{\nu_1} q_\mu) + q^4(\delta_{\mu\nu_3}\delta_{\nu_1\nu_2} + \delta_{\nu_3\nu_2}\delta_{\mu\nu_1})\} + + (e^{ix\varphi}-1) \times \left\{\sum_{i=1}^{n} e^{ixm_i^2} Sp[\gamma_\mu \gamma q \gamma_{\nu_1} \gamma q \gamma_{\nu_2} \gamma q \gamma_{\nu_3} \gamma q] - - \sum_{i=1}^{N} e^{ixM_i^2}\Big(\pi^{(3)}(qqqq) - q^2 \sum_i \delta_{\nu_i \nu_{i-1}} \times \times \pi^{(3)}_{i,i-1}(qq) + \frac{1}{2}\sum_{i\neq j} \delta_{\nu_i\nu_{i-1}} \delta_{\nu_j\nu_{j-1}}\Big).\right\}\right] \quad (69)$$

The last term in (69) is convergent as $e^{i\varphi x}-1$ vanishes linear at the origin. The first term can be made convergent too by the aid of the assumption that

$$\lim_{x\to 0}\frac{1}{x}\left[2\sum_{i=1}^{n} e^{i x M_i^2}-\sum_{i=1}^{N} e^{i x M_i^2}\right] \tag{70}$$

is finite. This is the case if

$$2\,n = N \tag{71}$$

which is one of the conditions of JOST and RAYSKI. Their other condition

$$2\sum_{i=1}^{n} m_i^2 = \sum_{i=1}^{N} M_i^2 \tag{72}$$

makes (70) vanish, but this assumption is not needed to make (69) converge.

The symmetrisation of (51) that is necessary to get (69) is certainly allowed. In fact, the expressions given by (7) can always be made symmetric in the variables and the whole calculation can be carried through in this way. We have purpously destroyed this symmetry (equations (22) and (55)) to get formulae, which are more easy to handle.

All the integrals appearing in the kernel of the e^4 approximatior are now convergent and hence our formal proof of the gauge invariance may be applied in this case too. The only remaining di vergent (but actually gauge invariant) expression in the phenomen is the charge renormalisation term in the e^2 approximation.

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Appendix.

Proof of equation (23).

From the formulae (17), (20) and (21) the symbols $E_k^{(n)}$ in (22) may be computed. The result is (for n an odd number)

$$E^{(n)}_{(n+1-k)} = (-1)^k [\varepsilon(0\,n)\,\varepsilon(n,\,n-1)\cdots\varepsilon(n-k,\,1)\,\varepsilon(12)\,\varepsilon(23)\cdots \\ \cdots\varepsilon(n-k-2,\,n-k-1) + \varepsilon(0\,n)\,\varepsilon(n,n-1)\cdots \\ \cdots\varepsilon(n-k+1,\,1)\,\varepsilon(1,\,n-k)\,\varepsilon(n-k,\,2)\,\varepsilon(23)\cdots \\ \cdots\varepsilon(n-k-2,\,n-k-1)+\cdots] \tag{73}$$

Denoting

$$(-1)^k [E^{(n)}_{(n+1-k)} + \varepsilon(01)\,\varepsilon(12)\cdots\varepsilon(n\text{-}k\text{-}1,\,n\text{-}k)\,\varepsilon(n\text{-}k\text{+}1,\,n\text{-}k\text{+}2)\cdots \\ \cdots\varepsilon(n-1,n)\;\;\varepsilon(n\,0)]$$

with

$$S^{(n)\,(k)}(01\cdots n-k-1;\quad n-k\cdots n)$$

we get from formula (73) the following recursion formulae

$$(k \neq 0)\; S^{(n)\,(k)}(01\cdots n-k-1;\; n-k\cdots n) = \varepsilon(0\,n)\,S^{(n-1)\,(k-1)}(n\,12\cdots \\ \cdots n-k-1;\; n-k\cdots n-1) + \varepsilon(01)\,\varepsilon(1n)\,S^{(n-2)\,(k-1)}(n\,23\cdots \\ \cdots n-k-1;\; n-k\cdots n-1) + \cdots + \varepsilon(01)\,\varepsilon(12)\cdots \\ \cdots\varepsilon(n-k-2,\,n)\,S^{(k+1)\,(k-1)}(n\,n-k-1;\; n-k\cdots n-1) + \\ + (-1)^k\,\varepsilon(n-k,\,n-k+1)\cdots\varepsilon(n-1,\,n)\times \\ \times S^{(n-k)\,(0)}(012\cdots n-k-1;\,n) \tag{74}$$

and

$$S^{(n)\,(0)}(01\cdots n-1,\,n) = \varepsilon(01)\,S^{(n-1)\,(0)}(12\cdots n-1;\,n) + \varepsilon(12)\cdots \\ \cdots\varepsilon(n-2,\,n-1)\,S^{(n-k)\,(0)}(012\cdots n-k-1;\;n) \tag{75}$$

From the identity

$$S^{(2)\,(0)}(01;\,2) = \varepsilon(02)\,\varepsilon(21) + \varepsilon(01)\,\varepsilon(12) + \varepsilon(01)\,\varepsilon(20) = -1 \tag{76}$$

and equation (75) it is imidiately seen, that $S^{(n)\,(0)}$ can be expressed as a sum of terms, each of which do not contain more than n-2 factors ε. From (74) the same statement is seen to be true also for the general expression $S^{(n)\,(k)}$. Using the property that no real pairs can be created, we now get (23) from (22).

Proof of equation (63).

The symbol $\overline{P}^{(n)}_{\mu\nu_1\cdots\nu_n}$ of equation (62) can be computed from equations (54)—(60). The result is (for n odd)

$$\overline{P}^{(n)}_{\mu\nu_1\cdots\nu_n} = \pi^{(n)} - \sum_i \delta_{4\,\nu_i}\,\delta_{4\,\nu_{i-1}}\,(p^{i-1^2}+m^2)\Big[\pi^{(n)}_{i,\,i-1} -$$
$$-\frac{1}{2}\sum_j \delta_{4\,\nu_j}\,\delta_{4\,\nu_{j-1}}\,(p^{j-1^2}+m^2)\left[\pi^{(n)}_{i,\,i-1,\,j,\,j-1} - \frac{1}{3}\sum[\cdots]\right]\Big] +$$
$$+\sum_i \xi(i,\,i-1)\,(p^{i-1^2}+m^2)\Big[\pi^{(n)}_{i,\,i-1} - \sum_j \delta_{4\,\nu_j}\,\delta_{4\,\nu_{j-1}}\,(p^{j-1^2}+m^2)\times$$
$$\times\left[\pi^{(n)}_{i,\,i-1,\,j,\,j-1} - \frac{1}{2}\sum[\cdots]\right]\Big] + \frac{1}{2!}\sum_{i,j}\xi(i,\,i-1)\,\xi(j,\,j-1)\times$$
$$\times(p^{i-1^2}+m^2)\,(p^{j-1^2}+m^2)\left[\pi^{(n)}_{i,\,i-1,\,j,\,j-1} - \sum[\cdots]\right] +$$
$$+\frac{1}{3!}\sum[\cdots] + \cdots \tag{77}$$

We here use the definition (3c) of $\xi\,(ij)$ and write down the coefficient for a term consisting of $2\,l$ factors $\delta_{4\,\nu_i}\;\delta_{4\,\nu_{i-1}}$ and λ factors $\delta_{\nu_i\,\nu_{i-1}}$. This coefficient is

$$\frac{(-1)^\lambda}{\lambda!}\sum_{s=0}^{l}\frac{(-1)^s}{s!\,(l-s)!} = \begin{cases} \dfrac{(-1)^\lambda}{\lambda!}\,\dfrac{(1-1)^l}{l!} = 0 & l \neq 0 \\[2ex] \dfrac{(-1)^\lambda}{\lambda!} & l = 0 \end{cases} \tag{78}$$

The only non-vanishing terms in (77) thus consist only of factors $\delta_{\nu_i\,\nu_{i-1}}$ with coefficients given by (78). This is formula (63).

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Paper [1950b]: Mass and charge-renormalizations in quantum electrodynamics without the use of the interaction representation

Arkiv för Fysik 2 (1950) 187

In December 1949, Källén wrote to Pauli that he has found a way to simplify certain calculations in quantum electrodynamics by going to the Heisenberg representation while previous calculations had been done in the interaction representation. This is what Christian Møller calls Källén-Yang-Feldman formalism (see Chap. 65 and 71). In his subsequent papers, Källén made an extensive use of this new approach.

C. Jarlskog (Ed.), *Portrait of Gunnar Källén*, DOI 10.1007/978-3-319-00627-7_88,

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Mass- and charge-renormalizations in quantum electrodynamics without use of the interaction representation

By G. Källén

Summary

The differential equations of quantum electrodynamics are solved in a few special cases without use of the interaction representation of Tomonaga and Schwinger. The results agree with those obtained earlier and the calculations are, at least in the problems treated here, somewhat simpler than before. It may thus be pointed out that the mass- and charge-renormalizations can be carried out without any reference to a system of space-like surfaces. This is especially convenient when particles of integer spin are present, as the interaction operator of Schwinger then contains the normal unit vectors of these surfaces. The end formulae, however, must be independent of the normals.

I. Introduction

During the last years considerable progress in the field of quantum electrodynamics has been made due to the conceptions of mass- and charge-renormalizations. With the use of these ideas it has been possible to separate the divergent parts of cross-sections, energy-level shifts and so on, and to extract physically significant results from the remaining terms. As far as those formulae have been compared with experiments the agreement is good. In all the calculations referred to above, the differential equations of quantum electrodynamics have been solved in two steps. The electromagnetic and the electronic field operators, which are originally given in a Heisenberg representation, are first transformed to an intermediate representation (the interaction representation) where the field equations are the same as those for the free fields.[1] The remaining time dependence is taken over by the state vector $|\psi\rangle$, the variation of which is given by

$$i\frac{\delta|\psi\rangle}{\delta\,\sigma(x)} = H(x)\,|\psi\rangle. \tag{1}$$

The solution of equation (1) is the second step in the calculation. The usual procedure is to transform the state vector by a series of canonical transformations,

[1] J. Schwinger: Phys. Rev. *74*, 1439 (1948).

until the right-hand side of the equation of motion is of a sufficiently small order of magnitude and can be neglected. The new state vector is then a constant — that is, one has returned to the original Heisenberg representation. An arbitrary operator F is then given as a power series in the coupling constant e

$$F = F^{(0)} + e\, F^{(1)} + e^2\, F^{(2)} + \cdots \tag{2}$$

where the $F^{(i)}$ are built up of commutators and ε-functions or of P-symbols.[2]

It might be of interest to investigate, if it is possible to treat the same equations without using the interaction representation. In the calculations below we will retain the Heisenberg representation and substitute the expansions (2) directly into the original equations.[3] The coefficients of different powers of e will then be put equal to zero, and the formulae obtained in this way will be shown to be identical with those of SCHWINGER.

II. Vacuum polarization in an external field for particles of spin $\frac{1}{2}$

We begin with this simple problem where the only operator equation is

$$\left(\gamma \frac{\partial}{\partial x} + m\right) \psi(x) = i\, e\, \gamma\, A(x)\, \psi(x). \tag{3}$$

The potentials $A_\nu(x)$ are here given functions of space and time and no operators. We put

$$\psi(x) = \psi^{(0)}(x) + e\, \psi^{(1)}(x) + \cdots \tag{4}$$

and obtain from (3)

$$\left(\gamma \frac{\partial}{\partial x} + m\right) \psi^{(n+1)}(x) = i\, \gamma\, A(x)\, \psi^{(n)}(x) \tag{5 a}$$

$$\left(\gamma \frac{\partial}{\partial x} + m\right) \psi^{(0)}(x) = 0. \tag{5 b}$$

The operators $\psi^{(0)}(x)$ here correspond to the operators $\psi(x)$ of SCHWINGER and the same argument as used by him will give the formulae[1,4]

$$\{\bar{\psi}^{(0)}_{\alpha}(x),\, \psi^{(0)}_{\beta}(x')\} = -\, i\, S_{\beta\alpha}(x' - x) \tag{6 a}$$

$$\langle [\bar{\psi}^{(0)}_{\alpha}(x),\, \psi^{(0)}_{\beta}(x')] \rangle_0 = S^{(1)}_{\beta\alpha}(x' - x). \tag{6 b}$$

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[2] F. J. DYSON: Phys. Rev. *75*, 486 (1949).

[3] A similar formalism has been developed independently by C. N. YANG, Princeton, and I should like to take this opportunity of thanking him for sending me his unpublished results.

[4] J. SCHWINGER: Phys. Rev. *75*, 651 (1949).

As boundary conditions for the equation (5 a) we use ($n \neq 0$)

$$\psi^{(n)}(x) \to 0 \quad \text{for} \quad x_0 \to -\infty \tag{7 a}$$

$$\psi^{(n)}(x) \,|\, \psi \rangle \to 0 \quad \text{for} \quad x_0 \to +\infty. \tag{7 b}$$

The last equation expresses the absence of real effects and is, in fact, a condition for the given external field. Equation (7 a) is then the important boundary condition for the operators. With the aid of the formulae

$$\left(\gamma \frac{\partial}{\partial x} + m\right) S(x) = 0 \tag{8 a}$$

$$\left(\gamma \frac{\partial}{\partial x} + m\right) \bar{S}(x) = -\delta(x) \tag{8 b}$$

we now obtain from (5 a) a recursion formula for $\psi^{(n)}(x)$

$$\psi^{(n+1)}(x) = -i \int S_R(x-x')\,\gamma A(x')\,\psi^{(n)}(x')\,dx' \tag{9 a}$$

$$S_R(x) = \bar{S}(x) - \tfrac{1}{2} S(x). \tag{9 b}$$

From (9 b) it is immediately seen that (7 a) is fulfilled. Equation (7 b) gives

$$\int S(x-x')\,\gamma A(x')\,\psi^{(n)}(x')\,dx' \,|\, \psi \rangle = 0 \tag{10}$$

and hence we always get the correct *expectation values* if we use

$$\psi^{(n+1)}(x) = -i \int \bar{S}(x-x')\,\gamma A(x')\,\psi^{(n)}(x')\,dx' \tag{9 c}$$

as the solution of (5 a). From (9 c) we obtain without difficulty

$$\psi^{(n)}(x) = (-i)^n \int \cdots \int dx' \cdots dx^n\, \bar{S}(x-x')\,\gamma A(x')\,\bar{S}(x'-x'') \cdot \\ \cdot \gamma A(x'') \cdots \bar{S}(x^{n-1}-x^n)\,\gamma A(x^n)\,\psi^{(0)}(x^n). \tag{11}$$

Writing the expectation value of the current operator as

$$\langle j_\mu(x) \rangle_0 = \sum_{n=0}^{\infty} e^{n+1} \langle j_\mu^{(n)}(x) \rangle_0 \tag{12}$$

we get

$$\langle j_\mu^{(n)}(x) \rangle_0 = \frac{(-i)^{n-1}}{2} \sum_{m=0}^{n} \int \cdots \int dx' \cdots dx^n\, Sp\,[\gamma_\mu \bar{S}(x-x')\,\gamma A(x') \cdots \\ \cdots \gamma A(x^m)\,S^{(1)}(x^m - x^{m+1})\,\gamma A(x^{m+1}) \cdots \gamma A(x^n)\,\bar{S}(x^n - x)]. \tag{13}$$

This formula has earlier been obtained by the author using the original formulation of SCHWINGER.[5]

[5] G. KÄLLÉN: Helv. Phys. Acta 22, 637 (1949).

III. Vacuum polarization in an external field for particles of spin 0

In this case the differential equation for the operator is

$$(\square - m^2)\,\varphi(x) = i\,e\left[A_\nu(x)\frac{\partial\,\varphi(x)}{\partial\,x_\nu} + \frac{\partial}{\partial\,x_\nu}(A_\nu(x)\,\varphi(x))\right] + e^2 A_\nu(x)\,A_\nu(x)\,\varphi(x). \quad (14)$$

Putting

$$\varphi(x) = \sum e^n\,\varphi^{(n)}(x) \quad (15)$$

as before, we get

$$\varphi^{(n+1)}(x) = -\,i\int A_\nu(x')\left[\overline{\triangle}(x-x')\frac{\partial}{\partial\,x'_\nu} + \frac{\partial\,\overline{\triangle}(x-x')}{\partial\,x_\nu}\right]\varphi^{(n)}(x')\,d\,x' +$$

$$+\,(-\,i)^2\int\int A_{\nu_1}(x')\,A_{\nu_2}(x'')\,\overline{\triangle}(x-x')\,\delta_{\nu_1\nu_2}\,\delta(x'-x'')\,\varphi^{(n-1)}(x'')\,d\,x'\,d\,x'' \quad (16)$$

and hence

$$\varphi^{(n)}(x) = (-\,i)^n\int\cdots\int d\,x'\cdots d\,x^n\,A_{\nu_1}(x')\cdots A_{\nu_n}(x^n)\left\{\left[\overline{\triangle}(x-x')\frac{\partial}{\partial\,x'_{\nu_1}} + \right.\right.$$

$$\left.+\,\frac{\partial\,\overline{\triangle}(x-x')}{\partial\,x_{\nu_1}}\right]\left[\overline{\triangle}(x'-x'')\frac{\partial}{\partial\,x''_{\nu_2}} + \frac{\partial\,\overline{\triangle}(x'-x'')}{\partial\,x'_{\nu_2}}\right]\cdots\left[\overline{\triangle}(x^{n-1}-x^n)\frac{\partial}{\partial\,x^n_{\nu_n}} + \right.$$

$$\left.+\,\frac{\partial\,\overline{\triangle}(x^{n-1}-x^n)}{\partial\,x^{n-1}_{\nu_n}}\right] + \sum_{i=1}^{n-1}\left[\overline{\triangle}(x-x')\frac{\partial}{\partial\,x'_{\nu_1}} + \frac{\partial\,\overline{\triangle}(x-x')}{\partial\,x_{\nu_1}}\right]\cdots$$

$$\cdots\left[\overline{\triangle}(x^{i-2}-x^{i-1})\frac{\partial}{\partial x^{i-1}_{\nu_{i-1}}} + \frac{\partial\,\overline{\triangle}(x^{i-2}-x^{i-1})}{\partial\,x^{i-2}_{\nu_{i-1}}}\right]\overline{\triangle}(x^{i-1}-x^i)\,\delta_{\nu_i\nu_{i+1}}\,\delta(x^i-x^{i+1})\,\cdot$$

$$\cdot\left[\overline{\triangle}(x^{i+1}-x^{i+2})\frac{\partial}{\partial x^{i+2}_{\nu_{i+2}}} + \frac{\partial\,\overline{\triangle}(x^{i+1}-x^{i+2})}{\partial\,x^{i+1}_{\nu_{i+2}}}\right]\cdots$$

$$\cdots\left[\overline{\triangle}(x^{n-1}-x^n)\frac{\partial}{\partial\,x^n_{\nu_n}} + \frac{\partial\,\overline{\triangle}(x^{n-1}-x^n)}{\partial\,x^{n-1}_{\nu_n}}\right] +$$

$$+\sum_{i<j}\cdots\overline{\triangle}(x^{i-1}-x^i)\,\delta_{\nu_i\nu_{i+1}}\,\delta(x^i-x^{i+1})\cdots\overline{\triangle}(x^{j-1}-x^j)\,\delta_{\nu_j\nu_{j+1}}\,\delta(x^j-x^{j+1})\cdots$$

$$\left.+\sum_{i<j<k}\cdots+\cdots\right\}\varphi^{(0)}(x^n). \quad (17)$$

Rearranging the terms in the hermitian conjugate of (17) according to the scheme

$$\left(f(x)\frac{d}{d\,x} - \frac{d\,f(x)}{d\,x}\right)g(x) = -\left(g(x)\frac{d}{d\,x} - \frac{d\,g(x)}{d\,x}\right)f(x)$$

we obtain

$$\varphi^{(n)*}(x) = (-i)^n \int \cdots \int dx' \cdots dx^n A_{\nu_n}(x^n) \cdots A_{\nu_1}(x') \left\{ \left[\varphi^{(0)*}(x^n) \frac{\partial}{\partial x^n_{\nu_n}} - \right.\right.$$

$$\left. - \frac{\partial \varphi^{(0)*}(x^n)}{\partial x^n_{\nu_n}} \right] \left[\overline{\triangle}(x^n - x^{n-1}) \frac{\partial}{\partial x^{n-1}_{\nu_{n-1}}} + \frac{\partial \overline{\triangle}(x^n - x^{n-1})}{\partial x^n_{\nu_{n-1}}} \right] \cdots$$

$$\left. \cdots \left[\overline{\triangle}(x'' - x') \frac{\partial}{\partial x'_{\nu_1}} + \frac{\partial \overline{\triangle}(x'' - x')}{\partial x''_{\nu_1}} \right] \overline{\triangle}(x' - x) + \sum_i \cdots + \cdots \right\}. \tag{18}$$

Writing, for convenience, the current operator as

$$j_\mu(x) = \frac{ie}{2} \left(\left\{ \frac{\partial \varphi^*(x)}{\partial x_\mu}, \varphi(x) \right\} - \left\{ \frac{\partial \varphi(x)}{\partial x_\mu}, \varphi^*(x) \right\} \right) -$$

$$- e^2 A_\mu(x) \{\varphi^*(x), \varphi(x)\} = \sum e^{n+1} j_\mu^{(n)}(x) \tag{19}$$

we get in momentum space

$$\langle j_\mu^{(n)}(x) \rangle_0 = \frac{1}{2} \left(\frac{1}{2\pi} \right)^{4n+3} \int \cdots \int dp\, dp' \cdots dp^n e^{ix(p-p^n)} A_{\nu_1}(p'-p) \cdots$$

$$\cdots A_{\nu_n}(p^n - p^{n-1}) \cdot P^{(n)}_{\mu \nu_1 \ldots \nu_n} \cdot \left\{ \frac{\delta(p^2 + m^2)}{(p'^2 - p^2) \cdots (p^{n^2} - p^2)} + \cdots \right.$$

$$\left. \cdots + \frac{\delta(p^{n^2} + m^2)}{(p^2 - p^{n^2}) \cdots (p^{n-1^2} - p^{n^2})} \right\} \tag{20}$$

where

$$P^{(n)}_{\mu \nu_1 \ldots \nu_n} = \pi^{(n)} - \sum_i \delta_{\nu_i \nu_{i+1}} (p^{i^2} + m^2) \pi^{(n)}_{i, i+1} + \sum_{i<j} \delta_{\nu_i \nu_{i+1}} \cdot$$

$$\cdot \delta_{\nu_j \nu_{j+1}} (p^{i^2} + m^2)(p^{j^2} + m^2) \pi^{(n)}_{i, i+1, j, j+1} - \sum \cdots + \cdots \tag{21}$$

and

$$\pi^{(n)} = (p'_{\nu_1} + p_{\nu_1})(p''_{\nu_2} + p'_{\nu_2}) \cdots (p^n_{\nu_n} + p^{n-1}_{\nu_n})(p_\mu + p^n_\mu) \tag{22 a}$$

$$\pi^{(n)}_{i, j, \ldots} = \frac{\pi^{(n)}}{(p^i_{\nu_i} + p^{i-1}_{\nu_i})(p^j_{\nu_j} + p^{j-1}_{\nu_j}) \cdots}. \tag{22 b}$$

These formulae have also been obtained earlier with the formalism of SCHWINGER.[5] However, the calculations are here simpler, because we have no trouble with the normals of the space-like surfaces of TOMONAGA and SCHWINGER.

IV. The first radiative corrections to the current operator and the mass-renormalization for particles of spin $\frac{1}{2}$

We now turn to the more complicated problem of the radiative corrections to the current operator for particles of spin $\frac{1}{2}$. In this case both the electron field and the electromagnetic field are operators and we write their equations of motion as

$$\left(\gamma \frac{\partial}{\partial x} + m\right) \psi(x) = \frac{i e}{2} \{A_\nu(x), \gamma_\nu \psi(x)\} + \delta m \, \psi(x) \tag{23}$$

$$\Box A_\mu(x) = -\frac{i e}{2} [\bar{\psi}(x), \gamma_\mu \psi(x)]. \tag{24}$$

The constant m in equation (23) is the experimental mass of the particle and the singular functions, which appear below, must be constructed with this mass. The δm of the right-hand side of (23) must, if possible, be determined in such a way that no divergences appear in the end formulae (except charge-renormalization factors). As before we expand $\psi(x)$, $A_\nu(x)$ and δm in powers of e, and obtain

$$\left(\gamma \frac{\partial}{\partial x} + m\right) \psi^{(n+1)}(x) = \frac{i}{2} \sum_{m=0}^{n} \{A_\mu^{(m)}(x), \gamma_\mu \psi^{(n-m)}(x)\} + \sum_{m=1}^{n+1} \delta m^{(m)} \psi^{(n+1-m)}(x) \tag{25}$$

$$\Box A_\mu^{(n+1)}(x) = -\frac{i}{2} \sum_{m=0}^{n} [\bar{\psi}^{(m)}(x), \gamma_\mu \psi^{(n-m)}(x)]. \tag{26}$$

The first approximations are

$$\psi^{(1)}(x) = -i \int \bar{S}(x-x') \gamma A^{(0)}(x') \psi^{(0)}(x') \, dx' - \delta m^{(1)} \int \bar{S}(x-x') \psi^{(0)}(x') \, dx' \tag{27}$$

$$A_\mu^{(1)}(x) = \frac{i}{2} \int \bar{D}(x-x') [\bar{\psi}^{(0)}(x'), \gamma_\mu \psi^{(0)}(x')] \, dx'. \tag{28}$$

For the corresponding term of the current operator we obtain

$$j_\mu^{(1)}(x) = \tfrac{1}{2} \int [\bar{\psi}^{(0)}(x), \gamma_\mu \bar{S}(x-x') \gamma_\nu \psi^{(0)}(x')] A_\nu^{(0)}(x') \, dx' - i \, \delta m^{(1)} \tfrac{1}{2} \cdot$$
$$\cdot \int [\bar{\psi}^{(0)}(x), \gamma_\mu \bar{S}(x-x') \psi^{(0)}(x')] \, dx' + \text{herm. conj.} \tag{29}$$

To determine $\delta m^{(1)}$ we compute the one-electron part of (29) and, due to the factor $A_\nu^{(0)}(x')$, get zero for the first term and its hermitian conjugate. Hence

$$\delta m^{(1)} = 0. \tag{30}$$

(As is easily seen from an analogous argument we also obtain, more generally

$$\delta m^{(2n+1)} = 0 \tag{30 a}$$)

In the next approximation we get

$$\psi^{(2)}(x) = -\tfrac{1}{2}\int\int \bar{S}(x-x')\,\gamma_{\nu_1}\bar{S}(x'-x'')\,\gamma_{\nu_2}\psi^{(0)}(x'')\,\{A^{(0)}_{\nu_1}(x'),\ A^{(0)}_{\nu_2}(x'')\}\,dx'\,dx'' +$$
$$+\tfrac{1}{4}\int\int \bar{S}(x-x')\,\gamma_{\nu_1}\{\psi^{(0)}(x')\,[\bar{\psi}^{(0)}(x''),\ \gamma_{\nu_2}\psi^{(0)}(x'')]\}\,\delta_{\nu_1\nu_2}\bar{D}(x'-x'')\,dx'\,dx'' -$$
$$-\delta m^{(2)}\int \bar{S}(x-x')\,\psi^{(0)}(x')\,dx'. \qquad (31)$$

As our theory is gauge invariant we can use the formula

$$\langle\{A^{(0)}_{\nu_1}(x'),\ A^{(0)}_{\nu_2}(x'')\}\rangle_0 = \delta_{\nu_1\nu_2}D^{(1)}(x'-x'') \qquad (32)$$

and obtain from (31) with the aid of (32)

$$\frac{i}{2}[\bar{\psi}^{(0)}(x),\ \gamma_\mu\psi^{(2)}(x)]_{1,0} = -\frac{i}{4}\int\int[\bar{\psi}^{(0)}(x),\ \gamma_\mu\bar{S}(x-x')\,\gamma_{\nu_1}\bar{S}(x'-x'')\,\gamma_{\nu_2}\psi^{(0)}(x'')]_1\cdot$$
$$\cdot D^{(1)}(x'-x'')\,\delta_{\nu_1\nu_2}dx'\,dx'' + \frac{i}{4}\int\int\{Sp\,[S^{(1)}(x'-x)\,\gamma_\mu\bar{S}(x-x')\,\gamma_{\nu_1}]\cdot$$
$$\cdot[\bar{\psi}^{(0)}(x''),\ \gamma_{\nu_2}\psi^{(0)}(x'')]_1 - [\bar{\psi}^{(0)}(x),\ \gamma_\mu\bar{S}(x-x')\,\gamma_{\nu_1}S^{(1)}(x'-x'')\,\gamma_{\nu_2}\psi^{(0)}(x'')]_1 -$$
$$-[\bar{\psi}^{(0)}(x''),\ \gamma_{\nu_2}S^{(1)}(x''-x)\,\gamma_\mu\bar{S}(x-x')\,\gamma_{\nu_1}\psi^{(0)}(x')]_1\}\,\delta_{\nu_1\nu_2}\bar{D}(x'-x'')\,dx'\,dx'' -$$
$$-\frac{i}{2}\delta m^{(2)}\int[\bar{\psi}^{(0)}(x),\ \gamma_\mu\bar{S}(x-x')\,\psi^{(0)}(x')]_1\,dx'. \qquad (33)$$

Further

$$\frac{i}{2}[\bar{\psi}^{(1)}(x),\ \gamma_\mu\psi^{(1)}(x)]_{1,0} = -$$
$$-\frac{i}{4}\int\int[\bar{\psi}^{(0)}(x''),\gamma_{\nu_2}\bar{S}(x''-x)\,\gamma_\mu\bar{S}(x-x')\,\gamma_{\nu_1}\psi^{(0)}(x')]_1\,\delta_{\nu_1\nu_2}D^{(1)}(x'-x'')\,dx'\,dx'' \qquad (34)$$

and we obtain the second order radiative correction to the current operator in the one-particle case as

$$\langle j^{(2)}_\mu(x)\rangle_{1,0} = \frac{i}{2}\int[\bar{\psi}^{(0)}(x),\ \gamma_\mu\bar{S}(x-x')\,(\phi(x')-\delta m^{(2)}\psi^{(0)}(x'))]_1\,dx' +$$
$$+\frac{i}{2}\int[(\bar{\phi}(x')-\delta m^{(2)}\bar{\psi}^{(0)}(x'))\,\bar{S}(x'-x)\,\gamma_\mu,\ \psi^{(0)}(x)]_1\,dx' -$$
$$-\frac{i}{4}\int\int[\bar{\psi}^{(0)}(x'),\ K_\mu(x'-x,\ x-x'')\,\psi^{(0)}(x'')]_1\,dx'\,dx'' + \text{vac. pol.} \qquad (35)$$

$$\phi(x) = -\tfrac{1}{2}\int\gamma_\lambda[S^{(1)}(x-x')\,\bar{D}(x'-x) + \bar{S}(x-x')\,D^{(1)}(x'-x)]\,\gamma_\lambda\psi^{(0)}(x')\,dx' \qquad (36)$$

$$K_\mu(x'-x,\,x-x'') = \gamma_\lambda S^{(1)}(x'-x)\,\gamma_\mu \bar{S}(x-x'')\,\gamma_\lambda \bar{D}(x''-x') + \\ + \gamma_\lambda \bar{S}(x'-x)\,\gamma_\mu S^{(1)}(x-x'')\,\gamma_\lambda \bar{D}(x''-x') + \\ + \gamma_\lambda \bar{S}(x'-x)\,\gamma_\mu \bar{S}(x-x'')\,\gamma_\lambda D^{(1)}(x''-x'). \qquad (37)$$

These equations are the same as those obtained by SCHWINGER and used by him to compute the anomalous magnetic moment of the electron, the radiative correction to the scattering cross-section etc.[6]

It is not difficult to obtain a similar formula for the radiative correction to the current operator for particles of spin 0. Instead of the factors involving γ-matrices we get expressions of the form (21) and the numerical factors in the formula are slightly changed. In addition, of course, anticommutators must be substituted throughout for the commutators.

V. Conclusions

In the examples treated above the calculations are comparatively simple, when the formalism that has been developed here is used. This is, however, also the case in the original formulation of SCHWINGER. The higher approximations of vacuum polarization in an external field are possibly somewhat simpler here, as we have avoided the ε-functions of SCHWINGER and their rather complicated treatment.[5, 6] This is also achieved in the formalism of DYSON[2] and it remains to compare the merits of the two methods in the higher approximations of the radiative corrections to various operators. DYSON's method, however, does not avoid the complication involved in the normals of the TOMONAGA-SCHWINGER space-like surfaces.

Department of Mechanics and Mathematical Physics, University of Lund.

[6] J. SCHWINGER: Phys. Rev. *76*, 790 (1949).

Tryckt den 19 maj 1950

Uppsala 1950. Almqvist & Wiksells Boktryckeri AB

89

Paper [1950f]: Formal Integration of the Equations of Quantum Theory in the Heisenberg Representation

Arkiv för Fysik 2 (1950) 371

In this paper Källén does a detailed study of quantum electrodynamic in the Källén-Yang-Feldman formalism. His purpose is a better understanding of the new approach and comparison of the results with those in the interaction picture. He also treats the case of a spin zero particle interacting with the electromagnetic field.

While looking in the cellar of the Department of Theoretical Physics in Lund for perhaps forgotten articles by Källén , I (CJ) found an old dusty piece of paper which turned out to be Källén's personal "errata" for this paper. It is reproduced here below, after the article.

C. Jarlskog (Ed.), *Portrait of Gunnar Källén*, DOI 10.1007/978-3-319-00627-7_89,

ARKIV FÖR FYSIK Band 2 nr 37

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Formal integration of the equations of quantum theory in the Heisenberg representation

By GUNNAR KÄLLÉN

With 13 figures in the text

Summary

The differential equations of quantum electrodynamics and meson theory are formally integrated in the higher approximations without use of the interaction representation. It is first shown, that the commutator formulae of SCHWINGER and the P-symbols of DYSON satisfy the recurrence formulae in the HEISENBERG representation, and the explicit calculations needed are at least not more complicated than before. This method of integration, however, has the disadvantage of not avoiding the complications involved in the normals of the space-like surfaces, as we have to make an explicit use of the interaction Hamiltonian. The real advantage of the HEISENBERG representation is thus lost. In the second part the differential equations are therefore solved directly by being substituted into themselves. Explicit formulae are given in the simplest cases and the analysis is carried so far, that the general structure also of the more complicated expressions can be inferred. The calculations are at this point a little more delicate than the use of the $S_F(x)$ and $D_F(x)$ functions (at least for particles of spin $\frac{1}{2}$, where no surface terms appear) but give, on the other hand, an interesting insight into some of the prescriptions of the earlier method. At last the theory for particles of integer spin is also investigated, and the similarity to the case of spin $\frac{1}{2}$ is stressed. In the appendix the removal of a special form of indeterminacy in the theory is illustrated by a simple example.

1. Introduction

In a previous paper[1] the author has developed a method of solving the differential equations of quantum electrodynamics (including the renormalization of charge and mass) without using the interaction representation. In (I) a few simple examples were discussed, and it was shown that the formulae obtained agreed with those derived by SCHWINGER and others. In this paper we want to examine the formal solutions a little more closely in the higher approximations and to compare the results of the investigation with the calculations of

[1] G. KÄLLÉN, Arkiv för fysik Bd. 2, Nr. 19 (1950), here quoted as (I).

DYSON.[2,3] It will appear, that our solution is always real, and that it agrees with the formulae of DYSON, when no real processes can take place.

The method referred to above consists essentially in expanding every operator (in the HEISENBERG representation) in a power series in the coupling constant and to substitute the expressions into the differential equations. If the coefficients of every power of e is then put equal to zero, we obtain ((I) equations (25) and (26))

$$\left(\gamma \frac{\partial}{\partial x} + m\right) \psi^{(n+1)}(x) = \frac{i}{2} \sum_{m=0}^{n} \left\{A_\nu^{(m)}(x), \gamma_\nu \psi^{(n-m)}(x)\right\} + \sum_{m=2}^{n+1} \delta m^{(m)} \psi^{(n+1-m)}(x) \quad (1)$$

$$\Box A_\mu^{(n+1)}(x) = -\frac{i}{2} \sum_{m=0}^{n} [\bar{\psi}^{(m)}(x), \gamma_\mu \psi^{(n-m)}(x)]. \quad (2)$$

These equations are the starting point for the discussion given below, and we want to study their formal solution in some details.

The notations used are essentially the same as those of SCHWINGER and DYSON. We thus denote the space-coordinates by (x_1, x_2, x_3) and use $x_4 = i x_0 = i t$ as time coordinate. The four-vectors will not be distinguished in any special way, as in any case they are generally easily recognized. The scalar product between two such vectors a and b is simply written as

$$a b = a_1 b_1 + a_2 b_2 + a_3 b_3 - a_0 b_0.$$

However, if we want especially to emphasize the spatial part of some four-vector, we denote it by a bar

$$\bar{a} = (a_1, a_2, a_3) \text{ and } a b = \bar{a} \bar{b} - a_0 b_0 .$$

The four-dimensional integration element is, as long as no error may arise, simply denoted by $d x$. An expression of the form

$$\int d p \frac{e^{i p x}}{p^2 + m^2}$$

is thus to be interpreted as

$$\iiiint d p_1 d p_2 d p_3 d p_0 \frac{e^{i(p_1 x_1 + p_2 x_2 + p_3 x_3 - p_0 x_0)}}{p_1^2 + p_2^2 + p_3^2 - p_0^2 + m^2}$$

and so on.

As the diagonal elements of the metric tensor are all equal to one (and the non-diagonal equal to zero) we do not distinguish between covariant and contravariant indices.

The matrices γ_ν are the well-known ones, with four rows and four columns, fulfilling

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[2] F. J. DYSON, Phys. Rev. *75*, 486 (1949).
[3] F. J. DYSON, Phys. Rev. *75*, 1736 (1949).

$$\gamma_\nu \gamma_\mu + \gamma_\mu \gamma_\nu = 2 g_{\mu\nu} = 2 \delta_{\mu\nu}$$

and *e.g.* the expression

$$\gamma \frac{\partial}{\partial x} \psi(x)$$

has to be read as

$$\sum_{\nu, \beta} (\gamma_\nu)_{\alpha\beta} \frac{\partial \psi_\beta(x)}{\partial x_\nu}.$$

etc. The $A_\mu(x)$ operators are the electromagnetic potentials, the $\psi(x)$ operator the usual DIRAC spinor and $\psi^*(x)$ its Hermitian conjugate. The quantity $\bar{\psi}(x)$ is defined as

$$\bar{\psi}(x) = \psi^*(x)\gamma_4.$$

The commutator

$$[\bar{\psi}(x), \gamma_\nu \psi(x)]$$

stands for

$$\bar{\psi}(x)\gamma_\nu \psi(x) - \psi(x)\gamma_\nu^T \bar{\psi}(x) = \sum_{\alpha, \beta} (\gamma_\nu)_{\alpha\beta} (\bar{\psi}_\alpha(x)\psi_\beta(x) - \psi_\beta(x)\bar{\psi}_\alpha(x)).$$

The other notations are either obvious or explained in the text. Natural units ($\hbar = c = 1$) are used throughout.

2. The singular functions

In what follows we will frequently make use of the various singular functions defined by SCHWINGER and others. For the convenience of the reader a short exposé of these functions will be given here, although we have nothing essentially new to say at this point.

We begin with the function $\overline{\triangle}(x)$ defined, more or less arbitrarily, as the solution of

$$(\Box - m^2)\overline{\triangle}(x) = -\delta(x) = -\frac{1}{(2\pi)^4}\int e^{ipx}\, dp \tag{3}$$

which is given by

$$\overline{\triangle}(x) = \frac{1}{(2\pi)^4} P \int \frac{e^{ipx}\, dp}{p^2 + m^2} \tag{4}$$

(compare SCHWINGER[4], appendix). The letter P in front of the integral sign indicates, that the principal value of the integral has to be taken in the points, where the denominator vanishes. The p-integrations have to be performed in the following order. The p_0-integration is done at first, and the principal value is taken at the poles. After that, the integrations over the three-dimensional

[4] J. SCHWINGER, Phys. Rev. *75*, 651 (1949).

angles are carried through and, finally, we may then integrate over the absolute value of the three-dimensional vector $\bar{p}$.

It is now convenient to write

$$\overline{\triangle}(x) = \tfrac{1}{2}[\triangle_R(x) + \triangle_A(x)] \tag{5}$$

where the two new functions $\triangle_R(x)$ and $\triangle_A(x)$ are given by

$$\triangle_R(x) = \frac{1}{(2\pi)^4}\int_{C_R} \frac{e^{ipx}}{p^2+m^2}\,dp \tag{6}$$

$$\triangle_A(x) = \frac{1}{(2\pi)^4}\int_{C_A} \frac{e^{ipx}}{p^2+m^2}\,dp\,. \tag{7}$$

The p_0-integrations are here performed along the two complex contours shown in figure 1.

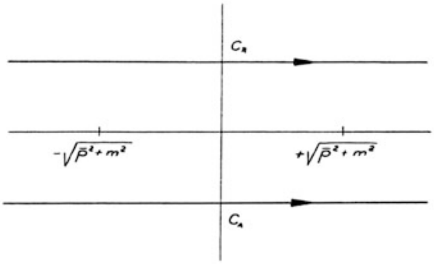

Fig. 1.

We will also make use of the functions

$$\triangle(x) = \triangle_A(x) - \triangle_R(x) = \frac{1}{(2\pi)^4}\int_{C} \frac{e^{ipx}}{p^2+m^2}\,dp \tag{8}$$

and

$$\triangle^{(1)}(x) = \frac{1}{i}\frac{1}{(2\pi)^4}\int_{C_1} \frac{e^{ipx}}{p^2+m^2}\,dp \tag{9}$$

where the paths C and C_1 are illustrated in figures 2 and 3.

The factor $\frac{1}{i}$ in (9) is inserted to secure agreement with the notations of SCHWINGER. Obviously, all these functions are not independent, and we have

$$\triangle_R(x) = \overline{\triangle}(x) - \tfrac{1}{2}\triangle(x) \tag{10}$$

$$\triangle_A(x) = \overline{\triangle}(x) + \tfrac{1}{2}\triangle(x)\,. \tag{11}$$

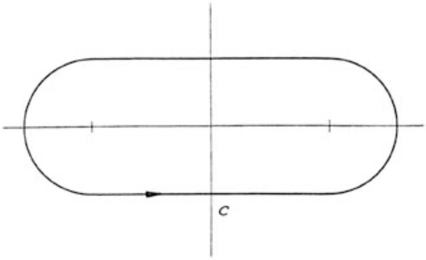

Fig. 2.

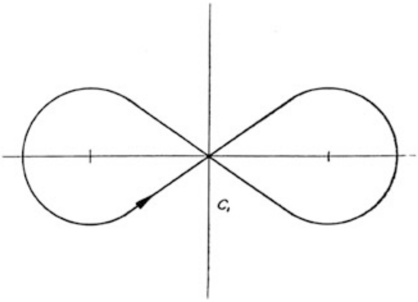

Fig. 3.

Further

$$(\Box - m^2)\,\triangle_R(x) = (\Box - m^2)\,\triangle_A(x) = -\,\delta(x) \tag{12}$$

$$(\Box - m^2)\,\triangle(x) = (\Box - m^2)\,\triangle^{(1)}(x) = 0. \tag{13}$$

Due to the factor $e^{ikx} = e^{i\bar{k}\bar{x} - ik_0x_0}$ the integral over a circle of very large radius will disappear in the upper half-plane if $x_0 < 0$ and in the lower one if $x_0 > 0$. This means that

$$\triangle_A(x) = 0 \tag{14 a}$$

$$\overline{\triangle}(x) = -\tfrac{1}{2}\,\triangle(x) \tag{14 b}$$

$$\triangle_R(x) = -\,\triangle(x) \tag{14 c}$$

if $x_0 > 0$ and

$$\triangle_R(x) = 0 \tag{15 a}$$

$$\overline{\triangle}(x) = \tfrac{1}{2}\,\triangle(x) \tag{15 b}$$

$$\triangle_A(x) = \triangle(x) \tag{15 c}$$

if $x_0 < 0$. Hence

$$\overline{\triangle}(x) = -\tfrac{1}{2}\,\varepsilon(x)\,\triangle(x) \tag{16}$$

where

$$\varepsilon(x) = \frac{x_0}{|x_0|}. \tag{16 a}$$

From the integral representations (4) and (8) we further infer, not only that

$$\triangle(-x) = -\,\triangle(x) \tag{17}$$

$$\overline{\triangle}(-x) = \overline{\triangle}(x) \tag{17 a}$$

but also that

$$\triangle(\bar{x}, -x_0) = -\,\triangle(\bar{x}, x_0). \tag{18}$$

Hence

$$\triangle(\bar{x}, 0) = 0 \tag{19}$$

which, due to reasons of invariance, can be generalized to

$$\triangle(x) = \overline{\triangle}(x) = 0, \text{ when } x^2 > 0. \tag{20}$$

Formula (20) is necessary to make equation (16) truly invariant.[4]

The integrals (8) and (9) acquire contributions only from the poles inside the contours and, as a simple investigation shows, these functions can equally well be represented by the formulae of SCHWINGER.[4]

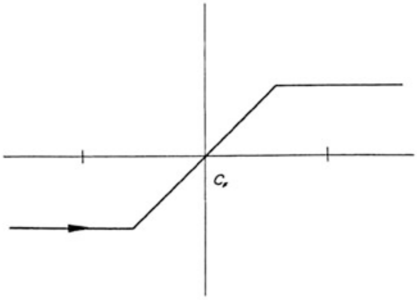

Fig. 4.

$$\triangle(x) = \frac{-i}{(2\pi)^3} \int d\,p\, e^{ipx}\, \delta(p^2 + m^2)\, \varepsilon(p) \tag{21}$$

$$\triangle^{(1)}(x) = \frac{1}{(2\pi)^3} \int d\,p\, e^{ipx}\, \delta(p^2 + m^2) \cdot \tag{22}$$

As last we also introduce the function

$$\triangle_F(x) = \frac{2}{i} \frac{1}{(2\pi)^4} \int\limits_{C_F} \frac{e^{ipx}}{p^2 + m^2}\, d\,p \tag{23}$$

which has been extensively used by FEYNMAN[5] and DYSON.[2,3]

The functions $S(x)$, $\bar{S}(x)$, ... etc. are given by

$$S(x) = \left(\gamma \frac{\partial}{\partial x} - m\right) \triangle(x)$$

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[5] R. F. FEYNMAN, Phys. Rev. *76*, 749 (1949), ibid. *76*, 769 (1949).

$$\bar{S}(x) = \left(\gamma \frac{\partial}{\partial x} - m\right) \overline{\triangle}(x)$$
$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$

and $D(x)$, $\bar{D}(x)$, ... are obtained from $\triangle(x)$, $\overline{\triangle}(x)$, ... if we put $m = 0$.

3. The solution of Schwinger

In this section we want to verify that the formal solution of SCHWINGER (compare *e.g.* DYSON[2] equation (25)) satisfies our recursion formulae (1) and (2). Although this is more or less evident, due to the fact that the two expressions have been derived from the same starting-point, it may be of some interest to consider the detailed calculations necessary. For this purpose we require a theorem, which can be stated as follows.

If $A(x)$, $B(x)$ and $H(x)$ are three operators, and if we define two sets of new operators according to the scheme

$$A^{(n)}(x) = i^n \int_{-\infty}^{x_0} d\,x' \int_{-\infty}^{x'_0} d\,x'' \ldots \int_{-\infty}^{x_0^{n-1}} d\,x^n \,[H(x^n), [\ldots [H(x'), A(x)] \ldots]] \quad (24)$$

and

$$B^{(n)}(x) = i^n \int_{-\infty}^{x_0} d\,x' \int_{-\infty}^{x'_0} d\,x'' \ldots \int_{-\infty}^{x_0^{n-1}} d\,x^n \,[H(x^n), [\ldots [H(x'), B(x)] \ldots]] \quad (25)$$

we have identically

$$(A(x)\,B(x))^{(n)} = i^n \int_{-\infty}^{x_0} d\,x' \int_{-\infty}^{x'_0} d\,x'' \ldots \int_{-\infty}^{x_0^{n-1}} d\,x^n \,[H(x^n), [\ldots [H(x'),$$
$$A(x)\,B(x)] \ldots]] = \sum_{m=0}^{n} A^{(m)}(x)\,B^{(n-m)}(x). \quad (26)$$

To prove this formula we expand the iterated commutator in the following way

$$[H(x^n), [\ldots [H(x'), A(x)\,B(x)] \ldots]] = [H(x^n), [\ldots [H(x'), A(x)] \ldots$$
$$\ldots]]\,B(x) + \sum_{i=1}^{n} [H(x^n), [\ldots [H(x^{i+1}), [H(x^{i-1}), [\ldots [H(x'), A(x)] \ldots$$
$$\ldots]]] \ldots]]\,[H(x^i), B(x)] + \sum_{i>j} [H(x^n), [\ldots [H(x^{i+1}), [H(x^{i-1}), [\ldots$$
$$\ldots [H(x^{j+1}), [H(x^{j-1}), [\ldots [H(x'), A(x)] \ldots]]] \ldots]]] \ldots]] \cdot$$
$$\cdot [H(x^i), [H(x^j), B(x)]] + \sum_{i>j>k} \ldots + \ldots \quad (27)$$

The first term in (27) gives, when integrated, directly $A^{(n)}(x)\cdot B(x)$. The second term gives a contribution of the following form

$$\int_{-\infty}^{x_0} d\,x' \int_{-\infty}^{x'_0} d\,x'' \ldots \int_{-\infty}^{x_0^{n-1}} d\,x^n \sum_{i=1}^{n} f(x^n \ldots x^{i+1}\,x^{i-1} \ldots x')\,g(x^i). \tag{28}$$

This expression, however, is equal to

$$\int_{-\infty}^{x_0} d\,x' \int_{-\infty}^{x'_0} d\,x'' \ldots \int_{-\infty}^{x_0^{n-2}} d\,x^{n-1} f(x^{n-1} \ldots x') \int_{-\infty}^{x_0} d\,x^n\, g(x^n) \tag{29}$$

which is easily shown from n to $n+1$. Supposing (28) and (29) to be equal up to some value n, we get

$$\int_{-\infty}^{x_0} d\,x' \int_{-\infty}^{x'_0} d\,x'' \ldots \int_{-\infty}^{x_0^{n}} d\,x^{n+1} \sum_{i=1}^{n+1} f(x^{n+1} \ldots x^{i+1}\,x^{i-1} \ldots x')\,g(x^i) =$$

$$= \int_{-\infty}^{x_0} d\,x^{n+1} \int_{-\infty}^{x_0^{n+1}} d\,x' \ldots \int_{-\infty}^{x_0^{n-1}} d\,x^n f(x^n \ldots x')\,g(x^{n+1}) +$$

$$+ \int_{-\infty}^{x_0} d\,x' \int_{-\infty}^{x'_0} d\,x'' \ldots \int_{-\infty}^{x_0^{n-1}} d\,x^n f(x^n \ldots x') \int_{-\infty}^{x'_0} d\,x^{n+1} g(x^{n+1}) =$$

$$= \int_{-\infty}^{x_0} d\,x' \ldots \int_{-\infty}^{x_0^{n-1}} d\,x^n f(x^n \ldots x') \int_{-\infty}^{x_0} d\,x^{n+1} g(x^{n+1}). \tag{29 a}$$

The first term in (29 a) is obtained from $i = 1$ only through a renumeration of the variables. The second will appear, if we apply our theorem (which was supposed valid up to the integer n) for the variables $x'', \ldots x^{n+1}$. As (28) and (29) are equal *e.g.* for $n = 2$, they are thus equal for all integers n.

In a quite analogous way we obtain, more generally

$$\int_{-\infty}^{x_0} d\,x' \ldots \int_{-\infty}^{x_0^{n-1}} d\,x^n \sum_{i_1 > i_2 > \cdots > i_r} f(x^n \ldots x^{i_1+1}\,x^{i_1-1} \ldots\ldots x^{i_r+1}\,x^{i_r-1} \ldots x')\,\cdot$$

$$\cdot\, g(x^{i_1} \ldots x^{i_r}) = \int_{-\infty}^{x_0} d\,x' \ldots \int_{-\infty}^{x_0^{n-r-1}} d\,x^{n-r} f(x^{n-r} \ldots x') \int_{-\infty}^{x_0} d\,x^{n-r+1} \ldots$$

$$\ldots \int_{-\infty}^{x_0^{n-1}} d\,x^n\, g(x^n \ldots x^{n-r+1}). \tag{30}$$

Equation (30) is proved by observing that if it is valid for some integer r, we have

$$\int_{-\infty}^{x_0} d x' \ldots \int_{-\infty}^{x_0^{n-1}} d x^n \sum_{i_1 > \cdots > i_{r+1}} f(x^n \ldots x^{i_1+1} x^{i_1-1} \ldots \ldots x^{i_{r+1}+1} x^{i_{r+1}-1} \ldots x') \cdot$$

$$\cdot g(x^{i_1} \ldots x^{i_{r+1}}) = \sum_{i_{r+1}=1}^{n-r} \int_{-\infty}^{x_0} d x' \ldots \int_{-\infty}^{x_0^{i_{r+1}-1}} d x^{i_{r+1}} \int_{-\infty}^{x_0^{i_{r+1}}} d x^{i_{r+1}+1} \ldots$$

$$\ldots \int_{-\infty}^{x_0^{n-r-1}} d x^{n-r} f(x^{n-r} \ldots x^{i_{r+1}+1} x^{i_{r+1}-1} \ldots x') \int_{-\infty}^{x_0^{i_{r+1}}} d x^{n-r+1} \ldots$$

$$\ldots \int_{-\infty}^{x_0^{n-1}} d x^n g(x^n \ldots x^{n-r+1} x^{i_{r+1}}). \tag{31}$$

The right-hand side of equation (31), however, is of the same form as (28), and we infer with the aid of (29) that (30) is valid also for $r+1$. For $r = 1$ formula (30) is proved earlier, and it is thus correct for all integers r. If we now integrate both sides of equation (27) over the $4n$-dimensional "tetrahedron"

$$\int_{-\infty}^{x_0} d x' \int_{-\infty}^{x'_0} d x'' \ldots \int_{-\infty}^{x_0^{n-1}} d x^n$$

we obtain from each term of the right-hand side an integral of the form (30), that is, a product of two operators $A^{(m)}(x)$ and $B^{(n-m)}(x)$. Obviously, all values of m less than n will appear, and we get

$$(A(x) B(x))^{(n)} = \sum_{m=0}^{n} A^{(m)}(x) B^{(n-m)}(x)$$

as desired.

After this preliminary calculation we have no difficulty in showing, that the expressions

$$\psi^{(n)}(x) = i^n \int_{-\infty}^{x_0} d x' \ldots \int_{-\infty}^{x_0^{n-1}} d x^n [H(x^n), [\ldots [H(x') \psi^{(0)}(x)] \ldots]] \tag{32}$$

and

$$A_\mu^{(n)}(x) = i^n \int_{-\infty}^{x_0} d x' \ldots \int_{-\infty}^{x_0^{n-1}} d x^n [H(x^n), [\ldots [H(x'), A_\mu^{(0)}(x)] \ldots]] \tag{33}$$

satisfy the recursion formulae (1) and (2), if the part containing the mass-renormalization term is dropped, and if $H(x)$ is given by

$$H(x) = - j_\nu^{(0)}(x) A_\nu^{(0)}(x). \tag{34}$$

We first consider the equation for $\psi(x)$ and obtain from

$$\int_{-\infty}^{x_0}[H(x'), \psi^{(0)}(x)]\,dx' = -\int_{-\infty}^{x_0} dx'\, A_\nu^{(0)}(x')\,\frac{i}{2}[[\bar{\psi}^{(0)}(x'), \gamma_\nu\,\psi^{(0)}(x')], \psi^{(0)}(x)] =$$

$$= \int_{-\infty}^{x_0} dx'\, S(x-x')\,\gamma\, A^{(0)}(x')\,\psi^{(0)}(x')\,dx' =$$

$$= -\int_{-\infty}^{+\infty} S_R(x-x')\,\gamma\, A^{(0)}(x')\,\psi^{(0)}(x')\,dx' \tag{35}$$

immediately

$$\left(\gamma\frac{\partial}{\partial x} + m\right)\psi^{(n+1)}(x) = i^{n+1}\int dx'\,\delta(x-x')\int_{-\infty}^{x'_0} dx'' \ldots \int_{-\infty}^{x_0^n} dx^{n+1}\,[H(x^{n+1}),$$

$$[\ldots[H(x''), A_\nu^{(0)}(x')\,\gamma_\nu\,\psi^{(0)}(x')]\ldots]] = \frac{i}{2}\sum_{m=0}^{n}\left\{A_\nu^{(m)}(x), \gamma_\nu\,\psi^{(n-m)}(x)\right\}. \tag{36}$$

The calculations for $A_\mu(x)$ are similar. We have

$$\int_{-\infty}^{x_0} dx'\,[H(x'), A_\mu^{(0)}(x)] = -\frac{i}{2}\int_{-\infty}^{x_0}[\bar{\psi}^{(0)}(x'),\gamma_\nu\,\psi^{(0)}(x')]\cdot$$

$$\cdot[A_\nu^{(0)}(x'), A_\mu^{(0)}(x)]\,dx' = -\frac{1}{2}\int_{-\infty}^{x_0} D(x-x')\,[\bar{\psi}^{(0)}(x'), \gamma_\mu\,\psi^{(0)}(x')]\,dx' =$$

$$= \frac{1}{2}\int_{-\infty}^{+\infty} D_R(x-x')\,[\bar{\psi}^{(0)}(x'), \gamma_\mu\,\psi^{(0)}(x')]\,dx' \tag{37}$$

and hence

$$\Box\, A_\mu^{(n+1)}(x) = -\frac{i^{n+1}}{2}\int_{-\infty}^{+\infty}\delta(x-x')\,dx'\int_{-\infty}^{x'_0} dx'' \ldots$$

$$\ldots\int_{-\infty}^{x_0^n} dx^{n+1}\,[H(x^{n+1}), [\ldots[H(x''), [\bar{\psi}^{(0)}(x'), \gamma_\mu\,\psi^{(0)}(x')]]\ldots]] =$$

$$= -\frac{i}{2}\sum_{m=0}^{n}[\bar{\psi}^{(m)}(x), \gamma_\mu\,\psi^{(n-m)}(x)]. \tag{38}$$

The equations (32), (33) and (26) together supply the complete formalism of SCHWINGER.

4. The S-matrix of Dyson

By starting from our equations (1) and (2), we want in this paragraph to obtain the solution given by DYSON.[2] For this purpose it is most convenient to calculate the elements of the $\boldsymbol{S}$-matrix directly. This quantity satisfies the following equations, given by YANG.[6]

$$[\boldsymbol{S}, \psi^{(0)}(x)] = -i e \boldsymbol{S} \int S(x-x') \gamma A(x') \psi(x') d x' \tag{39}$$

$$[\boldsymbol{S}, A_\mu^{(0)}(x)] = \frac{i e}{2} \boldsymbol{S} \int D(x-x') [\bar{\psi}(x'), \gamma_\mu \psi(x')] d x'. \tag{40}$$

For the derivation of these formulae the reader must be referred to a forthcoming paper by YANG and FELDMAN. Expanding all operators in a power series in e, we have

$$[\boldsymbol{S}^{(n+1)}, \psi^{(0)}(x)] = -i \sum_{m=0}^{n} \boldsymbol{S}^{(m)} \int S(x-x') \gamma_\nu (A_\nu(x') \psi(x'))^{(n-m)} d x' \tag{41}$$

$$[\boldsymbol{S}^{(n+1)}, A_\mu^{(0)}(x)] = \frac{i}{2} \sum_{m=0}^{n} \boldsymbol{S}^{(m)} \int D(x-x') [\bar{\psi}(x'), \gamma_\mu \psi(x')]^{(n-m)} d x'. \tag{42}$$

DYSON's solution is

$$\boldsymbol{S}^{(n)} = \frac{(-i)^n}{n!} \int \cdots \int d x' \ldots d x^n P(H(x^n) \ldots H(x')) \tag{43}$$

where $H(x)$ is given by (34) and the P-symbol is defined in the following way[2]

$$P(A(x) B(x')) = \begin{cases} A(x) B(x') & \text{when } x_0' > x_0 \\ B(x') A(x) & \text{when } x_0 > x_0'. \end{cases} \tag{44}$$

The generalization for more than two factors is obvious. From the definition of the P-symbol we find, without difficulty, for arbitrary operators $H(x)$ and $F(x)$

$$\int \cdots \int d x \, d x' \ldots d x^n P(H(x^n) \ldots H(x') F(x)) = \int \cdots \int d x \, d x' \ldots d x^n \cdot$$

$$\cdot P(H(x^n) \ldots H(x')) F(x) - n \int_{-\infty}^{+\infty} d x \int_{-\infty}^{x_0} d x' \left\{ \int d x'' \ldots \int d x^n P(H(x^n) \ldots \right.$$

$$\ldots H(x'')) \cdot [H(x'), F(x)] - (n-1) \int_{-\infty}^{x_0'} d x'' \left[\int d x''' \ldots \int d x^n P(H(x^n) \ldots \right.$$

[6] Compare (I) note 3 and a forthcoming paper by YANG and FELDMAN.

$$\ldots H(x''')\,[H(x''),\,[H(x'),\,F(x)]] - \cdots]\} =$$
$$= \int \cdots \int d\,x \ldots d\,x^n\, P(H(x^n) \ldots H(x'))\,F(x) -$$
$$- n \int d\,x \int\limits_{-\infty}^{x_0} d\,x' \int d\,x'' \ldots \int d\,x^n \cdot P(H(x^n) \ldots H(x''))\,[H(x'),\,F(x)] +$$
$$+ n(n-1) \int d\,x \int\limits_{-\infty}^{x_0} d\,x' \int\limits_{-\infty}^{x'_0} d\,x'' \int d\,x''' \ldots \int d\,x^n \cdot$$
$$\cdot P(H(x^n) \ldots H(x''')) \cdot [H(x''),\,[H(x'),\,F(x)]] - \cdots \tag{45}$$

If we substitute equation (43) into the left-hand side of (41) and apply (45) with $F(x) = \gamma A^{(0)}(x)\,\psi^{(0)}(x)$ we obtain, using the results of the previous section

$$\frac{(-i)^{n+1}}{(n+1)!} \int \cdots \int d\,x' \ldots d\,x^{n+1}\,(n+1)\,P(H(x^{n+1}) \ldots H(x'')\,[H(x'),\,\psi^{(0)}(x)]) =$$
$$= \frac{(-i)^{n+1}}{n!} \int \cdots \int d\,x' \ldots d\,x^{n+1}\, S(x-x')\,P(H(x^{n+1}) \ldots H(x'')\,\gamma A^{(0)}(x')\,\psi^{(0)}(x')) =$$
$$= -i \sum_{m=0}^{n} \boldsymbol{S}^{(m)} \int d\,x'\, S(x-x')\,\gamma_\nu\,(A_\nu(x')\,\psi(x'))^{(n-m)}. \tag{46}$$

Substituting (43) into (42), we obtain in the same way

$$\frac{(-i)^{n+1}}{(n+1)!} \int \cdots \int d\,x' \ldots d\,x^{n+1}\, P(H(x^{n+1}) \ldots H(x'')\,[H(x'),\,A_\mu^{(0)}(x)])\,(n+1) =$$
$$= \frac{(-i)^{n+1}}{n!}\left(-\tfrac{1}{2}\right) \int \cdots \int d\,x' \ldots d\,x^{n+1}\, D(x-x')\,P(H(x^{n+1}) \ldots H(x'')\,[\bar{\psi}^{(0)}(x'),$$
$$\gamma_\mu\,\psi^{(0)}(x')]) = \frac{i}{2} \sum_{m=0}^{n} \boldsymbol{S}^{(m)} \int d\,x'\, D(x-x')\,[\bar{\psi}(x'),\,\gamma_\mu\,\psi(x')]^{(n-m)}. \tag{47}$$

The calculations given in the last two sections verify that the integration of our equations (1) and (2) is exactly equivalent to the solution of the equation of motion in the interaction representation[7]

$$i\,\frac{\delta\,|\,\psi[\sigma]>}{\delta\,\sigma(x)} = H(x)\,|\,\psi[\sigma]>. \tag{48}$$

(The notations are explained in the work by SCHWINGER.[7]) Our problem can, of course, be considered as solved, when we have obtained the expressions (32), (33), (26) and (43). In practical calculations, however, the iterated commutators give rise to rather lengthy computations, especially concerning the ε-functions. The P-symbols of DYSON are much more simple to handle and the ε-functions

[7] J. SCHWINGER, Phys. Rev. *74*, 1439 (1948).

are there cause of considerably less trouble. Both methods, however, make use of the interaction Hamiltonian $H(x)$, and although this fact is no serious drawback as long as we are only considering particles of spin $\frac{1}{2}$, a quite different situation arises, when particles of integer spin are also treated. In this case the Hamiltonian contains terms, which are dependent of the surface σ (in the notations of SCHWINGER) and thus not truly invariant. As a matter of fact, these non-invariant terms will all drop out of the end formula but only after a long and comparatively complicated calculation.[8] The disappearance of the non-invariant terms is closely connected with the derivatives of the ε-functions, the occurrence of which in the formalisms of SCHWINGER and DYSON comes from the boundaries in the x_0-integrations. The integrals of equations (24) and (43) are effectively invariant only if the operators $H(x)$ commute on every space-like surface, and this is not the case if the interaction energy contains derivatives of the field operators. The mission of the non-invariant terms of $H(x)$ is only to counterbalance these inconvenient properties of the integrals. On the other hand, it has been shown in the paper referred to earlier[1] that, for the case of scalar particles in interaction with an electromagnetic field, it is possible to make calculations in the HEISENBERG representation and obtain invariant results directly without reference to any system of space-like surfaces. We therefore wish to solve our equations in a manner more similar to the procedure used in (I) and do not want to make use of the integral formulae above.

5. Direct integration of the differential equations of quantum electrodynamics

As has been proved earlier[1], we can construct a solution to our equations in the following way

$$\psi^{(n+1)}(x) = -\frac{i}{2}\int S_R(x-x')\sum_{m=0}\{A_\nu^{(m)}(x'),\gamma_\nu\psi^{(n-m)}(x')\}\,d x' - \\ -\int S_R(x-x')\sum_{m=2}^{n+1}\delta m^{(m)}\psi^{(n+1-m)}(x')\,d x' \qquad (49)$$

$$A_\mu^{(n+1)}(x) = \frac{i}{2}\int D_R(x-x')\,\delta_{\mu\nu}\sum_{m=0}^{n}[\psi^{(m)}(x'),\gamma_\nu\psi^{(n-m)}(x')]\,d x'. \qquad (50)$$

Equations (49) and (50) permit us to compute the $n+1^{\text{st}}$ approximations of $\psi(x)$ and $A_\mu(x)$, when the operators $A_\mu^{(0)}(x)$, $\psi^{(0)}(x)$, ... $A_\mu^{(n)}(x)$, $\psi^{(n)}(x)$ are already known. The constants $\delta m^{(m)}$ have to be determined in such a way that all remaining divergences can be interpreted as charge-renormalization factors. The mass-renormalization is thus completely absorbed in the (infinite) quantity δm. As is easily shown[1] $\delta m^{(2n+1)}$ always vanish, and the summa-

[8] P. T. MATTHEWS, Phys. Rev. *76*, 1657 (1949), ibid. *76*, 684 (1949). Compare also R. JOST—J. RAYSKI, Helv. Phys. Acta *22*, 457 (1949) and G. KÄLLÉN, Helv. Phys. Acta *22*, 637 (1949).

tion in the last integral of (49) is effectively restricted to even integers m. If n is even, m takes the values 2, 4, ... n, and if n is odd, the corresponding set will be 2, 4, ... $n + 1$. The solutions given above fulfil, from a formal point of view, the following boundary conditions

$$\left.\begin{array}{l}\lim \psi^{(n)}(x) \to 0 \\ \lim A_\mu^{(n)}(x) \to 0\end{array}\right\} \begin{array}{l} x_0 \to -\infty \\ n \neq 0.\end{array} \tag{51}$$

The incoming fields ($x_0 \to -\infty$) are thus described entirely by the terms $\psi^{(0)}(x)$ and $A_\mu^{(0)}(x)$. It is, however, also possible to construct other solutions to the differential equations, using either the advanced functions $S_A(x)$ and $D_A(x)$ or the expressions $\bar{S}(x)$ and $\bar{D}(x)$ instead of the retarded functions $S_R(x)$ and $D_R(x)$. In those cases the operators $\psi^{(0)}(x)$ and $A_\mu^{(0)}(x)$ will have another physical meaning. If the advanced functions are used, they will describe the outgoing fields ($x_0 \to +\infty$) instead of the incoming ones, and if the solutions are built up with the aid of the functions $\bar{S}(x)$ and $\bar{D}(x)$ the corresponding operators are given by half the sum of the incoming and the outgoing fields. As has been pointed out by YANG[6], both the incoming and the outgoing field operators will fulfil the usual commutation relations (equations (57) and (58) below) and are thus connected through a canonical transformation[9]

$$\psi^{(\text{out})}(x) = \boldsymbol{S}^{-1}\,\psi^{(\text{in})}(x)\,\boldsymbol{S}$$

$$A_\mu^{(\text{out})}(x) = \boldsymbol{S}^{-1}\,A_\mu^{(\text{in})}(x)\,\boldsymbol{S}.$$

The matrix $\boldsymbol{S}$ above is identical with the $\boldsymbol{S}$-matrix of HEISENBERG. If we use the singular functions $\bar{S}(x)$ and $\bar{D}(x)$, we get for the corresponding commutators

$$\begin{aligned}[A_\mu^{(0)}(x), A_\nu^{(0)}(x')] = \tfrac{1}{4}\big([A_\mu^{(\text{in})}(x), A_\nu^{(\text{in})}(x')] + [A_\mu^{(\text{out})}(x), A_\nu^{(\text{out})}(x')] +\\ + [A_\mu^{(\text{out})}(x), A_\nu^{(\text{in})}(x')] + [A_\mu^{(\text{in})}(x), A_\nu^{(\text{out})}(x')]\big).\end{aligned} \tag{52}$$

As $\boldsymbol{S}$ is unitary, half the *sum* of the two last terms in (52) is equal to the first term, and we obtain the same commutators for $A_\mu^{(0)}(x)$ as for $A_\mu^{(\text{in})}(x)$ and $A_\mu^{(\text{out})}(x)$. Hence, we also have

$$A_\mu^{(0)}(x) = \boldsymbol{Q}^{-1}\,A_\mu^{(\text{in})}(x)\,\boldsymbol{Q}.$$

A similar argument gives the corresponding connection between $\psi^{(0)}(x)$ and $\psi^{(\text{in})}(x)$. From the equations

$$A_\mu^{(0)}(x) = A_\mu^{(\text{in})}(x) + \frac{i}{4}\int D(x-x')\,[\bar{\psi}(x'), \gamma_\mu\,\psi(x')]\,dx'$$

[9] Compare P. A. M. DIRAC, The Principles of Quantum Mechanics, third edition p. 106. (Oxford 1947).

$$A_\mu^{(\text{out})}(x) = A_\mu^{(0)}(x) + \frac{i}{4}\int D(x-x')\,[\bar\psi(x'),\,\gamma_\mu\,\psi(x')]\,dx'$$

we infer, that we must also have

$$\begin{aligned} A_\mu^{(\text{out})}(x) &= \boldsymbol{Q}^{-1}\,A_\mu^{(0)}(x)\,\boldsymbol{Q} \\ \psi^{(\text{out})}(x) &= \boldsymbol{Q}^{-1}\,\psi^{(0)}(x)\,\boldsymbol{Q} \end{aligned} \qquad (53\text{ a})$$

with the same $\boldsymbol{Q}$ in (53) and (53 a). We thus obtain[10]

$$\boldsymbol{Q}^2 = \boldsymbol{S}. \qquad (54)$$

If no real processes can take place, the $\boldsymbol{S}$-matrix and thus also the $\boldsymbol{Q}$-matrix are effectively equal to one, when we want to compute the expectation value of some operator. As long as we are only treating self-energy problems it is thus possible to simplify the solutions (49) and (50) in the following way

$$\psi^{(n+1)}(x) = -\frac{i}{2}\int \bar S(x-x')\sum_{m=0}^{n}\left\{A_\nu^{(m)}(x'),\,\gamma_\nu\,\psi^{(n-m)}(x')\right\}dx' - \int \bar S(x-x')\cdot \sum_{m=2}^{n+1}\delta m^{(m)}\,\psi^{(n+1-m)}(x')\,dx' \qquad (55)$$

$$A_\mu^{(n+1)}(x) = \frac{i}{2}\int \bar D(x-x')\,\delta_{\mu\nu}\sum_{m=0}^{n}[\bar\psi^{(m)}(x'),\,\gamma_\nu\,\psi^{(n-m)}(x')]\,dx'. \qquad (56)$$

The operators $A_\mu^{(0)}(x)$ and $\psi^{(0)}(x)$ fulfil the following equations[1]

$$[A_\mu^{(0)}(x),\,A_\nu^{(0)}(x')] = -i\,\delta_{\mu\nu}\,D(x'-x) \qquad (57)$$

$$\{\bar\psi_\alpha^{(0)}(x),\,\psi_\beta^{(0)}(x')\} = -i\,S_{\beta\alpha}(x'-x) \qquad (58)$$

$$<\{A_\mu^{(0)}(x),\,A_\nu^{(0)}(x')\}>_0 = \delta_{\mu\nu}\,D^{(1)}(x'-x) \qquad (59)$$

$$<[\bar\psi_\alpha^{(0)}(x),\,\psi_\beta^{(0)}(x')]>_0 = S_{\beta\alpha}^{(1)}(x'-x). \qquad (60)$$

Equation (59) is actually incomplete as it stands but will always give correct results, if the theory is truly gauge-invariant.[11] Formulae (57) and (58) have already been used several times in the calculations above.

For what follows it is convenient to introduce the notations

$$\frac{1}{2^{n-1}}\left\{A_{\nu_1}^{(0)}(x'),\,\left\{\cdots\left\{A_{\nu_{n-1}}^{(0)}(x^{n-1})\;A_{\nu_n}^{(0)}(x^n)\right\}\cdots\right\}\right\} = A\,(1,\,2,\,\ldots\,n). \qquad (61)$$

[10] Cf. Dyson[2], note to section V.
[11] Compare F. J. Dyson, Phys. Rev. 77, 420 (1950).

This symbol is actually symmetrical in all the variables x and indices ν. This can be inferred, if we consider the difference

$$\{A, \{B, C\}\} - \{B, \{A, C\}\} = [C, [B, A]] \tag{62}$$

where A, B and C are arbitrary operators. Putting

$$A = A^{(0)}_{\nu_1}(x')$$

$$B = A^{(0)}_{\nu_2}(x'')$$

$$C = A(3, 4, \ldots n)$$

we get

$$A(1, 2, 3, \ldots n) - A(2, 1, 3, \ldots n) = \\ = -i\,\delta_{\nu_1 \nu_2}[A(3, \ldots n), D(x' - x'')] = 0. \tag{63}$$

As $A(1, 2)$ is symmetrical in x' and x'', we conclude from (63) that $A(1, 2, 3)$ is completely symmetrical in all the variables x', x'' and x'''. From this fact the corresponding property of symmetry can be deduced for the iterated anticommutator of four potentials and so on for any value of n in (61).

The vacuum expectation value of the symbol, defined in (61), is easily computed according to well-known rules.[4] The operators have to be taken together in pairs in all possible ways, and the expectation value of every pair can then be calculated separately according to (59). If n in (61) is odd, the corresponding vacuum expectation value is obviously zero. If n is even, we obtain a sum of terms, one of which is given by

$$\left(\tfrac{1}{2}\right)^{\frac{n}{2}} \delta_{\nu_1 \nu_2} D^{(1)}(x' - x'')\,\delta_{\nu_3 \nu_4} D(x''' - x'''') \ldots \delta_{\nu_{n-1} \nu_n} D(x^{n-1} - x^n). \tag{64}$$

The vacuum expectation value of the iterated anticommutator contains $(n-1)$ $(n-3) \ldots 5 \cdot 3$ terms of the form (64), one for each way, in which it is possible to combine the numbers $1, 2, \ldots n$ in pairs.

In our calculations, commutators and anticommutators of the operators (61) will frequently appear. These quantities can be calculated successively in the following way. We begin by showing that

$$\tfrac{1}{2}[A^{(0)}_{\nu_1}(x'), A(2, 3, \ldots n)] = -\frac{i}{2}\sum_{m=2}^{n} \delta_{\nu_1 \nu_m} D(x^m - x')\, A^{(m)}(2, 3, \ldots n) \tag{65}$$

where

$$A^{(m)}(1, 2, \ldots n) = A(1, 2, \ldots m-1, m+1, \ldots n). \tag{66}$$

Supposing (65) to be valid for some value n, we have

$$\tfrac{1}{2}\left[A^{(0)}_{\nu_1}(x'),\ A(2, 3, \ldots n+1)\right] = \tfrac{1}{4}\left\{A^{(0)}_{\nu_2}(x''),\ \left[A^{(0)}_{\nu_1}(x'),\ A(3, \ldots n+1)\right]\right\} +$$
$$+ \tfrac{1}{4}\left\{A(3, \ldots n+1)\left[A^{(0)}_{\nu_1}(x'),\ A^{(0)}_{\nu_2}(x'')\right]\right\} =$$
$$= -\frac{i}{4}\sum_{m=3}^{n+1}\left\{A^{(0)}_{\nu_2}(x''),\ A^{(m)}(3, \ldots n+1)\right\}\cdot\delta_{\nu_1\nu_m}\cdot$$
$$\cdot D(x^m - x') - \frac{i}{2}A(3, \ldots n+1)\,\delta_{\nu_1\nu_2}D(x''-x') =$$
$$= -\frac{i}{2}\sum_{m=2}^{n+1}\delta_{\nu_1\nu_m}D(x^m - x')\,A^{(m)}(2, \ldots n+1). \tag{67}$$

As (65) is certainly correct for $n = 1$, it is thus proved for all values of n.

Using this result we are now able to compute

$$\tfrac{1}{2}\left\{A(1, 2),\ A(3, 4, \ldots n)\right\} = A(1, 2, \ldots n) +$$
$$+ \tfrac{1}{4}\left[A^{(0)}_{\nu_2}(x''),\ \left[A^{(0)}_{\nu_1}(x'),\ A(3, 4, \ldots n)\right]\right] = A(1, 2, \ldots n) +$$
$$+ \left(-\frac{i}{2}\right)^2\sum_{m+k=3}^{n}\delta_{\nu_1\nu_m}D(x^m - x')\,\delta_{\nu_2\nu_k}D(x^k - x'')\,A^{(m)(k)}(2, 3, \ldots n). \tag{68}$$

Continuing in this way and calculating alternatively the commutators and the anticommutators, we obtain the following formulae

$$\tfrac{1}{2}\left\{A(1, 2, \ldots m),\ A(m+1, \ldots n)\right\} = A(1, 2, \ldots n) +$$
$$+ \left(-\frac{i}{2}\right)^2\sum_{i<j=1}^{m}\ \sum_{\substack{r+s=\\=m+1}}^{n}\delta_{\nu_i\nu_r}D(x^r - x^i)\cdot\delta_{\nu_j\nu_s}D(x^s - x^j)\cdot$$
$$\cdot A^{(i)(j)(r)(s)}(1, 2, \ldots n) + \left(-\frac{i}{2}\right)^4\sum_{\substack{i<j<k<\\<l=1}}^{m}\ \sum_{\substack{r+s+t+\\+u=m+1}}^{n}\delta_{\nu_i\nu_r}D(x^r - x^i)\cdot$$
$$\cdot\delta_{\nu_j\nu_s}D(x^s - x^j)\,\delta_{\nu_k\nu_t}D(x^t - x^k)\,\delta_{\nu_u\nu_l}D(x^u - x^l)\cdot$$
$$\cdot A^{(i)(j)(k)(l)(r)(s)(t)(u)}(1, 2, 3 \ldots n) + \left(-\frac{i}{2}\right)^6\sum\sum\cdots+\cdots \tag{69}$$

and

$$\tfrac{1}{2}\left[A(1, 2, \ldots m),\ A(m+1, \ldots n)\right] = -\frac{i}{2}\sum_{i=1}^{m}\ \sum_{r=m+1}^{n}\delta_{\nu_i\nu_r}D(x^r - x^i)\cdot$$
$$\cdot A^{(i)(r)}(1, 2, \ldots n) + \left(-\frac{i}{2}\right)^3\sum_{i<j<k=1}^{m}\ \sum_{\substack{r+s+t=\\=m+1}}^{n}\delta_{\nu_i\nu_r}D(x^r - x^i)\,\delta_{\nu_j\nu_s}D(x^s - x^j)\cdot$$
$$\cdot\delta_{\nu_k\nu_t}D(x^t - x^k)\,A^{(i)(j)(k)(r)(s)(t)}(1, 2, \ldots n) + \left(-\frac{i}{2}\right)^5\sum\sum\cdots+\cdots. \tag{70}$$

The summations over $i, j, \ldots$ and $r, s, \ldots$ have to be done in such a way that all possible combinations of the differences $x^\xi - x^\eta$, where η belongs to the set $i, j, \ldots$ and ξ to $r, s, \ldots$, appear once and only once.

For the operators $\psi^{(0)}(x)$ and $\bar{\psi}^{(0)}(x)$ the definitions similar to (61) will be

$$J(1, 2, \ldots i_1; i_1 + 1, \ldots i_2; \ldots \quad \ldots i_r; i_r + 1, \ldots n) =$$

$$= \frac{1}{2^{2r+1}} [\bar{\psi}^{(0)}(x'), \bar{S}(1, 2, \ldots i_1) \{\psi^{(0)}(x^{i_1}), [\bar{\psi}^{(0)}(x^{i_1+1}),$$

$$\bar{S}(i_1 + 1, \ldots i_2) \{\psi^{(0)}(x^{i_2}), [\ldots \quad \ldots [\bar{\psi}^{(0)}(x^{i_r+1}),$$

$$\bar{S}(i_r + 1, \ldots n)\, \psi^{(0)}(x^n)] \ldots]\}]\}] \tag{71}$$

where

$$\bar{S}(1, 2, \ldots n) = \gamma_{\nu_1} \bar{S}(x' - x'') \gamma_{\nu_2} \ldots \gamma_{\nu_{n-1}} \bar{S}(x^{n-1} - x^n) \gamma_{\nu_n}. \tag{72}$$

The vacuum expectation value of (71) is easily found to be a sum of $(r + 1)!$ terms, one typical of which can be written

$$\frac{(-1)^r}{2^{r+1}} Sp\, [S^{(1)}(x^n - x') \bar{S}(1, 2, \ldots i_1) S^{(1)}(x^{i_1} - x^{i_1+1}) \bar{S}(i_1 + 1, \ldots i_2) \ldots$$

$$\ldots S^{(1)}(x^{i_r} - x^{i_r+1}) \bar{S}(i_r + 1, \ldots n)]. \tag{73}$$

The other terms are obtained, if the field operators are combined in different ways and can contain a product of two or more traces. The numerical value of the factor in front of the expression is always 2^{-r-1} ($r + 1$ is the number of $S^{(1)}$-functions in the expectation value) but the sign will be dependent on the number of traces in the product. As a careful inspection shows, the sign is given by

$$(-1)^{r-l+1} \tag{74}$$

where l is the number of factors in our expression. For the commutators and anticommutators between two J-symbols we get formulae similar to (69) and (70). Arguing in the same way as before, we obtain *e.g.*

$$\tfrac{1}{2} \{J(1, 2, \ldots m), J(m+1, \ldots i_1; i_1+1, \ldots i_2; \ldots \quad \ldots i_r; i_r+1, \ldots n)\} =$$

$$= J(1, 2, \ldots m; m + 1, \ldots i_1; \ldots \quad \ldots i_r; i_r + 1, \ldots n) +$$

$$+ \left(-\frac{i}{2}\right)^2 \sum_{\alpha, \beta} J(m + 1, \ldots i_1; \ldots$$

$$\ldots i_\alpha; i_\alpha + 1; \ldots \boldsymbol{i_{\alpha+1}}; \mathbf{1}, 2, \ldots \boldsymbol{m}; \boldsymbol{i_\beta} + \mathbf{1}, \ldots \quad \ldots i_r; i_r + 1, \ldots n) \tag{75}$$

where

$$J(1, 2, \ldots i_1; i_1 + 1, \ldots \quad \ldots \boldsymbol{i_\alpha}; \boldsymbol{i_\alpha} + \mathbf{1}, \ldots \boldsymbol{i_\beta}; \boldsymbol{i_\beta} + \mathbf{1}, \ldots$$

$$\ldots i_r; i_r + 1, \ldots n) = \frac{1}{2^{2r-1}} [\bar{\psi}^{(0)}(x'), \bar{S}(1, 2, \ldots i_1) \{\psi^{(0)}(x^{i_1}),$$

$$[\cdots \quad \cdots \{\psi^{(0)}(x^{i_{\alpha-1}}), [\bar{\psi}^{(0)}(x^{i_{\alpha-1}+1}), \bar{S}(i_{\alpha-1} + 1, \ldots i_\alpha) S(x^{i_\alpha} - x^{i_\alpha+1}) \cdot$$

$$\cdot \bar{S}(i_\alpha + 1, \ldots i_\beta) S(x^{i_\beta} - x^{i_\beta+1}) \bar{S}(i_\beta + 1, \ldots i_{\beta+1}) \{\psi^{(0)}(x^{i_{\beta+1}}),$$

$$[\cdots \quad \cdots [\bar{\psi}^{(0)}(x^{i_r+1}), \bar{S}(i_r + 1, \ldots n) \psi^{(0)}(x^n)] \cdots]\}]\} \cdots]\}]. \tag{76}$$

Equation (76) corresponds to (68), and the appearance of the two $S(x)$-functions in (76) is quite analogous to the existence the $D(x)$-functions in (68). One important difference between the two formulae, however, lies in the fact, that the summation in the last term of (75) has to include also the possibilities $\alpha = \beta$. In these cases a trace of the form

$$Sp[S(x^{i_{\alpha+1}} - x') \bar{S}(1, 2, \ldots n) S(x^n - x^{i_\alpha+1}) \bar{S}(i_\alpha + 1, \ldots i_{\alpha+1})] \tag{77}$$

has to be substituted in (76) in the place of the expression containing the $S(x)$-functions. This difference between (75) and (68) has its origin in the fact, that the symbols J contain two kinds of operators ($\bar{\psi}^{(0)}(x)$ and $\psi^{(0)}(x)$), while the expressions A in (61) are built up from only one type of electromagnetic potentials.

We will make no attempt at writing down explicitly the commutators and the other, more general anticommutators between the J-symbols, as they will be only rather trivial modifications of the formulae (69) and (70) but will nevertheless give rise to comparatively complicated notations. The main structure of those expressions is, however, easily inferred from what has already been said.

We are now able to turn to the direct solution of the equations (1) and (2). As we do not want to introduce too many complications at the beginning, for the moment we drop the mass-renormalization terms and write (55) and (56) as

$$\psi^{(n+1)}(x) = -\frac{i}{2} \int \bar{S}(x - x') \sum_{m=0}^{n} \{A_\nu^{(m)}(x'), \gamma_\nu \psi^{(n-m)}(x')\} \, dx' \tag{78}$$

$$A_\mu^{(n+1)}(x) = \frac{i}{2} \int \bar{D}(x - x') \sum_{m=0}^{n} [\bar{\psi}^{(m)}(x'), \gamma_\mu \psi^{(n-m)}(x')] \, dx'. \tag{79}$$

From these equations we get successively

$$\psi^{(1)}(x) = -i \int \bar{S}(x - x') \gamma A^{(0)}(x') \psi^{(0)}(x') \, dx' \tag{80 a}$$

$$A_\mu^{(1)}(x) = \frac{i}{2} \int \bar{D}(x - x') [\bar{\psi}^{(0)}(x'), \gamma_\mu \psi^{(0)}(x')] \, dx' \tag{80 b}$$

$$\psi^{(2)}(x) = (-i)^2 \int\int \bar{S}(x-x')\,[\bar{S}(1,2)\,\psi^{(0)}(x'')\,A(1,2) - \tfrac{1}{4}\gamma_{\nu_1}\{\psi^{(0)}(x'),\\ [\bar{\psi}^{(0)}(x''),\gamma_{\nu_2}\psi^{(0)}(x'')]\}\,\delta_{\nu_1\nu_2}\bar{D}(x'-x'')]\,dx'\,dx'' \quad (81\text{ a})$$

$$A_\mu^{(2)}(x) = -i^2\int\int J(1,2)\,[\delta_{\mu\nu_1}\bar{D}(x-x')\,A_{\nu_2}^{(0)}(x'') + \delta_{\mu\nu_2}\bar{D}(x-x'')\cdot A_{\nu_1}^{(0)}(x')] \quad (81\text{ b})$$

$$\psi^{(3)}(x) = (-i)^3\int\int\int \bar{S}(x-x')\,[\bar{S}(1,2,3)\,\psi^{(0)}(x''')\,A(1,2,3) - \tfrac{1}{2}\bar{S}(1,2)\cdot\\ \cdot\{\psi^{(0)}(x''),J(3)\}\,(A_{\nu_1}^{(0)}(x')\,\delta_{\nu_2\nu_3}\bar{D}(x''-x''') + A_{\nu_2}^{(0)}(x'')\,\delta_{\nu_1\nu_3}\bar{D}(x'-x''')) -\\ -\tfrac{1}{2}\gamma_{\nu_1}\{\psi^{(0)}(x'),J(2,3)\}\,(A_{\nu_2}^{(0)}(x'')\,\delta_{\nu_1\nu_3}\bar{D}(x'-x''') + A_{\nu_3}^{(0)}(x''')\,\delta_{\nu_1\nu_2}\cdot\\ \cdot\bar{D}(x'-x''))]\,dx'\,dx''\,dx''' \quad (82\text{ a})$$

$$A_\mu^{(3)}(x) = i^3\int\int\int\{J(1,2,3)\,(\delta_{\mu\nu_1}\bar{D}(x-x')\,A(2,3) + \delta_{\mu\nu_2}\bar{D}(x-x'')\,A(1,3) +\\ +\delta_{\mu\nu_3}\bar{D}(x-x''')\,A(1,2)) - J(1,2;3)\,(\delta_{\mu\nu_1}\bar{D}(x-x')\,\delta_{\nu_2\nu_3}\bar{D}(x''-x''') +\\ +\delta_{\mu\nu_2}\bar{D}(x-x'')\,\delta_{\nu_1\nu_3}\bar{D}(x'-x'''))\}\,dx'\,dx''\,dx''' + \frac{i}{4}\int\int\int\bar{D}(x-x')\cdot\\ \cdot\delta_{\mu\nu_1}Sp\,[S(x''-x''')\,\bar{S}(3,2,1)]\,\delta_{\nu_2\nu_3}D(x''-x''')\,dx'\,dx''\,dx'''. \quad (82\text{ b})$$

The last term in (82 b) is obtained from the commutator between $\bar{\psi}^{(1)}(x)$ and $\psi^{(1)}(x)$. This expression gives

$$\tfrac{1}{2}[\bar{\psi}^{(1)}(x'),\gamma_{\nu_1}\psi^{(1)}(x')] = \frac{(-i)^2}{4}\int\int dx''\,dx'''\,\{[\bar{\psi}^{(0)}(x'''),\bar{S}(3,1,2)\,\psi^{(0)}(x'')]\cdot\\ \cdot 2A(3,2) + \{\bar{\psi}^{(0)}(x'''),\bar{S}(3,1,2)\,\psi^{(0)}(x'')\}\cdot[A_{\nu_3}^{(0)}(x'''),A_{\nu_2}^{(0)}(x'')]\}. \quad (83)$$

The first term in (83) is equal to

$$-\int\int dx''\,dx'''\,J(3,1,2)\,A(3,2) \quad (84)$$

and the second one gives with the aid of (57) and (58)

$$\tfrac{1}{4}\int\int dx''\,dx'''\,Sp\,[S(x''-x''')\,\bar{S}(3,1,2)]\,\delta_{\nu_2\nu_3}D(x''-x'''). \quad (85)$$

Up till now the formulae obtained have contained only the singular functions $\bar{S}(x)$ and $\bar{D}(x)$, but in (82 b) and (85) also the functions $S(x)$ and $D(x)$ have appeared. When we proceed to still higher approximations, more and more terms of this form will turn up from the commutators and anticommutators in (78) and (79). Computing, as a simple example, the commutator

$$\tfrac{1}{2}[\bar{\psi}^{(0)}(x')\,\bar{S}(1,2,\ldots m)\,A(1,2,\ldots m),\\ A(m+1,\ldots n)\,\bar{S}(m+1,\ldots n)\,\psi^{(0)}(x^n)] \quad (86)$$

we obtain

$$J(1, 2, \ldots n)\tfrac{1}{2}\{A(1, 2, \ldots m), A(m+1, \ldots n)\} -$$
$$-\frac{i}{4} Sp[S(x^n - x')\bar{S}(1, 2, \ldots n)] \cdot [A(1, 2, \ldots m),$$
$$A(m+1, \ldots n)] = J(1, 2, \ldots n)[A(1, 2, \ldots n) +$$
$$+\left(-\frac{i}{2}\right)^2 \sum_{i<j=1}^{m} \sum_{\substack{r+s=\\=m+1}}^{n} \delta_{\nu_i \nu_r} D(x^r - x^i)\,\delta_{\nu_j \nu_s} D(x^s - x^j) \cdot$$
$$A^{(i)(j)(r)(s)}(1, 2, \ldots n) + \cdots] + Sp[S(x^n - x')\bar{S}(1, 2, \ldots n)] \cdot$$
$$\cdot\left[\left(-\frac{i}{2}\right)^2 \sum_{i=1}^{m} \sum_{r=m+1}^{n} \delta_{\nu_i \nu_r} D(x^r - x^i) A^{(i)(r)}(1, 2, \ldots n) + \cdots\right]. \tag{87}$$

If an expectation value of the first term is calculated, we obtain, according to the rules given above, a sum of terms, each of which contains only $\bar{S}(x)$, $S^{(1)}(x)$, $\bar{D}(x)$ and $D^{(1)}(x)$ functions. The variables of those singular functions are always differences between two coordinates $x^i - x^j$. If some $j < m$ and at the same time the attached $i > m$, there is a simple correspondence between these terms and the terms obtained from the expectation value of the remaining expression in (87). The latter are obtained from the former, if we in every term substitute two, four, six etc. "unbarred functions" for the $D^{(1)}$ and $S^{(1)}$ functions in all possible ways, only remembering the conditions for i and j. The same correspondence is easily seen to hold even if the more general expressions

$$\tfrac{1}{2}\bar{S}(1, \ldots n)\{\psi^{(0)}(x^n), J(n+1, \ldots; \ldots \ \ldots)\} \tag{88}$$

are used in (86) instead of $\bar{S}(1, \ldots n)\,\psi^{(0)}(x^n)$.

It is now convenient to split our operators $\psi^{(n)}(x)$ and $A_\mu^{(n)}(x)$ into two parts, one of which (${}^{(1)}\psi^{(n)}(x)$ and ${}^{(1)}A_\mu^{(n)}(x)$) contains no "unbarred functions", and write

$$\psi^{(n)}(x) = {}^{(1)}\psi^{(n)}(x) + {}^{(2)}\psi^{(n)}(x) \tag{89 a}$$

$$A_\mu^{(n)}(x) = {}^{(1)}A_\mu^{(n)}(x) + {}^{(2)}A_\mu^{(n)}(x). \tag{89 b}$$

From the definitions given earlier we immediately infer

$${}^{(1)}\psi^{(n)}(x) = (-i)^n \int \cdots \int dx' \ldots dx^n \tfrac{1}{2}\bar{S}(x - x') \sum_r \sum_{i_1 \ldots i_r} \bar{S}(1, 2, \ldots i_1) \cdot$$
$$\cdot \{\psi^{(0)}(x^{i_1}), J(i_1+1, \ldots i_2; i_2+1, \ldots; \ldots \ \ldots i_r; i_r+1, \ldots n)(-1)^r \cdot$$
$$\cdot \sum_{\alpha_1 \ldots \alpha_{2r}} \delta_{\nu_{\alpha_1} \nu_{\alpha_2}} \bar{D}(x^{\alpha_1} - x^{\alpha_2})\,\delta_{\nu_{\alpha_3} \nu_{\alpha_4}} \bar{D}(x^{\alpha_3} - x^{\alpha_4}) \ldots \delta_{\nu_{\alpha_{2r-1}} \nu_{\alpha_{2r}}} \cdot$$
$$\cdot \bar{D}(x^{\alpha_{2r-1}} - x^{\alpha_{2r}}) A^{(\alpha_1) \cdots (\alpha_{2r})}(1, 2, \ldots n). \tag{90}$$

The summation over r has to be performed from 0 to $\frac{n}{2}$, if n is even, and from 0 to $\frac{n-1}{2}$ if n is odd. The numbers $i_1, \ldots i_r$ have to be summed over all possible combinations for a fixed value of r. The last sum over the α_j has to include all possibilities, where each one of the sets $1, \ldots i_1;\ i_1+1, \ldots i_2;\ \ldots\ \ldots;\ i_r+1, \ldots n$ contains at least one of the numbers $\alpha_1 \ldots \alpha_{2r}$. The summations over α_j are also restricted through another condition, which can be formulated in the following way. Consider one of the sets $i_j+1, \ldots i_{j+1}$. To this set belongs at least one value α_j and a function $\bar{D}(x^{\alpha_j} - x^{\alpha_k})$, where the number α_k is included in another set $i_k+1, \ldots i_{k+1}$. We then say, that the two sets $i_j+1, \ldots i_{j+1}$ and $i_k+1, \ldots i_{k+1}$ are "connected". If a third set is now connected, in the sense just defined, to one of the two mentioned earlier, we say that it is also connected to the other one etc. The remaining restriction for the α-summations can then be so formulated, that none of the sets $i_j+1, \ldots i_{j+1}$ is to be connected to itself. As an example of a term, which does *not* appear according to this restriction, we can mention

$$-\tfrac{1}{2}(-i)^3 \int\int\int \bar{S}(x-x')\,\gamma_{\nu_1}\{\psi^{(0)}(x'), J(2,3)\}\cdot$$
$$.\,A^{(0)}_{\nu_1}(x')\,\delta_{\nu_2\nu_3}\bar{D}(x''-x''')\,dx'\,dx''\,dx'''.$$

(Compare equation (82 a)).

The corresponding formula for the electromagnetic potentials is given by

$$\begin{aligned}{}^{(1)}A^{(m)}_\mu(x) &= (-i)^n \int\cdots\int dx' \ldots dx^n \sum_r \sum_{i_1\ldots i_r} J(1,2,\ldots i_1;\ i_1+1,\ldots\\ &\ldots i_r;\ i_r+1,\ldots n)\,(-1)^{r+1} \sum_{\alpha_1\ldots\alpha_{2r}} \delta_{\nu_{\alpha_1}\nu_{\alpha_2}} \bar{D}(x^{\alpha_1}-x^{\alpha_2}) \ldots \delta_{\nu_{\alpha_{2r-1}}\nu_{\alpha_{2r}}}\cdot\\ &\cdot\bar{D}(x^{\alpha_{2r-1}}-x^{\alpha_{2r}}) \sum_j{}' \delta_{\mu\nu_j}\bar{D}(x-x^j)\,A^{(j)(\alpha_1)\ldots(\alpha_{2r})}(1,2,\ldots n).\end{aligned} \tag{91}$$

The same conventions for the summations over $r, i_1, \ldots i_r, \alpha_1, \ldots \alpha_{2r}$ are valid in (91) and (92). The index j in (91) has to be summed over all values from 0 to n, excepting the numbers $\alpha_1, \ldots \alpha_{2r}$. The proof of (90) and (91) is conveniently done from n to $n+1$ in the manner used several times earlier and will not be given explicitly. In this calculation it is permitted to disregard all terms in the right-hand side of (69) and (70) except the first one in (69), as those terms will give contributions only to ${}^{(2)}\psi^{(n)}(x)$ and ${}^{(2)}A^{(n)}_\mu(x)$.

The remaining parts of the operators (${}^{(2)}\psi^{(n)}(x)$ and ${}^{(2)}A^{(n)}_\mu(x)$) cannot be written down in such a compact form, as was possible for ${}^{(1)}\psi^{(n)}(x)$ and ${}^{(1)}A^{(n)}_\mu(x)$. Their general properties, however, can easily be inferred from the remark made after equation (87). As these expressions will all appear from commutators of the form (86), it is very easy to include, later on, their contributions to the expectation values of our operators. We thus turn to the calculation of expectation values and choose for this purpose the current operator as a suitable expression. This operator has a fundamental significance as both the interaction Hamiltonian and the $\boldsymbol{S}$-matrix are closely connected with it. If we take only the part ${}^{(1)}j(x)$ defined analogously to (89), we obtain with the definition

$$ {}^{(1)}j_\mu(x) = \sum_{n=0}^{\infty} e^{n+1}\, {}^{(1)}j_\mu^{(n)}(x) \tag{92} $$

the formula

$$ {}^{(1)}j_\mu^{(n)}(x) = (-i)^n \int \cdots \int d\,x' \ldots d\,x^n \sum_{m=0}^{n}{}' \sum_{r} \sum_{i_1 \ldots i_r} (-i)^r J(1, 2, \ldots i_1; \ldots $$

$$ \ldots i_s;\, i_s + 1, \ldots m, 0, m+1, \ldots i_{s+1}; \ldots \quad \ldots i_r;\, i_r + 1, \ldots n) \cdot $$

$$ \cdot \sum_{\alpha_1 \ldots \alpha_{2r}} \delta_{\nu_{\alpha_1} \nu_{\alpha_2}} \bar{D}(x^{\alpha_1} - x^{\alpha_2}) \ldots \delta_{\nu_{\alpha_{2r-1}} \nu_{\alpha_{2r}}} \bar{D}(x^{\alpha_{2r-1}} - x^{\alpha_{2r}}) \cdot $$

$$ \cdot A^{(\alpha_1) \cdots (\alpha_{2r})}(1, 2, \ldots n). \tag{93} $$

The summation conventions for $r, \ldots \alpha_{2r}$ are similar to the earlier ones. The expectation value is now obtained as a sum of several terms, each of them containing $\frac{n}{2}$ functions $S^{(1)}(x)$ or $D^{(1)}(x)$. It is at this point most convenient to introduce the graphical language of DYSON[2, 3] and FEYNMAN.[5] If our expression contains $n + 1$ variables $x, x', \ldots x^n$, we represent it by $n + 1$ points in a graph, each point labelled with one of the letters $x, x', \ldots x^n$. For every function $\bar{S}(x^i - x^j)$ or $S^{(1)}(x^i - x^j)$ in the expectation value we draw a line (an electron line) from the point x^i to the point x^j, and for every function $\bar{D}(x^i - x^j)$ or $D^{(1)}(x^i - x^j)$ we draw a dotted line (a photon line) connecting the two points in question. For every operator representing a free particle we also draw a line (electron or photon) from the edge of our graph to the point that appears as a variable in the operator. In this way we obviously obtain a simple diagram representing our expectation value. It is, however, doubtful if this graph will also give a picture of the physical process underlying the theory. In the older formulation of quantum electrodynamics the higher radiative approximations were described with the aid of virtual photons and virtual pairs of electrons. In the papers quoted above, FEYNMAN[5] has developed an intuitive method of identifying the electron lines with the virtual pairs and the photon lines with the virtual quanta. From the point of view adopted here, this is, however, only an extra complication, as we have no need for the conceptions of virtual particles, and as the singular functions have the much more natural property of being either solutions to the inhomogenous differential equations or expectation values. Also in the older formulation of quantum electrodynamics the virtual particles were, even from a principal point of view, not observable and thus only a mathematical conception. We therefore prefer to regard our graphs only as a suitable representation of the mathematical structure of the theory.

It might be pointed out here, that it is also possible and in some respects convenient to represent the field operators themselves with the aid of graphical figures. If we use the same conventions as above concerning the $\bar{S}(x^i - x^j)$ and $\bar{D}(x^i - x^j)$ functions but write a letter $\bar{\psi}$, ψ or A for every operator $\bar{\psi}^{(0)}(x^i)$, $\psi^{(0)}(x^i)$ and $A_\mu^{(0)}(x^i)$, we can make us a diagram of the ${}^{(1)}\psi(x)$ and ${}^{(1)}A_\mu(x)$ in a simple way. For the operators given by (80), (81) and (82) we then obtain figures 5, 6, 7 and 8. The summation rules of equation (90) can

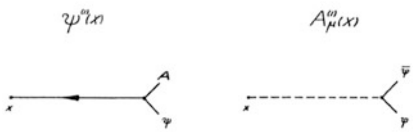

Fig. 5.

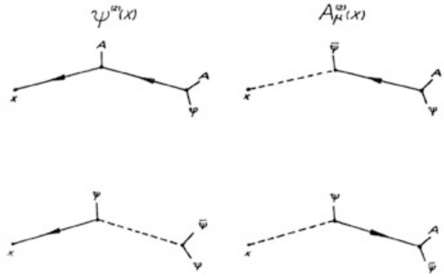

Fig. 6.

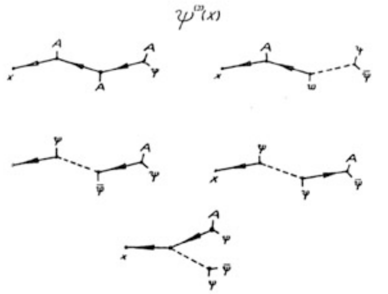

Fig. 7.

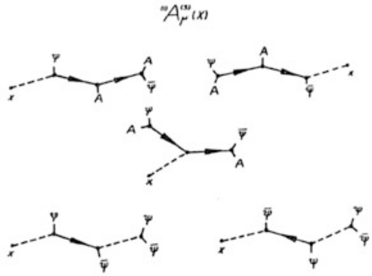

Fig. 8.

here be expressed in the following way. We sum over all possible combinations of either n operators A and one operator ψ or $(n-2)$ operators A, two operators ψ and one operator $\bar{\psi}$ or $(n-4)$ operators A, three operators ψ and two operators $\bar{\psi}$ etc., where the different operators are connected together with electron and photon lines. The restrictions on the α_i-variables correspond to the restriction to only "connected" graphs with no "closed loops".[12] In a similar way the ${}^{(1)}j_\mu^{(n)}(x)$ operators (not their expectation values) can be represented by graphs containing ψ and A as above. In the lowest approximations we obtain, corresponding to equation (93), figures 9 and 10.

We obtain the graphs for the expectation values from the graphs for the operators by combining the letters A (or $\bar{\psi}$ and ψ) together in pairs and sub-

[12] For the definition of these expressions see DYSON[2] p. 495—496.

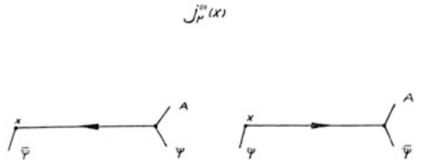

Fig. 9.

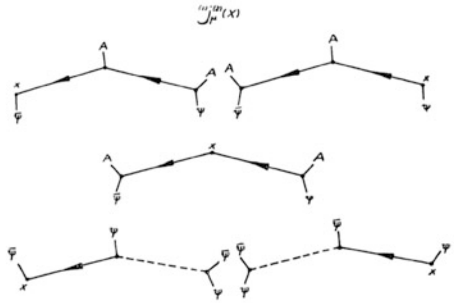

Fig. 10.

stituting a photon (electron) line for each pair. This procedure has to be performed in all possible ways, and it is easily seen, that all the connected graphs of DYSON[12] will appear as a result. No disconnected graph, however, will turn up, but these diagrams were shown by DYSON to give only an unimportant vacuum fluctuation and no contribution to the expectation value.[12] It might rather be considered as being to the advantage of our formalism, that these terms will not come out from the calculation.

For each connected graph of DYSON we will get, not only one term as he does, but several terms containing both the $S^{(1)}(x)$, $D^{(1)}(x)$ and $\bar{S}(x)$, $\bar{D}(x)$ functions. If we consider a graph belonging to the n^{th} approximation, this diagram does necessarily contain exactly $\frac{n}{2}$ closed loops, which may or may not have one or several lines in common. If we first consider the case, when all the loops are entirely separated from each other, we infer from the rules given above that the lines joining the loops must be represented by $\bar{S}(x)$ or $\bar{D}(x)$ functions. This follows from the fact, that the diagrams of the operators (*e.g.* figures 9 and 10) contain no disconnected parts. As a simple example we

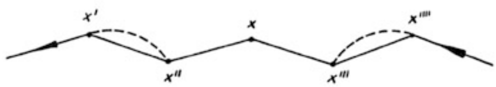

Fig. 11.

can consider the graph in figure 11, belonging to the one-particle part of ${}^{(1)}j_\mu^{(4)}(x)$. This diagram contains two loops, one between (x', x'') and the other between (x''', x''''). The lines joining the loops are the lines between the points (x'', x) and (x, x'''). These two last lines are thus represented by the functions $\bar{S}(x''-x)$ and $\bar{S}(x-x''')$. As the diagrams of the operators contain no closed loops, one and only one of the lines in every loop in the graph of an expectation value must be represented by a $S^{(1)}(x)$ or $D^{(1)}(x)$ function. It is also obvious, that all possible combinations will actually appear, and this means, that each loop is represented by a *factor* of the following general form

$$\bar{S}(1, 2, \ldots n)\,\delta_{\nu_n \nu_1} D^{(1)}(x^n - x') + \sum_{m=1}^{n-1} \bar{S}(1, 2, \ldots m)\, S^{(1)}(x^m - x^{m+1}) \cdot$$
$$\cdot \bar{S}(m+1, \ldots n) \cdot \delta_{\nu_1 \nu_n} \bar{D}(x^n - x'). \qquad (94)$$

The modifications necessary, when more or less than one of the lines are photon lines, are obvious. For the special graph of figure 11 we thus obtain, apart from a numerical factor

$$\int \cdots \int d x' \ldots d x'''' \,\bar{\psi}^{(0)}(x')\,\gamma_\lambda \{\bar{S}(x' - x'')\, D^{(1)}(x'' - x') +$$
$$+ S^{(1)}(x' - x'')\,\bar{D}(x'' - x')\} \cdot \gamma_\lambda \bar{S}(x'' - x)\,\gamma_\mu \bar{S}(x - x''')\,\gamma_\varrho \cdot$$
$$\cdot \{\bar{S}(x''' - x'''')\, D^{(1)}(x'''' - x''') + S^{(1)}(x''' - x'''')\,\bar{D}(x'''' - x''')\} \cdot$$
$$\cdot \gamma_\varrho \psi^{(0)}(x''''). \qquad (95)$$

Using the following integral representation, given by LUTTINGER[13]

$$\frac{\delta(a_1)}{a_2 \ldots a_n} + \frac{\delta(a_2)}{a_1 a_3 \ldots a_n} + \cdots + \frac{\delta(a_n)}{a_1 \ldots a_{n-1}} =$$
$$= (-1)^{n-1} \int_0^1 d x_1 \int_0^{x_1} d x_2 \ldots \int_0^{x_{n-2}} d x_{n-1}\, \delta^{(n-1)}(a_n x_{n-1} +$$
$$+ a_{n-1}(x_{n-2} - x_{n-1}) + \cdots + a_2(x_1 - x_2) + a_1(1 - x_1)) \qquad (96)$$

(the proof of equation (96) is conveniently done from n to $n+1$) we can, in momentum space, write the expression (94) as

$$\left(\frac{1}{2\pi}\right)^{4n-1} \int \cdots \int d p_1 \ldots d p_{n-1}\, d k\, e^{i p_1 (x' - x'') + i p_2 (x'' - x''') + \cdots + i p_{n-1}(x^{n-1} - x^n)} \cdot$$
$$\cdot e^{i k (x^n - x')} \cdot \gamma_{\nu_1}(i \gamma p_1 - m)\,\gamma_{\nu_2} \ldots \gamma_{\nu_{n-1}}(i \gamma p_{n-1} - m)\,\gamma_{\nu_n} \cdot$$
$$\cdot \delta_{\nu_1 \nu_n}(-1)^{n-1} \int_0^1 d\xi_1 \ldots \int_0^{\xi_{n-2}} d\xi_{n-1} \cdot \delta^{(n-1)}(k^2 \xi_{n-1} + p_{n-1}^2(\xi_{n-2} - \xi_{n-1}) +$$
$$+ \cdots + p_2^2(\xi_1 - \xi_2) + p_1^2(1 - \xi_1) + m^2(1 - \xi_{n-1})). \qquad (97)$$

For the special case of equation (95) we obtain in this way

$$\left(\frac{1}{2\pi}\right)^6 \int \int d k_1\, d k_2 \int \cdots \int d p_1 \ldots d p_4\, e^{i x (p_3 - p_2)}\, \bar{u}(p_1 - k_1)\,\gamma_{\lambda_1}(i \gamma p_1 - m) \cdot$$
$$\cdot \gamma_{\lambda_1}(i \gamma p_2 - m) \cdot \gamma_\mu (i \gamma p_3 - m)\,\gamma_{\lambda_2}(i \gamma p_4 - m)\,\gamma_{\lambda_2} u(p_4 - k_2) \cdot$$

[13] J. M. LUTTINGER, unpublished letter to the author.

$$\cdot \frac{\delta(k_1 - p_1 + p_2)\,\delta(p_4 - p_3 - k_2)}{(p_2^2 + m^2)\;(p_3^2 + m^2)} \cdot \int_0^1 d\xi\, \delta'(k_1^2 \xi + (p_1^2 + m^2)(1-\xi)) \cdot$$

$$\cdot \int_0^1 d\eta\, \delta'(k_2^2 \eta + (p_4^2 + m^2)(1-\eta)) = \frac{1}{(2\pi)^6} \int\int dq\, dq'\, e^{ix(q'-q)} \cdot$$

$$\cdot \int\int dk\, dk'\, \bar{u}(q)\, \gamma_{\lambda_1} (i\gamma(q+k) - m)\, \gamma_{\lambda_1} (i\gamma q - m) \cdot \gamma_\mu \cdot$$

$$\cdot (i\gamma q' - m)\, \gamma_{\lambda_2} (i\gamma(q'+k') - m)\, \gamma_{\lambda_2} u(q') \frac{1}{(q^2+m^2)(q'^2+m^2)} \int_0^1 d\xi\, \delta'(k^2\xi +$$

$$+ ((q+k)^2 + m^2)(1-\xi)) \int_0^1 d\eta\, \delta'(k'^2 \eta + ((q'+k')^2 + m^2)(1-\eta)) \tag{98}$$

where

$$u(q) = \frac{1}{(2\pi)^4} \int e^{-iqx}\, \psi^{(0)}(x)\, dx. \tag{98 a}$$

Introducing the notation

$$K(q) = -\int_0^1 d\xi \int dk\, \gamma_\lambda (i\gamma(q+k) - m)\, \gamma_\lambda\, \delta'(k^2\xi + ((q+k)^2 + m^2)(1-\xi)) \tag{99}$$

we obtain for the expression (98)

$$\left(\frac{1}{2\pi}\right)^6 \int\int dq\, dq'\, e^{ix(q'-q)}\, \bar{u}(q)\, K(q)\, \frac{i\gamma q - m}{q^2 + m^2}\, \gamma_\mu\, \frac{i\gamma q' - m}{q'^2 + m^2}\, K(q')\, u(q'). \tag{100}$$

Using the method of DYSON, we obtain instead of the expression (95) the formula

$$\left(\frac{i}{2}\right)^4 \int \cdots \int dx' \ldots dx''''\, \bar{\psi}^{(0)}(x')\, \gamma_\lambda S_F(x'-x'')\, \gamma_\lambda S_F(x''-x) \cdot$$

$$\cdot \gamma_\mu S_F(x - x''')\, \gamma_\varrho S_F(x''' - x'''')\, \gamma_\varrho\, \psi^{(0)}(x''''). \tag{101}$$

With the aid of the integral representations of FEYNMAN[14]

$$\frac{1}{a_1 \ldots a_n} = (n-1)! \int_0^1 dx_1 \int_0^{x_1} dx_2 \ldots \int_0^{x_{n-2}} dx_{n-1} \cdot$$

$$\cdot [a_n x_{n-1} + a_{n-1}(x_{n-2} - x_{n-1}) + \cdots + a_1(1 - x_1)]^{-n} \tag{102}$$

[14] R. P. FEYNMAN, Phys. Rev. *76*, 785 (1949). Compare also R. JOST—J. M. LUTTINGER, Helv. Phys. Acta *23*, 208 (1950).

this expression is, in momentum space, conveniently written in the form (100), with $K(q)$ given by

$$K(q) = \frac{1}{i\pi}\int_0^1 d\xi \int_{C_F} dk \, \frac{\gamma_\lambda (i\gamma(q+k) - m)\gamma_\lambda}{k^2\xi + ((q+k)^2 + m^2)(1-\xi)}. \tag{103}$$

Introducing the well-known translation of the origin $k \to k + q(1-\xi)$ and observing that

$$\int dk\, \delta^{(n)}(k^2 + A) f(k^2) = Re \int_{C_F} \frac{dk\, f(k^2)}{(k^2 + A)^{n+1}} \frac{(-1)^n n!}{i\pi} \tag{104}$$

we immediately infer, that the real part of (103) is equal to the expression (99). From a comparison of equations (96), (102) and (104) we further conclude, that this equality is not limited to the case of only two lines in the loop but quite general. If A is never zero in the interval $0 \leqq \xi \leqq 1$, the right-hand side of (104) is real, and the two kernels obtained are identical. However, if A does vanish in the interval in question, the integral ($n \geqq 2$)

$$\int_{C_F} \frac{dk}{(k^2 + A)^{n+1}} \tag{105}$$

becomes infinite as[14]

$$\frac{i\pi^2}{n!}\left[(n-2)!\, P\frac{1}{A^{n-1}} + (-1)^n i\pi\, \delta^{(n-2)}(A)\right]. \tag{106}$$

As

$$\int dk\, \delta^{(n)}(k^2 + A) \tag{107}$$

gives only the real part of the right-hand side of (104), we have

$$\int dk\, \delta^{(n)}(k^2 + A) = (-1)^n \pi (n-2)!\, P\frac{1}{A^{n-1}}. \tag{108}$$

The vanishing of A will be related to the occurrence of real processes[15] and gives no trouble in the simple form of diagrams investigated up till now. Both the formalism of DYSON and the integration method developed here are only applicable, when the elements of the **S**-matrix are effectively equal to one, and it is not astonishing that they will give different results, when this condition is not fulfilled.

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[15] Compare DYSON[3] p. 1744.

The method of parametrization used to obtain (100) from (101) with the aid of (102) has been found convenient, as it is then especially simple to identify the divergences occurring in the theory.[3] It is, however, not impossible to apply (102) in other ways too. At first it would possibly seem natural to use only one set of auxiliary variables and write for (101) *e.g.*

$$-\frac{1}{\pi^2}\left(\frac{1}{2\pi}\right)^6 \int\int dq\,dq'\,\bar{u}(q)\,K(q,q')\,u(q')\,e^{ix(q'-q)} \tag{109}$$

with

$$\begin{aligned} K(q,q') = 5!\int_0^1 dx_1 \int_0^{x_1} dx_2 \ldots \int_0^{x_4} dx_5 \int\int dk\,dk'\,\gamma_\lambda(i\gamma(q+k)-m)\,\cdot \\ \cdot\,\gamma_\lambda(i\gamma q-m)\,\gamma_\mu(i\gamma q'-m)\,\gamma_\varrho(i\gamma(q'+k')-m)\,\gamma_\varrho\,\cdot \\ \cdot\,\{[(q'+k')^2+m^2]\,x_5+(q'^2+m^2)(x_4-x_5)+(q^2+m^2)(x_3-x_4)+ \\ +((q+k)^2+m^2)(x_2-x_3)+k^2(x_1-x_2)+k'^2(1-x_1)\}^{-6}. \end{aligned} \tag{110}$$

In this formulation, however, the divergences are not so easily separated as before. In our formalism, on the other hand, only the parametrization given in (98) is natural, and this choice of the auxiliary variables corresponds exactly to the parametrization of equations (100) and (103).

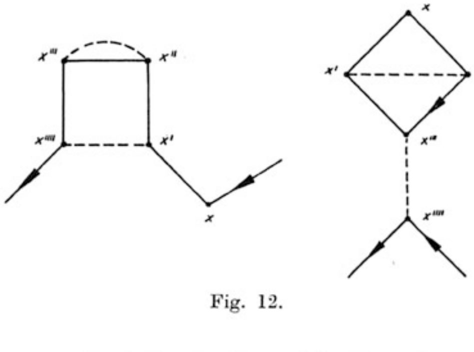

Fig. 12.

A slightly more complicated situation will arise, when two of the loops in the graph have one line in common. As simple examples of such diagrams we can use the two graphs in figure 12, both of which belong to the one-particle part of $j_\mu^{(4)}(x)$. From the calculations given earlier it can be seen without difficulties, that the expectation value of the ${}^{(1)}j_\mu^{(n)}(x)$ operator will give a sum of terms, each containing one $S^{(1)}(x)$ or $D^{(1)}(x)$ function in every loop. Each possible combination will occur, and *e.g.* for the first graph of figure 12 we obtain this expression is, in momentum space, conveniently written in the form (100), with $K(q)$ given by

$$K(q) = \frac{1}{i\pi}\int_0^1 d\xi \int_{C_F} dk \, \frac{\gamma_\lambda (i\gamma(q+k) - m)\gamma_\lambda}{k^2\xi + ((q+k)^2 + m^2)(1-\xi)}. \tag{103}$$

Introducing the well-known translation of the origin $k \to k + q(1-\xi)$ and observing that

$$\int dk\, \delta^{(n)}(k^2 + A) f(k^2) = Re \int_{C_F} \frac{dk\, f(k^2)}{(k^2 + A)^{n+1}} \frac{(-1)^n n!}{i\pi} \tag{104}$$

we immediately infer, that the real part of (103) is equal to the expression (99). From a comparison of equations (96), (102) and (104) we further conclude, that this equality is not limited to the case of only two lines in the loop but quite general. If A is never zero in the interval $0 \leqq \xi \leqq 1$, the right-hand side of (104) is real, and the two kernels obtained are identical. However, if A does vanish in the interval in question, the integral $(n \geqq 2)$

$$\int_{C_F} \frac{dk}{(k^2 + A)^{n+1}} \tag{105}$$

becomes infinite as[14]

$$\frac{i\pi^2}{n!}\left[(n-2)!\, P\frac{1}{A^{n-1}} + (-1)^n i\pi\, \delta^{(n-2)}(A)\right]. \tag{106}$$

As

$$\int dk\, \delta^{(n)}(k^2 + A) \tag{107}$$

gives only the real part of the right-hand side of (104), we have

$$\int dk\, \delta^{(n)}(k^2 + A) = (-1)^n \pi (n-2)!\, P\frac{1}{A^{n-1}}. \tag{108}$$

The vanishing of A will be related to the occurrence of real processes[15] and gives no trouble in the simple form of diagrams investigated up till now. Both the formalism of DYSON and the integration method developed here are only applicable, when the elements of the $\boldsymbol{S}$-matrix are effectively equal to one, and it is not astonishing that they will give different results, when this condition is not fulfilled.

[15] Compare DYSON[3] p. 1744.

The method of parametrization used to obtain (100) from (101) with the aid of (102) has been found convenient, as it is then especially simple to identify the divergences occurring in the theory.[3] It is, however, not impossible to apply (102) in other ways too. At first it would possibly seem natural to use only one set of auxiliary variables and write for (101) *e.g.*

$$-\frac{1}{\pi^2}\left(\frac{1}{2\pi}\right)^6 \iint d\,q\,d\,q'\,\bar{u}(q)\,K(q,q')\,u(q')\,e^{ix(q'-q)} \tag{109}$$

with

$$\begin{aligned} K(q,q') = 5!\int_0^1 d\,x_1 \int_0^{x_1} d\,x_2 \ldots \int_0^{x_4} d\,x_5 \iint d\,k\,d\,k'\,\gamma_\lambda\,(i\gamma(q+k)-m)\,\cdot \\ \cdot\,\gamma_\lambda\,(i\gamma q-m)\,\gamma_\mu\,(i\gamma q'-m)\,\gamma_\varrho\,(i\gamma(q'+k')-m)\,\gamma_\varrho\,\cdot \\ \cdot\,\{[(q'+k')^2+m^2]\,x_5+(q'^2+m^2)(x_4-x_5)+(q^2+m^2)(x_3-x_4)+ \\ +((q+k)^2+m^2)(x_2-x_3)+k^2(x_1-x_2)+k'^2(1-x_1)\}^{-6}. \end{aligned} \tag{110}$$

In this formulation, however, the divergences are not so easily separated as before. In our formalism, on the other hand, only the parametrization given in (98) is natural, and this choice of the auxiliary variables corresponds exactly to the parametrization of equations (100) and (103).

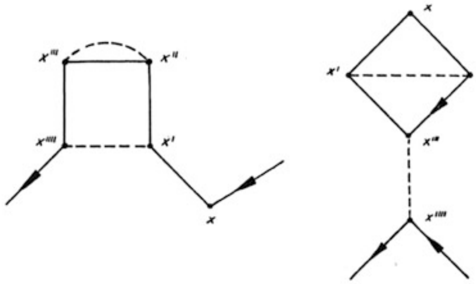

Fig. 12.

A slightly more complicated situation will arise, when two of the loops in the graph have one line in common. As simple examples of such diagrams we can use the two graphs in figure 12, both of which belong to the one-particle part of $j_\mu^{(4)}(x)$. From the calculations given earlier it can be seen without difficulties, that the expectation value of the ${}^{(1)}j_\mu^{(n)}(x)$ operator will give a sum of terms, each containing one $S^{(1)}(x)$ or $D^{(1)}(x)$ function in every loop. Each possible combination will occur, and *e.g.* for the first graph of figure 12 we obtain

$$\int \cdots \int d x' \ldots d x'''' \, \bar{\psi}^{(0)}(x'''') \, \gamma_\lambda \{ \bar{S}(x'''' - x''') \, \gamma_\nu \, \bar{S}(x''' - x'') \, \gamma_\nu \, \bar{S}(x'' - x') \cdot$$
$$\cdot D^{(1)}(x'''' - x') \, D^{(1)}(x''' - x'') + \bar{S}(x'''' - x''') \, \gamma_\nu \, \bar{S}(x''' - x'') \cdot$$
$$\cdot \gamma_\nu \, S^{(1)}(x'' - x') \, \bar{D}(x'''' - x') \, D^{(1)}(x''' - x'') + \bar{S}(x'''' - x''') \, \gamma_\nu \cdot$$
$$\cdot S^{(1)}(x''' - x'') \, \gamma_\nu \, \bar{S}(x'' - x') \, \bar{D}(x'''' - x') \, D^{(1)}(x''' - x'') +$$
$$+ S^{(1)}(x'''' - x''') \, \gamma_\nu \, \bar{S}(x''' - x'') \, \gamma_\nu \, \bar{S}(x'' - x') \, \bar{D}(x'''' - x') \cdot$$
$$\cdot D^{(1)}(x''' - x'') + \bar{S}(x'''' - x''') \, \gamma_\nu \, S^{(1)}(x''' - x'') \, \gamma_\nu \, S^{(1)}(x'' - x') \cdot$$
$$\cdot \bar{D}(x'''' - x') \, \bar{D}(x''' - x'') + S^{(1)}(x'''' - x''') \, \gamma_\nu \, S^{(1)}(x''' - x'') \cdot$$
$$\cdot \gamma_\nu \, \bar{S}(x'' - x') \, \bar{D}(x'''' - x') \, \bar{D}(x''' - x'') \} \, \gamma_\lambda \, \bar{S}(x' - x) \, \gamma_\mu \, \psi^{(0)}(x) \,. \qquad (111)$$

In these cases we also have contributions from the terms containing the operators ${}^{(2)}\psi^{(n)}(x)$ and ${}^{(2)}A_\mu^{(n)}(x)$. The expressions obtained in that way will contain one functions $S(x)$ or $D(x)$ in the line common to the two loops and another function of that kind in one of the other lines in the loop, not directly connected to the point x. Furthermore, those terms will appear with a minus-sign. For the graph belonging to equation (111) we get simply

$$- \int \cdots \int d x' \ldots d x'''' \, \bar{\psi}^{(0)}(x'''') \, \gamma_\lambda \, \bar{S}(x'''' - x''') \, \gamma_\nu \, S(x''' - x'') \, \gamma_\nu \cdot$$
$$\cdot \bar{S}(x'' - x') \, \gamma_\lambda \, \bar{S}(x' - x) \, \gamma_\mu \, \psi^{(0)}(x) \, \bar{D}(x'''' - x') \, D(x''' - x'') \,. \qquad (112)$$

The parametrization of the singular functions in momentum space can now be performed in the following way. For the terms containing a $S^{(1)}(x)$ or $D^{(1)}(x)$ function entirely in the loop directly connected to x, we combine all the expressions with this singular function in the same line with the aid of formula (96). If the momentum variable in the line in common is called p, and the corresponding variables in the loop not connected to x are $p + q'$, $p + q''$, $\ldots$ $p + q^n$, we obtain in this way

$$(-1)^n \int d p \int_0^1 d \xi_1 \int_0^{\xi_1} d \xi_2 \ldots \int_0^{\xi_{n-1}} d \xi_n \, \delta^{(n)}((p + Q)^2 + A) \qquad (113)$$

where

$$Q = q^n (\xi_{n-1} - \xi_n) + q^{n-1} (\xi_{n-2} - \xi_{n-1}) + \cdots + q' (1 - \xi_1) \qquad (113\text{ a})$$

and

$$A = m^2 + q^{n\,2} (\xi_{n-1} - \xi_n) + \cdots + q'^{\,2} (1 - \xi_1) - Q^2 \,. \qquad (113\text{ b})$$

The whole expression (113) is multiplied by

$$\frac{\delta(p'^{\,2})}{p''^{\,2} (p'''^{\,2} + m^2) \ldots (p^{i2} + m^2)} + \frac{\delta(p''^{\,2})}{p'^{\,2} (p'''^{\,2} + m^2) \ldots (p^{i2} + m^2)} + \cdots$$
$$\cdots + \frac{\delta(p^{i2} + m^2)}{p'^{\,2} p''^{\,2} \ldots ((p^{i-1})^2 + m^2)} \qquad (114)$$

where $p', \ldots p^i$ are the momentum variables of the loop connected to the point x. We have here made the assumption, that all the lines in the two loops whith the exception of two are electron lines. If some more of them are photon lines, equations (113 b) and (114) will be slightly changed, but as the modifications necessary are quite trivial, we will not investigate this possibility further. The remaining terms from ${}^{(1)}j_\mu^{(N)}(x)$ can obviously be written

$$(-1)^n \int d\,p\,\delta(p^2+m^2)\int_0^1 d\,\eta_1 \ldots \int_0^{\eta_{n-2}} d\,\eta_{n-1}\,\delta^{(n-1)}((p+Q')^2+A') \tag{115}$$

where

$$Q' = q^n\,\eta_{n-1} + q^{n-1}(\eta_{n-2}-\eta_{n-1}) + \cdots + q'(1-\eta_1) \tag{115 a}$$

and

$$A' = q^{n\,2}\,\eta_{n-1} + q^{n-1\,2}(\eta_{n-2}-\eta_{n-1}) + \cdots + q'^{\,2}(1-\eta_1) + m^2 - Q'^{\,2}. \tag{115 b}$$

The terms from ${}^{(2)}j_\mu^{(N)}(x)$ can analogously be combined in the following way

$$(-1)^n \int_0^1 d\,\eta_1 \ldots \int_0^{\eta_{n-2}} d\,\eta_{n-1} \int d\,p\,\delta(p^2+m^2)\,\varepsilon(p)\cdot$$
$$\cdot\,\delta^{(n-1)}((p+Q')^2+A')\,\varepsilon(p+Q') \tag{116}$$

with the same notations A' and Q' as before. The expression (116) is, if no real processes can take place ($A' > 0$), equal to

$$-\frac{(n-1)!}{\pi^2}\int_0^1 d\,\eta_1 \ldots \int_0^{\eta_{n-2}} d\,\eta_{n-1}\,P\int \frac{d\,p}{(p^2+m^2)((p+Q')^2+A')^n} \tag{117}$$

and thus (115) and (116) will combine to

$$(-1)^{n-1}\int_0^1 d\,\eta_1 \ldots \int_0^{\eta_{n-2}} d\,\eta_{n-1}\int d\,p\left\{\delta(p^2+m^2)\,\delta^{(n-1)}((p+Q')^2+A') - \right.$$
$$\left. - \frac{1}{\pi^2}\,\frac{(-1)^{n-1}(n-1)!}{(p^2+m^2)((p+Q')^2+A')^n}\right\}. \tag{118}$$

Using the well-known integral representations

$$\delta(a) = \frac{1}{2\pi}\int d\,w\,e^{i\,w\,a} \tag{119}$$

$$P\,\frac{1}{a} = \frac{1}{2\,i}\int d\,w\,\frac{w}{|w|}\,e^{i\,w\,a} \tag{120}$$

we now obtain

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$$\frac{(-1)^{n-1}}{4\pi^2}\int_0^1 d\eta_1 \ldots \int_0^{\eta_{n-2}} d\eta_{n-1} \iint dw_1\, dw_2 \int dp\, e^{iw_1(p^2+m^2)+iw_2((p+Q')^2+A')} \cdot$$

$$\cdot (i w_2)^{n-1}\left(1+\frac{w_1}{|w_1|}\cdot\frac{w_2}{|w_2|}\right) = \frac{(-i)^{n-1}}{2\pi^2}\int_0^1 d\eta_1 \ldots \int_0^{\eta_{n-2}} d\eta_{n-1} \cdot$$

$$\cdot \int_0^1 d\alpha\,(1-\alpha)^{n-1}\int dw\; w^{n-1}|w|\int dp\, e^{iw[p^2+2pQ'(1-\alpha)+m^2\alpha+(A'+Q'^2)(1-\alpha)]} =$$

$$= -\tfrac{1}{2}(-i)^n\int_0^1 d\alpha\,(1-\alpha)^{n-1}\int_0^1 d\eta_1 \ldots \int_0^{\eta_{n-2}} d\eta_{n-1}\int dw\; w^{n-2}\, e^{iwB} =$$

$$= (-1)^n\cdot\pi\int_0^1 d\alpha\,(1-\alpha)^{n-1}\int_0^1 d\eta_1 \ldots \int_0^{\eta_{n-2}} d\eta_{n-1}\,\delta^{(n-2)}(B) \tag{121}$$

where

$$B = m^2 - Q'^2(1-\alpha)^2 + (A'-m^2+Q'^2)(1-\alpha) = m^2 + (q^{n\,2}\eta_{n-1} + \cdots$$
$$\cdots + q'^2(1-\eta_1))(1-\alpha) - Q'^2(1-\alpha)^2. \tag{121 a}$$

In the derivation of (121) the transformation

$$\begin{aligned} w_1 &= w\alpha \\ w_2 &= w(1-\alpha) \end{aligned} \tag{121 b}$$

has been used. The new variable-transformation

$$\begin{aligned} \xi_{n-1}-\xi_n &= (1-\alpha)\,\eta_{n-1} \\ \xi_{n-2}-\xi_{n-1} &= (1-\alpha)(\eta_{n-2}-\eta_{n-1}) \\ &\cdots\cdots\cdots\cdots \\ \xi_2-\xi_1 &= (1-\alpha)(\eta_2-\eta_1) \\ 1-\xi_1 &= (1-\alpha)(1-\eta_1) \end{aligned} \tag{122}$$

will change B to A of (113 b) and $Q'(1-\alpha)$ to Q of (113 a). The expression (121) can then be written

$$(-1)^n\pi\int_0^1 d\xi_1 \ldots \int_0^{\xi_{n-1}} d\xi_n\,\delta^{(n-2)}(A). \tag{123}$$

The p-integration in (113) can be performed according to well-known rule and we obtain

$$\int d\,p\,\delta^{(n)}((p+Q)^2+A)=\frac{(-1)^{n-2}(n-2)!}{A^{n-1}}\,\pi. \tag{124}$$

The expression (123) is multiplied by

$$[p'^{\,2}\cdot p''^{\,2}\cdot(p'''^{\,2}+m^2)\,\ldots\,(p^{i\,2}+m^2)]^{-1} \tag{125}$$

where $p', \ldots p^i$ have the same meaning as in (114). Equations (114), (124), (123) and (125) thus combine to give

$$\pi\int\limits_0^1 d\,\xi_1\,\ldots\int\limits_0^{\xi_{n-1}} d\,\xi_n\left\{\left[\frac{\delta(p'^{\,2})}{p''^{\,2}\ldots(p^{i\,2}+m^2)}+\cdots+\frac{\delta(p^{i\,2}+m^2)}{p'^{\,2}\ldots((p^{i-1})^2+m^2)}\right]\cdot\right.$$
$$\left.\cdot\frac{(n-2)!}{A^{n-1}}+\frac{(-1)^n}{p'^{\,2}\ldots(p^{i\,2}+m^2)}\,\delta^{(n-2)}(A)\right\}. \tag{126}$$

With a simple modification of (96) (some of the numbers a_i equal) we can introduce a new set of auxiliary variables and perform the remaining momentum variable integrations without any new difficulties of a fundamental nature appearing. The parametrization obtained in this way again corresponds to the most convenient one used by DYSON[3], and the result of the integration is equal to the real part of the formula obtained with the use of the $S_F(x)$ and $D_F(x)$ functions.

At this point it is perhaps not necessary to go further and to write down explicitly the expectation values, when the loops have more than one line in common, or when more than two loops are mixed together. Also in these cases we get one term of the form A^{-n} from the "normal" terms in the loops and contributions containing $\delta^{(n-1)}(A)$ and $\delta^{(n-1)}(A)\cdot\varepsilon(Q)$ from the others. The last expression will appear, when more than two loops are mixed together. We then get terms of the following form

$$\int d\,p\,\delta(p^2+m^2)\,\delta^{(n-1)}((p+Q')^2+A')\,(\varepsilon(p+Q')-\varepsilon(p)) \tag{116'}$$

Comparing (115), (116) and (121), we conclude

$$\int d\,p\,\delta(p^2+m^2)\,\delta^{(n-1)}((p+Q')^2+A')\,(1-\varepsilon(p)\,\varepsilon(p+Q'))=$$
$$=\pi\int\limits_0^1 d\,\alpha\,(1-\alpha)^{n-1}\,\delta^{(n-2)}(A'(1-\alpha)+m^2\,\alpha+Q'^{\,2}\,\alpha\,(1-\alpha)). \tag{121'}$$

This result can be interpreted geometrically. To get a contribution from the left-hand side of (121'), we must have

$$p^2 + m^2 = 0$$

$$(p^2 + Q')^2 + A' = 0$$

$$\frac{p_0}{|p_0|} \cdot \frac{p_0 + Q'_0}{|p_0 + Q'_0|} = -1 .$$

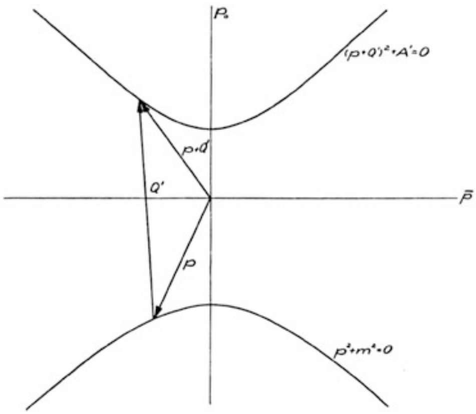

Fig. 13.

These equations cannot be fulfilled at the same time, if we do not have (compare figure 13)

$$|Q'_0| > m + \sqrt{A'}$$

in *all* coordinate systems. This can be written in an invariant way as

$$Q'^2 > -(m + \sqrt{A'})^2$$

which is exactly the condition, that

$$A'(1-\alpha) + m^2\alpha + Q'^2\alpha(1-\alpha)$$

will vanish for some value of α in the interval (0, 1). Now, the integral (116′) gets contributions from exactly the same values of p as the integral (121′), and the only difference between the two expressions lies in the signs. We thus obtain

$$\int dp\, \delta(p^2 + m^2)\, \delta^{(n-1)}((p + Q')^2 + A')\, (\varepsilon(p + Q') - \varepsilon(p)) =$$

$$= \pi \int_0^1 d\alpha\, (1-\alpha)^{n-1}\, \varepsilon(Q')\, \delta^{(n-2)}(A'(1-\alpha) + m^2\alpha + Q'^2\alpha(1-\alpha)). \qquad (121'')$$

The restrictions on the graphs introduced above are in no way essential for the argument and were only established in order that we should not be burdened with too complicated formulae. It seems to be a general property of the higher approximations of the present form of quantum theory that, although they can be comparatively simply understood from a general point of view, they give nevertheless rise to very cumbersome computations, if we try to make an explicit calculation for a special problem.

It now remains to include the mass-renormalization terms of (55) in the calculation. This can be done comparatively simply by noting that δm in the equation

$$\left(\gamma \frac{\partial}{\partial x} + m\right) \psi(x) = \frac{ie}{2} \{A_\mu(x), \gamma_\mu \psi(x)\} + \delta m \, \psi(x) \tag{127}$$

plays a role similar to that of $i e \gamma_\mu A_\mu(x)$. Introducing a new vector

$$A'_\mu(x) = A_\mu(x) - \frac{i}{4e} \gamma_\mu \cdot \delta m \tag{128}$$

we have

$$\left(\gamma \frac{\partial}{\partial x} + m\right) \psi(x) = \frac{ie}{2} \{A'_\mu(x), \gamma_\mu \psi(x)\} \tag{129}$$

$$\Box A'_\mu(x) = -\frac{ie}{2} [\bar{\psi}(x), \gamma_\mu \psi(x)]. \tag{130}$$

We thus obtain the solution to (127), if we substitute the *total* δm (including all powers of e) in one or more of the places for the operators $i e \gamma_\nu A_\nu^{(0)}(x)$ in the solution of the equation without mass-renormalization term. These substitutions have to be made in all possible ways and the results added together. We will not investigate this point further, as the calculation would be merely a repetition of the argument given by Dyson.

6. Particles with integer spin

In (I) the interaction of particles of spin zero with an external electromagnetic field was studied in some detail. It was, however, also observed, that no serious complications will arise, if the electromagnetic field is quantized. In this case we have the following system of equations

$$(\Box - m^2)\varphi(x) = \frac{ie}{2}\left(\left\{A_\nu(x), \frac{\partial \varphi(x)}{\partial x_\nu}\right\} + \frac{\partial}{\partial x_\nu}\{A_\nu(x), \varphi(x)\}\right) + \\ + \frac{e^2}{2}\{A_\nu(x) A_\nu(x), \varphi(x)\} \tag{131}$$

$$\Box A_\mu(x) = \frac{ie}{2}\left\{\left(\varphi^*(x) \frac{\partial}{\partial x_\mu} - \frac{\partial \varphi^*(x)}{\partial x_\mu}\right), \varphi(x)\right\} + e^2 A_\mu(x) \{\varphi^*(x), \varphi(x)\}. \tag{132}$$

Expanding in powers of e as before, we obtain for the first few approximations (compare (I) p. 190)

$$\varphi^{(1)}(x) = -i \int A_\nu^{(0)}(x') \left(\overline{\triangle}(x-x') \frac{\partial}{\partial x'_\nu} + \frac{\partial \overline{\triangle}(x-x')}{\partial x_\nu} \right) \varphi^{(0)}(x')\, dx' \quad (133)$$

$$A_\mu^{(1)}(x) = -\frac{i}{2} \int \bar{D}(x-x') \left\{ \left(\varphi^{(0)*}(x') \frac{\partial}{\partial x'_\nu} - \frac{\partial \varphi^{(0)*}(x')}{\partial x'_\nu} \right), \varphi^{(0)}(x') \right\} \delta_{\mu\nu}\, dx' \quad (134)$$

$$\varphi^{(2)}(x) = (-i)^2 \int\int dx'\, dx'' \left[A(1,2) \left\{ \left[\overline{\triangle}(x-x') \frac{\partial}{\partial x'_{\nu_1}} + \frac{\partial \overline{\triangle}(x-x')}{\partial x_{\nu_1}} \right] \cdot \right.\right.$$

$$\left. \cdot \left[\overline{\triangle}(x'-x'') \frac{\partial}{\partial x''_{\nu_2}} + \frac{\partial \overline{\triangle}(x'-x'')}{\partial x'_{\nu_2}} \right] + \overline{\triangle}(x-x')\, \delta(x'-x'')\, \delta_{\nu_1 \nu_2} \right\} \varphi^{(0)}(x'') +$$

$$+ \delta_{\nu_1 \nu_2} \bar{D}(x'-x'') \left[\overline{\triangle}(x-x') \frac{\partial}{\partial x'_{\nu_1}} + \frac{\partial \overline{\triangle}(x-x')}{\partial x_{\nu_1}} \right] \tfrac{1}{4} \cdot$$

$$\left. \cdot \left\{ \varphi^{(0)}(x'), \left\{ \left(\varphi^{(0)*}(x'') \frac{\partial}{\partial x''_{\nu_2}} - \frac{\partial \varphi^{(0)*}(x'')}{\partial x''_{\nu_2}} \right), \varphi^{(0)}(x'') \right\} \right\} \right] \quad (135)$$

$$A_\mu^{(2)}(x) = (-i)^2 \int\int dx'\, dx''\, \tfrac{1}{2} \left[\left\{ \left[\varphi^{(0)*}(x') \frac{\partial}{\partial x'_{\nu_1}} - \frac{\partial \varphi^{(0)*}(x')}{\partial x'_{\nu_1}} \right], \right.\right.$$

$$\left. \cdot \left[\overline{\triangle}(x'-x'') \frac{\partial}{\partial x''_{\nu_2}} + \frac{\partial \overline{\triangle}(x'-x'')}{\partial x'_{\nu_2}} \right] \varphi^{(0)}(x'') \right\} +$$

$$\left. + \delta(x'-x'')\, \delta_{\nu_1 \nu_2} \{ \varphi^{(0)*}(x'), \varphi^{(0)}(x'') \} \right] \cdot [\bar{D}(x-x')\, \delta_{\mu\nu_1} A_{\nu_2}^{(0)}(x'') +$$

$$+ \bar{D}(x-x'')\, \delta_{\mu\nu_2} A_{\nu_1}^{(0)}(x')]. \quad (136)$$

It is clear, that the general structure of the solutions to (131) and (132) is, apart from some differences in signs, very similar to that of the solutions to (78) and (79). We have only to introduce expressions of the form

$$\left[\overline{\triangle}(x-x') \frac{\partial}{\partial x'_{\nu_1}} + \frac{\partial \overline{\triangle}(x-x')}{\partial x_{\nu_1}} \right] \cdots \left[\overline{\triangle}(x^{n-1}-x^n) \frac{\partial}{\partial x^n_{\nu_n}} + \frac{\partial \overline{\triangle}(x^{n-1}-x^n)}{\partial x^{n-1}_{\nu_n}} \right] +$$

$$+ \sum_{i=1}^{n-1} \left[\overline{\triangle}(x-x') \frac{\partial}{\partial x'_{\nu_1}} + \frac{\partial \overline{\triangle}(x-x')}{\partial x_{\nu_1}} \right] \cdots$$

$$\cdots \left[\overline{\triangle}(x^{i-2}-x^{i-1}) \frac{\partial}{\partial x^{i-1}_{\nu_{i-1}}} + \frac{\partial \overline{\triangle}(x^{i-2}-x^{i-1})}{\partial x^{i-2}_{\nu_{i-1}}} \right] \cdot$$

$$\cdot \overline{\triangle}(x^{i-1}-x^i)\, \delta_{\nu_i \nu_{i+1}}\, \delta(x^i - x^{i+1}) \cdot$$

$$\cdot \left[\overline{\triangle} (x^{i+1} - x^{i+2}) \frac{\partial}{\partial x^{i+2}_{\nu_{i+2}}} + \frac{\partial \overline{\triangle} (x^{i+1} - x^{i+2})}{\partial x^{i+1}_{\nu_{i+2}}} \right] \cdots$$

$$\cdots \left[\overline{\triangle} (x^{n-1} - x^n) \frac{\partial}{\partial x^n_{\nu_n}} + \frac{\partial \overline{\triangle} (x^{n-1} - x^n)}{\partial x^{n-1}_{\nu_n}} \right] + \sum_{i<j} \cdots + \cdots \tag{137}$$

(compare (I) equations (17) and (18)) instead of the factors

$$\bar{S} (x - x') \gamma_{\nu_1} \bar{S} (x' - x'') \gamma_{\nu_2} \ldots \gamma_{\nu_{n-1}} \bar{S} (x^{n-1} - x^n) \gamma_{\nu_n} \tag{138}$$

and

$$\left\{ \left[\varphi^{(0)*} (x') \frac{\partial}{\partial x'_{\nu_1}} - \frac{\partial \varphi^{(0)*} (x')}{\partial x'_{\nu_1}} \right], \left[\overline{\triangle} (x' - x'') \frac{\partial}{\partial x''_{\nu_2}} + \frac{\partial \overline{\triangle} (x' - x'')}{\partial x'_{\nu_2}} \right] \cdots \right.$$

$$\left. \cdots \left[\overline{\triangle} (x^{n-1} - x^n) \frac{\partial}{\partial x^n_{\nu_n}} + \frac{\partial \overline{\triangle} (x^{n-1} - x^n)}{\partial x^{n-1}_{\nu_n}} \right] \varphi^{(0)} (x^n) \right\} +$$

$$+ \sum_{i=1}^{n-1} \left\{ \left[\varphi^{(0)*} (x') \frac{\partial}{\partial x'_{\nu_1}} - \frac{\partial \varphi^{(0)*} (x')}{\partial x'_{\nu_1}} \right], \ldots \overline{\triangle} (x^{i-1} - x^i) \cdot \right.$$

$$\left. \cdot \delta_{\nu_i \nu_{i+1}} \delta (x^i - x^{i+1}) \ldots \varphi^{(0)} (x^n) \right\} + \sum_{i<j} \cdots + \cdots \tag{139}$$

instead of

$$[\bar{\psi}^{(0)} (x'), \gamma_{\nu_1} \bar{S} (x' - x'') \ldots \bar{S} (x^{n-1} - x^n) \gamma_{\nu_n} \psi^{(0)} (x^n)]. \tag{140}$$

(The modifications necessary in (139) for i equal to one are obvious and, for a special case, written down explicitly in (136)). The analysis of the higher approximations is now identical for particles of spin $\frac{1}{2}$ and spin 0 and will not be repeated again.

It is obvious that an argument similar to that given above can be applied to all kinds of mesons in interaction with nucleons or an electromagnetic field. If only the operator equations are covariant, we will always get an invariant result out of the calculation without any elimination of "surface terms". As another simple example we can take the case of pseudoscalar mesons (operator $\varphi(x)$, mass m) in vector interaction with nucleons (operator $\psi(x)$, mass M). The differential equations are here

$$\left(\gamma \frac{\partial}{\partial x} + M \right) \psi (x) = \frac{i g}{2} \left\{ \frac{\partial \varphi (x)}{\partial x_\nu}, \gamma_5 \gamma_\nu \psi (x) \right\} \tag{141}$$

$$(\Box - m^2) \varphi (x) = \frac{i g}{2} \frac{\partial}{\partial x_\nu} [\bar{\psi} (x), \gamma_5 \gamma_\nu \psi (x)] \tag{142}$$

$$\gamma_5 = \gamma_1 \gamma_2 \gamma_3 \gamma_4 . \tag{143}$$

The recurrence formulae corresponding to (78) and (79) are

$$\psi^{(n+1)}(x) = -\frac{i g}{2}\int \bar{S}(x-x')\sum_{m=0}^{n}\left\{\frac{\partial \varphi^{(m)}(x')}{\partial x'_\nu},\ \gamma_5\gamma_\nu\psi^{(n-m)}(x')\right\}dx' \tag{144}$$

$$\varphi^{(n+1)}(x) = -\frac{i g}{2}\int \frac{\partial \overline{\triangle}(x-x')}{\partial x_\nu}\sum_{m=0}^{n}[\bar{\psi}^{(m)}(x'),\ \gamma_5\,\gamma_\nu\,\psi^{(n-m)}(x')]\,dx'. \tag{145}$$

It is now evident that we obtain the solutions to (144) and (145) from the solutions to (78) and (79) through the following replacements

$$A_\mu^{(0)}(x) \rightarrow \frac{\partial \varphi^{(0)}(x)}{\partial x_\mu} \tag{146 a}$$

$$\gamma_\mu \rightarrow \gamma_5\gamma_\mu \tag{146 b}$$

$$\bar{D}(x'-x'')\,\delta_{\nu_1\nu_2} \rightarrow \frac{\partial^2\,\overline{\triangle}(x'-x'')}{\partial x'_{\nu_1}\,\partial x''_{\nu_2}}. \tag{146 c}$$

From equation (146 c) we infer, that the expectation values will, in the n^{th} approximation, contain *n more* momentum variables than the corresponding expressions in quantum electrodynamics. The divergences are thus much more serious in this kind of meson theory than in the case of particles of spin one half in interaction with an electromagnetic field.[16]

The other cases of meson-nucleon interaction can be treated in similar ways.

7. Appendix

In this appendix we will investigate a point, which, although it lies a little outside the main discussion, is very conveniently studied in the HEISENBERG representation. We will not carry through the calculations in all approximations, but restrict ourselves to the e^2-terms. Let us consider the one-particle part of the current operator for particles of spin one half. In the notations used in (I) this expression can be written (compare (I) equations (35)–(37))

$$\begin{aligned} < j_\mu^{(2)}(x) >_1 = \frac{i}{2}\int [\bar{\psi}^{(0)}(x),\ \gamma_\mu\bar{S}(x-x')(\phi(x') - \delta m^{(2)}\psi^{(0)}(x'))]_1\,dx' + \\ + \frac{i}{2}\int [(\bar{\phi}(x') - \delta m^{(2)}\bar{\psi}^{(0)}(x'))\bar{S}(x'-x)\gamma_\mu,\ \psi^{(0)}(x)]_1\,dx' - \\ - \frac{i}{4}\int\int [\bar{\psi}^{(0)}(x'),\ K_\mu(x'-x,\ x-x'')\,\psi^{(0)}(x'')]_1\,dx'\,dx'' + \text{vac. pol.} \end{aligned} \tag{A. 1}$$

The operator $\phi(x)$ is logarithmically divergent but, using a regularization with respect to the photon mass[17], we obtain

[16] P. T. MATHEWS, Phil. Mag. *41*, 185 (1950).

[17] W. PAULI–F. VILLARS, Rev. Mod. Phys. *21* 434 (1949). Compare also the paper by JOST and LUTTINGER.[14]

$$\phi(x) = \frac{M^2}{(2\pi)^7} \int\int d\,p\,d\,k \int d\,x'\,(i\gamma p + 2\,m)\,e^{i(p+k)(x-x')}\,\psi^{(0)}(x') \cdot$$
$$\cdot \left\{ \frac{\delta(p^2+m^2)}{k^2(k^2+M^2)} + \frac{\delta(k^2)}{(p^2+m^2)(k^2+M^2)} + \frac{\delta(k^2+M^2)}{(p^2+m^2)\,k^2} \right\}. \quad \text{(A. 2)}$$

This expression can be calculated according to well-known rules, and we obtain

$$\phi(x) = \phi_1(x) + \phi_2(x) \quad \text{(A. 3)}$$

$$\phi_1(x) = \frac{\pi M^2}{(2\pi)^7} \int_0^1 d\,u \int_0^u d\,v \int d\,q \int d\,x'\,e^{iq(x-x')} \frac{u\,(i\gamma q + m)\,\psi^{(0)}(x')}{q^2 u(1-u) + m^2(1-u) + M^2 v} \quad \text{(A. 4)}$$

$$\phi_2(x) = \frac{\pi M^2}{(2\pi)^7} \int_0^1 d\,u \int_0^u d\,v \int d\,q \int d\,x'\,e^{iq(x-x')} \frac{(2-u)\,\psi^{(0)}(x')}{q^2 u(1-u) + m^2(1-u) + M^2 v}. \quad \text{(A. 5)}$$

As $\psi^{(0)}(x)$ fulfils the DIRAC equation for free particles, we can use

$$q^2 + m^2 = 0 \quad \text{(A. 6)}$$

in the *denominators*, and obtain

$$\phi_1(x) = \frac{1}{8\,\pi^2} \int_0^1 u\,d\,u\,\log\left(1 + \frac{M^2 u}{m^2(1-u)^2}\right)\left(\gamma\frac{\partial}{\partial x} + m\right)\psi^{(0)}(x) \quad \text{(A. 7)}$$

$$\phi_2(x) = \frac{m}{8\,\pi^2} \int_0^1 (2-u)\,d\,u\,\log\left(1 + \frac{M^2 u}{m^2(1-u)^2}\right)\psi^{(0)}(x). \quad \text{(A. 8)}$$

The infinity (dependence of M) in $\phi_2(x)$ can be cancelled, if we define

$$\delta m^{(2)} = \frac{m}{8\,\pi^2} \int_0^1 d\,u\,(2-u)\,\log\left(1 + \frac{M^2 u}{m^2(1-u)^2}\right). \quad \text{(A. 9)}$$

It is *not* permitted to put

$$\left(\gamma\frac{\partial}{\partial x'} + m\right)\psi(x') = 0 \quad \text{(A. 10)}$$

in (A. 7) as this expression is multiplied by $\bar{S}(x-x')$ and integrated over x'. As

$$\left(\gamma\frac{\partial}{\partial x} + m\right)\bar{S}(x-x') = -\,\delta(x-x') \quad \text{(A. 11)}$$

the term

$$\int \bar{S}(x-x')\,\phi_1(x')\,dx' \tag{A. 12}$$

is indeterminate and can be given any value of the form

$$-\xi\frac{1}{8\pi^2}\int_0^1 u\,du\,\log\left(1+\frac{M^2 u}{m^2(1-u)^2}\right)\psi^{(0)}(x) \tag{A. 13}$$

with ξ an arbitrary constant.

The term containing $K_\mu(x'-x;\,x-x'')$ in (A. 1) also contains a divergency. This can be separated in the usual way[18], and we get, after regularization

$$-\frac{1}{4\pi^2}\gamma_\mu\,\delta(x'-x)\,\delta(x-x'')\int_0^1 u\,du\,\log\left(1+\frac{M^2 u}{m^2(1-u)^2}\right). \tag{A. 14}$$

Collecting the terms from (A. 13), its Hermitian conjugate and (A. 14), we obtain

$$\frac{i}{16\pi^2}\int_0^1 du\cdot u\cdot\log\left(1+\frac{M^2 u}{m^2(1-u)^2}\right)(1-2\xi)\,[\bar{\psi}^{(0)}(x),\,\gamma_\mu\psi^{(0)}(x)]_1. \tag{A. 15}$$

The second approximation of the current operator thus contains an indeterminate multiple of the zeroth-approximation current. From a mathematical point of view, this corresponds to a solution of the homogeneous equation added to the expression given *e.g.* in equation (49). Such an extra term ought to have been avoided according to our boundary conditions (5) and the use of the retarded functions. This is, however, not the case and is probably connected with the fact that the operators $\psi^{(0)}(x)$ do not vanish for $x_0\to-\infty$. Hence, the solutions (49) and (50) do not strictly fulfil (51). According to Dyson[3], this indeterminacy is avoided by observing that the normalization of $\psi^{(0)}(x)$ must not be altered by the electromagnetic interaction. The normalization condition for the electron field is, however, just the time-component of the current operator, and hence (A. 15) must vanish. This means

$$\xi = \tfrac{1}{2}$$

and agrees in the e^2-approximation with the result of Dyson.[19]

Department of Mechanics and Mathematical Physics, University of Lund.

[18] J. Schwinger, Phys. Rev. *76*, 790 (1949).
[19] Cf. also R. Karplus–N. M. Kroll, Phys. Rev. *77*, 542 (1950).

Tryckt den 17 oktober 1950

Uppsala 1950. Almqvist & Wiksells Boktryckeri AB

Errata

p. 372 Top line: Read "theory".

p. 376 Line after Eq. 22: Read "At last".

p. 381 Eq (44): The two lines on the right-hand side must be interchanged.

p. 384 A factor $-e$ is missing in front of the integral sign in the last equation. The same factor is also missing in the first equation on the next page.

p. 386 Eq. (64): All the singular functions in this expression are $D^{(1)}$-functions.

p. 387 Eq. (68): Substitute $A^{(m)(k)}(3, \ldots n)$ for $A^{(m)(k)}(2,3, \ldots n)$.

p. 390 Eq. (81 b): A factor $dx'dx''$ is missing.
Eq. (82 b): Substitute $\bar{S}(3,1,2)$ for $\bar{S}(3,2,1)$.

p. 392 The index j in (91) has to be summed over all values from 1 to n excepting ...

p. 393 Eq. (92): Substitute $i(-1)^r$ for $(-i)^r$.

p. 397 Eq. (101): A factor $D_F(x'' - x') \cdot D_F(x'''' - x''')$ is missing.

p. 398 Eq. (103): Read $\dfrac{\gamma_\lambda(i\gamma(q+k) - m)\gamma_\lambda}{[k^2\xi + ((q+k)^2 + m^2)(1-\xi)]^2}$.

p. 401 Third line: Read "with".

p. 402 Last line: Read "according to well-known rules".

p. 404 Second line: Read $(p + Q')^2 + A' = 0$.
Middle of the page: Read $Q'^2 < -(m + \sqrt{A'})^2$.

p. 409 Eq. (A.5): Read $m(2-u)\psi^{(0)}(x')$.

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Paper [1952a]: On the Definition of the Renormalization Constants in Quantum Electrodynamics

Helv. Phys. Acta 25 (1952) 417

Pauli was very impressed by this paper which he labeled as Källén "looking behind the veil of Dyson's power series". He wrote about it to several people (see the chapter on "Non-perturbative Renormalization" in Part 4).

In this paper Källén takes his first steps to go beyond perturbation theory in quantum electrodynamics. He uses the Heisenberg representation for the electron and the photon field operators and expresses the renormalization constants in terms of spectral functions. The charge renormalization constant L emerges in the vacuum expectation value of the commutator of the photon field and its time-derivative (see Eq. (40)) and is related to Källén's weight function Π which appears in the vacuum expectation value of the commutator of the current operator at two different space-time points. The definition of the mass renormalization constant K is carried through in a similar way by considering the vacuum expectation value of the anticommutator of the fermionic current (the right-hand side of the Dirac equation) taken at two different space-time points.

C. Jarlskog (Ed.), *Portrait of Gunnar Källén*, DOI 10.1007/978-3-319-00627-7_90,

Separatum
HELVETICA PHYSICA ACTA
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On the Definition of the Renormalization Constants in Quantum Electrodynamics

by **Gunnar Källén.***)
Swiss Federal Institute of Technology, Zürich.

(14. II. 1952.)

Summary. A formulation of quantum electrodynamics in terms of the renormalized Heisenberg operators and the experimental mass and charge of the electron is given. The renormalization constants are implicitly defined and expressed as integrals over finite functions in momentum space. No discussion of the convergence of these integrals or of the existence of rigorous solutions is given.

Introduction.

The renormalization method in quantum electrodynamics has been investigated by many authors, and it has been proved by DYSON[1]) that every term in a formal expansion in powers of the coupling constant of various expressions is a finite quantity. No serious attempt at a discussion of the convergence of the series has been published, and the definition of the renormalization constants is always given as a formal series where every coefficient is infinite. It is the aim of the present paper to give a formulation of quantum electrodynamics where only the renormalized operators (in the Heisenberg representation) will appear and where the renormalization constants are defined in terms of these operators and the experimental mass and charge of the electron. There thus exists a possibility of studying the renormalized quantities directly without the aid of a power series expansion and especially to decide if they are really finite and not only a divergent sum of finite terms. No discussion of this point, however, will be given in this paper, only the formulation of the theory.

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*) On leave of absence from Department of Mechanics and Mathematical Physics, University of Lund, Sweden.

[1]) F. J. DYSON, Phys. Rev. **83**, 608; 1207 (1951) and earlier papers.

The starting point of our analysis is the following formal Lagrangian

$$\mathfrak{L} = -\frac{N^2}{4}\left[\bar{\psi}(x), \left(\gamma\frac{\partial}{\partial x}+m\right)\psi(x)\right] - \frac{N^2}{4}\left[-\frac{\partial\bar{\psi}(x)}{\partial x_\nu}\gamma_\nu + m\bar{\psi}(x), \psi(x)\right] +$$
$$+\frac{K}{2}\cdot N^2[\bar{\psi}(x),\psi(x)] - \frac{1}{2}\frac{\partial A_\nu(x)}{\partial x_\nu}\frac{\partial A_\mu(x)}{\partial x_\mu} -$$
$$-\frac{1-L}{4}\left(\frac{\partial A_\nu(x)}{\partial x_\mu}-\frac{\partial A_\mu(x)}{\partial x_\nu}\right)\left(\frac{\partial A_\nu(x)}{\partial x_\mu}-\frac{\partial A_\mu(x)}{\partial x_\nu}\right)+$$
$$+\frac{ie}{4}\cdot N^2\{A_\mu(x), [\bar{\psi}(x), \gamma_\mu\psi(x)]\}. \tag{1}$$

In this expression $A_\mu(x)$ is the renormalized vector-potential of the electromagnetic field, $\psi(x)$ the renormalized Dirac-operator of the electron-positron field, m and e the experimental mass and charge of the electron, and K, L and N three universal constants, the definition of which will be given later. The three quantities K, N^{-1} and $(1-L)^{-1}$ might be infinite but in spite of that, we will adopt the convention that the usual algebraic operations can be performed with them. K and L describe respectively the mass- and the charge-renormalization, and N is a normalization constant for the ψ-field. The other notations are nowadays standard symbols in quantum field theory.

From the above Lagrangian we obtain without difficulty the following equations of motion for the field operators in the Heisenberg representation

$$\left(\gamma\frac{\partial}{\partial x}+m\right)\psi(x) = \frac{ie}{2}\{A_\nu(x),\gamma_\nu\psi(x)\} + K\psi(x) \equiv f(x) \tag{2}$$

$$\Box A_\mu(x) = \frac{-ie}{2}N^2[\bar{\psi}(x),\gamma_\mu\psi(x)] + L\left(\Box A_\mu(x) - \frac{\partial^2 A_\nu(x)}{\partial x_\mu\partial x_\nu}\right) \equiv -j_\mu(x). \tag{3}$$

In Eq. (2) and (3) $f(x)$ and $j_\mu(x)$ are only to be considered as abbreviations for the right-hand sides.

From our Lagrangian we can also obtain the commutators for the electromagnetic operators and their time-derivatives in two points, the distance of which is space-like, and the corresponding anticommutators for the electron field. As the terms with K and L contain the time-derivatives of $A_\mu(x)$ but not of $\psi(x)$, the canonical commutators involving the electromagnetic potentials will be rather complicated and really meaningless if $L = 1$, but the anticommutators of the matter-field will have the simple form

$$\{\bar{\psi}_\alpha(x), \psi_\beta(x')\} = (\gamma_4)_{\beta\alpha}\cdot N^{-2}\cdot\delta(\bar{x}-\bar{x}') \quad \text{for } x_0 = x_0' \tag{4}$$

$$\delta(\bar{x}) = \delta(x_1)\,\delta(x_2)\,\delta(x_3). \tag{4a}$$

For the electromagnetic potentials we get for $(x - x')^2 > 0$

$$[A_\mu(x), A_\nu(x')] = 0 \tag{5}$$

$$\left[\frac{\partial A_\mu(x)}{\partial t}, A_\nu(x')\right] = -i\,\delta(\bar{x} - \bar{x}')\left[\frac{\delta_{\mu\nu}}{1-L} - \frac{L}{1-L}\,\delta_{\mu 4}\,\delta_{\nu 4}\right] \tag{6}$$

$$\left[\frac{\partial A_\mu(x)}{\partial t}, \frac{\partial A_\nu(x')}{\partial t'}\right] = \frac{L}{1-L}\left(\delta_{\mu 4}\frac{\partial}{\partial x_\nu} + \delta_{\nu 4}\frac{\partial}{\partial x_\mu}\right)\delta(\bar{x} - \bar{x}'). \tag{7}$$

Besides, every component A_μ will commute with every component of ψ on a space-like surface.

General Properties of the Operators.

The two equations of motion are formally integrated with the help of the retarded singular functions and the operators for the free fields

$$\psi(x) = \psi^{(0)}(x) \cdot N^{-1} - \int S_R(x - x')\,f(x')\,dx' \tag{8}$$

$$A_\mu(x) = A_\mu^{(0)}(x) + \int D_R(x - x')\,j_\mu(x')\,dx' \tag{9}$$

$$S_R(x - x') = \bar{S}(x - x') - \frac{1}{2}\,S(x - x') \tag{10}$$

$$D_R(x - x') = \bar{D}(x - x') - \frac{1}{2}\,D(x - x'). \tag{11}$$

The integral equations (8) and (9) have the same solutions as the differential equations (2) and (3) but contain also the boundary conditions for $t = -\infty$. The operators $\psi^{(0)}(x)$ fulfill the following formulae

$$\{\bar{\psi}_\alpha^{(0)}(x), \psi_\beta^{(0)}(x')\} = -i\,S_{\beta\alpha}(x' - x) \tag{12}$$

$$\langle 0 | [\bar{\psi}_\alpha^{(0)}(x), \psi_\beta^{(0)}(x')] | 0 \rangle = S_{\beta\alpha}^{(1)}(x' - x). \tag{13}$$

The properties of the operators $A_\mu^{(0)}(x)$ are a little more delicate. In practical calculations, we want to use the formulae

$$[A_\mu^{(0)}(x), A_\nu^{(0)}(x')] = -i\,\delta_{\mu\nu} D(x' - x) \tag{14}$$

$$\langle 0 | \{A_\mu^{(0)}(x), A_\nu^{(0)}(x')\} | 0 \rangle = \delta_{\mu\nu} D^{(1)}(x' - x) \tag{15}$$

but it is well-known that these formulae are inconsistent with the Lorentz-condition

$$\frac{\partial A_\mu(x)}{\partial x_\mu} | \psi \rangle = \frac{\partial A_\mu^{(0)}(x)}{\partial x_\mu} | \psi \rangle = 0. \tag{16}$$

On the other hand, it can be shown that this inconsistency is of no importance if only gauge-invariant expressions are calculated[2]). However, in what follows we will not only be interested in gauge-invariant quantities and are thus forced to discuss Eq. (14) and (15) in more detail. For our purpose, the most convenient way to do this will be to adopt the indefinite metric of GUPTA[3]) and BLEULER[4]). In this formalism the Lorentz-condition (16) is replaced by the weaker condition

$$\frac{\partial A_\mu^{(0)(+)}(x)}{\partial x_\mu} \mid \psi \rangle = 0 \tag{17}$$

and as a consequence of this, equation (14) can be fulfilled. ($F^{(+)}(x)$ means the positive-frequency part of the operator $F(x)$.) Further, if the vacuum is suitably defined (no scalar, transversal or longitudinal photons present) Eq. (15) follows from the formalism, but it must be understood that this is a non-gauge-invariant convention. The special gauge chosen corresponds to

$$\langle 0 \mid A_\mu(x) \mid 0 \rangle = \langle 0 \mid A_\mu^{(0)}(x) \mid 0 \rangle = 0\,. \tag{18}$$

In what follows we will, when necessary, use this gauge.

From our Lagrangian we can construct an energy-momentum tensor $T_{\mu\nu}$ and from this one a displacement operator P_μ fulfilling

$$[P_\mu, P_\nu] = 0 \tag{19}$$

$$[P_\mu, F(x)] = i\,\frac{\partial F(x)}{\partial x_\mu}\,. \tag{20}$$

In Eq. (20) $F(x)$ is an arbitrary operator depending on the point x. Eq. (19) thus expresses the fact that the P_μ's are constants of motion. As all the operators P_μ commute with each other, we can use a representation in the Hilbert space where every state vector is an eigenvector of all the P_μ's with the eigenvalues p_μ. In this representation Eq. (20) reads

$$\begin{aligned} \langle a \mid [P_\mu, F(x)] \mid b \rangle &= (p_\mu^{(a)} - p_\mu^{(b)}) \langle a \mid F(x) \mid b \rangle = \\ &= i\,\frac{\partial}{\partial x_\mu} \langle a \mid F(x) \mid b \rangle\,. \end{aligned} \tag{21}$$

Hence

$$\langle a \mid F(x) \mid b \rangle = \langle a \mid F \mid b \rangle e^{i(p^{(b)} - p^{(a)})x}\,. \tag{22}$$

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[2]) Cf. *e.g.* S. T. MA, Phys. Rev. **80**, 729 (1950) and other papers quoted by him.

[3]) S. N. GUPTA, Proc. Phys. Soc. London **63**, 681 (1950).

[4]) K. BLEULER, Helv. Phys. Acta **23**, 567 (1950).

Here $p_\mu^{(a)}$ and $p_\mu^{(b)}$ are the eigenvalues of P_μ in the states $| a \rangle$ and $| b \rangle$. In this representation, the x-dependence of an arbitrary operator is thus given by Eq. (22). The detailed form of the operators P_μ is of very little interest for the present investigation, and we will not write them down explicitly but make the following assumptions concerning the eigenvalues p_μ:

a) Every vector p_μ is time-like.

b) There exists a state with a smallest eigenvalue of the time-component p_0. This state by definition will be called the vacuum and, with a suitable renormalization of the energy, this eigenvalue of p_0 can be put equal to zero.

It is supposed that the above definition of the vacuum is not in contradiction with Eq. (18).

Definition of the Constant L.

We are now able to turn to the main problem of this paper, *i. e.* the definition of the universal constants K, L and N in the Lagrangian (1). We begin with the definition of L that describes the charge-renormalization. This is conveniently stated in terms of the matrix elements of the operators $A_\mu(x)$ between the vacuum state and a state where only one photon is present. (As we are working in the Heisenberg representation some care is necessary when we are speaking of a state with a given number of particles present. If, however, it is understood that we hereby always specify the system for $t = -\infty$, no ambiguities will arise. The occupation-number operators are then constructed from the special operators $A_\mu^{(0)}$ and $\psi^{(0)}$ introduced in Eq. (8) and (9).) At the first moment it would seem natural to introduce the following condition for the matrix elements

$$\langle 0 | A_\mu(x) | k \rangle = \langle 0 | A_\mu^{(0)}(x) | k \rangle \tag{23}$$

where $| k \rangle$ decribes a state with only one photon with energy-momentum vector k, but as the calculation below shows, this can only be fulfilled for the transversal photons. If also the longitudinal and scalar photons are considered, the correct condition for $\langle 0 | A_\mu(x) | k \rangle$ is

$$\langle 0 | A_\mu(x) | k \rangle = \left(\delta_{\mu\nu} + M \frac{\partial^2}{\partial x_\mu \partial x_\nu}\right) \langle 0 | A_\nu^{(0)}(x) | k \rangle \tag{24}$$

where M is another universal constant (*i. e.* independent of x and k). Eq. (24), together with the commutators (5), (6) and (7) and the equation of motion (9), determines the two constants L and M uniquely in terms of the matrix elements of the current operator in Eq. (3). As this contains the constant L explicitly and the other constants implicitly, the definitions of L and M are only implicit.

We now compute the vacuum expectation value of the commutator between the electromagnetic potentials and obtain

$$\begin{aligned}\langle 0 | [A_\mu(x), A_\nu(x')] | 0\rangle &= \langle 0 | [A_\mu^{(0)}(x), A_\nu^{(0)}(x')] | 0\rangle + \\ &+ \langle 0 | [A_\mu(x) - A_\mu^{(0)}(x), A_\nu^{(0)}(x')] | 0\rangle + \langle 0 | [A_\mu^{(0)}(x), A_\nu(x') - A_\nu^{(0)}(x')] | 0\rangle + \\ &+ \int\int dx''\, dx'''\, D_R(x - x'')\, D_R(x' - x''')\langle 0 | [j_\mu(x''), j_\nu(x''')] | 0\rangle . \end{aligned} \tag{25}$$

It is here convenient to introduce a special notation for the vacuum expectation value of the current commutator. Considering the definition of a matrix product and Eq. (22), we obtain

$$\begin{aligned}\langle 0 | [j_\mu(x), j_\nu(x')] | 0\rangle &= \sum_z \langle 0 | j_\mu | z\rangle\langle z | j_\nu | 0\rangle\, e^{-i p^{(z)}(x'-x)} - \\ &- \sum_z \langle 0 | j_\nu | z\rangle\langle z | j_\mu | 0\rangle\, e^{i p^{(z)}(x'-x)} \to \frac{-1}{(2\pi)^3}\Big\{\int_{p_0>0} dp\, e^{i p (x'-x)}\pi_{\mu\nu}^{(+)}(p) - \\ &- \int_{p_0<0} dp\, e^{i p(x'-x)}\pi_{\mu\nu}^{(-)}(p)\Big\}\end{aligned} \tag{26}$$

$$\pi_{\mu\nu}^{(+)}(p) \to V \sum_{p^{(z)}=p} \langle 0 | j_\nu | z\rangle\langle z | j_\mu | 0\rangle \tag{26a}$$

$$\pi_{\mu\nu}^{(-)}(p) \to V \sum_{p^{(z)}=-p} \langle 0 | j_\mu | z\rangle\langle z | j_\nu | 0\rangle . \tag{26b}$$

In Eq. (26a) and (26b) V is a large volume in which the fields are supposed to be enclosed, and summation over states and integration in p-space are freely interchanged. Due to the equation

$$\frac{\partial j_\mu(x)}{\partial x_\mu} = 0 \tag{27}$$

(which is easily verified from Eq. (3)) we must have

$$p_\mu \pi_{\mu\nu}^{(+)} = \pi_{\mu\nu}^{(+)} p_\nu = p_\mu \pi_{\mu\nu}^{(-)} = \pi_{\mu\nu}^{(-)} p_\nu = 0 . \tag{28}$$

From reasons of invariance, on the other hand, $\pi_{\mu\nu}^{(\pm)}$ must have the form

$$\pi_{\mu\nu}^{(\pm)}(p) = \delta_{\mu\nu} A^{(\pm)}(p^2) + p_\mu p_\nu B^{(\pm)}(p^2) . \tag{29}$$

Combining (28) and (29) we get

$$\pi^{(\pm)}_{\mu\nu}(p) = (-p^2\,\delta_{\mu\nu} + p_\mu\,p_\nu)\,\pi^{(\pm)}(p^2) \tag{30}$$

with

$$\pi^{(+)}(p^2) = \frac{V}{-3\,p^2} \sum_{p^{(z)}=p} \langle 0\,|\,j_\nu\,|\,z\rangle \langle z\,|\,j_\nu\,|\,0\rangle = \pi^{(-)}(p^2)\,. \tag{31}$$

We thus have

$$\langle 0\,|\,[\,j_\mu(x), j_\nu(x')]\,|\,0\rangle =$$

$$= \frac{-1}{(2\,\pi)^3} \int d\,p\,e^{i\,p\,(x'-x)}\,\varepsilon(p)\,(-p^2\,\delta_{\mu\nu} + p_\mu\,p_\nu)\,\pi(p^2) \tag{32}$$

$$\pi(p^2) \rightarrow \frac{V}{-3\,p^2} \sum_{p^{(z)}=p} \langle 0\,|\,j_\nu\,|\,z\rangle \langle z\,|\,j_\nu\,|\,0\rangle\,. \tag{32a}$$

Here it can be observed that if we compute the vacuum expectation value of the anticommutator instead of the commutator we get

$$\langle 0\,|\{\,j_\mu(x), j_\nu(x')\}\,|\,0\rangle =$$

$$= \frac{1}{(2\,\pi)^3} \int d\,p\,e^{i\,p\,(x'-x)}\,(-p^2\,\delta_{\mu\nu} + p_\mu\,p_\nu)\,\pi(p^2) \tag{33}$$

with the same function $\pi\,(p^2)$ in (33) as in (32). This follows immediately from the analysis above. Noting that $\pi(p^2) = 0$ unless $p^2 < 0$ (this follows from Eq. (32a) and (24)) we can further write

$$\langle 0\,|\,[\,j_\mu(x), j_\nu(x')]\,|\,0\rangle =$$

$$= \frac{-1}{(2\,\pi)^3} \int d\,p\,e^{i\,p\,(x'-x)}\,\varepsilon(p) \int_0^\infty d\,a\,\delta(p^2+a)\,(-p^2\,\delta_{\mu\nu} + p_\mu\,p_\nu)\,\pi(-a) =$$

$$= -i \int_0^\infty d\,a \left(\Box\,\delta_{\mu\nu} - \frac{\partial^2}{\partial\,x_\mu\,\partial\,x_\nu}\right) \Delta\,(x'-x,\,a)\,\pi(-a)\,. \tag{34}$$

Here $\Delta(x'-x,\,a)$ is the usual singular function constructed with the "mass" $\sqrt{a}$. Thus we also have

$$-\frac{i}{2}\,\varepsilon\,(x'-x) \langle 0\,|\,[\,j_\mu(x), j_\nu(x')]\,|\,0\rangle =$$

$$= \int_0^\infty d\,a \left(\Box\,\delta_{\mu\nu} - \frac{\partial^2}{\partial x_\mu\,\partial x_\nu}\right) \bar{\Delta}\,(x'-x,\,a)\,\pi(-a) =$$

$$= \frac{1}{(2\,\pi)^4} \int d\,p\,e^{i\,p\,(x'-x)}\,(-p^2\,\delta_{\mu\nu} + p_\mu\,p_\nu)\,\bar{\pi}(p^2) \tag{35}$$

$$\bar{\pi}\,(p^2) = P \int_0^\infty \frac{\pi(-a)}{p^2+a}\,d\,a\,. \tag{35a*}$$

---

*) The letter P in Eq. (35a) indicates that the principal value of the integral has to be taken in the point $a = -p^2$.

Returning to Eq. (25) we get with the aid of (24) and (32)

$$\langle 0 | [A_\mu(x), A_\nu(x')] | 0 \rangle = = \frac{-1}{(2\pi)^3} \int dp\, e^{ip(x'-x)} \varepsilon(p) \int_0^\infty da\, \delta(p^2+a) F_{\mu\nu} \tag{36}$$

$$F_{\mu\nu} = \delta(a)(\delta_{\mu\nu} - 2M p_\mu p_\nu) + \frac{\pi(-a)}{a}\left(\delta_{\mu\nu} + \frac{p_\mu p_\nu}{a}\right) = = \delta_{\mu\nu}\left(\delta(a) + \frac{\pi(-a)}{a}\right) + p_\mu p_\nu \left(\frac{\pi(-a)}{a^2} - 2M\delta(a)\right). \tag{37}$$

Putting $x_0' = x_0$ in (36) it follows from (5) that we must have

$$M = \frac{1}{2} \int_0^\infty \frac{\pi(-a)}{a^2}\, da. \tag{38}$$

The integral in (38) may diverge both for $a = 0$ and for $a = \infty$. The first divergency is of a kind usually classified as "infrared". It can always be avoided if we introduce a small photon mass μ. The function π will then vanish for $a < 9\mu^2$, and the denominator is zero only for $a = \mu^2$. We will not investigate this point further. The convergence of the integral at infinity will be discussed later in this paragraph.

Performing a differentiation with respect to the time t in (36) and putting the two times equal afterwards, we obtain

$$\left\langle 0 \left| \left[\frac{\partial A_\mu(x)}{\partial t}, A_\nu(x')\right] \right| 0 \right\rangle_{x_0 = x_0'} = -i\,\delta_{\mu\nu}\,\delta(\bar{x} - \bar{x}')\left(1 + \bar{\pi}(0)\right) - - \frac{i}{(2\pi)^3} \int dp\, e^{i\bar{p}(\bar{x}'-\bar{x})} \int_0^\infty da\, p_0 p_\mu p_\nu \varepsilon(p) \delta(p^2+a) \left[\frac{\pi(-a)}{a^2} - 2M\delta(a)\right]. \tag{39}$$

If μ and ν are both equal to 1, 2 or 3, the last integral in (39) is zero due to Eq. (38). If they are both equal to 4, we get

$$\frac{-i}{(2\pi)^3} \int d^3p\, e^{i\bar{p}(\bar{x}'-\bar{x})} \int_0^\infty da(-\bar{p}^2 - a)\left(\frac{\pi(-a)}{a^2} - 2M\delta(a)\right) = = i\,\delta(\bar{x}' - \bar{x})\,\bar{\pi}(0).$$

If only one of the indices μ and ν is equal to 4, the integral will vanish due to reasons of symmetry. We thus have

$$\left\langle 0 \left| \left[\frac{\partial A_\mu(x)}{\partial t}, A_\nu(x')\right] \right| 0 \right\rangle_{x_0 = x_0'} = = -i\,\delta(\bar{x}' - \bar{x})\left[\delta_{\mu\nu}\left(1 + \bar{\pi}(0)\right) - \delta_{\mu 4}\,\delta_{\nu 4}\,\bar{\pi}(0)\right]. \tag{40}$$

Equations (40) and (6) are identical if

$$1+\bar{\pi}(0)=\frac{1}{1-L} \tag{41}$$

which is our formula for L[5]. It now only remains to verify that Eq. (7) is consistent with the formulae (38) and (41). With the same method as used above we find from Eq. (36)

$$\left\langle 0\left|\left[\frac{\partial A_\mu(x)}{\partial t},\frac{\partial A_\nu(x')}{\partial t'}\right]\right|0\right\rangle=\bar{\pi}(0)\left[\delta_{\mu 4}\frac{\partial}{\partial x_\nu}+\delta_{\nu 4}\frac{\partial}{\partial x_\mu}\right]\delta(\bar{x}'-\bar{x}) \tag{42}$$

which is in fact identical with (7) due to (41).

If we consider the definition of the function $\pi(p^2)$ in Eq. (32a), we observe that it is defined as a sum over only a finite number of terms or rather as an integral over a finite domain in p-space. (The two surfaces $p^2=0$ and $p^2=-m^2$, where in fact an infinite number of states exist, are of no importance, as the first kind of matrix elements will vanish in view of Eq. (24) and the second kind will vanish as a consequence of the charge-invariance.) If a solution to our equations exists at all, $\pi(\mathrm{p}^2)$ is thus a finite quantity for all values of p^2. The question if L, M and $\bar{\pi}(p^2)$ are finite or not is thus answered, if we know the behaviour of $\pi(p^2)$ for large values of $-p^2$. The assumption of the renormalization method is that although $\bar{\pi}(0)$ might be infinite (and hence $L=1$) the difference $\bar{\pi}(p^2)-\bar{\pi}(0)$ (and thus also M) is finite. This means that $\pi(p^2)$ is *not* allowed to increase as strongly as $-p^2$ for large values of $-p^2$.

Concerning the general behaviour of the function $\pi(p^2)$, we will here only mention that it must be positive. This follows *e. g.* from the formula

$$V\sum_{p^{(z)}=p}\langle 0|j_\nu|z\rangle\langle z|j_\mu|0\rangle=(-p^2\,\delta_{\mu\nu}+p_\mu p_\nu)\,\pi(p^2) \tag{43}$$

if we here put $\mu=\nu=1$. As the x-component of the current operator is self-adjoint (not Hermitian, in view of the indefinite metric!) we have

$$\langle z|j_x|0\rangle=(\langle 0|j_x|z\rangle)^*(-1)^{N_4^{(z)}}. \tag{44}$$

In Eq. (44) $N_4^{(z)}$ means the number of scalar photons in the state $|z\rangle$. To obtain (44) we have made use of the explicit form of the

[5]) A similar formula for the charge-renormalization but in terms of the unrenormalized current operator has been given by UMEZAWA and KAMEFUCHI, Prog. Theor. Phys. **6**, 543 (1951).

metric operator as given by GUPTA[3]) and BLEULER[4]). Eq. (43) and (44) now give

$$\pi(p^2) = \frac{V}{p_z^2 - p^2} \sum_{p^{(z)}=p} |\langle 0 | j_x | z \rangle|^2 (-1)^{N_4^{(z)}} . \tag{45}$$

The negative contribution to the sum in (45) from a state with a scalar photon will exactly cancel with the contribution from a similar state with a longitudinal photon, if we observe that the current is a gauge-invariant operator, and hence that we can write

$$\langle 0 | j_\mu | z, k \rangle = F_{\mu\lambda} \langle 0 | A_\lambda^{(0)} | k \rangle \tag{46}$$

with

$$F_{\mu\lambda} k_\lambda = 0 . \tag{47}$$

In (46) $| z, k \rangle$ means a state with one photon with energy-momentum k and other particles present, some of which may also be photons. The quantity $F_{\mu\lambda}$ then depends on the vector k (but not on the polarization vector of the photon) and on the annihilation operators of the other particles. The only surviving terms in (45) will then be the contributions from the transversal photons and these terms are all positive. It thus follows

$$\pi(p^2) \geqq 0 \tag{48}$$

$$\bar{\pi}(0) = \int_0^\infty \frac{\pi(-x)}{x} dx > 0 \tag{49}$$

$$0 < \frac{\bar{\pi}(0)}{1+\bar{\pi}(0)} = L \leqq 1 . \tag{50}$$

This property of the charge-renormalization has earlier been proved by SCHWINGER (unpublished) in a somewhat different way. I am indebted to professor PAULI for information concerning SCHWINGER's proof.

Definition of the Constant K.

The definition of K (the mass-renormalization) can be carried out in a way similar to that of the definition of L. Here we state the renormalization in terms of the matrix elements of the operators ψ between the vacuum state and a state $| q \rangle$ with only one electron present

$$\langle 0 | \psi(x) | q \rangle = \langle 0 | \psi^{(0)}(x) | q \rangle \tag{51}$$

or

$$\langle 0 | f(x) | q \rangle = 0 . \tag{52}$$

To investigate the last condition we compute the vacuum expectation value of the anticommutator between $f(x)$ and $\bar{\psi}^{(0)}(x')$.

$$\langle 0|\{\bar{\psi}^{(0)}_\alpha(x'),\, f_\beta(x)\}|0\rangle = \sum_q (\langle 0|\bar{\psi}^{(0)}_\alpha(x')|q\rangle\langle q|f_\beta(x)|0\rangle + \langle 0|f_\beta(x)|q\rangle\langle q|\bar{\psi}^{(0)}_\alpha(x')|0\rangle). \tag{53}$$

With the aid of the formula

$$N^{-1}\cdot\bar{\psi}^{(0)}(x') = \int_{-\infty}^{x_0''=x_0} \bar{f}(x'')\, S(x''-x')\, dx'' - i \int_{x_0''=x_0} \bar{\psi}(x'')\,\gamma_4 S(x''-x')\, d^3x'' \tag{54}$$

(Eq. (54) follows from the equation of motion (8) — or rather its adjoint — and some well-known properties of the S-function) we can write

$$N^{-1}\cdot\langle 0|\{\bar{\psi}^{(0)}(x'), f(x)\}|0\rangle = \int_{-\infty}^{x} \langle 0|\{\bar{f}(x''), f(x)\}|0\rangle\, S(x''-x')\, dx'' - i \int_{x_0''=x_0} \langle 0|\{\bar{\psi}(x''), f(x)\}|0\rangle\,\gamma_4 S(x''-x')\, d^3x''. \tag{55}$$

The three-dimensional integral in (55) contains the anticommutator of operators for equal times and can thus be calculated with the aid of (4). In view of the definition of $f(x)$ in Eq. (2) we get

$$\{\bar{\psi}(x''), f(x)\}_{x_0=x_0''} = (ie\gamma A(x) + K)\,\gamma_4\,\delta(\bar{x}-\bar{x}'')\cdot N^{-2} \tag{56}$$

$$\langle 0|\{\bar{\psi}(x''), f(x)\}|0\rangle_{x_0=x_0''} = K'\gamma_4\,\delta(\bar{x}-\bar{x}'') \tag{57}$$

(in view of Eq. (18))

$$N^{-1}\langle 0|\{\bar{\psi}^{(0)}(x'), f(x)\}|0\rangle = \int_{-\infty}^{x} \langle 0|\{\bar{f}(x''), f(x)\}|0\rangle\, S(x''-x')\, dx'' - iK'S(x-x')\,;\quad (K' = K\cdot N^{-2})\,. \tag{58}$$

Here again it is convenient to introduce a special notation for the anticommutator in (58). In analogy with (26) we write

$$\langle 0|\{\bar{f}_\alpha(x''), f_\beta(x)\}|0\rangle = \frac{-1}{(2\pi)^3}\Bigg(\int_{p_0>0} dp\, e^{ip(x-x'')}\{\textstyle\sum_1^{(+)}(p^2) + (i\gamma p + m)\sum_2^{(+)}(p^2)\}_{\beta\alpha} + \displaystyle\int_{p_0<0} dp\, e^{ip(x-x'')}\{\textstyle\sum_1^{(-)}(p^2) + (i\gamma p + m)\sum_2^{(-)}(p^2)\}_{\beta\alpha}\Bigg) \tag{59}$$

$$\left(\sum_1^{(+)}(p^2)+(i\gamma p+m)\sum_2^{(+)}(p^2)\right)_{\beta\alpha}=-V\sum_{p^{(z)}=p}\langle 0|f_\beta|z\rangle\langle z|\bar{f}_\alpha|0\rangle \quad (59a)$$

$$\left(\sum_1^{(-)}(p^2)+(i\gamma p+m)\sum_2^{(-)}(p^2)\right)_{\beta\alpha}=-V\sum_{p^{(z)}=-p}\langle 0|\bar{f}_\alpha|z\rangle\langle z|f_\beta|0\rangle . \quad (59b)$$

In view of the charge-invariance of the theory, we must have

$$-V\sum_{p^{(z)}=-p}\langle 0|(C^{-1}f)_\alpha|z\rangle\langle z|(C\bar{f})_\beta|0\rangle =$$
$$=\sum_1^{(-)}(p^2)\,\delta_{\beta\alpha}+(i\gamma p+m)_{\beta\alpha}\sum_2^{(-)}(p^2) \quad (60)$$

where C is the charge-conjugation matrix of SCHWINGER[6]), which has the following properties

$$C^T=-C \quad (61)$$

$$-C^{-1}\gamma_\mu C=\gamma_\mu^T . \quad (62)$$

If we compare (60) and (59a) we get, considering (61) and (62)

$$\sum_1^{(-)}(p^2)=-\sum_1^{(+)}(p^2) \quad (63)$$

$$\sum_2^{(-)}(p^2)=-\sum_2^{(+)}(p^2) . \quad (63a)$$

We thus have

$$\langle 0|\{\bar{f}_\alpha(x''),f_\beta(x)\}|0\rangle =$$
$$=\frac{-1}{(2\pi)^3}\int dp\, e^{ip(x-x'')}\varepsilon(p)\left\{\sum_1(p^2)+(i\gamma p+m)\sum_2(p^2)\right\}_{\beta\alpha} . \quad (64)$$

As on page 423 we also have

$$\langle 0|[\bar{f}_\alpha(x''),f_\beta(x)]|0\rangle =$$
$$=\frac{1}{(2\pi)^3}\int dp\, e^{ip(x-x'')}\left\{\sum_1(p^2)+(i\gamma p+m)\sum_2(p^2)\right\}_{\beta\alpha} \quad (65)$$

and

$$-\frac{i}{2}\varepsilon(x-x'')\langle 0|\{\bar{f}_\alpha(x''),f_\beta(x)\}|0\rangle =$$
$$=\frac{1}{(2\pi)^4}\int dp\, e^{ip(x-x'')}\left\{\bar{\sum}_1(p^2)+(i\gamma p+m)\bar{\sum}_2(p^2)\right\}_{\beta\alpha} \quad (66)$$

$$\bar{\sum}_i(p^2)=P\int_{m^2}^{\infty}\frac{\Sigma_i(-a)}{p^2+a}\,da\,; \quad (i=1,2) . \quad (67)$$

[6]) J. SCHWINGER, Phys. Rev. **74**, 1439 (1948).

With these notations we can write Eq. (58) as

$$N^{-1}\langle 0|\{\bar{\psi}^{(0)}(x'),\, f(x)\}|0\rangle =$$
$$= \frac{-1}{(2\pi)^3}\int dx'' \int dp\, e^{ip(x-x'')}\frac{\varepsilon(p)}{2}\{\Sigma_1(p^2)+(i\gamma p+m)\Sigma_2(p^2)\}S(x''-x')+$$
$$+\frac{i}{(2\pi)^4}\int dx'' \int dp\, e^{ip(x-x'')}\{\bar{\Sigma}_1(p^2)+(i\gamma p+m)\,\bar{\Sigma}_2(p^2)\}\,S(x''-x')-$$
$$-iK'\,S(x-x')\,. \tag{68}$$

As

$$S(x) = \frac{-i}{(2\pi)^3}\int dp\, e^{ipx}\,\varepsilon(p)\,(i\gamma p-m)\,\delta(p^2+m^2)$$

Eq. (68) can also be written

$$N^{-1}\cdot\langle 0|\{\bar{\psi}^{(0)}(x'),\, f(x)\}|0\rangle =$$
$$= -i[K'-\bar{\Sigma}_1(-m^2)-i\pi\,\varepsilon(p)\,\Sigma_1(-m^2)]\,S(x-x')\,. \tag{69}$$

From Eq. (52), however, it follows that the right-hand side of (69) must vanish and, as $\Sigma_1(-m^2)=0$ in view of *e. g.* (59a), this means

$$K' = \bar{\Sigma}_1(-m^2) = \int_{m^2}^{\infty}\frac{\Sigma_1(-a)}{a-m^2}\,da = K\cdot N^{-2}\,. \tag{70}$$

Eq. (70) gives the formula for K. Returning now to Eq. (51), we can write the matrix element of ψ between the vacuum and an one-electron state

$$\langle 0|\psi(x)|q\rangle = \frac{1}{N}\langle 0|\psi^{(0)}(x)|q\rangle + \left(1-\frac{1}{N}\right)\langle 0|\psi^{(0)}(x)|q\rangle\,. \tag{71}$$

The normalization constant N can be determined from the anticommutator of $\bar{\psi}$ and ψ for equal times. Computing the vacuum expectation value of this quantity, we get in analogy with Eq. (25)

$$\langle 0|\{\bar{\psi}(x),\,\psi(x')\}|0\rangle = \frac{-i}{N^2}\,S(x'-x)\,[1+2(N-1)]+$$
$$+\int\int dx''\,dx'''\,S_R(x'-x'')\,\langle 0|\{f(x''),\bar{f}(x''')\}|0\rangle\,S_A(x'''-x) =$$
$$= \frac{-1}{(2\pi)^3}\int dp\, e^{ip(x'-x)}\,\varepsilon(p)\left[\delta(p^2+m^2)\,\frac{1+2(N-1)}{N^2}+\right.$$
$$\left.+\frac{i\gamma p-m}{(p^2+m^2)^2}\{\Sigma_1(p^2)+(i\gamma p+m)\Sigma_2(p^2)\}\right](i\gamma p-m)\,. \tag{72}$$

As

$$\frac{i\gamma p-m}{p^2+m^2}\,(\Sigma_1+(i\gamma p+m)\,\Sigma_2)\,\frac{i\gamma p-m}{p^2+m^2} =$$
$$= (i\gamma p-m)\left[\frac{-\Sigma_2}{p^2+m^2}-\frac{2m\,\Sigma_1}{(p^2+m^2)^2}\right]-\frac{\Sigma_1}{p^2+m^2} \tag{73}$$

we get for equal times with the aid of (4)

$$N^{-2}\gamma_4\,\delta(\bar{x}-\bar{x}')=\gamma_4\,\delta(\bar{x}-\bar{x}')\left[\frac{1+2(N-1)}{N^2}-\right.$$
$$\left.-\int\limits_{m^2}^{\infty}d\,a\left\{\frac{-\Sigma_2(-a)}{a-m^2}+\frac{2\,m\,\Sigma_1(-a)}{(a-m^2)^2}\right\}\right] \tag{74}$$

and hence

$$\frac{N-1}{N^2}=\frac{-1}{2}\left(\bar{\Sigma}_2(-m^2)+2\,m\,\bar{\Sigma}_1{}'(-m^2)\right) \tag{75}$$

$$\bar{\Sigma}_1{}'(-m^2)=-\int\limits_{m^2}^{\infty}\frac{\Sigma_1(-a)\,d\,a}{(a-m^2)^2}=\frac{d\,\bar{\Sigma}_1(p^2)}{d\,p^2}\Bigg|_{p^2=-m^2}. \tag{76}$$

As was the case with the function $\bar{\pi}(p^2)$, it is necessary if the renormalization method is consistent that the difference

$$\bar{\Sigma}_i(p^2)-\bar{\Sigma}_i(-m^2)\qquad(i=1,2)$$

is finite, or that the integrals

$$\int\limits_{m^2}^{\infty}\frac{\Sigma_i(-a)\,d\,a}{(p^2+a)\,(a-m^2)}$$

will converge. The last term in (75) is thus a finite quantity (apart from an infrared divergency for $a=m^2$) but the first integral might be infinite. This is, however, not serious, as the normalization constant itself is not observable. As a matter of fact, it has been shown by Ward[7]) that, for an observable quantity, all infinities of this kind will disappear from the coefficients in an expansion in powers of the charge.

We will end this paragraph with the observation that if one considers Eq. (68) as an identity in x', one concludes *e. g.*

$$\langle 0\,|\,f(x)\,|\,q\rangle=\frac{N}{(2\,\pi)^4}\int d\,x''\int d\,p\,e^{i\,p\,(x-x'')}\times$$
$$\times\left[K'-\bar{\Sigma}_1(p^2)-i\,\pi\,\varepsilon(p)\,\Sigma_1(p^2)-\right.$$
$$\left.-(i\,\gamma\,p+m)\left(\bar{\Sigma}_2(p^2)+i\,\pi\,\varepsilon(p)\,\Sigma_2(p^2)\right)\right]\langle 0\,|\,\psi^{(0)}(x'')\,|\,q\rangle \tag{77}$$

where the equation of motion for $\psi^{(0)}$ has *not* been used. One could

[7]) J. C. Ward, Phys. Rev. **78**, 182 (1950).

then try to compute the normalization constant N from (77) in the following way

$$\left(1-\frac{1}{N}\right)\langle 0\,|\,\psi^{(0)}(x)\,|\,q\rangle = -\int S_R(x-x')\,\langle 0\,|\,f(x')\,|\,q\rangle\,d\,x' =$$

$$= \frac{N}{(2\,\pi)^4}\int d\,x''\int d\,p\left[\frac{\bar{\Sigma}_1(p^2)-K'}{p^2+m^2}\,(i\gamma\,p-m)+i\,\pi\,\varepsilon(p)\,\frac{\Sigma_1(p^2)}{p^2+m^2}\,(i\gamma\,p-m)-\right.$$

$$\left.-\bar{\sum}_2(p^2)-i\,\pi\,\varepsilon(p)\sum_2(p^2)\right]\langle 0\,|\,\psi^{(0)}(x'')\,|\,q\rangle\,e^{i\,p\,(x-x'')} =$$

$$= N\left\{-\bar{\sum}_2(-m^2)-2\,m\lim_{p^2+m^2\to 0}\frac{\bar{\Sigma}_1(p^2)-\bar{\Sigma}_1(-m^2)}{p^2+m^2}\right\}\langle 0\,|\,\psi^{(0)}(x)\,|\,q\rangle. \quad (78)$$

In Eq. (78) the equation of motion for $\psi^{(0)}$ and the fact that $\Sigma_i(p^2)$ vanishes for $-p^2 < (m+\mu)^2$ has been used in the *last* step of the computation. (μ is the small photon mass introduced to avoid infrared divergencies.) The value obtained in this way for N is, however, not the correct value in Eq. (75). This error comes from the way in which we have ambiguously put

$$-\int S_R(x-x')\left(\gamma\,\frac{\partial}{\partial\,x'}+m\right)\psi^{(0)}(x')\,d\,x' = \psi^{(0)}(x'). \quad (79)$$

The left-hand side of (79) is not a well-defined mathematical symbol, and the example above shows that a formula of the kind of Eq. (79) is not always to be trusted. Similar observations have been made in the past by many authors[8]).

I want to express my deep gratitude to professor W. Pauli for his kind interest and valuable criticism and to the *Swedish Atomic Committee* for financial support.

Appendix.

The physical meaning of the functions $\pi(p^2)$ and $\bar{\pi}(p^2)$ can be made clearer if we consider a system with an external electromagnetic field. The influence of such a field can be taken into account if we add the following two terms to the Lagrangian (1)

$$\frac{i\,e\,N^2}{2}\,A_\mu^{(e)}(x)\,[\bar{\psi},\gamma_\mu\,\psi]+L\,A_\mu^{(i)}\,j_\mu^{(e)}(x)\,. \quad (\text{A. }1)$$

[8]) Cf. *e.g.* R. Karplus-N. M. Kroll, Phys. Rev. **77**, 542 (1950); F. J. Dyson, Phys. Rev. **75**, 1736 (1949) and G. Källén, Ark. f. Fys. **2**, 371 (1950).

We then get the following equations of motion

$$\left(\gamma \frac{\partial}{\partial x} + m\right) \psi(x) = \frac{i e}{2} \left\{A_\nu^{(i)}(x), \gamma_\nu \psi(x)\right\} + i e A_\nu^{(e)}(x) \gamma_\nu \psi(x) + K \psi(x) \tag{A. 2}$$

$$\Box A_\mu^{(i)}(x) = -\frac{i e N^2}{2} [\bar{\psi}(x), \gamma_\mu \psi(x)] + L\left(\Box A_\mu^{(i)}(x) - \frac{\partial^2 A_\nu^{(i)}(x)}{\partial x_\mu \partial x_\nu} - j_\mu^{(e)}(x)\right). \tag{A. 3}$$

In the formulae above, $A_\mu^{(e)}(x)$ is the external field and $A_\mu^{(i)}(x)$ the induced field. The former is a c-number and the latter an operator. The external current $j_\mu^{(e)}(x)$ is given by

$$j_\mu^{(e)}(x) = -\left(\Box A_\mu^{(e)}(x) - \frac{\partial^2 A_\nu^{(e)}(x)}{\partial x_\mu \partial x_\nu}\right). \tag{A. 4}$$

If we suppose that we know the solution ($\boldsymbol{\psi}(x)$ and $\boldsymbol{A}_\mu(x)$) when the external field is zero, and that we have a situation where the external field is very weak, we can expand the operators above in a power series of the *external field*. It can be verified without difficulty that the first two terms in such an expansion are respectively

$$\psi(x) = \boldsymbol{\psi}(x) - i \int_{-\infty}^{x} [\boldsymbol{j}_\nu(x'), \boldsymbol{\psi}(x)] A_\nu^{(e)}(x') \, d x' \tag{A. 5}$$

and

$$A_\mu^{(i)}(x) = \boldsymbol{A}_\mu(x) - i \int_{-\infty}^{x} [\boldsymbol{j}_\nu(x'), \boldsymbol{A}_\mu(x)] A_\nu^{(e)}(x') \, d x' + \frac{L}{1-L} (\delta_{\mu\nu} - \delta_{4\mu} \delta_{4\nu}) A_\nu^{(e)}(x). \tag{A. 6}$$

Substituting *e. g.* Eq. (A. 6) into the left-hand side of Eq. (A. 3) we obtain

$$\begin{aligned} \Box A_\mu^{(i)}(x) = &-\boldsymbol{j}_\mu(x) - \frac{e N^2}{2(1-L)} \int_{-\infty}^{x} [[\boldsymbol{j}_\nu(x'), \bar{\boldsymbol{\psi}}(x)], \gamma_\mu \boldsymbol{\psi}(x)] A_\nu^{(e)}(x') \, d x' - \\ &- \frac{e N^2}{2(1-L)} \int_{-\infty}^{x} [\bar{\boldsymbol{\psi}}(x) \gamma_\mu, [\boldsymbol{j}_\nu(x'), \boldsymbol{\psi}(x)]] A_\nu^{(e)}(x') \, d x' + \\ &+ i \int_{x_0 = x_0'} d^3 x' \left([\boldsymbol{j}_\nu(x'), \boldsymbol{A}_\mu(x)] \frac{\partial A_\nu^{(e)}(x')}{\partial t'} + \left[\boldsymbol{j}_\nu(x'), \frac{\partial \boldsymbol{A}_\mu(x)}{\partial t}\right] A_\nu^{(e)}(x')\right) + \\ &+ \frac{i L}{1-L} \int_{-\infty}^{x} \left[\boldsymbol{j}_\nu(x'), \frac{\partial^2 \boldsymbol{A}_\lambda(x)}{\partial x_\mu \partial x_\lambda}\right] A_\nu^{(e)}(x') \, d x' + \frac{L}{1-L} (\delta_{\mu\nu} - \delta_{\mu 4} \delta_{\nu 4}) \Box A_\nu^{(e)}(x). \end{aligned} \tag{A. 7}$$

Using Eq. (A. 5), (A. 6) and the formulae

$$i \int\limits_{x_0 = x_0'} d^3 x' \left([\boldsymbol{j}_\nu(x'), \boldsymbol{A}_\mu(x)] \frac{\partial A_\nu^{(e)}(x')}{\partial t'} + \left[\boldsymbol{j}_\nu(x'), \frac{\partial \boldsymbol{A}_\mu(x)}{\partial t} \right] A_\nu^{(e)}(x') \right) =$$

$$= \frac{-L}{1-L} \frac{\partial^2 A_\nu^{(e)}(x)}{\partial x_\mu \partial x_\nu} + \frac{L}{1-L} \delta_{\mu 4} \Box A_4^{(e)}(x) \qquad \text{(A. 8)}$$

and

$$\int\limits_{-\infty}^{x} \left[\boldsymbol{j}_\nu(x'), \frac{\partial^2 \boldsymbol{A}_\lambda(x)}{\partial x_\mu \partial x_\lambda} \right] A_\nu^{(e)}(x')\, dx' = \frac{\partial^2}{\partial x_\mu \partial x_\lambda} \int\limits_{-\infty}^{x} [\boldsymbol{j}_\nu(x'), \boldsymbol{A}_\lambda(x)]\, A_\nu^{(e)}(x')\, dx' +$$

$$+ \frac{i L}{1-L} \frac{\partial}{\partial x_\mu} \sum_{k=1}^{3} \frac{\partial A_k^{(e)}(x)}{\partial x_k} \qquad \text{(A. 9)}$$

we can simplify the right-hand side of (A. 7) to

$$\frac{-i e N^2}{2(1-L)} \left([\bar{\psi}(x), \gamma_\mu \psi(x)] - [\bar{\psi}(x) - \bar{\boldsymbol{\psi}}(x), \gamma_\mu (\psi(x) - \boldsymbol{\psi}(x))] \right) -$$

$$- \frac{L}{1-L} \left(\frac{\partial^2 A_\nu^{(i)}(x)}{\partial x_\mu \partial x_\nu} + j_\mu^{(e)}(x) \right). \qquad \text{(A. 10)}$$

This expression differs from the correct current only in terms which are of second order in $A_\mu^{(e)}(x)$. The verification of (A. 5) can be performed along similar lines and will not be given explicitly.

If we now compute the vacuum-expectation value of the current operator, we obtain from (A. 6) with the aid of (A. 8)

$$\langle 0 | j_\mu^{(i)}(x) | 0 \rangle = \frac{-i}{2} \int\limits_{-\infty}^{+\infty} \big(1 + \varepsilon(x - x')\big) \langle 0 | [\boldsymbol{j}_\nu(x'), \boldsymbol{j}_\mu(x)] | 0 \rangle\, A_\nu^{(e)}(x')\, dx' +$$

$$+ \frac{L}{1-L} j_\mu^{(e)}(x) \qquad \text{(A. 11)}$$

or

$$\langle 0 | j_\mu^{(i)}(x) | 0 \rangle =$$

$$= \frac{1}{(2\pi)^4} \int d p\, e^{i p x} \big(-\bar{\pi}(p^2) + \bar{\pi}(0) - i \pi\, \varepsilon(p)\, \pi(p^2) \big)\, j_\mu^{(e)}(p) \qquad \text{(A. 12)}$$

where

$$j_\mu^{(e)}(x) = \frac{1}{(2\pi)^4} \int d p\, e^{i p x}\, j_\mu^{(e)}(p). \qquad \text{(A. 13)}$$

The vacuum thus behaves as a medium with the complex dielectricity-constant

$$\varepsilon(p^2) = 1 - \bar{\pi}(p^2) + \bar{\pi}(0) - i \pi\, \varepsilon(p)\, \pi(p^2). \qquad \text{(A. 14)}$$

The connection between the real and the imaginary part of $\varepsilon(p^2)$, which is expressed in Eq. (35a), is very similar to a formula that has been given by KRAMERS[9]) for a dielectricum.

The expectation value of the energy which is transferred per unit time from the external field to the system of particles is given by

$$\int d^3x \langle 0 | \frac{\partial L}{\partial t} | 0 \rangle = \int d^3x \Big(\frac{\partial A_\nu^{(e)}(x)}{\partial t} \langle 0 | j_\nu^{(i)}(x) | 0 \rangle +$$

$$+ L\Big[\langle 0 | A_\nu^{(i)}(x) | 0 \rangle \frac{\partial j_\nu^{(e)}(x)}{\partial t} - \langle 0 | j_\nu^{(i)}(x) | 0 \rangle \frac{\partial A_\nu^{(e)}(x)}{\partial t} - j_\nu^{(e)}(x) \frac{\partial A_\nu^{(e)}(x)}{\partial t} \Big] \Big) . \quad (A.15)$$

The time average of the terms proportional to L is zero, and the average of the first term is equal to

$$\frac{1}{(2\pi)^4} \int dp \, \pi(p^2) \frac{|p_0|}{-p^2} \frac{1}{2} j_\nu^{(e)}(p) \, j_\nu^{(e)}(-p) \qquad (A.16)$$

which is thus the energy of the real particles (photons and electron pairs) which are created per unit time by the external field.

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[9]) M. H. A. KRAMERS, Cong. Int. d. Fisici, Como, Settembre 1927.

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Paper [1953a]: Charge Renormalization and the Identity of Ward

Helv. Phys. Acta 26 (1953) 755

In this paper Källén derives the famous Ward identity, $Z_1 = Z_2$, of quantum electrodynamics, using his general approach to non-perturbative renormalization introduced in his paper [1952a] and further developed in paper [1953c]. Both of these papers have been reprinted here below in this book.

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Gunnar Källén
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Charge Renormalization and the Identity of Ward

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(12. VIII. 1953.)

Summary. The importance of the identity of Ward for the consistency of the charge renormalization is pointed out. A proof of the identity is given without aid of perturbation theory.

Introduction.

Charge renormalization was introduced in quantum electrodynamics in a paper by Schwinger[1]), where it was defined in the e^2-approximation and for the problem of vacuum polarization in an external, electromagnetic field. It was, however, soon remarked by Dyson[2]) that, actually, one has to deal with two different kinds of charge renormalization in quantum electrodynamics, in his paper called "external" and "internal" renormalization. Dyson introduced two different (infinite) constants Z_1 and Z_2 to take care of these renormalizations, but conjectured that they were possibly equal. Later on, it was proved by Ward[3]) that Z_1 is actually equal to Z_2, and consequently Gupta[4]) introduced only one constant in his treatment of the charge renormalization. All these authors use, explicitly or implicitly, perturbation theory, and each renormalization constant is defined with the aid of a power series in e^2, where every coefficient is infinite[5]). In the treatment of the renormalization technique without aid of perturbation theory introduced by the author[6]), the Gupta formalism for the charge renormalization was followed, and only one constant L was introduced to handle this problem, in other words, the identity of Ward was implicitly assumed to hold. It is the aim of the following note to make a clear

1) J. Schwinger, Phys. Rev. **75**, 651 (1949). Cf. also V. F. Weisskopf, Dan. Mat. Fys. Medd. **14**, no. 6 (1936).
2) F. J. Dyson, Phys. Rev. **75**, 486, 1736 (1949).
3) J. C. Ward, Phys. Rev. **78**, 182 (1950); Proc. Phys. Soc. **A 64**, 54 (1951).
4) S. Gupta, Proc. Phys. Soc. **A 64**, 426 (1951).
5) Cf. also G. Takeda, Prog. Theor. Phys. **7**, 359 (1952), where an argument partly independent of perturbation theory is given.
6) G. Källén, Helv. Phys. Acta **25**, 417 (1952), here quoted as I; Dan. Mat. Fys. Medd. **27**, no. 12 (1953), here quoted as II.

distinction between the two kinds of charge renormalization and to give a proof of their equivalence without aid of perturbation theory. The proof, which uses charge conservation for the total system over finite time intervals, may also be of some methodological interest itself. In the discussion below, the content of papers I and II is assumed to be known. The notation of these papers is often used without further explanations.

The External Charge Renormalization.

The definition of the charge renormalization in quantum electrodynamics was given in I as

$$\langle 0 | A_\mu(x) | k \rangle = \left[\delta_{\mu\nu} + M \frac{\partial^2}{\partial x_\mu \partial x_\nu}\right] \langle 0 | A_\nu^{(0)}(x) | k \rangle . \tag{1}$$

This was shown to lead to the formula

$$\frac{1}{1-L} = 1 + \overline{\Pi}(0) \tag{2}$$

which can be understood as an implicit formula for L. The physical meaning of this somewhat abstract formalism is perhaps better understood after a discussion of the vacuum expectation value of the current operator in a system with a very weak external field. It was shown in the appendix of I that this quantity can be written

$$\langle 0 | j_\mu(x) | 0 \rangle = \frac{1}{(2\pi)^4} \int dp\, e^{ipx} j_\mu^{ext}(p)\, \varepsilon(p^2) \tag{3}$$

where

$$j_\mu^{ext}(x) = \frac{1}{(2\pi)^4} \int dp\, e^{ipx} j_\mu^{ext}(p) = \text{the external current} \tag{4}$$

and

$$\varepsilon(p^2) = 1 - \overline{\Pi}(p^2) + \overline{\Pi}(0) - i\pi\, \varepsilon(p)\, \Pi(p^2) = \\ = \text{the “dielectric constant of the vacuum”}. \tag{5}$$

The term $\overline{\Pi}(0)$ in (5) is a direct consequence of equation (2) and of the introduction of the renormalization term

$$-L\left(\Box A_\mu(x) - \frac{\partial^2 A_\nu(x)}{\partial x_\mu \partial x_\nu}\right) \tag{6}$$

in the definition of the current operator. Since the function $\Pi(p^2)$ is equal to zero for $-p^2 \leqq 0$, it follows that the dielectric constant of the vacuum is normalized to one for a light wave as a consequence of the charge renormalization. This shows the connection with SCHWINGER's way of defining the charge renormalization and corresponds to what DYSON calls “external” charge renormalization.

The Internal Charge Renormalization.

A quite different way of defining the charge renormalization in quantum electrodynamics would be to normalize the expectation value of the charge operator Q

$$Q = -i \int d^3x\, j_4(x) \tag{7}$$

to e for an one-electron state[7]). This corresponds to the "internal" charge renormalization of DYSON[2]). In order to discuss this point in more detail we note the following formulae in II (equations (23), (24) and (53)):

$$\begin{aligned}\langle 0 | j_\mu | q, q' \rangle = {} & \langle 0 | j_\mu^{(0)} | q, q' \rangle \left[\varepsilon\left((q+q')^2\right) + 2\,\frac{N-1}{1-L}\right] + \\ & + i\,e \langle 0 | \bar{\psi}^{(0)} | q' \rangle\, \Lambda_\mu(-q'; q)\, \langle 0 | \psi^{(0)} | q \rangle = \\ & = \langle 0 | j_\mu^{(0)} | q, q' \rangle \left[\varepsilon\left((q+q')^2\right) + 2\,\frac{N-1}{1-L} + R\left((q+q')^2\right)\right] + \\ & + \frac{e}{2m} S\left((q+q')^2\right)(q_\mu - q'_\mu)\, \langle 0 | \bar{\psi}^{(0)} | q' \rangle \langle 0 | \psi^{(0)} | q \rangle\,, \end{aligned} \tag{8}$$

where

$$\begin{aligned}\frac{i\,e}{(2\pi)^8} \int\!\!\int dp\, dp'\, e^{ip'(3x) + ip(x4)}\, \Lambda_\mu(p'; p) = {} & N^2 \theta(x3)\, \theta(x4) \langle 0 | \{f(3), \\ & [j_\mu(x), \bar{f}(4)]\} | 0 \rangle - N^2 \theta(x3)\, \theta(34) \langle 0 | [j_\mu(x), \{f(3), \bar{f}(4)\}] | 0 \rangle - \\ & - 2\,i\,e\,(N-1)\, \frac{L}{1-L}\, \delta_{\mu 4} \gamma_4\, \delta(x3)\, \delta(34)\,, \end{aligned} \tag{9}$$

$\varepsilon(p^2)$ is the function defined in equation (5), and where the functions $R(p^2)$ and $S(p^2)$ are defined with the aid of equation (8) above (or with the aid of equation (53) in II). From equation (22) in II we also conclude

$$\begin{aligned}\langle q | j_\mu | q' \rangle = {} & \langle q | j_\mu^{(0)} | q' \rangle \left[\varepsilon\left((q'-q)^2\right) + 2\,\frac{N-1}{1-L} + R\left((q'-q)^2\right)\right] + \\ & + \frac{e}{2m} S\left((q'-q)^2\right)(q'_\mu + q_\mu)\, \langle q | \bar{\psi}^{(0)} | 0 \rangle \langle 0 | \psi^{(0)} | q' \rangle = \\ & = \langle q | j_\mu^{(0)} | q' \rangle \left[\varepsilon\left((q'-q)^2\right) + 2\,\frac{N-1}{1-L} + R\left((q'-q)^2\right) + \right. \\ & \left. + S\left((q'-q)^2\right)\right] + \frac{e}{4m} S\left((q'-q)^2\right)(q'_\nu - q_\nu)\, \langle q | \bar{\psi}^{(0)} | 0 \rangle \times \\ & \times (\gamma_\nu \gamma_\mu - \gamma_\mu \gamma_\nu)\, \langle 0 | \psi^{(0)} | q' \rangle\,. \end{aligned} \tag{10}$$

In equation (10) both $|q\rangle$ and $|q'\rangle$ are one-electron states. The state $|q'\rangle$ in equation (8) is a one-positron state. If we put $\mu = 4$,

[7]) For the definition of particle numbers cf. I and G. KÄLLÉN, Physica **19** (1953).

$|q\rangle = |q'\rangle$ and integrate over three-dimensional space, we get from equation (10)

$$\langle q|Q|q\rangle = -i\int d^3x \langle q|j_4^{(0)}|q\rangle \left[\varepsilon(0) + 2\frac{N-1}{1-L} + R(0) + S(0)\right] = \\ = e\left[1 + 2\frac{N-1}{1-L} + R(0) + S(0)\right]. \qquad (11)$$

It now appears that the condition $\varepsilon(0) = 1$, which followed as a consequence of the charge renormalization, is not in itself sufficient to ensure the value e for the one-electron expectation value of the charge operator. To achieve this we must further have

$$R(0) + S(0) = -2\frac{N-1}{1-L}. \qquad (12)$$

Equation (12) corresponds in our notations to the identity of WARD.

One might be inclined to think that equation (12) could be understood as a consequence of a conservation of the charge of the state $|q\rangle$ from $-\infty$ to the time x_0. However, such an argument is not quite convincing. If the limit $t \to -\infty$ is performed in the formalism, certain formal prescriptions, usually described as an adiabatic switching off of the charge, have to be used to give the limit a well-defined meaning[7]). It appears at least doubtful to use a charge conservation for infinite time intervals under such circumstances. It has further been shown by SCHWINGER[8]) with the aid of an explicit calculation that one easily gets contradictions after a careless use of charge conservation over infinite time intervals. On the other hand, the argument given in the next section will prove equation (12) as a consequence of charge conservation over finite time intervals. The result of our analysis can then be interpreted as a conservation of the quantity $\langle q|Q|q\rangle/e$ also during the switching on process.

Proof of Equation (12).

We start by putting $\mu = 4$ and integrating over three-dimensional space in equation (9)

$$\frac{ie}{(2\pi)^5}\int\int dp\,dp' e^{-ip_0'(x_0^{\mathrm{III}}-x_0)-ip_0(x_0-x_0^{\mathrm{IV}})}\,\delta(\bar{p}-\bar{p}')\,e^{i\bar{p}(\bar{x}^{\mathrm{III}}-\bar{x}^{\mathrm{IV}})}\,\Lambda_4(p';p) = \\ = i N^2\theta(x3)\,\theta(x4)\,\langle 0|\{f(3),[Q,\bar{f}(4)]\}|0\rangle - \\ - i N^2\theta(x3)\,\theta(34)\,\langle 0|[Q,\{f(3),\bar{f}(4)\}]|0\rangle - \\ - 2ie(N-1)\frac{L}{1-L}\gamma_4\,\delta(34)\,\delta(x_0 - x_0^{\mathrm{III}}). \qquad (13)$$

[8]) J. SCHWINGER, Phys. Rev. **76**, 790 (1949), appendix.

As the operator Q is a constant of motion we can write

$$[Q, f(3)] = -i \int_{x_0^{III} = x_0} d^3x [j_4(x), f(3)] = -e\, f(3) \tag{14a}$$

and

$$[Q, \bar{f}(4)] = e\, \bar{f}(4)\,. \tag{14b}$$

Note that only the time independence of the operator Q over the finite time intervals $x_0 - x_0^{III}$ and $x_0 - x_0^{IV}$ is used in the argument. With the aid of equations (13), (14a) and (14b) we obtain

$$\frac{i e}{(2\pi)^5} \int\int dp\, dp'\, e^{ip(34) + i(p_0' - p_0)(x_0 - x_0^{III})}\, \delta(\bar{p} - \bar{p}')\, \Lambda_4(p'; p) =$$
$$= i e N^2 \theta(x3)\, \theta(x4) \langle 0 | \{f(3), \bar{f}(4)\} | 0 \rangle -$$
$$- 2\, ie\, (N-1) \frac{L}{1-L} \gamma_4\, \delta(34)\, \delta(x_0 - x_0^{III})\,. \tag{15}$$

Multiplying with $e^{iq_0 x_0}$ and integrating over x_0, we get

$$\frac{1}{(2\pi)^4} \int dp\, e^{ip(34) + iq_0 x_0^{III}} \Lambda_4(\bar{p}, p_0 - q_0; p) = N^2 \int dx_0 e^{iq_0 x_0}\, \theta(x3)\, \theta(x4) \times$$
$$\times \langle 0 | \{f(3), \bar{f}(4)\} | 0 \rangle - 2(N-1) \frac{L}{1-L} \gamma_4\, \delta(34)\, e^{iq_0 x_0^{III}}\,. \tag{16}$$

From the formula

$$\int dx_0\, e^{iq_0 x_0}\, \theta(x3)\, \theta(x4) = \Big\{\pi\, \delta(q_0) + i[\theta(34) +$$
$$+ \theta(43)\, e^{iq_0 (x_0^{IV} - x_0^{III})}]\, P \frac{1}{q_0} \Big\} e^{iq_0 x_0^{III}} \tag{17}$$

we obtain

$$\frac{1}{(2\pi)^4} \int \Lambda_4(\bar{p}, p_0 - q_0; p)\, dp\, e^{ip(34)} = \pi N^2 \delta(q_0) \frac{-1}{(2\pi)^3} \int dp\, e^{ip(34)} \times$$
$$\times\, \varepsilon(p) \{ \Sigma_1(p^2) + (i\gamma p + m)\, \Sigma_2(p^2) \} -$$
$$- \frac{N^2}{(2\pi)^4} \int dp\, e^{ip(34)} \Big\{ \bar{\Sigma}_1(p^2) + (i\gamma p + m)\, \bar{\Sigma}_2(p^2) +$$
$$+ i\pi\varepsilon(p) \big(\Sigma_1(p^2) + (i\gamma p + m)\, \Sigma_2(p^2)\big) -$$
$$- \bar{\Sigma}_1\big(\bar{p}^2 - (p_0 - q_0)^2\big) - \big[i\big(\gamma_k p_k + i\gamma_4 (p_0 - q_0)\big) + m\big] \times$$
$$\times\, \bar{\Sigma}_2\big(\bar{p}^2 - (p_0 - q_0)^2\big) + i\pi\varepsilon(p-q) \big(\Sigma_1\big(\bar{p}^2 - (p_0 - q_0)^2\big) +$$
$$+ \big[i\big(\gamma_k p_k + i\gamma_4 (p_0 - q_0)\big) + m\big]\, \Sigma_2\big(\bar{p}^2 - (p_0 - q_0)^2\big)\big)\Big\} \times$$
$$\times\, P \frac{1}{q_0} - 2(N-1) \frac{L}{1-L} \gamma_4 \frac{1}{(2\pi)^4} \int dp\, e^{ip(34)}\,. \tag{18}$$

Equation (18) can be solved directly for $\Lambda_4(\bar{p}, p_0 - q_0; p)$. The result is

$$\Lambda_4(\bar{p}, p_0 - q_0; p) = -N^2 \left\{ \frac{\bar{\Sigma}_1(p^2) - \bar{\Sigma}_1(p^2 + 2\, p_0 q_0 - q_0^2)}{q_0} + (i\gamma p + m) \times \right.$$
$$\left. \times \frac{\bar{\Sigma}_2(p^2) - \bar{\Sigma}_2(p^2 + 2\, p_0 q_0 - q_0^2)}{q_0} - \gamma_4 \bar{\Sigma}_2(p^2 + 2\, p_0 q_0 - q_0^2) \right\} -$$
$$- 2(N-1) \frac{L}{1-L} \gamma_4 . \tag{19}$$

Equation (19) holds in the domain

$$-p^2 \leqq (m+\mu)^2; \; -p^2 - 2\, p_0 q_0 + q_0^2 \leqq (m+\mu)^2 .$$

From our point of view, the interesting quantity is

$$ie \langle q | \bar{\psi}^{(0)} | 0 \rangle \Lambda_4(q; q) \langle 0 | \psi^{(0)} | q \rangle = \big(R(0) + S(0) \big) \langle q | j_4^{(0)} | q \rangle . \tag{20}$$

From equation (19) follows

$$ie \langle q | \bar{\psi}^{(0)} | 0 \rangle \Lambda_4(q; q) \langle 0 | \psi^{(0)} | q \rangle = \Big\{ N^2 [2m\, \bar{\Sigma}_1'(-m^2) +$$
$$+ \bar{\Sigma}_2(-m^2)] - 2 \frac{N-1}{1-L} L \Big\} \langle q | j_4^{(0)} | q \rangle . \tag{21}$$

(Note that

$$\langle q | \bar{\psi}^{(0)} | 0 \rangle q_0 \langle 0 | \psi^{(0)} | q \rangle = m \langle q | \bar{\psi}^{(0)} | 0 \rangle \gamma_4 \langle 0 | \psi^{(0)} | q \rangle) .$$

Using the definition of $R(p^2)$ and $S(p^2)$ we conclude from I equation (75)

$$R(0) + S(0) = N^2 [2m\, \bar{\Sigma}_1'(-m^2) + \bar{\Sigma}_2(-m^2)] - 2(N-1) \frac{L}{1-L} =$$
$$= -2 \frac{N-1}{1-L} . \tag{22}$$

Formula (22) is identical with (12) and we have now proved the equivalence of the "external" and the "internal" way of defining the charge renormalization.

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Paper [1953c]: On the Magnitude of the Renormalization Constants in Quantum Electrodynamics

Mat-Fys. Medd. Dan. Vid. Selesk. 27, No.12 (1953) 1; Reprinted in "Selected Papers on Quantum Electrodynamics", ed. J. Schwinger, Dover (1958) and in Selected Papers in Physics, Phys. Soc. of Japan

Here Källén argues, though without claiming absolute rigor, that quantum electrodynamics, by itself, is *not* a consistent theory. He writes:

> "The proof presented here makes no pretence at being satisfactory from a rigorous, mathematical point of view. It contains, for example, a large number of interchanges of orders of integrations, limiting processes and so on. From a strictly logical point of view we cannot exclude the possibility that a more singular solution exists where such formal operations are not allowed. It would, however, be rather hard to understand how the excellent agreement between experimental results and lowest order perturbation theory calculations could be explained on the basis of such a solution."

Later on, the question of consistency of quantum electrodynamics was on Källén's mind as long as he lived. See his talk at the 1956 CERN Conference, as well as the chapter on the Schladming School 1965, both in Part 2 of this book. This impressive result was challenged a few years later, as described in Part 4.

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ON THE MAGNITUDE OF THE RENORMALIZATION CONSTANTS IN QUANTUM ELECTRODYNAMICS

BY

GUNNAR KÄLLÉN

København
i kommission hos Ejnar Munksgaard
1953

With the aid of an exact formulation of the renormalization method in quantum electrodynamics which has been developed earlier, it is shown that not all of the renormalization constants can be finite quantities. It must be stressed that this statement is here made without any reference to perturbation theory.

Introduction.

In a previous paper[1], the author has given a formulation of quantum electrodynamics in terms of the renormalized Heisenberg operators and the experimental mass and charge of the electron. The consistency of the renormalization method was there shown to depend upon the behaviour of certain functions ($\Pi(p^2)$, $\Sigma_1(p^2)$ and $\Sigma_2(p^2)$) for large, negative values of the argument p^2. If the integrals

$$\int^{\infty} \frac{\Pi(-a)}{a}\, da, \quad \int^{\infty} \frac{\Sigma_i(-a)}{a}\, da \quad (i = 1, 2) \tag{1}$$

converge, quantum electrodynamics is a completely consistent theory, and the renormalization constants themselves are finite quantities. This would seem to contradict what has appeared to be a well-established fact for more than twenty years, but it must be remembered that all calculations of self-energies etc. have been made with the aid of expansions in the coupling constant e. Thus what we know is really only that, for example, the self-energy of the electron, considered as a function of e, is not analytic at the origin. It has even been suggested[2] that a different scheme of approximation may drastically alter the results obtained with the aid of a straightforward application of perturbation theory. It is the aim of the present paper to show—without any attempt at extreme mathematical rigour—that this is actually not the case in present quantum electrodynamics. The best we can

[1] G. Källén, Helv. Phys. Acta **25**, 417 (1952), here quoted as I.

[2] *Cf., e. g.*, W. Thirring, Z. f. Naturf. **6a** 462 (1951). N. Hu, Phys. Rev. **80**, 1109 (1950).

hope for is that the renormalized theory is finite or, in other words, that the integrals

$$\int^{\infty} \frac{\Pi(-a)}{a^2}\, da, \quad \int^{\infty} \frac{\Sigma_i(-a)}{a^2}\, da, \tag{2}$$

appearing in the renormalized operators, do converge. No discussion of this point, however, will be given here.

General Outline of the Method.

We start our investigation with the assumption that all the quantities K, $(1-L)^{-1}$ and $\frac{1}{N}$ (for notations, cf. I) are finite or that the integrals (1) converge. This will be shown to lead to a lower bound for $\Pi(p^2)$ which has a finite limit for $-p^2 \to \infty$, thus contradicting our assumption. In this way it is proved that not all of the three quantities above can be finite. Our lower bound for $\Pi(p^2)$ is obtained from the formula (cf. I, Eqs. (32) and (32 a))

$$\Pi(p^2) = \frac{V}{-3p^2} \sum_{p^{(z)}=p}{}' |\langle 0|j_\nu|z\rangle|^2 (-1)^{N_4^{(z)}}.^{1)} \tag{3}$$

It was shown in I that, in spite of the signs appearing in (3), the sum for $\Pi(p^2)$ could be written as a sum over only positive terms. Thus we get a lower bound for $\Pi(p^2)$, if we consider the following expression

$$\Pi(p^2) > \frac{V}{-3p^2} \sum_{q+q'=p}{}' |\langle 0|j_\nu|q, q'\rangle|^2. \tag{4}$$

In Eq. (4), $\langle 0|j_\nu|q, q'\rangle$ denotes a matrix element of the current (defined in I, Eq. (3)) between the vacuum and a state with one electron-positron pair (for $x_0 \to -\infty$). The energy-momentum vector of the electron is equal to q and of the positron is equal to q'. The sum is to be extended over all states for which $q+q'=p$. We can note here that, if we develop the function $\Pi(p^2)$ in powers of e^2 and consider just the first term in this expansion, only the states included in (4) will give a contribution. For this case, the sum is easily computed, *e. g.* in the following way:

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[1]) $\Sigma' |\langle 0|j_\nu|z\rangle|^2 = \sum \left(\sum_{k=1}^{3} |\langle 0|j_k|z\rangle|^2 - |\langle 0|j_4|z\rangle|^2 \right)$

$$\left.\begin{aligned}\Pi^{(0)}(p^2) &= \frac{V}{-3p^2}\sum_{q+q'=p}{}' |\langle 0 | j_\nu^{(0)} | q, q'\rangle|^2 \\ &= \frac{Ve^2}{-3p^2}\sum_{q+q'=p}{}' \langle 0 | \overline{\psi}^{(0)} | q'\rangle \gamma_\nu \langle 0 | \psi^{(0)} | q\rangle \langle q | \overline{\psi}^{(0)} | 0\rangle \gamma_\nu \langle q' | \psi^{(0)} | 0\rangle \\ &= \frac{e^2}{12\pi^2}\left(1-\frac{2m^2}{p^2}\right)\sqrt{1+\frac{4m^2}{p^2}}\,\frac{1}{2}\left[1-\frac{p^2+4m^2}{|p^2+4m^2|}\right].\end{aligned}\right\} \quad (5)$$

The function $\Pi^{(0)}(p^2)$ has the constant limit $\frac{e^2}{12\pi^2}$ for large values of $-p^2$. This corresponds, of course, to the well-known divergence for the first-order charge-renormalization. We shall see, however, that with the assumptions we have made here the lower bound for the complete $\Pi(p^2)$, obtained from (4), is rather similar to $\Pi^{(0)}(p^2)$.

An Exact Expression for the Matrix Element of the Current.

Our next problem is to obtain a formula for $\langle 0 | j_\nu | q, q'\rangle$ with which we can estimate the matrix element for large values of $-(q+q')^2$. For this purpose we first compute

$$\left.\begin{aligned}[j_\mu(x), \psi^{(0)}(x')] &= -N\int_{-\infty}^{x} S(13)\,[j_\mu(x), f(3)]\,dx''' \\ &\quad - iN\int_{x_0'''=x_0} S(13)\,\gamma_4\,[j_\mu(x), \psi(3)]\,d^3x'''.\end{aligned}\right\} \quad (6)$$

(Cf. I, Eq. (54).) The last commutator can be computed without difficulty if we introduce the following formula for $j_\mu(x)$

$$j_\mu(x) = \frac{ieN^2}{1-L}\xi_{\mu\lambda}s_\lambda(x) + \frac{L}{1-L}\xi_{\mu\lambda}\frac{\partial^2 A_\nu(x)}{\partial x_\lambda \partial x_\nu} - L\delta_{\mu 4}\Box A_4(x) \quad (7)$$

with

$$\xi_{\mu\lambda} = \delta_{\mu\lambda} - L\delta_{\mu 4}\delta_{\lambda 4} \quad (7\,a)$$

and

$$s_\lambda(x) = \frac{1}{2}[\overline{\psi}(x), \gamma_\lambda \psi(x)]. \quad (7\,b)$$

The expression (7) is written in such a way that the second time-derivatives of all the A_μ's drop out. With the aid of I, Eqs. (4)—(7) we now get

$$\left.\begin{aligned}[j_\mu(x), \psi(3)]_{x_0'''=x_0} &= \frac{ieN^2}{1-L}\xi_{\mu\lambda}[s_\lambda(x), \psi(3)] \\ &= -\frac{ie}{1-L}\xi_{\mu\lambda}\gamma_4\gamma_\lambda\psi(x)\,\delta(\bar{x}-\bar{x}''').\end{aligned}\right\} \quad (8)$$

It thus follows that

$$\left.\begin{aligned}[j_\mu(x), \psi^{(0)}(x')] = -N\int_{-\infty}^{x} S(13)\,[j_\mu(x), f(3)]\,dx''' \\ -\frac{eN}{1-L}\xi_{\mu\lambda}S(1\,x)\,\gamma_\lambda\,\psi(x).\end{aligned}\right\} \quad (9)$$

We then proceed by computing

$$\left.\begin{aligned}&\langle 0\,|\,\{[j_\mu(x), \psi^{(0)}(x')], \overline{\psi}^{(0)}(x'')\}\,|\,0\rangle \\ &= \frac{ieN}{1-L}\xi_{\mu\lambda}S(1x)\,\gamma_\lambda\,S(x\,2) - N\int_{-\infty}^{x} S(13)\,dx''' \\ &\times[\langle 0\,|\,[j_\mu(x), \{\overline{\psi}^{(0)}(2), f(3)\}]\,|\,0\rangle - \langle 0\,|\,\{[j_\mu(x), \overline{\psi}^{(0)}(2)], f(3)\}\,|\,0\rangle].\end{aligned}\right\} \quad (10)$$

If this expression is considered as an identity in x' and x'' it will obviously give us a formula for $\langle 0\,|\,j_\mu\,|\,q, q'\rangle$ and for $\langle q\,|\,j_\mu\,|\,q'\rangle$. (Cf. I, Eqs. (68) and (77).) We transform the right-hand side of (10) in the following way:

$$\{\overline{\psi}^{(0)}(2), f(3)\} = N\int_{-\infty}^{x'''}\{f(3), \overline{f}(4)\}\,S(42)\,dx^{\mathrm{IV}} - \frac{i}{N}[ie\gamma A(3) + K]\,S(32) \quad (11)$$

and, hence,

$$\left.\begin{aligned}\langle 0\,|\,[j_\mu(x), \{\overline{\psi}^{(0)}(2), f(3)\}]\,|\,0\rangle &= \frac{e}{N}\gamma_\lambda S(32)\,\langle 0\,|\,[j_\mu(x), A_\lambda(3)]\,|\,0\rangle \\ &+ N\int_{-\infty}^{x'''} dx^{\mathrm{IV}}\,\langle 0\,|\,[j_\mu(x), \{f(3), \overline{f}(4)\}]\,|\,0\rangle\,S(42).\end{aligned}\right\} \quad (12)$$

The last term in (10) can be treated in a similar way:

$$[j_\mu(x), \overline{\psi}^{(0)}(2)] = N\int_{-\infty}^{x}[j_\mu(x), \overline{f}(4)]\,S(42)\,dx^{\mathrm{IV}} + \frac{eN}{1-L}\overline{\psi}(x)\,\gamma_\lambda S(x\,2)\,\xi_{\lambda\mu} \quad (13)$$

and

$$\left.\begin{aligned}N\int_{-\infty}^{x} S(13)\,dx'''\,\langle 0\,|\,\{\overline{\psi}(x), f(3)\}\,|\,0\rangle = -\langle 0\,|\,\{\overline{\psi}(x), \psi^{(0)}(x') \\ + iN\int_{x_0'''=x_0} S(13)\,\gamma_4\,\psi(3)\,d^3x'''\}\,|\,0\rangle = iS(1\,x)\left[1-\frac{1}{N}\right].\end{aligned}\right\} \quad (14)$$

Collecting (12), (13) and (14) we get

$$\left.\begin{aligned} &\langle 0 \,|\, \{[j_\mu(x),\, \psi^{(0)}(x')],\, \overline{\psi}^{(0)}(x'')\} \,|\, 0\rangle \\ &= \frac{ie}{1-L}\,[1+2(N-1)]\,\xi_{\mu\lambda}\,S(1x)\,\gamma_\lambda\,S(x2) \\ &-e\int_{-\infty}^{x} S(13)\,\gamma_\lambda\,S(32)\,dx'''\,\langle 0\,|\,[j_\mu(x),\,A_\lambda(3)]\,|\,0\rangle \\ &-N^2\int_{-\infty}^{x} dx'''\int_{-\infty}^{x'''} dx^{\mathrm{IV}}\,S(13)\,\langle 0\,|\,[j_\mu(x),\,\{f(3),\,\bar f(4)\}]\,|\,0\rangle\,S(42) \\ &+N^2\int_{-\infty}^{x} dx'''\int_{-\infty}^{x} dx^{\mathrm{IV}}\,S(13)\,\langle 0\,|\,\{f(3),\,[j_\mu(x),\,\bar f(4)]\}\,|\,0\rangle\,S(42). \end{aligned}\right\} \tag{15}$$

The second term in (15) can be rewritten with the aid of the functions $\Pi(p^2)$ and $\overline{\Pi}(p^2)$.

$$\left.\begin{aligned} \langle 0\,|\,[j_\mu(x),\,A_\lambda(3)]\,|\,0\rangle &= \int D_R(34)\,\langle 0\,|\,[j_\mu(x),\,j_\lambda(4)]\,|\,0\rangle\,dx^{\mathrm{IV}} \\ &= \frac{-1}{(2\pi)^3}\int dp\,e^{ip(3x)}\,\varepsilon(p)\,[p_\mu p_\lambda - p^2\,\delta_{\mu\lambda}]\,\frac{\Pi(p^2)}{p^2}. \end{aligned}\right\} \tag{16}$$

We are, however, more interested in the expression

$$\left.\begin{aligned} \frac{1}{2}[1+\varepsilon(x3)]\,\langle 0\,|\,[j_\mu(x),\,A_\lambda(3)]\,|\,0\rangle &= \frac{i\delta_{\mu\lambda}}{(2\pi)^4}\int dp\,e^{ip(x3)}\,[\overline{\Pi}(p^2) \\ &+ i\pi\varepsilon(p)\,\Pi(p^2)] + \frac{1}{2}[1+\varepsilon(x3)]\,\frac{\partial^2\Phi(3x)}{\partial x_\mu\,\partial x_\lambda}, \end{aligned}\right\} \tag{17}$$

where

$$\Phi(x) = \frac{1}{(2\pi)^3}\int dp\,e^{ipx}\,\varepsilon(p)\,\frac{\Pi(p^2)}{p^2}. \tag{17 a}$$

Obviously, we have

$$\Phi(3x) = 0 \tag{18 a}$$

$$\frac{\partial\Phi(3x)}{\partial x_0'''} = -i\overline{\Pi}(0)\,\delta(\bar x - \bar x''') \tag{18 b}$$

for $x_0''' = x_0$. It thus follows

$$\varepsilon(x3)\,\frac{\partial^2\Phi(3x)}{\partial x_\mu\,\partial x_\lambda} = \frac{\partial^2}{\partial x_\mu\,\partial x_\lambda}\,[\varepsilon(x3)\,\Phi(3x)] + 2\,i\overline{\Pi}(0)\,\delta_{\mu 4}\delta_{\lambda 4}\delta(x3). \tag{19}$$

Using the equation

$$\frac{\partial}{\partial x_\lambda'''} S(13)\gamma_\lambda S(32) = 0, \tag{20}$$

we get

$$\left.\begin{aligned} -e\int_{-\infty}^{+\infty}\frac{1}{2}[1+\varepsilon(x\,3)]\,S(13)\gamma_\lambda S(32)\langle 0|[j_\mu(x), A_\lambda(3)]|0\rangle\,dx''' \\ = -\frac{ie}{(2\pi)^4}\int dx'''\int dp e^{ip(x3)} S(13)\gamma_\mu S(32)\,[\overline{\Pi}(p^2)+i\pi\varepsilon(p)\,\Pi(p^2)] \\ + i\delta_{\mu 4}\frac{L}{1-L}S(1\,x)\gamma_4 S(x\,2). \end{aligned}\right\} \tag{21}$$

Introducing (21) into (15) we obtain

$$\left.\begin{aligned} &\langle 0|\{[j_\mu(x), \psi^{(0)}(x')], \overline{\psi}^{(0)}(x'')\}|0\rangle \\ &= ie\int dx'''\int\frac{dp}{(2\pi)^4}e^{ip(x3)}S(13)\gamma_\mu S(32)\,[1-\overline{\Pi}(p^2) \\ &\qquad + \overline{\Pi}(0) - i\pi\varepsilon(p)\,\Pi(p^2)] \\ &- N^2\int_{-\infty}^{x}dx'''\int_{-\infty}^{x'''}dx^{\mathrm{IV}}\,S(13)\langle 0|[j_\mu(x), \{f(3), \bar{f}(4)\}]|0\rangle S(42) \\ &+ N^2\int_{-\infty}^{x}dx'''\int_{-\infty}^{x}dx^{\mathrm{IV}}\,S(13)\langle 0|\{f(3), [j_\mu(x), \bar{f}(4)]\}|0\rangle S(42) \\ &+\frac{2\,ie\,(N-1)}{1-L}\,\xi_{\mu\lambda}\,S(1\,x)\gamma_\lambda S(x\,2). \end{aligned}\right\} \tag{22}$$

The first term in (22) describes the vacuum polarization and is quite similar to the corresponding expression for a weak external field (cf. I, Appendix). The remaining terms contain the anomalous magnetic moment, the main contribution to the Lamb shift etc. Introducing the notation

$$\left.\begin{aligned} &- N^2\theta(x\,3)\,\theta(34)\langle 0|[j_\mu(x), \{f(3), \bar{f}(4)\}]|0\rangle \\ &+ N^2\theta(x\,3)\,\theta(x\,4)\langle 0|\{f(3), [j_\mu(x), \bar{f}(4)]\}|0\rangle \\ &\quad -\frac{2\,ie\,(N-1)}{1-L}L\delta_{\mu 4}\gamma_4\,\delta(x\,3)\,\delta(34) \\ &= \frac{ie}{(2\pi)^8}\iint dp\,dp' e^{ip'(3x)+ip(x4)}\Lambda_\mu(p', p) \end{aligned}\right\} \tag{23}$$

$$\theta(x) = \frac{1}{2}[1+\varepsilon(x)], \tag{23 a}$$

we obtain from (22)

$$\left.\begin{aligned} &\langle 0|j_\mu|q, q'\rangle \\ &= \langle 0|j_\mu^{(0)}|q, q'\rangle\left[1-\bar{\Pi}((q+q')^2)+\bar{\Pi}(0)-i\pi\Pi((q+q')^2)\right. \\ &\left.+2\frac{N-1}{1-L}\right]+ie\langle 0|\bar{\psi}^{(0)}|q'\rangle\Lambda_\mu(-q', q)\langle 0|\psi^{(0)}|q\rangle. \end{aligned}\right\} \tag{24}$$

This is the desired formula for the matrix element of the current.

Analysis of the Function $\Lambda\mu(p', p)$.

We now want to investigate the function $\Lambda\mu(p', p)$ in some detail, especially studying its behaviour for large values of $-(q+q')^2$ in (24). For simplicity, we put $\mu = k \neq 4$ and study

$$\left.\begin{aligned} ie\Lambda_k(p', p) = &\iint dx''' dx^{IV} e^{-ip'(3x)-ip(x4)} N^2\{\theta(x3)\theta(x4)\langle 0|\{f(3), \\ &[j_k(x), \bar{f}(4)]\}|0\rangle - \theta(x3)\theta(34)\langle 0|[j_k(x), \{f(3), \bar{f}(4)\}]|0\rangle\}. \end{aligned}\right\} \tag{25}$$

We treat the two terms in (25) separately. The first vacuum expectation value can be transformed to momentum space with the aid of the functions

$$A_k^{(+)}(p', p) = V^2 \sum_{\substack{p^{(z)}=p \\ p^{(z')}=p'}} \langle 0|f|z'\rangle\langle z'|j_k|z\rangle\langle z|\bar{f}|0\rangle \tag{26}$$

$$A_k^{(-)}(p', p) = V^2 \sum_{\substack{\cdots\cdot \\ \cdots\cdot}} \langle 0|\bar{f}|z'\rangle\langle z'|j_k|z\rangle\langle z|f|0\rangle \tag{27}$$

$$B_k^{(+)}(p', p) = V^2 \sum_{\substack{\cdots\cdot \\ \cdots\cdot}} \langle 0|f|z'\rangle\langle z'|\bar{f}|z\rangle\langle z|j_k|0\rangle \tag{28}$$

$$B_k^{(-)}(p', p) = V^2 \sum_{\substack{\cdots\cdot \\ \cdots\cdot}} \langle 0|j_k|z'\rangle\langle z'|\bar{f}|z\rangle\langle z|f|0\rangle. \tag{29}$$

It then follows that

$$\left.\begin{aligned} \langle 0|\{f(3), [j_k(x), \bar{f}(4)]\}|0\rangle = &\frac{1}{V^2}\sum_{p, p'}\{e^{ip'(3x)+ip(x4)}A_k^{(+)}(p', p) \\ &-e^{ip'(34)+ip(4x)}B_k^{(+)}(p', p)+e^{ip'(x4)+ip(43)}B_k^{(-)}(p', p) \\ &-e^{ip'(4x)+ip(x3)}A_k^{(-)}(p', p)\}. \end{aligned}\right\} \tag{30}$$

Our discussion started with the assumption that all the renormalization constants and, of course, all the matrix elements of the operators $j_\mu(x)$ and $f(x)$ are finite. As this is a condition on the behaviour of, for example, the function $\Pi(p^2)$ for large values of $-p^2$, and as this function is defined as a sum of matrix elements, it is clear that we also have a condition on the matrix elements themselves, *i. e.* on the functions A and B defined in (26)—(29) for large values of $-p^2, -p'^2$ and $-(p-p')^2$. To get more detailed information on this point we consider the expression

$$\left.\begin{aligned} &\langle z|[j_\mu(x), A_\nu^{(0)}(x')]|z\rangle \\ &= -i\frac{L}{1-L}\frac{\partial^2 D(x'-x)}{\partial x_\mu \partial x_\nu} + \int dx'' F_{\mu\nu}(x-x'') D(x'-x'') \end{aligned}\right\} \quad (31)$$

with

$$F_{\mu\nu}(x-x'') = \theta(x-x'')\langle z|[j_\mu(x), j_\nu(x'')]|z\rangle \quad (32)$$

(cf. I, Eq. (A. 8) and the equation of motion for $A_\mu(x)$). Supposing, for simplicity, that $|z\rangle$ does not contain a photon with energy-momentum vector k, we have

$$\left.\begin{aligned} &\langle z|j_\mu(x)|z, k\rangle \\ &= -\frac{L}{1-L}k_\mu k_\nu\langle 0|A_\nu^{(0)}(x)|k\rangle + i\int dx'' F_{\mu\nu}(x-x'')\langle 0|A_\nu^{(0)}(x'')|k\rangle. \end{aligned}\right\} \quad (33)$$

Writing

$$F_{\mu\lambda}(x-x'') = \theta(x-x'')\frac{-1}{(2\pi)^3}\int dp e^{ip(x-x'')} F_{\mu\lambda}(p) \quad (34)$$

and using the formula

$$\varepsilon(x-x'') = \frac{1}{i\pi}P\int\frac{d\tau}{\tau}e^{i\tau(x_0-x_0'')} \quad (35)$$

we get

$$iF_{\mu\lambda}(x-x'') = \frac{-1}{(2\pi)^4}\int dp e^{ip(x-x'')}\{\overline{F}_{\mu\lambda}(p) + i\pi F_{\mu\lambda}(p)\} \quad (36)$$

with

$$\overline{F}_{\mu\lambda}(p) = P\int\frac{d\tau}{\tau}F_{\mu\lambda}(\overline{p}, p_0+\tau). \quad (37)$$

We further note that from (34) it follows that

$$F_{\mu\lambda}(p) = V \sum_{p^{(z')} = p^{(z)} + p} \langle z | j_\lambda | z' \rangle \langle z' | j_\mu | z \rangle - V \sum_{p^{(z')} = p^{(z)} - p} \langle z | j_\mu | z' \rangle \langle z' | j_\lambda | z \rangle . \quad (38)$$

If every expression appearing in our formalism is finite, the integral in (37) must converge. This means that[1])

$$\lim_{p_0 \to \pm \infty} F_{\mu\lambda}(\bar{p}, p_0) = 0 . \quad (39)$$

Putting $\mu = \lambda = k$ we then get from (38) and (39)

$$\lim_{p_0 \to \infty} \sum_{p^{(z')} = p^{(z)} + p} | \langle z | j_k | z' \rangle |^2 (-1)^{N_4^{(z)} + N_4^{(z')}} = 0 \quad (40\,\mathrm{a})$$

and

$$\lim_{p_0 \to -\infty} \sum_{p^{(z')} = p^{(z)} - p} | \langle z | j_k | z' \rangle |^2 (-1)^{N_4^{(z)} + N_4^{(z')}} = 0 . \quad (40\,\mathrm{b})$$

If we first consider a state $|z\rangle$ with no scalar or longitudinal photons, it can be shown with the aid of the gauge-invariance of the current operator (cf. I, p. 426. Eq. (47) there can be verified explicitly with the aid of (32) and (33) above) that only states $|z'\rangle$ with transversal photons will give a non-vanishing contribution to (40 a) and (40 b), and these contributions are all positive. We thus obtain the result

$$\lim_{|p_0^{(z)} - p_0^{(z')}| \to \infty} | \langle z | j_k | z' \rangle |^2 = 0 \quad (41)$$

if none of the states $|z\rangle$ and $|z'\rangle$ contains a scalar or a longitudinal photon. Because of Lorentz invariance which requires that Eq. (41) is valid in every coordinate system, it follows, however, that (41) must be valid for all kinds of states. If we make a Lorentz transformation, the "transversal" states in the new coordinate system will in general be a mixture of all kinds of states in the old system. If (41) were not valid also for the scalar and longitudinal states in the old system, it could not hold for the transversal states in the new system.

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[1]) The case in which the integrals converge without the functions vanishing will be discussed in the Appendix.

From equation (41) we conclude that

$$\lim_{-(p-p')^2\to\infty} A_k^{(\pm)}(p',p) = 0 \tag{42 a}$$

$$\lim_{-p^2\to\infty} B_k^{(+)}(p',p) = 0 \tag{42 b}$$

$$\lim_{-p'^2\to\infty} B_k^{(-)}(p',p) = 0\,. \tag{42 c}$$

It is, of course, not immediately clear that the sum over all the terms in (26)—(29) must vanish because every term vanishes. What really follows from (40) is, however, that the sum of all the absolute values of $\langle z|j_\mu|z'\rangle$ must vanish. If the limits in A and B are then performed in such a way that p^2 and p'^2 are kept fixed for A and $(p-p')^2$ and one of the p^2's are kept fixed for the B's, equations (42) will follow.

To summarize the argument so far, we have shown that if we write

$$\langle 0|\{f(3),[j_k(x),\bar f(4)]\}|0\rangle = \frac{1}{(2\pi)^6}\iint dp\,dp'\,e^{ip'(3x)+ip(x4)}F_k(p',p) \tag{43}$$

we have

$$\lim_{-(p-p')^2\to\infty} F_k(p',p) = 0\,. \tag{44}$$

Introducing the notations

$$\bar F_k(p',p) = \int\frac{d\tau}{\tau}F_k(p'-\varepsilon\tau,\,p) \tag{45 a}$$

and

$$\tilde F_k(p',p) = \int\frac{d\tau}{\tau}F_k(p',p+\varepsilon\tau) \tag{45 b}$$

(ε is a "vector" with the components $\varepsilon_k = 0$ for $k \neq 4$ and $\varepsilon_0 = 1$) we find from (44) and the assumption that the integrals in (45) converge that

$$\lim_{-(p-p')^2\to\infty}\bar F_k(p',p) = \lim_{-(p-p')^2\to\infty}\tilde F_k(p',p) = 0 \tag{46}$$

(cf. the Appendix). With the aid of the notations (45) we can now write

$$\left.\begin{aligned}&\theta(x3)\,\theta(x4)\langle 0\,|\{f(3),[j_k(x),\bar{f}(4)]\}|\,0\rangle\\&=\frac{-1}{(2\pi)^8}\iint dp\,dp'\,e^{ip'(3x)+ip(x4)}\,[\tilde{\bar{F}}_k(p',p)\\&-\pi^2F_k(p',p)+i\pi(\bar{F}_k(p',p)+\tilde{F}_k(p',p))].\end{aligned}\right\}\quad(47)$$

In quite a similar way it can be shown that the second term in (25) can be written in a form analogous to (47) with the aid of a function $G_k(p',p)$ which also has the properties (44) and (46). It thus follows

$$\lim_{-(p-p')^2\to\infty} \Lambda_k(p',p) = 0. \quad (48)$$

It must be stressed that this property of the function $\Lambda_k(p',p)$ is a consequence of (41) and thus essentially rests on the assumption that all the renormalization constants are finite quantities.

It is clear from (24) that the function Λ_μ transforms as the matrix γ_μ under a Lorentz transformation. The explicit verification of this from (23) is somewhat involved but can be carried through with the aid of the identity

$$\left.\begin{aligned}&\theta(x3)\,\theta(x4)\{f(3),[j_\mu(x),\bar{f}(4)]\}-\theta(x3)\,\theta(34)\,[j_\mu(x),\{f(3),\bar{f}(4)\}]\\&=\theta(x4)\,\theta(x3)\{\bar{f}(4),[j_\mu(x),f(3)]\}-\theta(x4)\,\theta(43)\,[j_\mu(x),\{\bar{f}(4),f(3)\}]\end{aligned}\right\}\quad(49)$$

and the canonical commutators. Eq. (49) can also be used to prove the formula

$$-C^{-1}\Lambda_\mu(-q',q)\,C = \Lambda_\mu^T(-q,q') \quad (50)$$

which is, however, also evident from (24) and the charge invariance of the formalism. From the Lorentz invariance it follows that we can write

$$\Lambda_\mu(p',p) = \sum_{\varrho'=0,1}\sum_{\varrho=0,1}(i\gamma p'+m)^{\varrho'}\,[\gamma_\mu F^{\varrho'\varrho}+p_\mu G^{\varrho'\varrho}+p'_\mu H^{\varrho'\varrho}]\,(i\gamma p+m)^{\varrho} \quad (51)$$

where the functions F, G and H are uniquely defined and depending only on p^2, p'^2, $(p-p')^2$ and the signs $\varepsilon(p)$, $\varepsilon(p')$ and $\varepsilon(p-p')$. From (50) it then follows

$$F^{\varrho\varrho'}(-p, p') = F^{\varrho'\varrho}(-p', p) \tag{52 a}$$

$$G^{\varrho\varrho'}(-p, p') = H^{\varrho'\varrho}(-p', p). \tag{52 b}$$

Utilizing (51) and (52) we get

$$\left.\begin{aligned} ie\langle 0|\overline{\psi}^{(0)}|q'\rangle \Lambda_\mu(-q', q)\langle 0|\psi^{(0)}|q\rangle &= \langle 0|j_\mu^{(0)}|q, q'\rangle R((q+q')^2) \\ &+ \frac{e}{2m} S((q+q')^2)(q_\mu - q'_\mu)\langle 0|\overline{\psi}^{(0)}|q'\rangle\langle 0|\psi^{(0)}|q\rangle \end{aligned}\right\} \tag{53}$$

where, in view of (48),

$$\lim_{-(q+q')^2 \to \infty} R((q+q')^2) = \lim_{-(q+q')^2 \to \infty} S((q+q')^2) = 0. \tag{54}$$

The equations (53) and (54) are the desired result of this paragraph.

Completion of the Proof.

We are now nearly at the end of our discussion. From the assumptions made about $\Pi(p^2)$ (and its consequences for $\overline{\Pi}(p^2)$, cf. the Appendix), Eqs. (53) and (54), the limit of Eq. (24) reduces to

$$\left.\begin{aligned} \lim_{-(q+q')^2 \to \infty} \langle 0|j_\mu|q, q'\rangle &= \langle 0|j_\mu^{(0)}|q, q'\rangle \left[1 + \overline{\Pi}(0) + 2\frac{N-1}{1-L}\right] \\ &= \langle 0|j_\mu^{(0)}|q, q'\rangle \frac{2N-1}{1-L}. \end{aligned}\right\} \tag{55}$$

Our inequality (4) now gives

$$\left.\begin{aligned} \Pi(p^2) &> \frac{V}{-3p^2} \sum_{q+q'=p}{}' |\langle 0|j_\mu|q, q'\rangle|^2 \\ &\to \frac{V}{-3p^2} \sum_{q+q'=p}{}' |\langle 0|j_\mu^{(0)}|q, q'\rangle|^2 \left(\frac{2N-1}{1-L}\right)^2 \\ &= \Pi^{(0)}(p^2)\left(\frac{2N-1}{1-L}\right)^2 \to \frac{e^2}{12\pi^2}\left(\frac{2N-1}{1-L}\right)^2. \end{aligned}\right\} \tag{56}$$

Except for the possibility of N being exactly $\frac{1}{2}$ $\left(\text{independent of } e^2 \text{ and } \frac{m^2}{\mu^2}\right)$ we have then proved that, if all the renormalization constants K, $\frac{1}{N}$ and $\frac{1}{(1-L)}$ are finite, the function $\Pi(p^2)$ cannot approach zero for $-p^2 \to \infty$. This is an obvious contradiction and the only remaining possibility is that at least one (and probably all) of the renormalization constants is infinite.

The case $N = \frac{1}{2}$ is rather too special to be considered seriously. We can note, however, that N must approach 1 for $e \to 0$ and that one of the integrals in I Eq. (75) will diverge at the lower limit for $\mu \to 0$, independent of the value of e. The constant N could thus at the utmost be equal to $\frac{1}{2}$ for some special combination (or combinations) of e^2 and $\frac{m^2}{\mu^2}$. As μ is an arbitrarily small quantity it is hardly possible to ascribe any physical significance to such a solution,. even if it does exist.

The proof presented here makes no pretence at being satisfactory from a rigorous, mathematical point of view. It contains, for example, a large number of interchanges of orders of integrations, limiting processes and so on. From a strictly logical point of view we cannot exclude the possibility that a more singular solution exists where such formal operations are not allowed. It would, however, be rather hard to understand how the excellent agreement between experimental results and lowest order perturbation theory calculations could be explained on the basis of such a solution.

Appendix.

It has been stated and used above that: if

$$\bar{f}(x) = P\int_0^\infty \frac{f(y)}{y-x}\,dy \quad (f(0) = 0) \tag{A. 1}$$

where $f(x)$ is bounded and continuous for all finite values of x and fulfills

$$|f(x+y) - f(x)| < M\,|y| \quad \text{for all } x \tag{A. 2}$$

and if the integral converges, both $f(x)$ and $\bar{f}(x)$ will vanish for large values of the argument. This is not strictly true, and in this appendix we will study that point in some detail.

We begin by proving that if the integral in (A. 1) converges absolutely and if

$$\lim_{x\to\infty} \log x\,|f(x)| = 0 \tag{A. 3}$$

it follows that

$$\lim_{x\to\pm\infty} \bar{f}(x) = 0. \tag{A. 4}$$

(Note that the integral $\int^{\infty} \frac{dx}{x\cdot\log x}$ is *not* convergent and that the vanishing of $f(x)$ is already implicit in (A. 3).) To get an upper bound for $\bar{f}(x)$ when $x > 0$ we write

$$\bar{f}(x) = P\int_0^{\infty} \frac{f(y)}{y-x}\,dy = \left(\int_0^{x/2} + P\int_{x/2}^{3x/2} + \int_{3x/2}^{\infty}\right)\frac{f(y)}{y-x}\,dy. \tag{A. 5}$$

(The limit $x \to -\infty$ is simpler and need not be discussed explicitly.) The absolute value of the first term in (A. 5) is obviously less than

$$\frac{2}{x}\int_0^{x/2} |f(y)|\,dy < \text{const.}\,\frac{2}{x}\int_0^{x/2}\frac{dy}{\log y} \to 0. \tag{A. 6}$$

The last term can be treated in a similar way and yields the result

$$\left|\int_{3x/2}^{\infty}\frac{f(y)}{y-x}\,dy\right| \leq \int_{3x/2}^{\infty}\frac{|f(y)|}{y/3}\,dy \to 0. \tag{A. 7}$$

The remaining term can be written

$$\left.\begin{aligned}\left|P\int_{x/2}^{3x/2}\frac{f(y)}{y-x}\,dy\right| &= \left|\int_0^{x/2}\frac{dy}{y}\,[f(x+y)-f(x-y)]\right| \\ \leq \int_0^{\varepsilon}\frac{dy}{y}\left|f(x+y)-f(x-y)\right| &+ \int_{\varepsilon}^{x/2}\frac{dy}{y}\left|f(x+y)\right| + \int_{\varepsilon}^{x/2}\frac{dy}{y}\left|f(x-y)\right|.\end{aligned}\right\} \tag{A. 8}$$

In view of (A. 2) and (A. 3), the three terms in (A. 8) vanish separately for large values of x. It thus follows

$$\lim_{x \to \infty} \bar{f}(x) = 0 \qquad \text{q. e. d.}$$

As the function $\Pi(p^2)$ is positive the condition (A. 3) seems rather reasonable from a physical point of view. On the other hand, the functions F_k in (45) are not necessarily positive. It is, however, also possible to construct a more general argument where (A. 3) is not used, and where even the vanishing of $f(x)$ is not needed. Instead, we then require that from

$$\bar{f}(x) = P \int_0^{\infty} \frac{f(y)}{y - x}\, dy; \; f(y) = 0 \quad \text{for} \quad y \leqq 0 \tag{A. 9}$$

will follow

$$f(x) = -\frac{1}{\pi^2} P \int_{-\infty}^{+\infty} \frac{\bar{f}(y)}{y - x}\, dy \tag{A. 10}$$

where both $f(x)$ and $\bar{f}(x)$ are finite.

Note that

$$\left.\begin{aligned} &\frac{1}{\pi^2} P \int_{-\infty}^{+\infty} \frac{dz}{(z-x)(z-y)} \\ = &\frac{-1}{4\pi^2} \iint dw_1 dw_2 \int dz e^{i(w_1+w_2)z} \cdot e^{-iw_1 x - iw_2 y} \frac{w_1 w_2}{|w_1 w_2|} \\ &= \frac{1}{2\pi} \int dw_1 e^{iw_1(y-x)} = \delta(y-x). \end{aligned}\right\} \tag{A. 11}$$

It then follows that the integral

$$\int^{\infty} \frac{|1 + \bar{f}(x) + i\pi f(x)|^2}{x}\, dx > \int^{\infty} \frac{|1 + 2\bar{f}(x)|}{x}\, dx$$

is divergent, because the second term is convergent in view of (A. 10). This is everything that is needed for the proof.

It is, of course, possible to construct functions $f(x)$ where (A. 10) does not follow from (A. 9). In that case we are not allowed to interchange the order of the integrations in (A. 11); but we have already excluded such cases from our discussion.

For simplicity, the statement that the functions "vanish" for large values of the variables has been used in the text. If a more careful argument is wanted the phrase

"the functions have the property that the integral

$$\int^{\infty} \frac{f(x)}{x}\, dx$$

converges" should be substituted for the word "vanish" in many places.

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This is the most "useful" paper by Källén. It is written in collaboration with Sabry (see also the contribution by Sabry in Part 2). Its result is used not only in quantum electrodynamics but also in other areas such as quantum chromodynamics, atomic physics and exotic atoms, whenever precision is required. In atomic physics, the correction due to the fourth order is treated as an effective potential called Källén-Sabry potential.

C. Jarlskog (Ed.), *Portrait of Gunnar Källén*, DOI 10.1007/978-3-319-00627-7_93,

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FOURTH ORDER VACUUM POLARIZATION

BY

G. KÄLLÉN AND A. SABRY

CONTENTS

The real and imaginary parts of the kernel for the fourth order vacuum polarization are calculated for all values of the four-dimensional energy momentum vector. If an expansion in powers of the square of this quantity is used, the first coefficient agrees with a result previously obtained by BARANGER *et. al.*

I. Introduction.

In a previous paper, one of us[1] has developed a formulation of renormalized quantum electrodynamics that is slightly different from the standard techniques used by most authors. This modification was introduced because of its convenience in discussions of general principles. It has been applied, for example, to a discussion, avoiding perturbation theory, of the magnitude of the renormalization constants.[2] In the present paper, we wish to show that the new method can also be used with advantage in practical calculations in which perturbation theory is applied, and, as an illustration, the fourth order vacuum polarization has been chosen. BARANGER, DYSON and SALPETER[3] have computed those terms in this effect which are important in the Lamb shift. They present, however, only the result and very few intermediary steps of the calculation. On the other hand, we attempt to give a fairly detailed account of our calculations, and compute not only the terms of immediate experimental interest, but also the complete vacuum polarization kernel as a function of the four-dimensional momentum. As will be seen later, our calculation is simplified to a certain extent by the fact that we can use the result of an earlier calculation of the lowest-order radiative corrections to the current operator[4] and thereby avoid some

[1] G. KÄLLÉN, Helv. Phys. Acta **25**, 417 (1952), in the following quoted as I.
[2] G. KÄLLÉN, Dan. Mat. Fys. Medd. **27**, no. 12 (1953).
[3] M. BARANGER, F. J. DYSON, and E. E. SALPETER, Phys. Rev. 88, 680 (1952).
[4] J. SCHWINGER, Phys. Rev. **76**, 790 (1949).

integrations. Since the main work involved in the calculation of a high-order effect is connected with the integrations over the so-called "Feynman auxiliary variables", a simplification at this point is not without interest. A further advantage of our method is that the questions of regularization[1] and of the so-called "overlapping divergences"[2] are completely avoided. Finally, due to the application of the known expression for the current operator, we need not carry out any explicit mass renormalization in our calculations.

II. General Outline of the Method.

We start from the following formulae given in I:

$$\langle o|\overset{\text{ind}}{j_\mu}(x)|o\rangle = \frac{1}{(2\pi)^4}\int dp\, e^{ipx}(-\overline{\Pi}(p^2)+\overline{\Pi}(0)-i\pi\,\varepsilon(p)\,\Pi(p^2))\,\overset{\text{ext}}{j_\mu}(p), \tag{1}$$

$$\overline{\Pi}(p^2)-\overline{\Pi}(0) = -p^2\int_0^\infty \frac{da\,\Pi(-a)}{a(a+p^2)}, \tag{2}$$

$$\Pi(p^2) = \frac{V}{-3p^2}\sum_{p^{(z)}=p}\langle 0|j_\mu|z\rangle\langle z|j_\mu|0\rangle. \tag{3}$$

The notation is the same as in I and will be used here without further explanation. If the matrix elements of the current operator are expanded in powers of e,

$$j_\mu = ej_\mu^{(0)} + e^2 j_\mu^{(1)} + e^3 j_\mu^{(2)} + \cdots, \tag{4}$$

the first non-vanishing contribution to the function $\Pi(p^2)$ will be

$$\Pi^{(0)}(p^2) = \frac{Ve^2}{-3p^2}\sum_{p^{(z)}=p}\langle 0|j_\mu^{(0)}|z\rangle\langle z|j_\mu^{(0)}|0\rangle. \tag{5}$$

[1] W. Pauli and F. Villars, Rev. Mod. Phys. 21, 434 (1949). The regularization of the fourth order vacuum polarization has been discussed by E. Karlson, Arkiv f. Fysik 7, 221 (1954).

[2] A. Salam, Phys. Rev. 82, 217 (1951). For the special problem of fourth order vacuum polarization, the overlapping divergences have been discussed by R. Jost and J. M. Luttinger, Helv. Phys. Acta 23, 201 (1949).

This expression can be computed easily and gives

$$\Pi^{(0)}(p^2) = \frac{e^2}{12\pi^2}\left(1-\frac{2m^2}{p^2}\right)\sqrt{1+\frac{4m^2}{p^2}}\,\theta(-p^2-4m^2), \tag{6}$$

$$\theta(x) = \frac{1}{2}\left[1+\frac{x}{|x|}\right], \tag{6a}$$

$$\left.\begin{aligned}&\overline{\Pi}^{(0)}(p^2)-\overline{\Pi}^{(0)}(0) = \\ &-\frac{e^2}{12\pi^2}\left[\frac{4m^2}{p^2}-\frac{5}{3}+\left(1-\frac{2m^2}{p^2}\right)\sqrt{1+\frac{4m^2}{p^2}}\log\frac{1+\sqrt{1+\frac{4m^2}{p^2}}}{\left|1-\sqrt{1+\frac{4m^2}{p^2}}\right|}\right].\end{aligned}\right\} \tag{7}$$

The subsequent term in the expansion of the function $\Pi(p^2)$ is of order e^4, and contains the following terms:

$$\Pi^{(1)}(p^2) = \Pi_a^{(1)}(p^2)+\Pi_b^{(1)}(p^2), \tag{8}$$

$$\Pi_a^{(1)}(p^2) = \frac{Ve^4}{-3p^2}\sum_{p^{(z)}=p}\langle 0|j_\mu^{(1)}|z\rangle\langle z|j_\mu^{(1)}|0\rangle, \tag{9}$$

$$\Pi_b^{(1)}(p^2) = \frac{Ve^4}{-3p^2}\sum_{p^{(z)}=p}\langle 0|j_\mu^{(2)}|z\rangle\langle z|j_\mu^{(0)}|0\rangle+\text{complex conjugate}. \tag{10}$$

The expansion of the current operator has been computed earlier.[1] From these results it can be seen that the term (9) gets contributions from states with one in-coming pair and one incoming photon.[2] These matrix elements are

$$\left.\begin{aligned}\langle 0|j_\mu^{(1)}|q,q',k\rangle = \frac{e^2}{V^{3/2}}\frac{1}{\sqrt{2\omega}}\bar{u}^{(-)}(-q')\Big[\gamma_\mu\frac{i\gamma(q+k)-m}{2qk-\mu^2}\gamma e \\ -\gamma e\frac{i\gamma(q'+k)+m}{2q'k-\mu^2}\gamma_\mu\Big]u^{(+)}(q).\end{aligned}\right\} \tag{11}$$

The notation in the last expression is self-explanatory, except possibly for the quantities $u^{(\pm)}(q)$. These are the normalized

[1] Cf., e. g., G. Källén, Arkiv f. Fysik 2, 371 (1950).

[2] For the definition of particle numbers for these physical states, cf., e. g., G. Källén, Physica 19, 850 (1953).

plane-wave solutions of the free-particle Dirac equation. The index (+) refers to solutions with positive energy and the index (—) to solutions with negative energy. The vector e is the polarization vector of the photon; V is the volume of periodicity and μ a small photon mass introduced to handle infrared divergences. In the computation of the function $\Pi_a^{(1)}(p^2)$, we must "square" the expression (11) and sum over all states where $k+q+q'=p$. Using well-known properties of the functions u, and taking the limit $V\to\infty$, we can write this sum as an integral

$$\left.\begin{aligned} & e^4 V \sum_{q+q'+k=p} \langle 0 | j_\mu^{(1)} | q, q', k\rangle \langle k, q', q | j_\mu^{(1)} | 0\rangle = \\ & -\frac{e^4}{(2\pi)^6}\int dk\, dq\, dq'\, \delta(p-q-q'-k)\,\delta(q^2+m^2)\,\theta(q)\,\delta(q'^2+m^2)\,\theta(q') \\ & \times \delta(k^2+\mu^2)\,\theta(k)\, Sp\left[(i\gamma q'+m)\left(\gamma_\mu \frac{i\gamma(q+k)-m}{2\,qk-\mu^2}\gamma_\lambda\right.\right. \\ & -\left.\gamma_\lambda \frac{i\gamma(q'+k)+m}{2q'k-\mu^2}\gamma_\mu\right)(i\gamma q-m)\left(\gamma_\mu \frac{i\gamma(q'+k)+m}{2\,q'k-\mu^2}\gamma_\lambda\right. \\ & \left.\left. -\gamma_\lambda \frac{i\gamma(q+k)-m}{2\,qk-\mu^2}\gamma_\mu\right)\right]. \end{aligned}\right\} \quad (12)$$

The evaluation of this integral, which is the main task in our computation, is given in a later paragraph.

The first approximation to the current, $j_\mu^{(0)}$, has matrix elements which connect the vacuum only to states with one incoming pair. Hence, the expression (10) will reduce to a sum over states with one in-coming pair

$$\Pi_b^{(1)}(p^2) = \frac{Ve^4}{-3\,p^2}\sum_{q+q'=p}\langle 0|j_\mu^{(2)}|q,q'\rangle\langle q',q|j_\mu^{(0)}|0\rangle + \text{complex conjugate.} \quad (13)$$

As has been mentioned in the introduction, the matrix elements $\langle 0|j_\mu^{(2)}|q,q'\rangle$ have been computed by SCHWINGER.[1] We write his result as

$$\left.\begin{aligned} & e^2\langle 0|j_\mu^{(2)}|q,q'\rangle = [-\overline{\Pi}^{(0)}(p^2)+\overline{\Pi}^{(0)}(0)+\overline{R}(p^2)-\overline{R}(0)+\overline{S}(0)-i\pi(\Pi^{(0)}(p^2) \\ & -R(p^2))]\,\langle 0|j_\mu^{(0)}|q,q'\rangle-\frac{1}{2\,m}(q_\mu-q'_\mu)\,[\overline{S}(p^2)+i\pi\, S(p^2)]\,\langle 0|\overline{\psi}^{(0)}\psi^{(0)}|q,q'\rangle, \end{aligned}\right\} (14)$$

[1] Footnote 4, p. 3.

$$R(p^2) = \frac{e^2}{8\pi^2}\left\{\frac{1+\frac{2m^2}{p^2}}{1+\frac{4m^2}{p^2}}\log\left(-\frac{p^2+4m^2}{\mu^2}\right)-\frac{3}{2}\right\}\sqrt{1+\frac{4m^2}{p^2}}\,\theta(-p^2-4m^2), \quad (15)$$

$$\left.\begin{aligned}
\overline{R}(p^2)-\overline{R}(0) = \frac{e^2}{4\pi^2}&\left\{\frac{1+\frac{2m^2}{p^2}}{\sqrt{1+\frac{4m^2}{p^2}}}\left[-\Phi\left(-\frac{1-\sqrt{1+\frac{4m^2}{p^2}}}{1+\sqrt{1+\frac{4m^2}{p^2}}}\right)+\frac{\pi^2}{4}\theta(-p^2)\right.\right.\\
&-\frac{1}{4}\log^2\frac{1+\sqrt{1+\frac{4m^2}{p^2}}}{\left|1-\sqrt{1+\frac{4m^2}{p^2}}\right|}+\log\frac{1+\sqrt{1+\frac{4m^2}{p^2}}}{\left|1-\sqrt{1+\frac{4m^2}{p^2}}\right|}\cdot\log\frac{2\sqrt{1+\frac{4m^2}{p^2}}}{1+\sqrt{1+\frac{4m^2}{p^2}}}\Bigg]\\
&+\frac{3}{4}\left[\sqrt{1+\frac{4m^2}{p^2}}\log\frac{1+\sqrt{1+\frac{4m^2}{p^2}}}{\left|1-\sqrt{1+\frac{4m^2}{p^2}}\right|}-2\right]\\
&\left.+\log\frac{m}{\mu}\left[1-\frac{1+\frac{2m^2}{p^2}}{\sqrt{1+\frac{4m^2}{p^2}}}\log\frac{1+\sqrt{1+\frac{4m^2}{p^2}}}{\left|1-\sqrt{1+\frac{4m^2}{p^2}}\right|}\right]\right\},
\end{aligned}\right\} \quad (16)$$

$$S(p^2) = -\frac{e^2}{4\pi^2}\frac{m^2}{p^2}\frac{\theta(-p^2-4m^2)}{\sqrt{1+\frac{4m^2}{p^2}}}, \quad (17)$$

$$\overline{S}(p^2) = \frac{e^2}{8\pi^2}\frac{2\frac{m^2}{p^2}}{\sqrt{1+\frac{4m^2}{p^2}}}\log\frac{1+\sqrt{1+\frac{4m^2}{p^2}}}{\left|1-\sqrt{1+\frac{4m^2}{p^2}}\right|}. \quad (18)$$

The connection between the functions $R(p^2)$ and $\overline{R}(p^2)$, and between $S(p^2)$ and $\overline{S}(p^2)$, is the same as the connection between $\Pi(p^2)$ and $\overline{\Pi}(p^2)$, which is given in Eq. (2). This is a consequence of the "causal" structure of the theory which says that the value of the current in one point x can depend only on the previous history of the system inside the retarded light-cone belonging to x. If Eq. (14) is written in x-space, we get a relation of the form

$$\left.\begin{aligned} \langle 0 \,|\, j_\mu^{(2)}(x) \,|\, q, q' \rangle &= \int dx' F(x-x') \langle 0 \,|\, j_\mu^{(0)}(x') \,|\, q, q' \rangle \\ +\int G(x-x') \langle 0 \,|\, &\frac{\partial \overline{\psi}^{(0)}(x')}{\partial x'_\mu} \psi^{(0)}(x') - \overline{\psi}^{(0)}(x') \frac{\partial \psi^{(0)}(x')}{\partial x'_\mu} \,|\, q, q' \rangle . \end{aligned}\right\} \quad (14\text{a})$$

Causality requires $F(x)$ and $G(x)$ to vanish if $x_0 < 0$ and this gives, in a well-known way, the relations involving the Hilbert transformations. This offers a new possibility of computing the matrix element under discussion by first computing the "imaginary parts" $R(p^2)$ and $S(p^2)$, which can be obtained by integrating over finite domains in momentum space and, subsequently, computing the "real parts" with the aid of Hilbert transformations. Actually, a calculation of this kind has been performed. However, it has not been found to be much simpler than the standard methods for this problem. On the other hand, arranging the computation in this way is certainly not a more complicated procedure. We will not insist on this point here, but accept the results (14) — (18) as they stand. Consequently, the computation of the function $\Pi_b^{(1)}(p^2)$ will be reduced to simple algebraic manipulations of these expressions. The function $\Phi(x)$ in (16) is defined by the integral

$$\Phi(x) = \int_1^x \frac{dt}{t} \log |1+t| . \quad (19)$$

Hereby it is supposed that the argument x is real, *i. e.* that $1 + \frac{4m^2}{p^2} > 0$. This will be sufficient at this stage. The integral $\Phi(x)$ has many interesting properties which will be of some use in our calculation and that are discussed in the Appendix.

We now write the function $\Pi_b^{(1)}(p^2)$ as

$$\left.\begin{aligned} \Pi_b^{(1)}(p^2) = 2\, \Pi^{(0)}(p^2) \,[&-\overline{\Pi}^{(0)}(p^2) + \overline{\Pi}^{(0)}(0) + \overline{R}(p^2) \\ &-\overline{R}(0) + \overline{S}(0)] + 2\,\overline{S}(p^2)\, X(p^2), \end{aligned}\right\} \quad (20)$$

where

$$\left.\begin{aligned} X(p^2) = \frac{4e^2}{3\,p^2}\frac{1}{(2\,\pi)^3}\int dq\,dq'\,\delta(p-q-q') \\ \times\,\delta(q^2+m^2)\,\theta(q)\,\delta(q'^2+m^2)\,\theta(q')\,(qq'+m^2). \end{aligned}\right\} \quad (21)$$

The last expression is easily computed

$$X(p^2) = \frac{e^2}{24\,\pi^2}\left(1+\frac{4\,m^2}{p^2}\right)^{3/2}\theta(-p^2-4\,m^2). \quad (22)$$

Collecting all these results, we have

$$\left.\begin{aligned} \Pi_b^{(1)}(p^2) = \frac{e^4}{48\,\pi^4}\Big[\frac{\delta}{3}(3-\delta^2)\left(-\frac{17}{3}+\delta^2\right)+\frac{\delta^2}{2}\left(7-3\,\delta^2+\frac{1}{3}\delta^4\right)\log\frac{1+\delta}{1-\delta} \\ +\log\frac{m}{\mu}\,\delta\,(3-\delta^2)\left(1-\frac{1+\delta^2}{2\,\delta}\log\frac{1+\delta}{1-\delta}\right)-\frac{1}{2}(3-\delta^2)(1+\delta^2)\Big(\Phi\left(-\frac{1-\delta}{1+\delta}\right) \\ -\frac{\pi^2}{4}+\frac{1}{4}\log^2\frac{1+\delta}{1-\delta}-\log\frac{1+\delta}{1-\delta}\cdot\log\frac{1+\delta}{2\,\delta}\Big)\Big]\,\theta(1-\delta), \end{aligned}\right\} \quad (23)$$

where

$$\delta = \sqrt{1+\frac{4\,m^2}{p^2}} > 0. \quad (24)$$

III. Discussion of the Part $\Pi_a^{(1)}(p^2)$.

The remaining part of the function $\Pi(p^2)$, the integral (12), can be treated in the following way. We first compute the trace of the γ-matrices. This is a straightforward calculation and the necessary work can be considerably reduced by performing first the summations over the indices μ and λ. This can be done with the aid of the well-known formulae

$$\gamma_\lambda\gamma_{\nu_1}\gamma_{\nu_2}\cdots\gamma_{\nu_{2n+1}}\gamma_\lambda = -2\,\gamma_{\nu_{2n+1}}\cdots\gamma_{\nu_2}\gamma_{\nu_1} \quad (25)$$

$$\gamma_\lambda\gamma_{\nu_1}\gamma_{\nu_2}\gamma_\lambda = 4\,\delta_{\nu_1\nu_2}. \quad (26)$$

The complete trace can then be written

$$Sp[\cdots] = \frac{S^{(1)}(q,q')}{(2\,qk-\mu^2)^2} + \frac{S^{(1)}(q',q)}{(2\,q'\,k-\mu^2)^2} + \frac{S^{(2)}(q,q')+S^{(2)}(q',q)}{(2\,qk-\mu^2)(2q'k-\mu^2)}, \tag{27}$$

$$S^{(1)}(q,q') = -32\,[kq\cdot kq' + 2\,m^2\,qk + m^2\,q'k + m^2\,(qq'-2\,m^2)], \tag{28}$$

$$S^{(2)}(q,q') = -16\,[2\,(qq')^2 - 4\,m^2\cdot qq' + 2\,(kq+kq')\,qq' - m^2\,(kq+kq')]. \tag{29}$$

Terms containing μ^2 have been dropped in (28) and (29), as they will obviously vanish in the limit $\mu \to 0$.

Our next task is to compute an integral of the form

$$\left.\begin{aligned} J = \int dk\,dq\,dq'\,\delta(p-k-q-q')\,\delta(q^2+m^2)\,\delta(q'^2+m^2)\,\delta(k^2+\mu^2) \\ \times\,\theta(q)\,\theta(q')\,\theta(k)\,F(qk,q'k,qq'). \end{aligned}\right\} \tag{30}$$

This can conveniently be done in two steps. We first consider

$$\left.\begin{aligned} I(p'^2,kp') = \int dq\,\delta(q^2+m^2)\,\theta(q)\,\delta((p'-q)^2+m^2)\,\theta(p'-q) \\ \times\,F(qk,p'k-qk,p'q-q^2). \end{aligned}\right\} \tag{31}$$

This is a *finite* integral and we compute it in the special coordinate system where the space-like components of the vector p' vanish. We then obtain

$$I(-p_0'^2,-k_0p_0') = \pi\int_{x_1}^{x_2}\frac{dx}{2\,p_0'\,|\bar{k}|}\,F\left(x,-k_0p_0'-x,m^2-\frac{1}{2}\,p_0'^2\right)\theta\,(p_0'^2-4\,m^2), \tag{32}$$

$$x_{1,2} = -\frac{1}{2}\,k_0\,p_0' \mp |\bar{k}|\sqrt{\frac{1}{4}\,p_0'^2-m^2}. \tag{32a}$$

We now write this result in an invariant way, as

$$I(p'^2,kp') = \frac{\pi}{2\sqrt{(kp')^2+\mu^2p'^2}}\int_{x_1}^{x_2}dx\,F\left(x,kp'-x,m^2+\frac{1}{2}\,p'^2\right)\theta(-p'^2-4\,m^2), \tag{33}$$

$$x_{1,2} = \frac{1}{2} kp' \mp \frac{1}{2} \sqrt{(kp')^2 + \mu^2 p'^2} \cdot \sqrt{1 + \frac{4\,m^2}{p'^2}}, \qquad (33\,\mathrm{a})$$

and treat the next integration similarly. The result is

$$\left.\begin{aligned} J &= \int dk\, \delta(k^2+\mu^2)\, \theta(k)\, I((p-k)^2, kp+\mu^2) = \\ &-\frac{\pi^2}{2\,p^2} \int_{\mu\sqrt{-p^2}}^{-\frac{1}{2}(p^2+4\,m^2)} dy \int_{-\xi}^{+\xi} dz\, F\left(\frac{z-y+\mu^2}{2}, \frac{-z-y+\mu^2}{2}, y+m^2+\frac{p^2-\mu^2}{2}\right), \end{aligned}\right\} \qquad (34)$$

$$\xi = \sqrt{1 + \frac{4\,m^2}{p^2 - \mu^2 + 2\,y}} \cdot \sqrt{y^2 + \mu^2 p^2}. \qquad (34\,\mathrm{a})$$

Applying this technique to the integral (12), we get

$$\Pi_a^{(1)}(p^2) = \frac{e^2}{12\,\pi^4} \frac{1}{p^4} (A+B)\, \theta(-p^2 - 4\,m^2), \qquad (35)$$

$$A = \int_{\mu\sqrt{-p^2}}^{-\frac{1}{2}(p^2+4\,m^2)} dy \int_{-\xi}^{+\xi} dz \left[-\frac{1}{2} + \frac{m^2-y}{z-y} + \frac{y}{y^2-z^2}(p^2-m^2)\right], \qquad (36)$$

$$B = \int_{\mu\sqrt{-p^2}}^{-\frac{1}{2}(p^2+4\,m^2)} dy \int_{-\xi}^{+\xi} dz\, (p^2-2\,m^2) \left[\frac{m^2}{(z-y)^2} + \frac{1}{2}\frac{p^2+2\,m^2}{y^2-z^2}\right]. \qquad (37)$$

To obtain these expressions, we have introduced the quantities y and z into (27), which becomes

$$\left.\begin{aligned} Sp\,[\cdots] = -32 \Big\{ &-\frac{1}{2} + \frac{m^2-y}{z-y} + \frac{m^2(p^2-2\,m^2)}{(z-y)^2} \\ &+ \frac{1}{y^2-z^2}\left(y\,(p^2-m^2) + \frac{1}{2}(p^4 - 4\,m^4)\right)\Big\}. \end{aligned}\right\} \qquad (38)$$

The quantity A will stay finite in the limit $\mu \to 0$ and can be expressed in elementary functions. After some straightforward calculations we get the result

$$A = \frac{3}{64} p^4 \left[2\delta\,(5 - 3\,\delta^2) - (5 + 6\,\delta^2 - 3\,\delta^4) \log \frac{1+\delta}{1-\delta} \right]. \tag{39}$$

The quantity B is a little more tricky to handle and the limit $\mu \to 0$ cannot be performed in all terms. We write (37) as

$$B = \frac{1}{8} p^4 (3 - \delta^2) \left[-(1-\delta^2)\, B^{(1)} + (1+\delta^2)\, B^{(2)} \right], \tag{40}$$

where

$$B^{(1)} = 2\, I(1), \tag{41}$$

$$B^{(2)} = \int_{-1}^{+1} I(z)\, dz, \tag{42}$$

$$I(z) = \int_{\mu\sqrt{-p^2}}^{-\frac{p^2}{2}\delta^2} \frac{\xi\, dy}{y^2 - \xi^2 z^2} = \int_{\varepsilon}^{\delta^2} dy \frac{\sqrt{y^2 - \varepsilon^2} \cdot \sqrt{1 - \frac{1-\delta^2}{1-y}}}{y^2 - z^2 (y^2 - \varepsilon^2) \left[1 - \frac{1-\delta^2}{1-y} \right]}, \tag{43}$$

$$\varepsilon = \frac{2\mu}{\sqrt{-p^2}}. \tag{43 a}$$

The term containing the logarithmic dependence on μ in $I(z)$ can be split off in the following way:

$$I(z) = \int_{\varepsilon}^{\delta^2} \frac{\sqrt{y^2 - \varepsilon^2} \cdot \delta\, dy}{y^2 - \delta^2 z^2 (y^2 - \varepsilon^2)} + \int_0^{\delta^2} \frac{dy}{y} \left[\frac{\sqrt{1 - \frac{1-\delta^2}{1-y}}}{1 - z^2 \left(1 - \frac{1-\delta^2}{1-y} \right)} - \frac{\delta}{1 - \delta^2 z^2} \right]. \tag{44}$$

In the first integral, we make the transformation $1 - \frac{\varepsilon^2}{y^2} = t^2$ and rewrite it as

$$\left. \begin{aligned} \int_{\varepsilon^2}^{\delta^2} \frac{dy}{y} \frac{\delta \cdot \sqrt{1 - \varepsilon^2/y^2}}{1 - z^2 \delta^2 (1 - \varepsilon^2/y^2)} &= \frac{\delta}{1 - z^2 \delta^2} \int_0^{\sqrt{1-\varepsilon^2/\delta^2}} \frac{dt}{1-t} \\ + \delta \int_0^1 \frac{dt}{1-t} &\left[\frac{2\, t^2}{(1+t)(1 - z^2 \delta^2 t^2)} - \frac{1}{1 - z^2 \delta^2} \right]. \end{aligned} \right\} \tag{45}$$

The remaining integrations can now be performed without difficulty, and $I(z)$ is found to be

$$I(z) = \frac{1}{1-z^2}\left(z\log\frac{1+\delta z}{1-\delta z} - \log\frac{1+\delta}{1-\delta}\right) - \left(\frac{1}{2z} + \frac{3\delta^2 z}{2(1-\delta^2 z^2)}\right)\log\frac{1+\delta z}{1-\delta z} + \frac{\delta}{1-\delta^2 z^2}\log\frac{8\delta^2}{\varepsilon(1-\delta^2)}. \tag{46}$$

The last integration in (42) introduces again the function $\Phi(x)$. With the aid of the formulae given in the Appendix we can write the result as

$$B^{(2)} = -3\,\Phi\left(-\frac{1-\delta}{1+\delta}\right) - 2\,\Phi\left(\frac{1-\delta}{1+\delta}\right) - \frac{3}{4}\pi^2 + \log\frac{1+\delta}{1-\delta}\left[\log\frac{m}{\mu} + \frac{1}{4}\log\frac{1+\delta}{1-\delta} + \log\frac{(1+\delta)^2}{4\delta}\right]. \tag{47}$$

The remaining part of the calculation is purely algebraic in nature. Collecting previous results, we get

$$\begin{aligned} \Pi_a^{(1)}(p^2) = \frac{e^4}{48\pi^4}\Big[&\frac{\delta}{8}(39-17\delta^2) + \frac{1}{16}(33-10\delta^2+\delta^4)\log\frac{1+\delta}{1-\delta} \\ &-\frac{\delta}{2}(3-\delta^2)\log\frac{64\delta^4}{(1-\delta^2)^3} - \delta(3-\delta^2)\left[1-\frac{1+\delta^2}{2\delta}\log\frac{1+\delta}{1-\delta}\right]\cdot\log\frac{m}{\mu} \\ &-\frac{(3-\delta^2)(1+\delta^2)}{2}\left(3\,\Phi\left(-\frac{1-\delta}{1+\delta}\right) + 2\,\Phi\left(\frac{1-\delta}{1+\delta}\right) + \frac{3}{4}\pi^2 - \frac{1}{4}\log^2\frac{1+\delta}{1-\delta} \right.\\ &\left.- \log\frac{1+\delta}{1-\delta}\log\frac{(1+\delta)^2}{4\delta}\right)\Big]\,\theta(1-\delta). \end{aligned} \tag{48}$$

Adding (48) and (23), we find that the terms depending on μ cancel. In this way we obtain

$$\begin{aligned} \Pi^{(1)}(p^2) = \frac{\alpha^2}{3\pi^2}\Big\{&\delta\left[-\frac{19}{24}+\frac{55}{72}\delta^2 - \frac{\delta^4}{3} - \frac{3-\delta^2}{2}\log\frac{64\delta^4}{(1-\delta^2)^3}\right] \\ &+\log\frac{1+\delta}{1-\delta}\left[\frac{33}{16}+\frac{23}{8}\delta^2 - \frac{23}{16}\delta^4 + \frac{1}{6}\delta^6 + \left(\frac{3}{2}+\delta^2-\frac{\delta^4}{2}\right)\log\frac{(1+\delta)^3}{8\delta^2}\right] \\ &-\left(\frac{3}{2}+\delta^2-\frac{1}{2}\delta^4\right)\left[4\,\Phi\left(-\frac{1-\delta}{1+\delta}\right) + 2\,\Phi\left(\frac{1-\delta}{1+\delta}\right) + \frac{\pi^2}{2}\right]\Big\}\,\theta(1-\delta), \end{aligned} \tag{49}$$

$$\alpha = \frac{e^2}{4\pi}. \tag{49a}$$

This is our expression for the imaginary part of the kernel (1). According to (2), the real part is obtained after a Hilbert transformation of this expression. This will be discussed in the next paragraph.

IV. The Real Part of the Vacuum Polarization Kernel.

So far, all our results are given as functions of the quantity δ defined in (24). It is therefore convenient to introduce a new variable of integration instead of a in (2). If we put

$$1 - \frac{4m^2}{a} = z^2, \tag{50}$$

we get

$$\overline{\Pi}^{(1)}(p^2) - \overline{\Pi}^{(1)}(0) = 2\int_0^1 \frac{zdz}{z^2-\delta^2}\,\Pi^{(1)}(\delta = z). \tag{51}$$

Not all the integrations in (51) can be carried out explicitly, with the result expressed by elementary functions or by the function $\Phi(x)$. The new integrals which appear can be written in the standard form

$$F(x, y) = \int_0^1 \frac{dt}{t}\log|1+xt|\cdot\log|1+yt|. \tag{52}$$

All the necessary integrals over the function $\Phi(x)$ can be expressed in terms of this $F(x, y)$

$$P\int_0^a \frac{dz}{z+b}\,\Phi(z) = \Phi(a)\log\left|1+\frac{a}{b}\right| - F\left(a, \frac{a}{b}\right). \tag{53}$$

In our final result, one of the variables in $F(x, y)$ has only a very small number (three) of different values. We therefore introduce the following three integrals, each of which depends on only one variable:

$$F(x) = \int_{-1}^{+1} \frac{dt}{t} \log(1+t) \log\left|1 - \frac{t^2}{x}\right|, \tag{54}$$

$$G(x) = \int_{-1}^{+1} \frac{dt}{1+t} \log\frac{1-t}{2} \cdot \log\left|1 - \frac{t^2}{x}\right|, \tag{55}$$

$$H(x) = \int_{-1}^{+1} \frac{dt}{1+t} \log|t| \log\left|1 - \frac{t^2}{x}\right|. \tag{56}$$

The remaining integrations are then straightforward, and not too time consuming. The result can be written as

$$\begin{aligned}\overline{\Pi}^{(1)}(p^2) - \overline{\Pi}^{(1)}(0) = \frac{\alpha^2}{3\pi^2}\Big\{&-\frac{13}{108} + \frac{11}{72}\delta^2 - \frac{1}{3}\delta^4 + \delta\left(\frac{19}{24} - \frac{55}{72}\delta^2\right. \\ &\left.+\frac{1}{3}\delta^4\right)\log\frac{1+\delta}{|1-\delta|} - \left(\frac{33}{32} + \frac{23}{16}\delta^2 - \frac{23}{32}\delta^4 + \frac{\delta^6}{12}\right)\left(\log^2\frac{1+\delta}{|1-\delta|} - \pi^2\theta(1-\delta)\right) \\ &+ \delta(3-\delta^2)\left[\Phi\left(\frac{1-\delta}{1+\delta}\right) + 2\Phi\left(-\frac{1-\delta}{1+\delta}\right) + \frac{\pi^2}{4} - \frac{3}{4}\pi^2\theta(1-\delta)\right. \\ &\left.-\frac{3}{4}\log^2\frac{1+\delta}{|1-\delta|} + \frac{1}{2}\log\frac{1+\delta}{|1-\delta|}\log\frac{64\,\delta^4}{|1-\delta^2|^3}\right] \\ &+ (3 + 2\delta^2 - \delta^4)\left[F(\delta^2) + \frac{3}{2}G(\delta^2) - H(\delta^2)\right]\Big\}.\end{aligned} \tag{57}$$

If this expression is expanded in powers of δ^{-1}, the first non-vanishing term will be of order δ^{-2}. The same conclusion can also be obtained from a study of Eq. (51). If this expression is expanded in powers of δ^{-1}, we get immediately

$$\overline{\Pi}^{(1)}(p^2) - \overline{\Pi}^{(1)}(0) = -\frac{2}{\delta^2}\int_{-1}^{+1} z\,dz\,\Pi^{(1)}(\delta = z) + \cdots. \tag{58}$$

The numerical coefficient of the first power of δ^{-2} has been computed from Eq. (57) and with the aid of the integration indicated in Eq. (58). The agreement of the results serves as a check on the calculations. In either way we obtain

$$\overline{\Pi}^{(1)}(p^2) - \overline{\Pi}^{(1)}(0) = -\frac{1}{\delta^2}\cdot\frac{\alpha^2}{\pi^2}\cdot\frac{82}{81} + \cdots = -\frac{p^2}{m^2}\frac{\alpha^2}{\pi^2}\frac{41}{162} + \cdots. \quad (59)$$

This also agrees with the result obtained by BARANGER, DYSON, and SALPETER.[1]

In Eq. (57) it is supposed that δ is real, that is, p^2 is either positive or less than $-4\,m^2$. For $0 < -p^2 < 4\,m^2$, δ will be purely imaginary. In this case we have to substitute arctangent functions for logarithms, according to the following rules:

$$\delta \log\frac{1+\delta}{|1-\delta|} \rightarrow 2\,\eta \operatorname{arctang}\frac{1}{\eta}, \quad (\eta = i\,\delta > 0), \quad (60\text{ a})$$

$$\log^2\frac{1+\delta}{|1-\delta|} - \pi^2\,\theta\,(1-\delta) \rightarrow -4 \operatorname{arctang}^2\frac{1}{\eta}, \quad (60\text{ b})$$

$$\left.\begin{aligned} &\delta\left[\Phi\left(\frac{1-\delta}{1+\delta}\right) + 2\,\Phi\left(-\frac{1-\delta}{1+\delta}\right) + \frac{\pi^2}{4} - \frac{3}{4}\pi^2\,\theta\,(1-\delta) - \frac{3}{4}\log^2\frac{1+\delta}{|1-\delta|}\right.\\ &\left.+\frac{1}{2}\log\frac{1+\delta}{|1-\delta|}\log\frac{64\,\delta^4}{|1-\delta^2|^3}\right] \rightarrow \eta\left[\psi\left(2\operatorname{arctang}\frac{1}{\eta}\right) - 2\,\psi\,(2\operatorname{arctang}\eta)\right.\\ &\qquad\left. + \operatorname{arctg}\frac{1}{\eta}\log\frac{64\,\eta^4}{(1+\eta^2)^3}\right], \end{aligned}\right\} \quad (60\text{ c})$$

$$\psi(x) = \sum_{1}^{\infty} \frac{\sin(nx)}{n^2}. \quad (60\text{ d})$$

At the point $p^2 = -4\,m^2$, or $\delta = 0$, the expression (57) has a logarithmic singularity. If, during the calculation, the photon mass μ had been kept different from zero in *all* places, our result would have shown a finite peak at this point. For practical applications, the weak logarithmic infinity will not be very harmful, as one is in general interested in convolution integrals involving the function $\overline{\Pi}(p^2) - \overline{\Pi}(0)$. In such expressions, the result (57) will be sufficient. For large values of $|p^2/m^2|$, our function behaves as $\log^2|p^2/m^2|$. Fig. 1 gives a qualitative idea of the behaviour of the fourth approximation of the vacuum polarization kernel as a function of $-p^2/m^2$. A figure of the corresponding behaviour of the functions $\Pi^{(0)}(p^2)$ and $\overline{\Pi}^{(0)}(p^2) - \overline{\Pi}^{(0)}(0)$ would be rather

[1] Footnote 3, page 3.

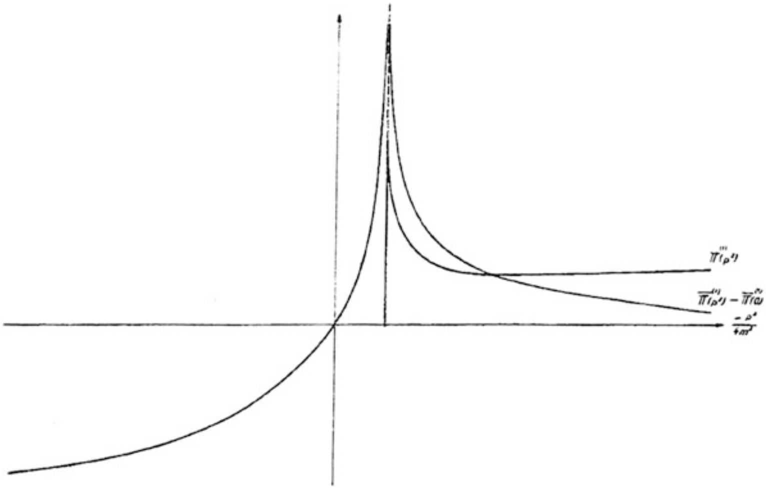

Fig. 1. Qualitative behaviour of the e^4 approximation of the real and of the imaginary parts of the vacuum polarization kernel.

similar to Fig. 1. The only qualitative difference would be that the function $\Pi^{(0)}(p^2)$ vanishes at the point $-p^2 = 4\,m^2$ and that the function $\overline{\Pi}^{(0)}(p^2) - \overline{\Pi}^{(0)}(0)$ has a finite peak at this point.

Appendix.

In the following are given some formulae involving the function $\Phi(x)$, defined in Eq. (19). Although practically all these expressions can be found in the literature,[1] we add this summary for the reader's convenience.

If x is real, our function is defined by

$$\Phi\,(x) = \int_1^x \frac{dz}{z} \log|\,1+z\,|. \tag{A. 1}$$

If we consider

$$\Phi\left(\frac{1}{x}\right) = \int_1^{1/x} \frac{dz}{z} \log|\,1+z\,|, \tag{A. 2}$$

[1] Cf., *e.g.*, K. Mitchell, Phil. Mag. **40**, 351 (1949) and W. Gröbner, N. Hofreiter, Integraltafeln, Wien and Innsbruck, 1950.

and make the variable transformation $t = z^{-1}$, we get the fundamental relation

$$\left.\begin{aligned}\Phi\left(\frac{1}{x}\right) &= -\int_1^x \frac{dt}{t}\log\left|\frac{1+t}{t}\right| - 2\,\theta(-x)\int_{-1}^{+1}\frac{dz}{z}\log|1+z| \\ &= -\Phi(x) + \frac{1}{2}\log^2|x| - \frac{\pi^2}{2}\,\theta(-x)\end{aligned}\right\} \quad (A.\ 3)$$

or

$$\Phi(x) + \Phi\left(\frac{1}{x}\right) = \frac{1}{2}\log^2|x| - \frac{\pi^2}{2}\theta(-x). \qquad (A.\ 4)$$

An integration by parts in the definition (A. 1) will give another useful formula

$$\left.\begin{aligned}\Phi(x) &= \Big[\log|z|\log|1+z|\Big]_1^x - \int_1^x \frac{dz}{1+z}\log|z| \\ &= \log|x|\cdot\log|1+x| - \int_2^{1+x}\frac{dz}{z}\log|1-z|,\end{aligned}\right\} \quad (A.\ 5)$$

or

$$\Phi(x) + \Phi(-1-x) = -\frac{\pi^2}{3} + \log|x|\cdot\log|1+x|. \qquad (A.\ 6)$$

Besides (A. 4) and (A. 6), we also mention the formula

$$\left.\begin{aligned}\Phi(x) + \Phi(-x) &= \int_1^x \frac{dz}{z}\log|1-z^2| + \int_{-1}^{+1}\frac{dz}{z}\log|1-z| \\ &= \frac{1}{2}\Phi(-x^2) - \frac{\pi^2}{8}.\end{aligned}\right\} \quad (A.\ 7)$$

Another relation which has been of some use in the calculations can be obtained in the following way:

$$\Phi(x) - \Phi(-x) = \int_1^x \frac{dz}{z}\log\left|\frac{1+z}{1-z}\right| + \frac{\pi^2}{4}. \qquad (A.\ 8)$$

The transformation $1 + t = \frac{1-z}{1+z}$ transfers this integral to

$$\left.\begin{aligned}\Phi(x) - \Phi(-x) &= \frac{\pi^2}{4} - \int_{-1}^{-\frac{2x}{1+x}} dt \left[\frac{1}{t} - \frac{1}{2+t}\right] \log|1+t| \\ -\theta(-1-x)\int_{-\infty}^{+\infty} \frac{2\,dt}{t(1+t)} \log|1+t| &= \frac{\pi^2}{4} - \pi^2\,\theta(-1-x) \\ &\quad - \Phi\left(\frac{-2x}{1+x}\right) + \Phi\left(\frac{-2}{1+x}\right).\end{aligned}\right\}\quad (\text{A. }9)$$

Using (A. 6), we can write (A. 9) as

$$\left.\begin{aligned}\Phi(x) - \Phi(-x) &= \frac{\pi^2}{4} - \pi^2\,\theta(-1-x) + \Phi\left(-\frac{1-x}{1+x}\right) \\ &\quad - \Phi\left(\frac{1-x}{1+x}\right) + \log|x| \cdot \log\left|\frac{1+x}{1-x}\right|.\end{aligned}\right\}\quad (\text{A. }10)$$

For complex values of x we can still define the function $\Phi(x)$ as the integral (A. 1), making this definition unique with the aid of a cut along the real axis below the point -1. This function fulfils an equation similar to (A. 4),

$$\Phi(x) + \Phi\left(\frac{1}{x}\right) = \frac{1}{2}\log^2 x, \qquad (\text{A. }11)$$

where the definition of the logarithm is made unique by the prescription just mentioned. From (A. 11), we conclude that

$$Re\,\Phi(e^{i\vartheta}) = -\frac{1}{4}\vartheta^2. \qquad (\text{A. }12)$$

For $|x| \leqq 1$, we have the power series expansion

$$\Phi(x) = -\frac{\pi^2}{12} + \sum_1^{\infty} \frac{(-1)^{n+1} \cdot x^n}{n^2}. \qquad (\text{A. }13)$$

From (A. 13), it follows that

$$Im\,\Phi(-e^{i\vartheta}) = -\sum_{1}^{\infty}\frac{\sin(n\vartheta)}{n^2} \equiv -\psi(\vartheta). \qquad \text{(A. 14)}$$

Numerical values of $\Phi(x)$ for real x can be obtained from the paper by MITCHELL. The function $\psi(\vartheta)$ in (A. 14) has been tabulated by CLAUSEN[1].

[1] T. CLAUSEN, Jour. f. Math. (Crelle) 8, 298 (1832).

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Paper [1955b]: On the Mathematical Structure of T. D. Lee's Model of a Renormalizable Field Theory

(with W. Pauli) Mat. Fys. Medd. Dan. Vid. Selesk. 30, No. 7 (1955) dedicated to Niels Bohr on his 70th birthday

The motivation for this work has been discussed several times in the earlier chapters in this book. In Part 3, see the article by Steven Weinberg. In Part 4, see the chapter "Why the Lee Model".

In a nutshell, Källén was looking for clues to help him make progress in his program on non-perturbative renormalization in quantum electrodynamics. This work made Pauli feel again "young and enthusiastic", a few years before he passed away. See the Källén-Pauli correspondence on the Lee Model, presented in Part 4.

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DEDICATED TO PROFESSOR NIELS BOHR ON THE OCCASION OF HIS 70TH BIRTHDAY

ON THE MATHEMATICAL STRUCTURE OF T.D. LEE'S MODEL OF A RENORMALIZABLE FIELD THEORY

BY

G. KÄLLÉN AND W. PAULI

København 1955
i kommission hos Ejnar Munksgaard

It is shown that the appropriate mathematical formalism of the field theoretical model recently proposed by T. D. Lee must use an indefinite metric to describe the norm of the state vector in the Hilbert space. The appearance of the indefinite metric is intimately connected with a new state of the V-particle having an energy that is below the mass of the "normal" V-particle. It is further shown that the S-matrix for this model is not unitary and that the probability for an incoming V-particle in the normal state and a boson, to make a transition to an outgoing V-particle in the new state and another boson, must be negative if the sum of all transition probabilities for the incoming state mentioned shall add up to one.

Introduction.

In a recent paper[1]), T. D. Lee has suggested a very interesting model of a renormalizable field theory. This model is simple enough to allow a more or less explicit solution, but complicated enough to contain many features characteristic of more realistic theories. It uses not only a renormalization of the mass of one kind of particles involved, but also a renormalization of the coupling constant g describing the interaction between the particles. In the explicit solution found by Lee, the ratio between the square of the renormalized coupling constant g and the square of the unrenormalized coupling constant g_0 is given by an expression of the form

$$\frac{g^2}{g_0^2} = 1 - A \cdot g^2, \qquad (1)$$

where A is a divergent integral. The ratio (1) is thus equal to $-\infty$. This is a very remarkable result, as according to very general principles[2]), this ratio should lie between one and zero. It is the aim of the present note to investigate the mathematical origin of the result (1) and to show that the violation of general principles implied by (1) also has observable consequences insofar as the S-matrix of the theory turns out not to be unitary.

To avoid the manipulation of divergent integrals we introduce a cut-off factor in the interaction. It will then appear that abnormal values of the ratio (1) are also obtained for a finite value of the cut-off and are not immediately connected with the infinities in the original formulation. To make our discussion reasonably self-contained we start with a survey of the foundations of the Lee model and with an outline of the way in which the renormalizations have to be performed in this case.

I. Renormalization of the Lee Model.

Let us consider a system with three different kinds of particles which, following LEE, we call V-particles, N-particles, and θ-particles. To each kind of particles corresponds a field that will be denoted by ψ_V, ψ_N, and a, respectively. The system is governed by the following *unrenormalized* Hamiltonian:

$$H = H_0 + H_{\text{int}}, \tag{2}$$

$$\left.\begin{aligned} H_0 = & \sum_{\bar{p}} E_V(\bar{p})\,\psi_V^*(\bar{p})\,\psi_V(\bar{p}) + \sum_{\bar{p}} E_N(\bar{p})\,\psi_N^*(\bar{p})\,\psi_N(\bar{p}) \\ & + \sum_{\bar{k}} \omega(\bar{k})\,a^*(\bar{k})\,a(\bar{k}), \end{aligned}\right\} \tag{3}$$

$$H_{\text{int}} = -\frac{g_0}{\sqrt{V}} \sum_{\bar{p}=\bar{p}'+\bar{k}} \frac{f(\omega)}{\sqrt{2\,\omega}} \left(\psi_V^*(\bar{p})\,\psi_N(\bar{p}')\,a(\bar{k}) + a^*(\bar{k})\,\psi_N^*(\bar{p}')\,\psi_V(\bar{p})\right). \tag{4}$$

The operators in (3) and (4) can be thought of as being written in p-space and in a Schrödinger representation. The model does not have invariance with respect to the Lorentz group and it will not be necessary to use the more sophisticated representations of relativistic field theories. The energies $E_V(\bar{p})$, $E_N(\bar{p})$, and $\omega(\bar{k})$ are, in principle, arbitrary functions of the momenta involved and the theory can be treated for any form of these functions. However, for our purpose, it will be sufficient to consider the following special case,

$$E_V(\bar{p}) = E_N(\bar{p}) = m \quad \text{(independent of } \bar{p}\text{)}, \tag{5}$$

$$\omega(\bar{k}) = \sqrt{\bar{k}^2 + \mu^2}. \tag{6}$$

In particular, $Eq.$ (5) will simplify the formal expressions to some extent without interfering with the interesting features of the result. If one wishes, this choice of the energies as functions of the momenta can be thought of as giving a model for the interaction of very heavy V- and N-particles (with equal masses) with light, relativistic θ-particles. The function $f(\omega)$ in (4) is the cut-off function mentioned earlier and is introduced to make the sums, appearing later, convergent. The quantity V is the volume of periodicity.

The field operators obey the following commutation and anticommutation relations:

$$\left\{\psi_V^*(\bar{p}),\, \psi_V(\bar{p}')\right\} = \left\{\psi_N^*(\bar{p}),\, \psi_N(\bar{p}')\right\} = \delta_{\bar{p},\bar{p}'}, \tag{7}$$

$$\left\{\psi_V(\bar{p}),\, \psi_V(\bar{p}')\right\} = \left\{\psi_V(\bar{p}),\, \psi_N(\bar{p}')\right\} = \cdots\cdots = 0, \tag{8}$$

$$[a(\bar{k}),\, a^*(\bar{k}')] = \delta_{\bar{k},\bar{k}'}, \tag{9}$$

$$[a(\bar{k}),\, \psi_V(\bar{p})] = [a(\bar{k}),\, \psi_N(\bar{p}')] = \cdots\cdots = 0. \tag{10}$$

With the aid of these commutators we can set up a representation in the Hilbert space, where each state is characterized by the number of particles present. Further, each state in this representation is an eigenstate of the free-particle Hamiltonian H_0 in (3), but not of the total Hamiltonian (2). Let us denote these states by

$$\left| n_V,\, n_N,\, n_k \right\rangle, \tag{11}$$

where n_V, n_N, and n_k are the numbers of "free" V-particles, N-particles, and θ-particles present[3)].

With the aid of (7)—(10) it can easily be verified that the following two operators commute with the total Hamiltonian.

$$Q_1 = \sum_{\bar{p}} \psi_V^*(\bar{p})\,\psi_V(\bar{p}) + \sum_{\bar{p}} \psi_N^*(\bar{p})\,\psi_N(\bar{p}), \tag{12}$$

$$Q_2 = \sum_{\bar{p}} \psi_N^*(\bar{p})\,\psi_N(\bar{p}) - \sum_{\bar{k}} a^*(\bar{k})\,a(\bar{k}), \tag{13}$$

$$[H,\, Q_i] = 0, \quad i = 1,2. \tag{14}$$

As each state (11) is also an eigenstate of the operators Q_i, it follows that the eigenstates of the total Hamiltonian H can be built up as linear combinations of states (11) belonging to the same eigenvalue q_i. This will considerably simplify the problem of diagonalizing the total Hamiltonian and, in some cases, even give an explicit solution. As an example, we may mention that there is only one of the states (11) which has $q_1 = q_2 = 0$, *viz.* the state $|0, 0, 0\rangle$ or the "free-particle vacuum". Hence, this state is also an eigenstate of the total Hamiltonian, and a simple calculation gives the eigenvalue zero for this operator. The "physical vacuum" is thus the same as the free-particle vacuum for this model. In the same way, we can show that the physical N-particle states and the physical θ-particle states are identical with the corresponding free-particle states, but that the free V-particle states are *not* eigenstates of the total Hamiltonian. It will be necessary to consider a linear combination of the states $|1_V, 0, 0\rangle$ and $|0, 1_N, 1_k\rangle$ to construct an eigenstate of the total Hamiltonian for this case. We shall later return to this point. For the moment we only remark that, under these circumstances, it will not be necessary to introduce renormalizations of the masses of the N-particles or the θ-particles. The mass renormalization in the model is now performed by adding the following term to the Hamiltonian (this term will *not* change the conservation equations (14)):

$$\delta H = -\delta m \sum_{\bar{p}} \psi_V^*(\bar{p}) \psi_V(\bar{p}). \tag{15}$$

The constant δm in (15) should, if possible, be determined in such a way that the state corresponding to the physical V-particle has the mass m appearing in H_0. Following the custom in quantum electrodynamics, we also introduce a renormalization of the coupling constant g_0 and of the field operator ψ_V by a factor N in the following way:

$$g = g_0 \cdot N, \tag{16}$$

$$\psi'_V(\bar{p}) = \psi_V(\bar{p}) \frac{1}{N}. \tag{17}$$

It is important to realize that the constant N in (16) and (17) can by definition be chosen to be real, as there is always an arbitrary phase factor in the field operators. The choice of a real N only fixes the phase connection between ψ_V and ψ'_V and can have no physical consequences. The value of N is determined by the condition[4)]

$$\langle 0 \,|\, \psi'_V(\bar{p}) \,|\, V \rangle = 1. \tag{18}$$

The state $|V\rangle$ in (18) is the physical V-particle state and the state $|0\rangle$ the physical vacuum. In what follows, we drop the dash on the renormalized ψ_V-operator as the corresponding unrenormalized operator will not be used again. In terms of our renormalized quantities the Hamiltonian and the canonical commutators will now read

$$H = H_0 + H_{\text{int}} + \delta H, \tag{19}$$

$$H_0 = mN^2 \sum_{\bar{p}} \psi_V^*(\bar{p})\,\psi_V(\bar{p}) + m \sum_{\bar{p}} \psi_N^*(\bar{p})\,\psi_N(\bar{p}) + \sum_{\bar{k}} \omega(\bar{k})\,a^*(\bar{k})\,a(\bar{k}), \tag{20}$$

$$H_{\text{int}} = -\frac{g}{\sqrt{V}} \sum_{\bar{p}=\bar{p}'+\bar{k}} \frac{f(\omega)}{\sqrt{2\omega}} \left(\psi_V^*(\bar{p})\,\psi_N(\bar{p}')\,a(\bar{k}) + a^*(\bar{k})\,\psi_N^*(\bar{p}')\,\psi_V(\bar{p})\right), \tag{21}$$

$$\delta H = -\delta m\, N^2 \sum_{\bar{p}} \psi_V^*(\bar{p})\,\psi_V(\bar{p}), \tag{22}$$

$$\left\{\psi_V^*(\bar{p}),\ \psi_V(\bar{p}')\right\} = \frac{1}{N^2}\,\delta_{\bar{p},\bar{p}'} \quad \text{(other commutators unchanged).} \tag{23}$$

Eqs. (19)—(23) will be the foundation for the following discussion.

II. The Physical V-Particle States and the States Describing the Scattering of one N-Particle and one θ-Particle.

We now try to find an eigenstate of the total Hamiltonian of the form

$$|z\rangle = |1_V, 0, 0\rangle + \sum_{\bar{k}} \Phi(\bar{k})\,|0, 1_N, 1_k\rangle. \tag{24}$$

In this expression all terms have the same total momentum. In the following formulae, a factor expressing conservation of three-dimensional momentum is very often left out. Calling the eigenvalue of the state (24) $m + \omega_0$, and using (19)—(23), we obtain after some straightforward calculations

$$\omega_0 + \delta m = -\frac{g}{N \cdot \sqrt{V}} \sum_{\bar{k}} \frac{\Phi(\bar{k}) f(\omega)}{\sqrt{2\,\omega}}, \tag{25}$$

$$(\omega - \omega_0)\,\Phi(\bar{k}) = \frac{g}{N\sqrt{V}} \frac{f(\omega)}{\sqrt{2\,\omega}}. \tag{26}$$

Eliminating $\Phi(\bar{k})$ from (25) and (26) we get the following equation for the determination of the eigenvalue ω_0:

$$\omega_0 + \delta m + \frac{g^2}{2\,N^2\,V} \sum_{\bar{k}} \frac{f^2(\omega)}{\omega} \frac{1}{\omega - \omega_0} = 0. \tag{27}$$

The constant δm is now determined from the condition that $\omega_0 = 0$ should be one solution of (27). The corresponding eigenstate (24) is, when properly normalized, the physical V-particle state. This gives us

$$\delta m = \frac{-g^2}{2\,V} \frac{1}{N^2} \sum_{\bar{k}} \frac{f^2(\omega)}{\omega^2}, \tag{28}$$

$$|\,V\rangle = C\left[\,|\,1_V, 0, 0\rangle + \frac{g}{N\sqrt{2\,V}} \sum_{\bar{k}} \frac{f(\omega)}{\omega^{3/2}}\,|\,0, 1_N, 1_k\rangle\right], \tag{29}$$

$$C^{-2} = 1 + \frac{g^2}{2\,VN^2} \sum_{\bar{k}} \frac{f^2(\omega)}{\omega^3}. \tag{30}$$

Furthermore, using *Eq.* (18), we get

$$C = N \tag{31}$$

or

$$|\,V\rangle = N\,|\,1_V, 0, 0\rangle + \frac{g}{\sqrt{2\,V}} \sum_{\bar{k}} \frac{f(\omega)}{\omega^{3/2}}\,|\,0, 1_N, 1_k\rangle, \tag{32}$$

$$N^2 = 1 - \frac{g^2}{2\,V} \sum_{\bar{k}} \frac{f^2(\omega)}{\omega^3}. \tag{33}$$

The results obtained so far in this paragraph correspond exactly to those obtained by LEE. In particular, *Eqs.* (33) and (16) together give LEE's result (1) if the form factor is put equal to unity for all values of ω. However, if we have a finite cut-off, *Eq.* (33) can be written

$$N^2 = 1 - \frac{g^2}{g_{\text{crit}}^2}, \tag{34}$$

$$g_{\text{crit}}^{-2} = \frac{1}{2V} \sum_{\bar{k}}{}' \frac{f^2(\omega)}{\omega^3}. \tag{34a}$$

The value (34) of N^2 lies between zero and one, as was to be expected, only if the renormalized coupling constant g is less than a critical value g_{crit} depending on the cut-off function and defined by (34a). If there is no cut-off, the critical value of the coupling is zero. Further, if the renormalization of the coupling constant is not performed explicitly, but if all quantities are expressed in terms of the original constant g_0, we have to substitute the expression

$$g^2 = \frac{g_0^2 \cdot g_{\text{crit}}^2}{g_0^2 + g_{\text{crit}}^2} \tag{35}$$

for g^2 everywhere in our formulae above. *Eq.* (35) contains the definite prediction that the renormalized coupling is always less than the critical coupling if the Hamiltonian is hermitian, *i.e.* if g_0 is real. As stressed by LEE, it is of some interest to investigate also the case of the renormalized coupling being larger than the critical value and the Hamiltonian being non-hermitian. The crucial question to be answered is whether this violation of the ordinary methods of quantum mechanics will have any observable consequences or if we are able in this way to get an at least partially satisfactory theory.

We now turn to the investigation of the other solutions to the eigenvalue problem (27). Making use of (28) and (33) we can rewrite *Eq.* (27) in the following way:

$$h(\omega_0) \equiv \omega_0 \left[1 + \frac{g^2}{2V} \sum_{\bar{k}}{}' \frac{f^2(\omega)\,\omega_0}{\omega^3(\omega - \omega_0)} \right] = 0. \tag{36}$$

The second factor in (36) has a pole each time $\omega_0 = \omega_i$, where ω_i is an eigenvalue of the unperturbed Hamiltonian H_0. As the derivative of the last factor in (36) with respect to ω_0 is always positive, this factor must vanish once, and only once, in each interval (ω_i, ω_{i+1}). The corresponding eigenstates (24) describe the scattering of one N-particle and one θ-particle. After some formal manipulations these states can be written,

$$|N, \theta\rangle = |0, 1_N, 1_k\rangle + \sum_{\bar{k}'} \alpha(\bar{k}, \bar{k}')|0, 1_{N'}, 1_{k'}\rangle + \beta(\bar{k}) N |1_V, 0, 0\rangle, \qquad (37)$$

$$\alpha(\bar{k}, \bar{k}') = \frac{g}{\sqrt{2V}} \frac{\beta(\bar{k}) f(\omega')}{\sqrt{\omega'}} \left\{ P \frac{1}{\omega' - \omega} + i\pi\delta(\omega' - \omega) \right\}, \qquad (38)$$

$$\beta(\bar{k}) = -\frac{g f(\omega)}{\sqrt{2V}\omega^{3/2}} \left[1 + \frac{g^2 \omega}{2V} \sum_{\bar{k}'} \frac{f^2(\omega')}{\omega'^3} \left(P \frac{1}{\omega' - \omega} + i\pi\delta(\omega' - \omega) \right) \right]^{-1}. \qquad (39)$$

In (38) and (39), the limit $V \to \infty$ has been anticipated and these equations contain a prescription how the denominators must be treated when the integration over $\bar{k}'$ is performed. This prescription corresponds to only outgoing waves in the second term of (37). The only incoming particles in these states have momentum $\bar{k}$. From the formulae above it is possible to compute that part of the S-matrix which corresponds to the scattering of N-particles and θ-particles by each other. The result is the unitary matrix

$$\langle N, \theta | S | N', \theta' \rangle = \delta_{\bar{k}, \bar{k}'} + \frac{i\pi g^2}{V} \frac{f^2(\omega)}{\omega} \frac{\delta(\omega' - \omega)}{h(\omega) + i\frac{g^2}{4\pi}|\bar{k}| f^2(\omega)}. \qquad (40)$$

From (40) we get the differential cross section

$$\frac{d\sigma}{d\Omega} = \frac{1}{|\bar{k}|^2} \sin^2 \delta \qquad (41)$$

with

$$\operatorname{tg} \delta = \frac{g^2}{4\pi} \frac{|\bar{k}| f^2(\omega)}{h(\omega)}. \qquad (42)$$

Again, this corresponds exactly to the results obtained by LEE. In the last three formulae, the limit $V \to \infty$ is performed, and the integral appearing in $h(\omega)$ (*Eq.* (36)) is defined to be a principal value.

It remains to discuss the important question whether the states (32) and (37) obtained so far form a complete set or if *there are possibly other states of the form* (24) *which are also eigenstates of the total Hamiltonian.* If other states exist, they must correspond to other solutions of the eigenvalue problem (36). We therefore begin by a more detailed discussion of this equation. The argument given so far has exhausted all roots of this equation in the domain $\omega_0 > \mu$*. For $\omega_0 < \mu$, we find that the second factor of (36) still has a positive derivative and that it approaches the value $N^2 = 1 - g^2/g^2_{\text{crit}}$ for very large values of $|\omega_0|$. If the coupling constant is less than the critical value, we have no extra root of (36) and the states considered so far form a complete set. On the other hand, *if the coupling is larger than the critical coupling, there will be exactly one extra root of* (36) *for* $\omega_0 < \mu$. The corresponding eigenstate is not a scattering state, but will represent another state of the V-particle.† This state can be constructed explicitly from the formalism given here, and the result is

$$|V_{-\lambda}\rangle = \frac{1}{\sqrt{|h'(-\lambda)|}}\left[N\cdot|1_V,0,0\rangle + \frac{g}{\sqrt{2V}}\sum_{\bar{k}}\frac{f(\omega)}{\sqrt{\omega}}\frac{1}{\omega+\lambda}|0,1_N,1_k\rangle\right], \quad (4$$

$$h(-\lambda) = 0; \quad \lambda > 0. \quad (44)$$

The normalization of the state (43) is chosen in a way that will be justified in the next paragraph.

It will be shown in Appendix I that *Eq.* (36) has no non-real roots.

* If the cut-off function vanishes exactly for ω larger than some value Ω, the domain $\omega_0 > \Omega$ needs a special discussion, as the argument after *Eq.* (36) will not be valid there. Actually, it can be shown that there is an extra root in this domain if g is less than the critical value g_{crit}. To avoid inessential complications of the argument, we therefore consider only cut-off functions that have a long tail as, *e. g.*, $f(\omega) = e^{-\omega/\Omega}$, where this question will not appear.

† In footnote 4 of Lee's paper, the possibility of another stable state of the V-particle is briefly mentioned, but no detailed investigation of its properties is given. In our discussion, this state will be of paramount importance.

III. Introduction of an Indefinite Metric in the Hilbert Space.

The negative sign for N^2 in (34), if g is larger than g_{crit}, obviously leads to difficulties with the normalization of the physical V-particle state (32). If we try to correct the normalization of this state by multiplying it with a suitable factor, we are ultimately led to a modification of our renormalization prescriptions insofar as we can no longer use the same factor in (16) and (17) to renormalize the coupling constant and the field operator ψ_V. In this case, extra factors have to be inserted in the interaction Hamiltonian (21), and it can easily be seen that it is not possible in this way to make the theory mathematically consistent. The only possibility of saving the normalization of the state (32) is then to *define* the norm of a state $\alpha \,|\, n_V, n_N, n_k\rangle$ to be $|\alpha|^2(-1)^{n_V}$. As N^2 in our case is real and negative, this indefinite metric will be the appropriate mathematical framework for the Lee model.[5] The introduction of this device will not change many of the formal operations performed earlier, and particularly the scattering states (37) and the S-matrix (40) will be uninfluenced by it. On the other hand, the norm of the state (32) will be one as it stands in the new metric. The norm of the state (43) will be

$$\left.\begin{aligned} &\frac{1}{|h'(-\lambda)|}\left[N^2 + \frac{g^2}{2V}\sum_{\bar{k}} \frac{f^2(\omega)}{\omega(\omega+\lambda)^2}\right] \\ &= \frac{1}{|h'(-\lambda)|}\left[1 + \frac{g^2}{2V}\sum_{\bar{k}} \frac{f^2(\omega)}{\omega}\left[\frac{1}{(\omega+\lambda)^2} - \frac{1}{\omega^2}\right]\right] \\ \frac{1}{|h'(-\lambda)|}\frac{g^2}{2V}&\sum_{\bar{k}} \frac{f^2(\omega)}{\omega}\left[\frac{1}{(\omega+\lambda)^2} - \frac{1}{\omega^2} + \frac{\lambda}{\omega^2(\omega+\lambda)}\right] = \frac{h'(-\lambda)}{|h'(-\lambda)|} = -1. \end{aligned}\right\} \quad (45)$$

The norm of the state $|\, V_{-\lambda}\rangle$ *is negative* and has been normalized to -1 in (43).

To make the formal discussion as simple as possible it will now be convenient to introduce a "metric operator" η[5] which has the following matrix elements for the free-particle states (11):

$$\langle n_V, n_N, n_k |\, \eta \,| n'_V, n'_N, n'_k \rangle = \delta_{n_V n'_V} \cdot \delta_{n_N n'_N} \cdot \delta_{n_k n'_k} \cdot (-1)^{n_V}. \quad (46)$$

For the physical states considered up till now, we have

$$\langle V | \eta | V \rangle = \langle N, \theta | \eta | N, \theta \rangle = 1, \tag{47}$$

$$\langle V_{-\lambda} | \eta | V_{-\lambda} \rangle = -1. \tag{48}$$

The non-diagonal elements of η between these states are all zero. The condition for an operator F to have real expectation values is no longer that it is hermitian, but rather that it is "self-adjoint" in the following sense:

$$F = F^{+} \equiv \eta F^{*} \eta. \tag{49}$$

A detailed examination of the foregoing calculations shows that the introduction of the indefinite metric will make the mathematics formally consistent if the adjoint operators ψ_V^{+}, ψ_N^{+}, and a^{+} are introduced in *Eqs.* (20)—(23) instead of the operators ψ_V^{*}, ψ_N^{*}, and a^{*}. This will make the Hamiltonian self-adjoint. On the other hand, the right-hand side of (23) will no longer have a definite sign, and a negative value of this c-number will not necessarily be inconsistent with the foundations of the theory. A special case of the expectation value of this anticommutator is examined in Appendix I.

If the transformation leading from the free particle states $| n \rangle$ to the physical states $| P \rangle$ is written as a matrix U,

$$| P \rangle = \sum_{| n \rangle} | n \rangle \langle n | U | P \rangle, \tag{50}$$

this matrix will not be unitary, but have the property

$$U^{+} U = \eta U^{*} \eta \, U = 1. \tag{51}$$

It is then important to decide whether the S-matrix of the theory also has the property (51) rather than being unitary. This expectation is not in contradiction with the result (40), as the operator η has only matrix elements $+1$ for the physical states involved there. *Eq.* (51) will have non-trivial consequences only if *physical* states with a non-positive norm are involved. The simplest process of this kind is the scattering of a θ-particle by a V-particle either in its normal state or in the state $| V_{-\lambda} \rangle$. In the former case, it is to be expected that transitions of the V-particle to its new state take place and that these transitions possibly occur with "negative probabilities". The following paragraph is devoted to a discussion of these problems.

IV. The Scattering of θ-Particles by V-Particles.

We will now study eigenvectors of the total Hamiltonian of the form

$$|z\rangle = \sum_{\bar{k}} \Phi_1(\bar{k}) \cdot N \cdot | 1_V, 0, 1_k \rangle + \sum_{\bar{k}, \bar{k}'} \Phi_2(\bar{k}, \bar{k}') | 0, 1_N, 1_k, 1_{k'} \rangle. \quad (52)$$

If the eigenvalue is again called $m + \omega_0$, a straightforward calculation will yield the following equations for the coefficients in (52):

$$\Phi_1(\bar{k})(\omega - \omega_0 - \delta m) = \frac{1}{N^2} g \sqrt{\frac{2}{V}} \sum_{\bar{k}'} \Phi_2(\bar{k}, \bar{k}') \frac{f(\omega')}{\sqrt{\omega'}}, \quad (53)$$

$$\Phi_2(\bar{k}, \bar{k}')(\omega + \omega' - \omega_0) = \frac{g}{\sqrt{2V}} \cdot \frac{1}{2} \left[\Phi_1(\bar{k}) \frac{f(\omega')}{\sqrt{\omega'}} + \Phi_1(\bar{k}') \frac{f(\omega)}{\sqrt{\omega}} \right]. \quad (54)$$

In this case, we are not interested in the complete set of states (52), but will only try to find those special states corresponding to the scattering of a θ-particle by a V-particle in its normal state. In other words, we look for solutions to (53) and (54) where $\Phi_1(\bar{k})$ is of the form

$$\Phi_1(\bar{k}, \bar{k}_0) = \delta_{\bar{k}, \bar{k}_0} + \psi(\bar{k}, \bar{k}_0) \quad (55)$$

with outgoing waves only in $\psi(\bar{k}, \bar{k}_0)$ and in $\Phi_2(\bar{k}, \bar{k}')$. The last condition gives us

$$\left. \begin{aligned} \Phi_2(\bar{k}, \bar{k}', \bar{k}_0) = \frac{g}{\sqrt{2V}} \cdot \frac{1}{2} \cdot \Bigg[\Phi_1(\bar{k}, \bar{k}_0) \frac{f(\omega')}{\sqrt{\omega'}} \\ + \Phi_1(\bar{k}', \bar{k}_0) \frac{f(\omega)}{\sqrt{\omega}} \Bigg] \left[P \frac{1}{\omega + \omega' - \omega_0} + i\pi\delta(\omega + \omega' - \omega_0) \right] \end{aligned} \right\} \quad (56)$$

or, using (28) and (33),

$$\left.\begin{aligned}&\Phi_1(\bar{k},\bar{k}_0)\,h(\omega_0-\omega)\\ &=\frac{g^2}{2V}\frac{f(\omega)}{\sqrt{\omega}}\sum_{\bar{k}'}\frac{f(\omega')\,\Phi_1(\bar{k}',\bar{k}_0)}{\sqrt{\omega'}}\left[P\frac{1}{\omega+\omega'-\omega_0}+i\pi\delta(\omega+\omega'-\omega_0)\right].\end{aligned}\right\}\quad(57)$$

Contrary to the situation in paragraph II, it will not be possible to find an explicit solution to *Eq.* (57). However, this will not be necessary for our purpose, as it is sufficient here to investigate the properties of the S-matrix. This can be done with a method very similar to MØLLER's proof of the unitarity of the S-matrix if the Hamiltonian is hermitian.[6)] Following MØLLER, we introduce the following quantity

$$\left.\begin{aligned}&U(\bar{k},\bar{k}_0)\\ &=i\frac{g^2}{2V}\frac{f(\omega)}{\sqrt{\omega}}\sum_{\bar{k}'}\frac{f(\omega')\,\Phi_1(\bar{k}',\bar{k}_0)}{\sqrt{\omega'}}\left[P\frac{1}{\omega+\omega'-\omega_0}+i\pi\delta(\omega+\omega'-\omega_0)\right].\end{aligned}\right\}\quad(58)$$

From *Eq.* (57) we then conclude

$$\left.\begin{aligned}\sum_{\bar{k}}\Phi_1^*(\bar{k},\bar{k}_0)\,U(\bar{k},\bar{k}_0') &= i\frac{g^2}{2V}\sum_{\bar{k},\bar{k}''}\frac{\Phi_1^*(\bar{k},\bar{k}_0)\,f(\omega)}{\sqrt{\omega}}\,\frac{f(\omega'')\,\Phi_1(\bar{k}'',\bar{k}_0')}{\sqrt{\omega''}}\\ &\times\left[P\frac{1}{\omega+\omega''-\omega_0'}+i\pi\delta(\omega+\omega''-\omega_0')\right],\end{aligned}\right\}\quad(59)$$

$$\left.\begin{aligned}\sum_{\bar{k}''}U^*(\bar{k}'',\bar{k}_0)\,\Phi_1(\bar{k}'',\bar{k}_0') &= -i\frac{g^2}{2V}\sum_{\bar{k},\bar{k}''}\frac{\Phi_1^*(\bar{k},\bar{k}_0)\,f(\omega)}{\sqrt{\omega}}\,\frac{f(\omega'')\,\Phi_1(\bar{k}'',\bar{k}_0')}{\sqrt{\omega''}}\\ &\times\left[P\frac{1}{\omega+\omega''-\omega_0}-i\pi\delta(\omega+\omega''-\omega_0)\right].\end{aligned}\right\}\quad(60)$$

The sum of (59) and (60) vanishes, as does the corresponding sum in MØLLER's paper, only if $\omega_0 < 2\,\mu$. In this case, $\omega+\omega''-\omega_0$ never vanishes in the physical interval (μ,∞) of the frequencies ω,ω'', and the transition $V+\theta\rightarrow N+\theta'+\theta''$ cannot occur on the energy shell. In the opposite case, $\omega_0 > 2\,\mu$, this transition causes a slight complication and we get the former case, it is to be expected that transitions of the V-particle to its new state take place and that these transitions possibly occur with "negative probabilities". The following paragraph is devoted to a discussion of these problems.

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If the eigenvalue is again called $m + \omega_0$, a straightforward calculation will yield the following equations for the coefficients in (52):

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$$\Phi_2(\bar{k}, \bar{k}')(\omega + \omega' - \omega_0) = \frac{g}{\sqrt{2V}} \cdot \frac{1}{2} \left[\Phi_1(\bar{k}) \frac{f(\omega')}{\sqrt{\omega'}} + \Phi_1(\bar{k}') \frac{f(\omega)}{\sqrt{\omega}} \right]. \quad (54)$$

In this case, we are not interested in the complete set of states (52), but will only try to find those special states corresponding to the scattering of a θ-particle by a V-particle in its normal state. In other words, we look for solutions to (53) and (54) where $\Phi_1(\bar{k})$ is of the form

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with outgoing waves only in $\psi(\bar{k}, \bar{k}_0)$ and in $\Phi_2(\bar{k}, \bar{k}')$. The last condition gives us

$$\left. \begin{aligned} \Phi_2(\bar{k}, \bar{k}', \bar{k}_0) = \frac{g}{\sqrt{2V}} \cdot \frac{1}{2} \cdot \Big[\Phi_1(\bar{k}, \bar{k}_0) \frac{f(\omega')}{\sqrt{\omega'}} \\ + \Phi_1(\bar{k}', \bar{k}_0) \frac{f(\omega)}{\sqrt{\omega}} \Big] \left[P \frac{1}{\omega + \omega' - \omega_0} + i\pi\delta(\omega + \omega' - \omega_0) \right] \end{aligned} \right\} \quad (56)$$

or, using (28) and (33),

$$\left.\begin{aligned}&\Phi_1(\bar{k},\bar{k}_0)\,h(\omega_0-\omega)\\=\;&\frac{g^2}{2V}\frac{f(\omega)}{\sqrt{\omega}}\sum_{\bar{k}'}\frac{f(\omega')\,\Phi_1(\bar{k}',\bar{k}_0)}{\sqrt{\omega'}}\left[P\frac{1}{\omega+\omega'-\omega_0}+i\pi\delta(\omega+\omega'-\omega_0)\right].\end{aligned}\right\}\quad(57)$$

Contrary to the situation in paragraph II, it will not be possible to find an explicit solution to *Eq.* (57). However, this will not be necessary for our purpose, as it is sufficient here to investigate the properties of the *S*-matrix. This can be done with a method very similar to MØLLER's proof of the unitarity of the *S*-matrix if the Hamiltonian is hermitian.[6] Following MØLLER, we introduce the following quantity

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From *Eq.* (57) we then conclude

$$\left.\begin{aligned}\sum_{\bar{k}}\Phi_1^*(\bar{k},\bar{k}_0)\,U(\bar{k},\bar{k}_0')&=i\frac{g^2}{2V}\sum_{\bar{k},\bar{k}''}\frac{\Phi_1^*(\bar{k},\bar{k}_0)\,f(\omega)}{\sqrt{\omega}}\frac{f(\omega'')\,\Phi_1(\bar{k}'',\bar{k}_0')}{\sqrt{\omega''}}\\&\times\left[P\frac{1}{\omega+\omega''-\omega_0'}+i\pi\delta(\omega+\omega''-\omega_0')\right],\end{aligned}\right\}\quad(59)$$

$$\left.\begin{aligned}\sum_{\bar{k}''}U^*(\bar{k}'',\bar{k}_0)\,\Phi_1(\bar{k}'',\bar{k}_0')&=-i\frac{g^2}{2V}\sum_{\bar{k},\bar{k}''}\frac{\Phi_1^*(\bar{k},\bar{k}_0)\,f(\omega)}{\sqrt{\omega}}\frac{f(\omega'')\,\Phi_1(\bar{k}'',\bar{k}_0')}{\sqrt{\omega''}}\\&\times\left[P\frac{1}{\omega+\omega''-\omega_0}-i\pi\delta(\omega+\omega''-\omega_0)\right].\end{aligned}\right\}\quad(60)$$

The sum of (59) and (60) vanishes, as does the corresponding sum in MØLLER's paper, only if $\omega_0<2\,\mu$. In this case, $\omega+\omega''-\omega_0$ never vanishes in the physical interval (μ,∞) of the frequencies ω, ω'', and the transition $V+\theta\rightarrow N+\theta'+\theta''$ cannot occur on the energy shell. In the opposite case, $\omega_0>2\,\mu$, this transition causes a slight complication and we get

$$\left.\begin{aligned}&\delta(\omega_0-\omega_0')\Big[\sum_{\bar k}\Phi_1^*(\bar k,\bar k_0)\,U(\bar k,\bar k_0')+\sum_{\bar k}U^*(\bar k,\bar k_0)\,\Phi_1(\bar k,\bar k_0')\Big]\\ &=-\frac{\pi g^2}{V}\,\delta(\omega_0-\omega_0')\sum_{\bar k,\bar k''}\Phi_1^*(\bar k,\bar k_0)\,\frac{f(\omega)\,f(\omega'')}{\sqrt{\omega\omega''}}\,\Phi_1(\bar k'',\bar k_0')\,\delta(\omega+\omega''-\omega_0).\end{aligned}\right\}\quad(61)$$

With the aid of (55), (57), (58), and the vanishing of $h(0)$, we have

$$\psi(\bar k,\bar k_0)\,h(\omega_0-\omega)=i\,U(\bar k,\bar k_0).\qquad(62)$$

We write the solution of (62) symbolically as

$$\psi(\bar k,\bar k_0)=i\,\frac{U(\bar k,\bar k_0)}{h(\omega_0-\omega)_+},\qquad(63)$$

where the plus sign indicates that outgoing waves are to be chosen at the zeros of $h(\omega_0-\omega)$. Using this result, we can write (61) as

$$\left.\begin{aligned}&\delta(\omega_0-\omega_0')\,[U(\bar k_0,\bar k_0')+U^*(\bar k_0',\bar k_0)]\\ &+i\,\delta(\omega_0-\omega_0')\sum_{\bar k}U^*(\bar k,\bar k_0)\,U(\bar k,\bar k_0')\left[\frac{1}{h(\omega_0-\omega)_+}-\frac{1}{h(\omega_0-\omega)_-}\right]\\ &+\frac{\pi g^2}{V}\,\delta(\omega_0-\omega_0')\sum_{\bar k,\bar k''}\Phi_1^*(\bar k,\bar k_0)\,\frac{f(\omega)\,f(\omega'')}{\sqrt{\omega\omega''}}\,\Phi_1(\bar k'',\bar k_0')\,\delta(\omega+\omega''-\omega_0)=0.\end{aligned}\right\}\quad(64)$$

The second bracket of (64) can be rewritten in the following way:

$$\frac{1}{h(\omega_0-\omega)_+}-\frac{1}{h(\omega_0-\omega)_-}=-2\pi i\sum_{\varrho_i}\frac{1}{h'(\varrho_i)}\,\delta(\omega_0-\omega-\varrho_i),\qquad(65)$$

where the summation is over all the roots of the equation $h(x)=0$. To simplify the notations further, we introduce the matrices

$$\langle V,\theta\,|\,R^{(1)}\,|\,V',\theta'\rangle=2\pi\delta(\omega-\omega')\,U(\bar k,\bar k'),\qquad(66)$$

$$\langle V_{-\lambda},\theta\,|\,R^{(2)}\,|\,V,\theta'\rangle=2\pi\delta(\omega+\lambda-\omega')\,\frac{U(\bar k,\bar k')}{\sqrt{-h'(-\lambda)}},\qquad(67)$$

$$
\left.\begin{aligned}
&\langle N, \theta', \theta'' \,|\, R^{(3)} \,|\, V, \theta \rangle \\
&= 2\pi\,\delta(\omega' + \omega'' - \omega)\,\frac{g}{\sqrt{2V}}\,\frac{1}{2}\left[\Phi_1(\bar{k}', \bar{k})\,\frac{f(\omega'')}{\sqrt{\omega''}} + \Phi_1(\bar{k}'', \bar{k})\,\frac{f(\omega')}{\sqrt{\omega'}}\right].
\end{aligned}\right\} \qquad (68)
$$

It can be shown that the sum over all the roots in (65) corresponding to the scattering states in paragraph II and the last term of (64) can be expressed in terms of the matrix $R^{(3)}$. Using this, we can write (64) as

$$
\left.\begin{aligned}
&\langle V, \theta \,|\, R^{(1)} + R^{(1)*} + R^{(1)*} R^{(1)} \,|\, V', \theta' \rangle \\
&- \langle V, \theta \,|\, R^{(2)*} R^{(2)} \,|\, V', \theta' \rangle + \langle V, \theta \,|\, R^{(3)*} R^{(3)} \,|\, V', \theta' \rangle = 0.
\end{aligned}\right\} \qquad (69)
$$

It now follows that *the S-matrix of the Lee model* which, for the states considered in this paragraph, is given by

$$
S = 1 + R^{(1)} + R^{(2)} + R^{(3)}, \qquad (70)
$$

is not unitary, because the probability for the transitions $V + \theta \rightarrow V_{-\lambda} + \theta'$ is to be counted negative in (69). As was suggested earlier, we see instead that the S-matrix has the property

$$
\eta S^* \eta S = 1 \qquad (71)
$$

if the diagonal elements of η belonging to the states $|V_{-\lambda}, \theta\rangle$ are put equal to -1. It can also be shown that, if transitions *from* the states $|V_{-\lambda}, \theta\rangle$ are considered, a similar result will be obtained. The non-unitariness of the transformation (50) between the free-particle states and the physical states has its close correspondence in the non-unitariness of the S-matrix and makes the model unacceptable for physical reasons.

At this stage, one might ask if it is not possible to reinterpret the formalism with the aid of an argument similar to hole theory in quantum electrodynamics. One would then, *e. g.*, call the state $|V_{-\lambda}\rangle$ the vacuum, and the state which is here called the vacuum a state with one "anti-particle". However, it is easily seen that it is not possible to make the formalism consistent in this way as no reinterpretation along such lines will ever change the non-unitary properties of the S-matrix in (69).

The conclusion of our discussion is then that the model suggested by T. D. Lee is in accordance with the physical probability concept only if a cut-off is introduced and if the renormalized coupling constant is less than the critical value given by *Eq.* (34a). In this case, the constant N^2 lies between zero and one, as is expected from general arguments.[2] If there is no cut-off, the critical value of the coupling constant is zero.

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Zürich, Switzerland.

References.

(1) T. D. Lee, Phys. Rev. **95**, 1329 (1954).
(2) This was first shown by J. Schwinger (unpublished) and has since been found by several authors. Cf. H. Umezaha and S. Kamefuchi, Progr. Theor. Phys. **6**, 543 (1951); G. Källén, Helv. Phys. Acta **25**, 417 (1952); H. Lehmann, Nuovo Cimento **11**, 342 (1954); M. Gell-Man and F. E. Low, Phys. Rev. **95**, 1300 (1954). Appendix II of the paper by Lee[1] also contains a proof of this theorem.
(3) The "free-particle states" introduced in this way are of the same kind as the free-particle states used, *e.g.*, in the Tamm-Dancoff method, but entirely different from the so-called "incoming (or outgoing) free-particle states" used in other formulations of relativistic field theories. As will be seen below, the Tamm-Dancoff approach gives the exact solution for this model.
(4) G. Källén, Helv. Phys. Acta **25**, 417 (1952).
(5) An indefinite metric has earlier been used in quantum field theory by P. A. M. Dirac, Proc. Roy. Soc. **A 180**, 1 (1942) in a connection which is not too different from the one used here. Cf. also W. Pauli, Rev. Mod. Phys. **15**, 175 (1943). A result of R. P. Feynman, Phys. Rev. **76**, 749 (1949) particularly p. 756 implies the implicit use of an indefinite metric. Cf. W. Pauli, Prog. Theor. Phys. **5**, 526 (1950). An indefinite metric has also been used in quantum electrodynamics for a treatment of scalar photons. Cf. S. N. Gupta, Proc. Phys. Soc. **53**, 681 (1950) and K. Bleuler, Helv. Phys. Acta **23**, 567 (1950).
(6) C. Moller, Dan. Mat. Fys. Medd. **23**, no. 1 (1945); ibid. **22**, no. 19 (1946).

Appendix I.

In this appendix, we show by an explicit calculation how the indefinite metric is able to account for the negative sign on the right hand of the anticommutator

$$\left\{\psi_V^+(\bar{p}),\, \psi_V(\bar{p}')\right\} = \delta_{\bar{p},\bar{p}'}\frac{1}{N^2}. \tag{A.1}$$

We compute the vacuum expectation value of this quantity for $g > g_{\text{crit}}$ and $p = p'$, and obtain

$$\langle 0 | \left\{\psi_V^+(\bar{p}),\, \psi_V(\bar{p})\right\} | 0 \rangle = \sum_{|z\rangle} |\langle 0 | \psi_V(\bar{p}) | z \rangle|^2 \langle z | \eta | z \rangle. \tag{A.2}$$

In (A.2) the summation is performed over any complete set of states. We can, *e.g.*, sum over all physical states and get contributions from the physical V-particle state, the state $|V_{-\lambda}\rangle$, and the scattering states $|N, \theta\rangle$. According to the result of paragraph II, these contributions will be

$$\left.\begin{aligned} \langle 0 | \left\{\psi_V^+(\bar{p}),\, \psi_V(\bar{p})\right\} | 0 \rangle &= 1 + \sum_{\bar{k}} |\beta(\bar{k})|^2 - \frac{1}{|h'(-\lambda)|} \\ &= 1 + \sum_{\bar{k}} |\beta(\bar{k})|^2 + \frac{1}{h'(-\lambda)}. \end{aligned}\right\} \tag{A.3}$$

If there were no indefinite metric, the right-hand side would be positive and larger than one. This is also the usual proof[2)] that N^2 is a positive number less than one. In our case, the last term has a negative sign, and there is no general principle according to which the right-hand side of (A.3) has a definite sign. We shall now show explicitly that this quantity has the correct value given by *Eq.* (33). The proof is essentially based on the fact that the function $h(z)$ defined by (36) and extended to the complex plane by

$$h(z) = z\left[1 + \frac{g^2}{2V}\sum_{\bar{k}}\frac{f^2(\omega)\,z}{\omega^3(\omega - z)}\right] \tag{A.4}$$

has zeros only on the real axis. Indeed, one has with $z = x + iy$,

$$Im\,\frac{h(z)}{z} = \frac{g^2}{2V}\,Im\sum_{\bar{k}}\frac{f^2(\omega)\,z}{\omega^3(\omega - z)} = \frac{g^2}{2V}\sum_{\bar{k}}\frac{f^2(\omega)\,y}{\omega^2[(\omega - x)^2 + y^2]}, \tag{A.5}$$

which is always different from zero for $y \neq 0$.

Moreover, passing to the limit $V \to \infty$, $h(z)$ transforms into an analytic function given by

$$h(z) = z\left[1 + \gamma z\int_{\mu}^{\infty} f^2(\omega)\,\frac{\sqrt{\omega^2 - \mu^2}\,d\omega}{\omega^2(\omega - z)}\right] \tag{A.4a}$$

$\left(\text{with the abbreviation } \gamma = \frac{g^2}{4\pi^2}\right)$ which is unique in the complex plane cut along the real axis from μ to positive infinity. The imaginary part of $h(z)$ is discontinuous at this part of the real axis, having opposite signs in the upper and the lower half plane, whilst the real part is continuous. To this ambiguity of $h(z)$ corresponds the circumstance that $z = \mu$ is a branching point of the square root type of $h(z)$ (*cf.* the explicit form given in Appendix II for the particular case $f(\omega) = 1$).

These properties of $h(z)$ enable us to evaluate the integral

$$\frac{1}{2\pi i}\int_C \frac{dz}{h(z)}$$

along the path illustrated in Fig. 1 in two different ways. We first remark that

$$\left.\begin{aligned}\sum_{\bar{k}} |\beta(\bar{k})|^2 &= \gamma\int_{\mu}^{\infty} f^2(\omega)\sqrt{\omega^2 - \mu^2}\,d\omega\left[h^2(\omega) + \left(\frac{\pi\gamma}{\omega}f^2(\omega)\sqrt{\omega^2 - \mu^2}\right)^2\right]^{-1}\\ &= \frac{1}{\pi}\lim_{\varepsilon\to 0} Im\int_{\mu}^{\infty}\frac{d\omega}{h(\omega - i\varepsilon)}.\end{aligned}\right\} \tag{A.6}$$

We now divide the path C into two parts. One of them, C_1, starts from a point $z = R - i\varepsilon$ with arbitrarily large R and arbitrarily

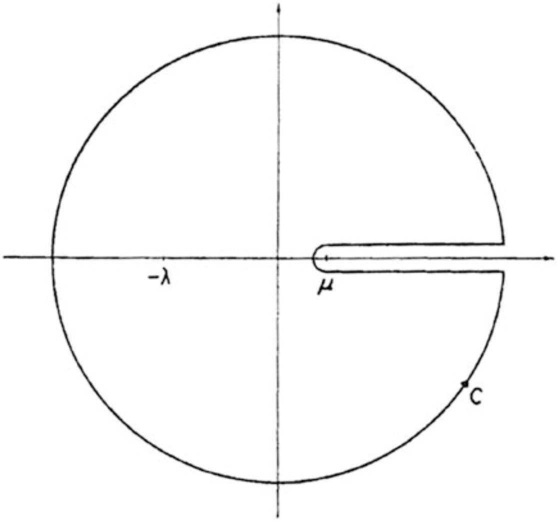

Fig. 1. The path C in *Eq.* (A. 9).

small, positive ε, goes below the real axis at a distance ε from it, encircles the point $z = \mu$ in the negative direction, returns above the real axis at a distance ε, and ends at the point $z = R + i\varepsilon$. The second part, C_R, is a large circle with radius R of which a small part near the positive real axis is omitted.

Performing the limiting process $\varepsilon \to 0$, in which the contribution of the circular arc of C_1 gets arbitrarily small, one first obtains

$$\lim_{\varepsilon \to 0} \int_{C_1} \frac{dz}{h(z)} = -2\,i \lim_{\varepsilon \to 0} Im \int_{\mu}^{\infty} \frac{dz}{h(z - i\varepsilon)} = -2\,\pi i \sum_{\bar{k}} |\beta(\bar{k})|^2. \quad \text{(A.7)}$$

In this limit, the second part C_R of C goes over into the full circle C_R. The corresponding integral is easily evaluated with the aid of the asymptotic form of the function $h(z)$ (cf. the remarks before *Eq.* (43)) and gives

$$\int_{C_R} \frac{dz}{h(z)} = 2\,\pi i \frac{1}{N^2}. \quad \text{(A.8)}$$

Hence, in this way we obtain

$$\frac{1}{2\pi i}\int_C \frac{dz}{h(z)} + \sum_{\bar{k}} |\beta(\bar{k})|^2 = \frac{1}{N^2}. \tag{A.9}$$

On the other hand, the absence of non-real zeros of $h(z)$ and a knowledge of the residues of $h(z)^{-1}$ at the poles $z = 0$ and $z = -\lambda$ permits a direct evaluation of the integral

$$\frac{1}{2\pi i}\int_C \frac{dz}{h(z)} = 1 + \frac{1}{h'(-\lambda)}. \tag{A.10}$$

Hence,

$$1 + \sum_{\bar{k}} |\beta(\bar{k})|^2 + \frac{1}{h'(-\lambda)} = \frac{1}{N^2}. \tag{A.11}$$

Eqs. (A.11) and (A.3) together give the expected result (A.1). If the coupling constant is less than the critical value, the integrand in (A.9) will have no pole at $z = -\lambda$, and the last term in (A.10) will be missing. Other matrix elements of the commutators and anticommutators can be treated in similar ways.

Appendix II.

In the *particular case of no cut-off* $f(\omega) = 1$, $1/N = 0$ the function $h(z)$ (cf. (A.4a)) can be expressed in closed form:

$$\left.\begin{aligned} h(\omega \pm i\varepsilon) &= \omega + \gamma\left[\omega + \frac{\pi\mu}{2} - \sqrt{\omega^2-\mu^2}\left(\log\frac{\omega+\sqrt{\omega^2-\mu^2}}{\mu} \mp i\pi\right)\right] \\ &\text{if} \quad \omega > \mu \quad \text{and} \quad \varepsilon > 0, \end{aligned}\right\} \tag{A.12}$$

$$h(-\lambda) = -\lambda + \gamma\left[-\lambda + \frac{\mu\pi}{2} + \sqrt{\lambda^2-\mu^2}\log\frac{\lambda+\sqrt{\lambda^2-\mu^2}}{\mu}\right] \quad \text{if} \quad \lambda > \mu. \tag{A.13}$$

Apart from the imaginary part in (A. 12) these two cases can also be represented by the same fomula if an absolute value is taken for the argument under the logarithm. For the third interval of the real axis, one has

$$h(\omega) = \omega + \gamma \left[\omega - \sqrt{\mu^2 - \omega^2} \arcsin \frac{\omega}{\mu} + \frac{\pi}{2} \frac{\omega^2}{\mu + \sqrt{\mu^2 - \omega^2}} \right] \text{ if } -\mu < \omega < \mu. \quad \text{(A.1}$$

These expressions can be used to find the position of the root

$$h(-\lambda) = 0 \tag{A.15}$$

both in the weak and in the strong coupling limit. For weak coupling, we find from (A.13)

$$\lambda \approx \frac{\mu}{2} e^{1/\gamma} \quad \text{if } \gamma << 1, \tag{A.16}$$

which *excludes any kind of power series expansion.** In the strong coupling limit the application of (A.14) gives the following expression for the root:

$$-\omega \equiv \lambda \approx \frac{4}{\pi} \frac{\mu}{\gamma} \quad \text{if } \gamma >> 1 \tag{A.17}$$

with a possibility of an expansion in powers of γ^{-1}.

* This is of some interest in connection with the failure to obtain a power series with a finite radius of convergence by application of perturbation methods to some examples of renormalizable field theories. Cf. C. A. Hurst, Proc. Cambr. Phil. Soc. 48, 625 (1952); W. Thirring, Helv. Phys. Acta 26, 33 (1953); A. Petermann, Phys. Rev. 89, 1160 (1953), and R. Utiyama and T. Imamura, Prog. Theor. Phys. 9, 431 (1953).

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Additional Commentaries

In this book, the page limit constrains us to presenting reprints of only a few of Källén's papers. However, for the benefit of our readers who are in the field and who wish to know more about what Källén's other papers are about we include, in the following pages, commentaries about some of them. These concern:

[1950a]: The second approximation of the asymptotic phase for the Yukawa potential ...
[1950c]: Higher approximations of the vacuum-polarization in an external field
[1950d]: La Renormalization de Masse et de Charge en ...
[1950e]: Formal Integration of the Equations of Quantum Theory in the Heisenberg ...
[1952b]: The Bethe-Salpeter Equation
[1953b]: Non-Perturbation Theory Approach to Renormalization Technique
[1953d]: Quantum Electrodynamics
[1954]: The Coupling Constant in Field Theory
[1956d]: The Concept of Particles in Quantum Field Theories
[1956e]: Selected Topics in Field Theory
[1957a]: A Model of an Unstable Particle
[1957c]: L'Électrodynamique Quantique
[ ]: General Commentary on the Vacuum Expectation Value Program
[1960b]: Properties of Vacuum Expectation Values of Field Operators
[1959c]: Selected Problems in Renormalization Theory
[1961c]: Renormalization Theory
[1962]: Topics in Quantum Electrodynamics
[1966a]: Intuitive Analyticity
[1966b]: On the Calculation of Some Holomorphy Envelopes of Interest in Physics

C. Jarlskog (Ed.), *Portrait of Gunnar Källén*, DOI 10.1007/978-3-319-00627-7_95,

[1967c]: Different Approaches to Field Theory, Especially Quantum Electrodynamics
[1967g]: Old and New ideas in Field Theory
[1968a]: Gradient Terms in Commutators of Currents and Fields

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Commentaries on Papers from the Period 1950–1953

Commentary on Paper [1950a]

The second approximation of the asymptotic phase for the Yukawa potential, treated with Laplace-transformations

Arkiv för Fysik 2 (1950) 33

This early paper already reveals Källén's "taste" in physics: his love of equations and elegant excursions in the complex plane, as well as methods to produce numerical results. His summary reads:

> "The Schrödinger equation is here solved with the aid of an expansion in the coupling constant. Every term in the series is written as a Laplace-integral, and from this the asymptotical behaviour of the solution is easily obtained. The asymptotical phase will then also be expressed as a power series in the coupling constant. The first term corresponds to the usual Born's approximation, the higher terms can be considered as corrections for this formula. In this paper the explicit calculations are carried out only to the second approximation and for small angular momenta (S-, P-, and D-states). The numerical results are compared with earlier approximate formulae of Pais and Ramsey and with the more accurate calculations made by the variational method of Hulthén."

When Källén started his studies at the "Department of Mechanics and Mathematical Physics" in Lund, Hulthén (whom he refers to here above) was already there as a research associate. Actually, in this paper Källén thanks him for his "kind interest" in this work, which implies that the idea of doing this work was Källén's. Although this work was the young man's first research project, it received lower priority than his work on vacuum polarization, after he went to Zürich. It was submitted to publication three months later than his paper [1949].

C. Jarlskog (Ed.), *Portrait of Gunnar Källén*, DOI 10.1007/978-3-319-00627-7_96,

Lamek Hulthén in his short opening address[1] at the 1980 Källén Memorial Conference "Perspectives in Modern Field Theories" singled out this paper by stating:

> "... [after] passing his degree in electrical engineering in 1948. Then he went straight to Lund and it didn't take him two years to become a full fledged theoretical physicist. In the title of his first paper, published in the Swedish journal 'Arkiv för Fysik', 'The second approximation of the asymptotic phase for the Yukawa potential, treated with Laplace transformations' one may trace the electrical engineer, trained in exploiting Laplace transforms. ..."

Källén's references in his abstract, to Pais, Ramsey and Hulthén read:

A. Pais, Proc. Cambridge Phil. Soc. 42 (1946) 45,
W. Ramsey, Proc. Cambridge Phil. Soc. 44 (1948) 87,
L. Hulthén, Ark, f. Mat., Astr. och Fys. 35A (1948) no. 25.

Commentary on Paper [1950c]

Higher approximations of the vacuum-polarization in an external field

Arkiv för Fysik 2 (1950) 228

This is a half page "bulletin board" kind of activity report on some of the results published in Paper [1949].

Commentary on Paper [1950d]

La Renormalization de Masse et de Charge en ...

Colloques Internationaux du C.N.R.S. sur Particules Fondamentales et Noyaux, Paris (1950), éditions du C. N. R. S. (1953), p. 83, in French

Källén gives a talk at a meeting of the French national organization for scientific research (CNRS) on his work reproduced in this book as paper [1950b]. Due to strict French linguistic rules, prevailing in those days, his talk is translated and published in French.

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[1] L. Hulthén *in* proceedings of International Symposium "Perspectives in Modern Field Theories", edited by B. Nagel and H. Snellman, Physica Scripta, Vol. 24, No 5 (1981). See also the chapter "Källén as a Young University Student" in Part 1.

Commentary on Paper [1950e]

Formal Integration of the Equations of Quantum Theory in the Heisenberg Representation

For his doctoral dissertation, Källén had to submit, in addition to publications that he wanted to be considered, also a brief summary thereof. Paper [1950e] is his three-page summary. He submits two papers (papers [1950b] and [1950f]), both published in 1950 and reprinted in this book. Thus his thesis was "homogeneous, i.e., solely on "Källén-Yang-Feldman formalism" and based on papers published in the Swedish journal, Arkiv för Fysik. By tradition, the occasion called for extending one's gratitude to some people. Källén thanked professors Torsten Gustafson, Wolfgang Pauli and Marcel Riesz.

Källén defended his thesis on 24 November 1950, with Sven Bertil Nilsson (1920–2010) from Lund as his major cross-examiner. As was expected, he passed his exam with flying colors. Nilsson[2] told me many years later (CJ) that the discussion had been lively. He had had no difficulty in finding interesting questions to ask.

Commentary on Paper [1952b]

The Bethe-Salpeter Equation

Lectures delivered to CERN Theoretical Study Group,
CERN report CERN/T/GK-2, November 17 (1952)

Källén considers the interaction of pointlike protons and neutrons with a fictitious scalar field. He defines the Bethe-Salpeter wave functions and derives differential as well as integral equations for them. Then he addresses the question of the eigenvalues of these wavefunctions. He ends his lectures with the following remarks:

What we know about the Bethe-Salpeter method:

1) The formal mathematics is quite clear.
2) The "ladder-approximation" gives in the non-relativistic limit an eigenvalue problem for the binding energy. The equation obtained is identical with the usual "adiabatic" Schrödinger equation.

[2] Nilsson had been sent to Pauli in Zürich, already in 1946. Pauli was very interested in his work on renormalization, using a method proposed by the mathematician Marcel Riesz. Among theorists in Lund, Nilsson was known as the man who knows everything but never says anything, unless asked to express himself.

What we do not know about the Bethe-Salpeter method:

1) Is the equation really an eigenvalue problem also in the extreme relativistic case?
2) What is the physical meaning of the approximations made? How far can the "corrections" to the "adiabatic" result be trusted?
3) What is the physical meaning of the "wave-function"?

Of these three questions the last one seems to be least important and the answer is possibly "none"! The first question is not unimportant. It is possible that a closer inspection of the connection between the Dancoff method and the Bethe-Salpeter method could answer this question. Let us guess that the answer is "yes". The second question is certainly the most important one. Before that one is answered no reliable calculations can be made based on this method.

This concludes our direct quotation from Källén's lectures, which include only three references, viz., to Bethe-Salpeter, Gell-Mann-Low and Dancoff. As usual, Källén gives few references which he considers to be truly relevant, in order for his readers to have a chance to study them in detail. He used to say that you should not refer to papers that you have not read or have read but not understood. Unfortunately, nowadays it is common that authors refer to papers they have never seen. All it takes is cutting and pasting from other sources.

Commentary on Paper [1953b]

Non-Perturbation Theory Approach to Renormalization Technique

Physica **19** (1953) 850–858 (Lorentz-Kamerlingh Onnes Conference)

At this Conference, Källén gives a brief report on the non-perturbative theory approach to the renormalization technique. He uses the adiabatic switching on and off of the coupling constant. The renormalization constants are expressed in terms of certain integrals of the weight functions (spectral representation). He sees three possibilities:

(a) The integral $\int da \frac{\Sigma(-a)}{a}$ and $\int da \frac{\Pi(-a)}{a}$ are finite and the renormalization constants are finite quantities.

(b) The integrals above diverge, but $\int da \frac{\Sigma(-a)}{a^2}$ and $\int da \frac{\Pi(-a)}{a^2}$ are finite. The renormalization constants will be infinite, but observable quantities are finite.
(c) Also the last integrals diverge whereby the equations have no solutions which are physically acceptable.

The following discussions (pp. 857–858) took place after Källén's talk:

Peierls: The method of Källén is well suited to exclude case (a) but if case (b) is realized one still has divergent equations which do not have any meaning without a further prescription. Such a prescription is being formulated by Dr. Valatin in Birmingham as a generalization of the Heisenberg-Dirac method of treating the vacuum polarization in an external field. The method is always to handle expressions like $G(x, x)$ which are divergent where G is some Green's function by starting from $G(x_1, x_2)$ which is finite, and subtracting a second term which has the same singularities at $x_1 = x_2$. One can show that the finite equations which result are identical with the current renormalization theory if expanded in a power series.

Källén: Case (a) is not in itself very interesting. But this problem is much simpler to treat than a decision between (b) and (c).

I admit that in the derivation of the formalism above we have been handling the constants K, N^{-1} and $(1-L)^{-1}$ as if they were finite quantities. However, taken as they stand, the equations have in fact, some features which are quite similar to the Heisenberg-Dirac formalism for vacuum polarization. In the definition of the functions $\Pi(p^2)$ and $\Sigma_i(p^2)$ [Eqs. (21) and (25)] we compute the vacuum expectation value of the product of two operators $j_\mu(x)$ or $f(x)$ in two different space-time points x and x'. If case (b) occurs, the functions Π and Σ_i are thus finite functions for all finite p^2. The only point, where infinities can occur, is then in the integrations over the variables a [Eqs. (20) and (24)]. If the renormalization method works (cf. the remarks made after Eq. (25)), we never have to compute K etc. explicitly but only integrals like $\int da \frac{\Pi(-a)}{a(p^2+a)}$ which are convergent in case (b). Alternatively we can, of course, cut off all the integrals at some high value A and then let this cut off value go to infinity in the final result. Thus the formalism developed above might be quite suitable for the discussion of case (b).

Heisenberg: If the integration in your function $\Pi(a)$ is carried only to a finite limit A, not infinity, does that mean that the theory would change into a non-local theory, and what would be the properties of this non-local theory?

Källén: Such a cut off convention would certainly be a departure from the local theory and hence, by definition, be a non-local theory. On the other hand, I know very little about the actual properties of such a theory but I do not think it would be of the usual form, discussed *e.g.* by Møller and Kristensen.

Pais: The question arises what moral can be drawn from the fact that certain of the constants are either finite or infinite. Owing to the inherently non linear nature of the theory one has of course to consider that the electron-photon system is not a closed one and that in a more refined version also the nucleons and mesons must contribute to the renormalization constants.

Källén: In the theory of the future we probably have to consider the coexistence of all particles in nature. I hope that the study of the present form of quantum electrodynamics, where only electrons and photons are considered, will give us some insight in the somewhat intricate mathematical problems of a field theory.

In the same Proceedings, there is the written version of a talk by L. Rosenfeld on "Problems of Interpretation of Quantum Electrodynamics" (p. 859–868). In a footnote on p. 866, Rosenfeld acknowledges that he has weakened the statement he made in the original text of the report after the pertinent objections presented by Källén and Fierz in the discussion.

Källén: We must not forget the great difference between the simple model of Thirring and Hurst and quantum electrodynamics. Some signs which are very essential for the argument are quite different in the model and in quantum electrodynamics. (p. 867)

At this Conference, W. Heisenberg gives the summary talk of the Conference, entitled "Doubts and Hopes in Quantum Electrodynamics" in which he says the following about Källén's contribution:

> "At the conference we had a very important contribution from Källén who looked into this matter very carefully without using an expansion into powers of α. Then apparently there are three possibilities:
>
> (a) If we add up all the terms with different powers of α the infinities disappear, so that actually m' and e' are finite, although they will probably be different from the experimental mass and charge of the electron, the differences being finite quantities. In this case we would have a consistent theory.
> (b) The situation resembles the one we get by expanding with respect to α. This would mean that those quantities which can be observed are always finite but m' and e' are infinite.
> (c) The series with respect to α will diverge and so the theory has no meaning as long as we try to confine our attention to electrons and light-quanta only. One result of the paper of Källén is that possibility (a) is really ruled

out. It is thus certainly impossible that m' and e' are finite quantities and that we have a finite and simple mathematical scheme."

Commentary on Paper [1953d]

Quantum Electrodynamics

Mimeographed notes of lectures delivered in 1952–53 to the CERN Theoretical Study Group, CERN report CERN/T/GK-1 (1953); reprinted at CERN, Geneva (1956)

These lecture notes (29 pages) manifest Källén's train of thought on his path to his famous work on renormalization constants in quantum electrodynamics. He starts these lectures with A-B-C of quantum electrodynamics and ends up introducing his famous weight function π, its Hilbert transform $\overline{\pi}$ as well as his constant M and discusses their properties.

These lecture notes are available at CERN, though they are not (yet) on internet.[3]

[3] Private communication by Ms Anita Hollier (archivist at CERN) to me (CJ). I wish to thank Ms Hollier for her kind and efficient help.

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Commentaries on Papers from the Period 1954–1961

Commentary on Paper [1954]

The Coupling Constant in Field Theory

Nuovo Cimento 12, 217–225 (1954)

Källén summary reads:

> "The various possible definitions of the coupling constant in quantum electrodynamics and in meson theory are reviewed and their properties are discussed. It is pointed out that the gauge invariance of quantum electrodynamics makes the definition of the renormalized charge unique, at least in a certain sense. This simplification does not occur in meson theory where the definition of the coupling constant is more ambiguous. A recent attempt at comparing different mesic charges is discused."

See also Steven Weinberg's article in Part 3 of this book.

Commentary on Paper [1956d]

The Concept of Particles in Quantum Field Theories

Proc. of Math. and Phys. Soc. of Egypt 5, No.4, 101–111 (1956), Cairo University, Press (1957)

Källén has been invited to Egypt by his collaborator Afaf Sabry. He knows that in order to get his ideas across he has to be very pedagogical. His article has the following abstract:

> "Different possible definitions of states characterized by a definite number of particles are reviewed from the standpoint of the theory of quantized fields. These concepts are illustrated with the aid of two special models both of which can be solved exactly and are therefore suitable for a discussion of this kind."

C. Jarlskog (Ed.), *Portrait of Gunnar Källén*, DOI 10.1007/978-3-319-00627-7_97,

He discusses the dual concept of particle number, as defined through a free field and that associated with a state of an oscillator.

Topics discussed in this paper are:

I. The Particle Properties of a Non-Interacting Field
II. Generalization for Interacting Fields
III. Asymptotic Particles in Collision States
IV. A Scalar Field in Interaction with a Time Independent, External Source
V. The Lee Model

Commentary on Paper [1956e]

Selected Topics in Field Theories

Cairo Univ. Press (1956) 1

This article is 83 pages long and resembles a mini-textbook on field theory. What is strange is that Källén starts by recommending the following five general references:

- His own Handbuch Article (in German)
- W. Heitler, The Quantum Theory of Radiation (third edition, 1954)
- G. Wentzel, Quantum Theory of Fields (1950)
- W. Thirring, Einführung in die Quantenelektrodynamik (1955) (in German)
- I. Akiezer and V. B. Beresteckij, Kvantovaja Elektrodinamika (1953) (in Russian)

This would mean that he expected his Egyptian audience to be able to read and understand German and Russian! His own lectures are very pedagogical and cover the following topics:

- Classical field theory in canonical form
- Quantization of classical theory
- The free Maxwell field
- The free Dirac field
- The interacting fields [here he does perturbation expansion]
- The S-Matrix [here he teaches Feynman rules]
- Scattering of an electron by an external field
- Pair creation by a time dependent external field [here he derives his weight function Π]

- Møller scattering
- The Compton effect [here he compares theory with experimental results at "highest energies", $\gamma = 5$, i.e., about 2.5 MeV]
- Bremsstrahlung
- Pair creation by γ-rays passing through an external field
- Tamm-Dancoff and Bethe-Salpeter formalisms [emphasizes that perturbation expansion is useless for treatment of bound states]
- Vacuum polarization [uses the language of virtual pairs for short time and uncertainty relations]
- Charge renormalization
- Applications to the energy levels of the π-mesic atoms and hydrogen atom

For mesic atoms, he notes that the results have been checked, experimentally by Benedetti[1] et al PR 95 (1954) 1353. The agreement with the measurements is much better when the effects of the vacuum polarization is taken into account than when it is neglected. Also for hydrogen atom (Lamb shift) the situation is similar.

- The Lee Model [he notes that charge renormalization generates a new state in the theory which gives "negative contribution to the sum". In scattering of a theta-particle against a V-particle, there is a certain probability that the V-particle flips over to this new state. The cross section for this flipping is negative.]
- General outlook

Commentary on Paper [1957a]

(with V. Glaser)

A Model of an Unstable Particle

Nucl. Phys. 2, 706–722 (1957)

Glaser and Källén discuss the case where the V-particle, in the Lee Model, is unstable (i.e., when $m_V > m_N + m_\Theta$). The authors state:

> "This seems to us to be of some interest as it is the only case known to us where a theory with an unstable particle can be solved exactly. Especially we want to study the behaviour of the particular state that corresponds to the V-particle.

[1] This reference should read M. Stearns et al., Phys. Rev. 95 (1954) 1353

> Below the threshold[2] of (1) this state is an eigenstate of the Hamiltonian, but above the threshold this is not the case any more."

The computations are done with the help of a cut-off and the renormalized coupling constant is taken to be below the critical value, to avoid complication due to negative probability (ghost state). The authors find that there are no surprises – i.e., the results were in accord with what they had expected.

Commentary on Paper [1957c]

L'Électrodynamique Quantique

Colloque International (Lille, 1957) sur les Problèmes Mathématique de la Théorie Quantique des Champs, C.N.R.S. (1959), pp. 109–117

Källén gives a brief review of quantum electrodynamics, based on his Papers [1952a] and [1953c], at the 1957 Lille Conference.

He discusses his favorite conjecture concerning the Born approximation.[3] For a fuller account see Källén's 1958 Varenna Summer School Lectures, paper [1959c], Nuovo Cimento Suppl. Ser. 10, Vol. 14 (1959) 105–130.

General Commentary on the Vacuum Expectation Value Program

After the epochs of quantum electrodynamics and the Lee Model, on Källén's scientific path, followed an era of "vacuum expectation values". In a letter, dated 27 January 1964 and addressed to his collaborator Arthur Wightman, he complained about an article written by him that he had just received and in passing touched on what had attracted him to the Vacuum Expectation Value Program. He wrote:

> "... One thing which is completely missing both on that page and also in the rest of your discussion is any mentioning of the reduction formulae. They are easily (and originally) obtained using adiabatic arguments. After all, from the practical point of view, these expressions are very important and, from my point of view, the real motivation for my being interested in vacuum expec-

[2] Here the authors mean when the V-particle is too light to decay, as was assumed in earlier papers.
[3] Källén would argue that at "high energies" two particles colliding with each other have little time to interact. Therefore, the lowest order in perturbation theory (i.e., the Born approximation) should give an accurate description of their interaction.

tation values at all. The representations we are trying to work out for these quantities using analyticity arguments and what else we can find, have as their ultimate goal (still, of course, speaking from my own point of view) the purpose of giving an insight into scattering amplitudes and off shell quantities."

By adiabatic arguments, Källén means adiabatic switching on and off of the electric charge at remote past and future respectively, i.e., replacing the electric charge e by e exp $(-\epsilon\,|t\,|)$, where t denotes time and the limit $\epsilon \to 0$ is taken at the end of the computations. Källén thought that perhaps this new program would give further insight into his Born approximation hypothesis, and hopefully provide guiding principles for constructing physical models.

In this program, one studies the vacuum expectation value of the product of n scalar fields in the configuration space, i.e., the quantity

$$< 0|A_1(x_1)\ldots A_n(x_n)|0 >$$

where x_k are the coordinates. Källén used to call this quantity the n-point function. Some authors referred to it as the Wightman function.

These functions naturally appear in field theory, such as in propagators, vertex functions, etc. One wished to explore the analytic properties of these functions, by extending the coordinates into complex variables, and to examine what happens when a number of fundamental restrictions is imposed, such as Poincaré invariance, locality, and existence of a unique vacuum. Assuming translation invariance, which is always done, the point functions depend on the differences $\xi_k = x_k - x_{k+1}$ which are extended to complex values.

For more on Källén's work in this field see the contributions by Wightman (Chapters 80 and 66). As mentioned before, Källén ended up by being highly disappointed by lack of progress in this field as it was not leading to any insight into physics of elementary particles. Subsequently, he decided to change his field of research.

Commentary on Paper [1959c]

Selected Problems in Renormalization Theory

Nuovo Cimento Supp. Series X, 14, 105–130 (1959), (1958 Varenna Summer School Lectures)

This publication gives an account of lectures given by Källén at the 1958 Varenna Summer School. Several of Källén's close scientific friends and acquaintances were among lecturers: Lars Gårding, Rudolf Haag, Harry

Lehmann, Louis Michel, Wolfgang Pauli and Arthur Wightman. Pauli gave a lecture on the F. Gürsey's work on "Group Structure of Elementary Particles". Its abstract in the proceedings of the school reads:

> "The contribution of W. Pauli to this report was not intended for publication, however, it was decided to publish it, in the form in which the talk was given, as a document of His last activities." Indeed, Pauli had passed away in December 1958, i.e., before the publication of the proceedings of the school.

Several people that our readers have met in this book were present in this School as participants, among them Raymond Stora and Stephen Gasiorowicz. The latter took notes at Källén's lectures. Källén discussed the following five topics:

- Classical field theory and its quantization.
- A scalar field in interaction with a c-number source.
- Calculation of vacuum polarization by an external field.
- The Lee model.
- Remarks on quantum electrodynamics.

Commentary on Paper [1960b]

Properties of Vacuum Expectation Values of Field Operators

(1960 Les Houches Lectures) in Relations de Dispersion et Particules Élémentaires, Hermann et Cie (1960), pp. 389–454

The 1960 Les Houches Lectures were devoted to the dispersion relations and elementary particles. Beside Källén, the other lecturers were M. L. Goldberger, A. S. Wightman, R. Omnès, G. F. Chew, S. B. Treiman, and Y. Yamaguchi.

Källén addressed the following topics:

- The field concept.
- The particle concept.
- General physical assumptions.
- Reduction formulae.
- The two-point function.
- The three-point function.
- An integral representation for the three-point function.
- Some properties of the N-point functions.
- Some properties of the analyticity domain of the four-point function.

Commentary on Paper [1961c]

Renormalization Theory

1961 Brandeis Summer Institute Lecture, published in "Lectures in Theoretical Physics", W. A. Benjamin, New York (1962), Vol. 1, pp. 169–256

On this occasion, Källén lectured on:

1) The Lee Model
2) Scalar field in interaction with a c-number source
3) Electrodynamics
4) Reduction technique and its applications.

These lectures were well received and his broad knowledge of physics as well as great enthusiasm were much appreciated by the young participants. No wonder that already for the following year he was invited back as a lecturer. He accepted and gave a series of lectures on "Topics in Quantum Electrodynamics" at the 1962 Brandeis Summer Institute.

Other 1961 Brandeis Lecturers were:

- R. J. Eden (Complex Variable Theory & Elementary Particle Physics)
- J. C. Polkinghorne (Analytic Properties in Perturbation Theory)
- J. J. Sakurai (Elementary Particle Physics)
- M. E. Rose (Polarization Phenomena in Beta-Decay and Gamma Emission)
- E. C. G. Sudarshan (Structure of Dynamical Theories; Relativistic Particle Interactions).

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Commentaries on Papers from the Period 1962–1968

Commentary on Paper [1962]

Topics in Quantum Electrodynamics

1962 Brandeis Summer Institute Lecture, in "Elementary Particle Physics & Field Theory", W. A. Benjamin, New York (1963), V.1, pp. 123–262

The subject matter is perhaps best described by Källén's Introduction:

> "This course intends to give a survey of some of the standard problems in quantum electrodynamics treated by techniques which perhaps are not quite standard. After a short summary of the properties of free fields, we shall discuss the polarization of the vacuum, the anomalous magnetic moment of the electron, the Lamb shift, and electron-proton scattering, making extensive use of general arguments such as invariances of different kind and causality. This line of reasoning leads naturally to spectral representations for various quantities, and the weight functions in these formulae are computed with perturbative expansions.
>
> We feel that such an exposition is of value not only for those immediately interested in applications of the formalism but perhaps even more for students who want to investigate the general properties of quantized fields. The spectral representations and the related analyticity properties we are working with here play an essential role in such a general abstract approach. Someone working with the general formalism but without a good understanding of how it looks in practical application is liable either to flounder in very sophisticated mathematical problems or – what is nearly the same thing – to be doing complete nonsense. At the same time we hope to demonstrate that this way of handling practical problems is no more complicated than the standard techniques.
>
> The course is intended to be a short survey and does not attempt to encompass all problems in the field. Consistency problems, especially those related to a nonperturbative treatment, are not discussed. Some of the pitfalls which exist for the special problems of vacuum polarization were discussed in a course

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given by the author at the Brandeis summer school of 1961. For a fuller account of these and other problems not mentioned here, we refer the reader to the article Quantenelektrodynamik in Handbuch der Physik, Band V, Teil 1, Springer-Verlag (1958). References to the original papers usually are not given in this course but can be found in the above-mentioned article."

By the above-mentioned article Källén means his Handbook Article, which is in German. After his death it was translated into English. See G. Källén, "Quantum Electrodynamics", translated by C. K. Iddings & M. Mizushima, Springer-Verlag, New York (1972).

Other 1962 Brandeis Lecturers and their topics, in the session on Elementary Particle Physics and Field Theory, were:

- T. Fulton: Resonances in Strong Interaction Physics
- J. D. Jackson: Weak Interactions
- C. Fronsdal: Group Theory and Applications to Particle Physics

Commentary on Paper [1966a]

Intuitive Analyticity

in Preludes in Theoretical Physics (in honor of V. F. Weisskopf)
Editors: A. De-Shalit, H. Feshbach & L. van Hove,
North-Holland Publ. Co., Amsterdam (1966), pp. 100–109

Källén wrote this article in honor of Viki Weisskopf, on the occasion of his returning to MIT after a five year tour of duty (1961–65) as the Director-General of CERN. He had been called upon to succeed Cornelius J. Bakker who had died unexpectedly in a plane crash in the USA on his way to a meeting of the American Physical Society. Källén was surprised that CERN was celebrating that Weisskopf was leaving! Didn't they like him, he wondered.[1]

In paper [1966a] vacuum polarization in quantum electrodynamics and the vacuum expectation value of a product of two operators are discussed as simple examples of intuitive analyticity. In the concluding remarks, Källén states that:

[1] Victor F. Weisskopf (1908–2002) was very popular at CERN during his regency. The budget was increasing and many top technicians appreciated the fact that he would delegate "everything" to them. He would say, I am a theorist – you do it. He would also leave theorists alone, so that they could pursue what they wished to do. Weisskopf concentrated on international relations and other topics of utmost importance for CERN's future. See, CERN Courier, Special Issue 2002, "Victor Weisskopf (1908–2002)".

"The main purpose has been to illustrate how the analyticity concept which plays a significant role in some modern approaches to quantized field theory can be understood and interpreted on an intuitive level."

Commentary on Paper [1966b]

On the Calculation of Some Holomorphy Envelopes of Interest in Physics

in Proc. of Conf. on Mathematical Theory of Elementary Particles
Editors: R. Goodman & I. Segal, M.I.T. Press (1966) pp. 59–67

Invited by one of his scientific friends, Irving Segal (1918–1998), Källén gave a talk at the September 1965 MIT Conference on the Mathematical Theory of Elementary Particles. This became his last published account of his efforts in the domain of *n*-point functions. See further the Chap. 80, 66.

Commentary on Paper [1967c]

Different Approaches to Field Theory, Especially Quantum Electrodynamics

Proc. of 14th Conference on Physics (Brussels 1967), published as Fundamental Problems in Elementary Particle Physics, Interscience Publisher (1968), pp. 33–47; & discussions pp. 48–51

This was the second as well as the last Solvay Conference that Källén attended. The first one was the 12th Solvay conference in 1961 (Paper [1961b]) which has been extensively discussed in Part 2 of this book.

Other speakers and their topics at the 1967 Conferences were:

- G. F. Chew: S-Matrix Theory with Regge Poles
- H. P. Dürr: Goldstone Theorem and Possible Applications to Elementary Particle Physics
- M. Gell-Mann: On Current Algebras (report not included in the Proceedings)
- A. Tavkhelidze: Simplest Dynamic Models of Composite Particles
- R. Haag: Mathematical Aspects of Quantum Field Theory
- I. Prigogine: Quantum Field Theory with Decaying Particles
- E. C. G. Sudarshan: Vector Dominance; Indefinite Metric and Nonlocal Field Theories

- W. Heisenberg: Nonlinear Spinor Theory of Elementary Particles
- S. L. Adler: Experimental Tests of the Local Current Algebra

In his report, Källén discussed the following topics:

- Introduction; The Original Formulation of Lagrangian Field Theory
- An alternative Formulation of Lagrangian Field Theory
- Some More Recent Developments

Following Källén's talk there were questions and/or comments by R. Omnès, W. Heisenberg, E. C. G. Sudarshan, R. E. Marshak, F. E. Low, R. Haag, and H. Umezawa.

Commentary on [1967g]

Old and New ideas in Field Theory

Proc. of International Conference on Particles and Fields (Rochester 1967), ed. by C.R. Hagen et al., Interscience Publishers (1967), pp. 178–184

Källén was on the Advisory Committee of the above Conference, which was not a part of the so-called "Rochester Conferences" i.e., the series of International Conferences on High energy Physics which started in Rochester in 1950 and turned into world-wide meetings that take place every even year. The Proceedings of the above conference were dedicated to the memory of J. Robert Oppenheimer (1904–1967). The speakers in the Session II (New Approaches to Field Theory) were:

- R. P. Feynman: Field Theory as a Guide to Strong Interactions
- J. S. Schwinger: Back to the Source
- Y. Katayama (reporting on his work with H. Yukawa): Space-Time Picture of Elementary Particles

Källén was asked to give a critical summary of the contributions of the previous three speakers – a task that suited him and he enjoyed to perform.

J. Mehra and K. Milton in their biography[2] describe this event as follows:

> "In his rather unpleasant summary talk at the above-mentioned 1967 Rochester conference, Gunnar Källén commented:

[2] See, J. Mehra and K. Milton, "Climbing the Mountain: The Scientific Biography of Julian Schwinger", Oxford U. Press, (2000)

> 'Lagrangian field theory was, shall we say, considerably improved twenty years ago through the work of Tomonaga, Feynman, Schwinger and many others. Twenty years ago we had what appeared to be two rather different formulations. One was Feynman's space-time approach with diagrams, which no one understood when it was first presented. The other formalism was very much easier to understand, it was Schwinger's approach with operators and fields. I think if someone had told us twenty years ago that in 1967 we would at the same conference hear a talk by Schwinger about a space-time approach to strong interactions with diagrams, and Feynman speaking about operators, commutators, singularities, and so on, at least I would not have believed it. However, that's life.'

Schwinger labeled this comment as 'amusing'; but went on to remark that he quite misunderstood then that in fact our two starting points had long since been amalgamated, at least from my point of view, in this general Green's function theory. I'm sure afterward we got together, Feynman and I, and had a good laugh. Perhaps exchanged a few points."

R. F. Streater, who also was present at the conference, recalls[3]:

> "Källén and Schwinger met again at the Rochester Conference on Particles and Fields (Rochester, N.Y., 28 Aug.–1 Sept. 1967). Schwinger had just invented his 'sourcery', which is a way round the divergences of quantum electrodynamics by neglecting certain terms corresponding to back reaction. He was a leading speaker at the conference, along with R. P. Fenyman, Y. Nambu, S. Mandelstam, A. S. Wightman, J. S. Bell, R. Haag, G. W. Mackey and Y. Neeman, J. Cronin and others. C. N. Yang and Hideki Yukawa were present, the latter's work being presented by his coauthor, Y. Katayama.
>
> Schwinger illustrated the use of sourcery in computing the decay rates of certain mesons. Källén asked the question, why is this new, since Ben Lee (and a coworker) had already computed these decay rates some time before. Schwinger replied that he did not know of this work, but it could not be the same, as sourcery had only been invented (by himself) a few months earlier. Källén insisted on a supplementary question: he said, that if you start with Lee's assumptions about the interaction, they were the same as Schwinger's; the first line of the calculation is the same; going through the calculations, said Källén, write Lee's working in the left-hand column, and sourcery in the right-hand column. The workings are, line by line, the same in Lee's paper as in Schwinger's lecture. And, the answer is the same. 'That may be so', said Schwinger, 'but one column is right and the other column is wrong'.

[3] See http://www.mth.kcl.ac.uk/~streater/Kaellen.html

> The laughter did not allow any further questions. Even Irving Segal, who was with me, smiled. This exchange is omitted in the proceedings of the conference [Proc. of the 1967 International Conference on Particles and Fields, Interscience, 1967; eds. C. R. Hagen, G. Guralnik, and V. S. Mathur]. The discussion presented there seems to be a sanitized version, in which Ben Lee gives a measured argument similar to Källén's, but more polite. His coauthor, by the way, was Nieh."

Mehra and Milton's statement that Källén gave an "unpleasant summary talk" is surprising. The Källén Collection testifies otherwise. For example, in a letter dated 19 March 1968, Abdus Salam writes to Källén, concerning a conference that he is organizing, in mid June 1968, in Trieste:

> "Dear Gunnar,
>
> … during the Symposium, in the morning of 14 June, F. Low will be speaking on quantum electrodynamics. We would very much appreciate if you could act as respondent to him. The brilliant performance which you gave at Rochester is remembered by everyone, and we naturally expect something even better."

Commentary on Paper [1968a]

Gradient Terms in Commutators of Currents and Fields

Lectures at Karpacz and the 7th Schladming Winter Schools (1968), Acta Phys. Austriaca Suppl. V. (1968) published *in* "Particles, Currents, Symmetries" Springer (1968), pp. 268–319

In these lectures, Källén reviews some properties of the vacuum expectation values of products of two fields. As is his usual practice, for the spectral representation Källén gives the credit to Kamefuchi and Umezawa (S. Kamefuchi and H. Umezawa, Progr. Theor. Phys. 6 (1951) 543) and quotes his own [1952a] paper. What Källén referred to as the gradient term in Eq. (2.13) is commonly known as the Schwinger term in the literature. However, Källén credits such terms to the work of Goto and Imamura (T. Goto and T. Imamura, Progr. Theor. Phys. 14 (1955) 396). See also the chapter on Schladming Schools 1966–1968 in Part 2.

Källén's summary includes the statement:

> "For the particular case of quantum electrodynamics, it is further pointed out that the coefficient of the gradient term is related to the self mass of the photon which has to vanish in a consistent theory."

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