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 $\psi^{out} = S^{-1}\ \psi^{in}\ S$. 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

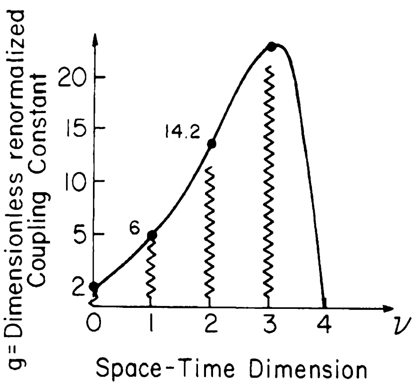

Fig. 1. Diagram representing the possible values of the renormalized coupling constant as a function of space-time dimensions in the $\lambda\phi^4$ theory.

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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