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THEORETICAL PHYSICS AT THE IHÉS. SOME RETROSPECTIVE REMARKS

by Arthur S. WIGHTMAN

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 invité permanent. 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 of 1960. At the time the Institute was still located in the Fondation Thiers at Rond Point Bugeaud on the right bank of the Seine. The practical arrangements were these. The Dieudonné and Grothendieck seminars, separated by tea, took place on Wednesday afternoons in an auditorium of the main building, where the Director of the IHÉS, Léon Motchane, and his secretary Annie Rolland had an office. 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 mathematics and physics interacted.

The discussion 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 $\mathcal{C}^3$ 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 representations 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 a 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 (1951-2) in Copenhagen on a National Research Council postdoctoral fellowship where I took advantage of an easy commute to Lund to work with Lars Gårding. 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

$$F^{(n)}(x_2 - x_1, x_3 - x_2, \ldots x_n - x_{n-1}) = (\Psi_o, \phi(x_1) \ldots \phi(x_n)\Psi_o)$$

where Ψ_o 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_+: $p \cdot p = (p^o)^2 - \vec{p}^{\,2} \geqslant 0, p^o \geqslant 0$) 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 $p_j \in V_+, j = 1, \ldots n-1$. 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 $\eta_1, \ldots \eta_{n-1} \in V_+$, 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:

$$F^{(n)}(\Lambda, z_1, \ldots \Lambda z_{n-1}) = F^{(n)}(z_1, \ldots z_{n-1}) .$$

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 complex Lorentz transformations and used to continue $F^{(n)}$ as a single-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

$$[\phi(x), \phi(y)] = 0 \text{ if } (x-y) \cdot (x-y) < 0 .$$

This implies

$$F^{(n)}(z_1, \dots z_{j-1}, z_j, z_{j+1}, \dots z_{n-1} = F^{(n)}(z_1, \dots z_{j-1} + z_j, \; - z_j, z_j + z_{j+1}) \dots z_{n-1}) \, .$$

When $z_1, \dots z_{n-1}$ runs over the extended tube,

$$z_1, \dots z_{j-1} + z_j, \; - z_j, z_j + z_{j+1}, \dots z_{n-1}$$

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 functions 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. That 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 Muenster 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 Muenster Heinrich Behnke, Hans Grauert, Reinhold Remmert and Friedrich Sommer. 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 technique and the use of what Behnke and Thullen had called the Kontinuitätsatz was regarded by the experts 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 yet 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 $F^{(3)}$ determined in [1], Källén and John Toll 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 representations 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.

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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