Copy for Gürsey

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Dear Landau:

I received your letter with some delay because of my trip to Berkeley, where I intend to stay until the end of May. I shall be back in Zurich by June 2nd, and I also intend to be present at the Conference in Geneva at the beginning of July. As long as I am here I prefer to write in English for the technical reason that it is easier to get typed.

From Weisskoff I hear that you are rather enthusiastic about the attempt of Heisenberg and me to incorporate the isogroup into the spinor model and to connect an indefinite metric with it. My own mood is rather fluctuating and at the moment I am again rather critical about the whole idea, while Heisenberg is always the optimistic partner. As you are on the list of the persons to whom preprints have been sent, I assume that you obtained it meanwhile and I shall make here some amendments and comments to it. This preprint has not been sent for publication yet because of the dark points which it contained. I shall make the final version for print as soon as I can overcome the remaining difficulties, which I shall mention.

1. I start now to report on other discussions which preceded this particular paper and which are connected with Heisenberg's paper on the Lee-model (Nuclear Physics 4, 532, 1957). I found that the method of this paper can be extended to the case of complex roots of $h(z) = 0$. The "dipole ghost" is a limiting case, which, however, is unessential in the Lee-model. (The double root has been introduced by Heisenberg in his earlier papers on the spinor model in order to obtain photons from it.) As the complex roots are never on a real energy shell it is trivial that the S-matrix on the latter stays unitary, but it is not trivial that this S-matrix can be obtained as a limit from a macroscopic space time description, which means from a U-matrix joining states at finite times (t_1, t_2), as in general exponential functions of time will occur which increase indefinitely in the limit $t_1 \to -\infty$, $t_2 \to +\infty$. Here, however, the partition of the Hilbert-space into the two equal halves of the upper and the lower complex plane comes into play. Every such half plane contains only zero states besides the ordinary physical states of positive norm and real energy similar to the Bleuler-Gupta metric in electrodynamics. One obtains a non-physical result only by a superposition of a state of the upper half plane with one in the lower half plane. Therefore it is possible to prescribe only the physical part of the initial state at t_1 and to choose the non-physical

part in one half-plane in such a way, that the final state at t_2 contains only non-physical states in the same half plane and not in the other. I have shown by a perturbation calculation that in this case the exponentials which increase with time do not occur in the transition matrix and that the S-matrix can be incorporated into a macroscopic space-time description. This perturbation calculation has been repeated independently and also somewhat extended by Källén with the same result. For small space-time regions many strange results appear in such a formalism and for the Lee-model also a violation of the T-invariance.

But in this way the door seems to be opened for further developments by searching for systems with an indefinite metric in Hilbert-space without single states with negative norm and real energy values. This is also the opinion of Källén, who favors now an indefinite metric of this kind but is at present opposed to the more special idea of a spinor-model. I define the latter by the special assumption that all fields should be derived from a single spinor field. Heisenberg and I are now trying how far one gets with the latter, but I am also tolerant if somebody wants to investigate models with a larger number of independent field operators. In any case, it is clear to me, that the indefinite metric in Hilbert-space is the last attempt to save a theoretical description using field operators, which depend continuously on space and time.

2. Last autumn I received the preprint of a paper by F. Gürsey, in which he showed the isomorphism between the isospin-rotation group and the group corresponding to baryon-conservation law with transformations which I had formulated in the very different connection with the free neutrino (Nuovo Cimento 6, 204, 1957). I guess that Gürsey's paper has appeared meanwhile in the Nuovo Cimento. (Gürsey is a younger man of Turkish nationality, who is now in this country and who is supposed to come to Berkeley soon.) At about the same time Heisenberg worked on the incorporation of the isospin into his spinor model and had already changed his Lagrangian for this purpose. The possibility of a synthesis of all these somewhat heterogenous ideas occurred to Heisenberg and me and the result of it is the preprint which you got.

But here already some criticism of mine starts. Heisenberg's Lagrangian, both the old and the new, is the sum of two terms which have logically no link with each other (of which the second term is multiplied by ℓ^2). This arbitrariness may be weakened by Heisenberg's assertion that the results don't change, if the second term is replaced by a certain class of function of it. But still, the sign of the second term relative to the first seems to be essential; a circumstance which I don't like at all.

Moreover, more recently Heisenberg has added the formalism with the $\tilde{\psi}$ to the manuscript, the consequences of which need further investigation. Whatever the connection "through the total Hilbert space" (p. 6) between the two spinors ψ and $\tilde{\psi}$ (and its ~~conjugate~~ adjoints ψ^+ and $\tilde{\psi}^+$) may be, one has in this way many more possibilities to construct Lagrangians which fulfill all claims of invariance.

3. The decisive key for further developments of theories of the kind proposed by Heisenberg and me (may it be the spinor model or a more general model) lies, according to my opinion, in our section 5. The methods for solving eigenvalue problems in theories without a priori given Hamiltonian or commutation rules are only sketched in this section. The state of these methods, as they have been developed until now by different groups of "experts" are in no way satisfactory. The main problem seems to me to study further what are the mathematical possibilities for the commutation relations, if the Lagrangian (and therefore the field equations) are given. Although Heisenberg theoretically emphasizes that it is not possible to introduce arbitrary assumptions for the invariant functions occurring in the vacuum expectation values, he nevertheless practically behaves as if such arbitrary assumptions were possible.

He does so in the course of the application of the so-called Tamm-Dancoff method of approximation. Quite independently of the name of this method (whether one calls it Tamm-Dancoff method or simply Tamm-method) I believe that it is one of the worst approximation-methods ever invented in the history of physics (an opinion which is in no way shared by Heisenberg. My very best regards to our excellent friend Tamm). Therefore I never had much interest in the earlier published results of Heisenberg on the spinor model nor do I participate in the calculations made at present in Goettingen by Heisenberg and his collaborators by applying this method.

The problem of the connection between the vacuum-expectation values (of two or more factors of localized field operators) on the one hand and the Lagrangian on the other hand is a very fundamental one. The idea seemed to me seductive, that in an indefinite metric it may be possible, that the former could have a lesser symmetry (group-invariance) than the latter and that in this way one could reach an interpretation of the gradual breakdown of invariances by passing from the strong interactions to the electromagnetic ones and again from these to the weak interactions. (In contrast to Heisenberg I don't believe that the particles with zero rest mass are of decisive importance in this connection.) But whether this idea can be actually carried through can only be decided if one succeeds in improving considerably the mathematical methods mentioned in our section 5. I don't believe, that I am able to do this alone and I am therefore much interested in your opinion on this point. Perhaps in your Institute one could also work on it. I also believe that only with better methods will it be possible to decide whether one particular model like the spinor model is better or worse than other more general models.

4. I want further to draw your attention to another question which goes more into detail and is specific for the spinor model, namely the problem, how to interpret the "strangeness" of ~~the~~ some elementary particles. With this I mean particularly the circumstance that there exist particles with half-integer ordinary spin and integer isospin and also the other way around. For this purpose Heisenberg invented the trick of a degenerate vacuum which you will find in the preprint. This is, however, an ad hoc hypothesis which in no way follows from the other assumptions of our paper. Until now no particular prescription is known to me, how one obtains the "symmetry changing $\tilde{\pi}$ " by applying $\psi \psi^{\dagger}$ to the degenerated vacuum. It is very doubtful to me whether this trick of vacuum-degeneration works at all. Gürsey proposed a different formalism with a simple vacuum, in which the neutrino produces the strangeness, but his proposal has also its disadvantages. Perhaps one meets here a necessity to generalize the spinor model by introducing at least one more field, which cannot be derived from a single spinor field.

These are the main features of my present attitude to a new situation, which is however far from being final, but on the contrary, is always changing. I shall let you know if new developments occur and please let me know your own views.

Yours,

W. Pauli

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