UNIVERSITY OF CALIFORNIA

DEPARTMENT OF PHYSICS
BERKELEY 4, CALIFORNIA

Febr. 10, 1958

Dear Dr. Gursey,

I was glad to have received today your letter of Febr. 6. Your letter to Zürich did not turn up until now, but possibly it will still come.

I would appreciate it if you could tell me more about your formalism with the non-degenerate vacuum, which I would prefer. (I have only the notes you gave me in New York.) Heisenberg seems to think, that one can explain with a degenerate vacuum only, that a halfinteger isospin ~~could~~ can be coupled with an integer spin (or the other way round). I am not so sure about this point and I would like to hear from you whether your formalism gives also this possibility of strange particles.

Your more modest way of attack has perhaps the better prospect than ~~t~~ my more fundamental one. At present I am worrying about

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many problems, but particularly on the "mirror-states", which actually do not occur in nature.

From the table in the (pre)^{n}-print one obtains them by changing the sign of I_3, $\frac{\ell}{2}$, Q, while keeping I_B, $\frac{\ell_B}{2}$, N fixed.

I don't see that I can introduce a doubling of pions, K-particles, photons which are identical with their mirror-states.

If a) the mirror states exist mathematically, these bosons should be able to generate also the (actually non-existing) mirror particles in pairs of particles-antiparticles.

If b) the mirror states do not ~~exist~~ even formally, how one can then construct the mentioned bosons out of the spinors states in such a unified model?

In the moment I don't see a satisfactory way out. Of course the difficulty does not occur, if one introduces the bosons extra, not in the way of a spinor-model.

I am also worrying, that the Lagrangian is not uniquely determined.

The question of your coming ~~tomorrow~~ here will be talked over tomorrow and you will ~~well~~ hear about it.

Yours sincerely W. Pauli

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Feb. 17th, 1958

Dear Dr. Gürsey,

Thanks for your letter of Feb. 14. – I am now eagerly awaiting your next letter, which should contain the discussion of the really important problems.

1) Ambiguity of Lagrangian for q-number ψ. (What you wrote on the c-number case was known to me). Meanwhile they will presumably ~~do~~ investigate this in Göttingen, too. I wonder what the result will be. – By the way, do you have a reprint of your Istanbul paper?

2) mirror problem. – Also about that I expect

was from Göttingen. A Dr. Dürr (who was earlier in Berkeley) is working here on your line. Very much depends for me on whether or not there is a satisfactory solution for it. If one can include the strange particles with a simple vacuum, I would prefer it. But I am not sure.

3) My scruple is, whether I can recommend the Princeton Institute to you, not the other way round. Do you know, who will be there (particularly younger people) during the winter term 58/59?

There is also a chance, that I shall get funds in Zürich. In this case I would certainly invite you (perhaps for spring 59). I hope to know more about it in the near future.

Hoping to hear from you soon again

Sincerely yours W. Pauli

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Continuation, Febr. 11, '58

1) Lagrangefunction.

I am interested in the question how many independent forms of the 4th degree have the invariant property of isoinvariance and b) ordinary Lorentz-invariance in spinor space.

The expression at the end of your last letter (of Feb. 6) seems to me correct.

Kroll's expression $\sum_{\mu} (\bar{\psi} i \gamma_\mu \gamma_5 \psi)^2$

$(\bar{\psi} = \psi^* \gamma_4)$ is another possibility.

How many possibilities exist? One can construct forms with squares of vectors, tensors, pseudovectors too – and look whether they are iso-invariant or not.

It is not excluded either, that there is an algebraic identity between Kroll's expression and yours.

I shall have a look to it myself, but two see more than one and I shall be glad, to hear from you about it.

2) Classification of elementary particles.

From the (wee)"-print, which you obtained in New York, you have seen, that we introduced

4 quantum numbers I_3, $\frac{l}{2}$ and I_B, $\frac{l_B}{2}$

connected with charge Q and baryon number N by

$$Q = I_3 + \frac{l}{2} \quad \text{and} \quad N = I_B + \frac{l_B}{2}$$

** Of course your $\frac{1}{2}t_3$ (in the Notes, you gave me in New York) is identical with I_3; but your L should be identical with l_B.

Now you write, l. c.

$$L = \frac{-u+f}{2}$$

$$Q = \frac{u+t_3}{2}$$

$$N = \frac{u+f}{2}$$

with only 3 quantum numbers.

As L, Q, N are all conserved in your formalism, the same would hold for f, t_3 and u.

Moreover one has $u \equiv l$, $Q \equiv \frac{1}{2}(N - L + t_3)$

This is certainly not generally true.

Moreover empirically t_3 is not conserved in weak interactions (see $\Lambda \to p + \pi^-$, etc.)

Therefore it is my impression that something is principally false in your 3-quantum number formalism and that one needs a 4-quantum-number formalism as proposed by us. (This is also connected with the question, whether the vacuum state is degenerated)

I would be glad to hear from you about it and also your opinion on my mirror world difficulty.

Again yours W. Pauli

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18 Febr 1958

Dear Dr Gürsey,

I looked meanwhile in the paper of Schwinger, „A Theory of Fundamental Interactions" Annals of Physics, 2, 407–434, 1957, which is independent of the spinor model.

In this paper an alternative way to treat the 'mirror problem' is developed. What I call 'mirror particles' exists actually in Schwinger's classification. Only the 'mirror particles' have different masses than the originals. In Schwinger's classification the Ξ^--particle is the 'mirror' to Proton, the μ^- is the 'mirror' to electron e^- (opposite leptonic charge, see l.c. p. 422). In the μ-decay one neutrino and one mirror-neutrino are emitted (not one neutrino and one anti-neutrino).

But Schwinger needs besides the conservation of the leptonic charge, a new conservation law called conservation of 'neutrinic charge'.

The difficulty for the spinor model is

are shifted to the new concept 'neutrinic charge' and its conservation law, I don't see how it can be obtained with the spinor model. Moreover I prefer the usual 'two component neutrino theory' because of its simplicity.(*)

But I am eager to hear your opinion on all that in your promised new letter.

With best wishes

Sincerely yours

W. Pauli

(*) P.S. The experimentalists in the Radiation Lab think, that Schwinger's 4 neutrinos have only a historical reason. So you can read this 'for the sake of completeness' only.

I intend to send a ~~[illegible]~~ copy of your letter of Feb. 22 to Goettingen.

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Feb. 27, 1958

Dear Dr. Gursey,

After I sent my letter to you yesterday, I realized, that I had forgotten to mention a few things.

1.) The mirror-~~pr~~oblem

I had received on Feb. 24 another letter of Heisenberg, written on Feb. 21 (probably without study of the Schwinger paper), where he proposed a solution of the mirror-problem very similar to yours: that the mirror-particles don't need to have the same mass as the original object. He also thinks, that the mirror's of ε are unobserved, possibly unstable and that the μ is something else.

2.) The group-invariance (~~problem~~ (symmetry)).

Here probably the transition from Ψ to Ψ' p. 4 below of your letter comes into play. One of these two matrices Ψ and Ψ' must have the γ5-invariance, the other must lead to this spinor (for instance for electron's e or for Λ0) which ~~[illegible]~~ obeys the ordinary Dirac-equation.

But this is your speciality and you can probably ~~[illegible]~~ give me the answer very quickly. I don't believe anymore that Källén's objection is a very profound one.

But I am looking forward very much to discuss with you ~~the~~ (in the following letters) the whole fundamental problem of symmetry (of the Lagrangian's group-invariance) and of the commutation-relations. Here comes also the formulation of the electromagnetic forces within the spinor model into play. I have only some rather provisional knowledge of it.

I don't believe the Tamm-Dancoff method (which the Goettingen group still applies), but I think that ~~both~~ the transformations corresponding to Q and to N should be generalized with x-dependent phases. (Does this leads to something similar as Schwinger's Z-particles?)

3.) I have, moreover, to report that I eventually obtained from Jordan your letter of Jan. 22d. It was interesting for me to learn the earlier history of your work (conform-group etc.). Do you have reprints of your earlier papers?

Hoping to hear from you soon

Sincerely yours

W. Pauli

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March 1st, 1958

Dear Dr Gursey,

Yesterday a group of theoreticians discussed with me your letter of Feb. 22d – We had some difficulty to understand I_3 (rather than $\frac{1}{2}t_3$) as the 3d component of the isospin, because in this case there must exist also operators I_1, I_2 which, together with I_3 generate the 3-dimens. iso-rotation group (for instance $\Sigma_{-1}, \Sigma_0, \Sigma_1$ is an isotriplet belonging to an absolute value 1 of the isospin)

./.

Moreover, ~~the~~ electron ε_+, ε_- and ν is widely separated in your scheme. ~~and~~ Bludman proposed an alternative scheme, with $\varepsilon_+, \varepsilon_-, \nu$ an isotriplet $|I| = 1$, and ν a Majorana-neutrino.

But the main problem is the Lagrangian and the commutation relations.

All good wishes

Sincerely yours

W. Pauli

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March 13, '58

Dear Dr Gursey.

Thanks for your letter of March 8. – Now again many things are clearer. – Källén's objection is "off." – I only want to add that the non-invariance of Ψ under $\Psi \to \gamma_5 \Psi$ seems to transform itself into a non-invariance of Ψ' under $\Psi' \to \Psi'^{C}$. But no objection springs from it.

What Bludman meant regarding the particle – antiparticle symmetry in your formalism I don't understand myself. – What was really new and clarifying to me was the section 6 of your letter on "The meaning of the operators corresponding to t_3 and I_3". (I propose than the notation N, I_N, l_N; Q, I_Q, l_Q – instead of $\underline{I}_3$ in your formalism.) - Unfortunately I am not very familiar with the paper by Salam, D'Espagnat and Prentki on "charge space", which you quote.

Ae Shalit at CERN seems to be also interested in the x-dependent gauge transformation.

Much will depend on Lagrangian and commutation relations. I wonder, whether Dürr makes headway in Göttingen with the competitive formalism with the analogous problem in the degenerated vacuum.

I enclose the copy of a letter to Landau, please show it also to others.

Looking very much forward to see you soon

Yours sincerely

W. Pauli

Physikalisches Institut
der Eidg. Technischen Hochschule
Zürich

ZÜRICH 7/6
Gloriastrasse 35 Dec. 5th, 1958

Dear Dr. Gursey:

It was nice to have news from you again. I count on it, that you will come here for the summer term (end of April). I can not yet give you details on the financial question, but I am sure, there will be no difficulty with that. I will come back to it later.

Now to physics. That Lee and Yang "disapprove strongly of your approach to physics" is, of course, a serious matter for me. Just with these two men I feel quite „at home" in all questions of taste and of instinct in physics. But, in order to be pedagogical ~~the~~ your statement is too general, we need something more specific as to how and where this disapproval is lying.

Now, I can well say, where I disagree with your approach: I believe, that the experiments of Schwartz and the Alvarez group will soon be confirmed and extended and that P-conservation is <u>not</u> generally true for strong interactions. I admit, that I am influenced in this direction by <u>Ledermann</u>, who was recently here in Zürich and ~~to~~ who seems to be rather convinced of this new P-violating experimental results. Moreover there exists a paper as preprint "Conservation of Parity and Strong interactions" of 1957 (which, as far as I know, never appeared) with the following <u>Summary</u>:

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"Time reversal together with certain assumptions about the form of the interaction energy assures the conservation of parity in strong pion nucleon interactions as well as in electrodynamics. The same arguments do not necessarily apply to those strong interactions which involve heavy mesons and hyperons. It therefore appears important to check the conservation of parity in the strong interactions of new particles. It is shown that up to now there exists no evidence to support the assumption of parity conservation in these interactions."

Indeed, I expect large deviations from P-conservation (due to Σ-Λ-π-interactions) for K-nucleon $\to \Sigma$ or Λ processes and also small P-deviation effects for pion-nucleon interaction. The "assumptions about the form of interaction", mentioned above concerns the non-derivative coupling, which however, is entirely arbitrary and, I think, should be replaced by derivative coupling. On the other hand, the T or CP-invariance seems to hold empirically for all kind of interactions. The experimental exploration of all these questions is ahead of us and. I believe that you should rewrite the whole argument in your paper. The symmetry conditions should be used as a guide for the construction of parity violating interactions: where one is forced to derivative coupling to get some P-violation? [NB. in your symmetry conditions (α) one should probably read $K^+ \to -\overline{K^+}$ (instead of $K^+ \to -K^+$) and in (β) $K^0 \to -\overline{K^0}$ (instead of $K^0 \to -K^0$)]

I read the preprint of Sakurai, which is written very agreeable and intelligent. However, I immediately

II

got a strange impression, that nature will be very different from what he believes.

√ The rule "as stronger the interaction, as larger the symmetry which it permits" seems to be false.

√ One more remark on the pion-nucleon scattering: I expect that the "large phases" of the scattering are parity conserving, but the small phases (and S-scattering) could be different.

The papers of Pais I have not yet read. In January Mr Dell' Antonio from Milan comes here, who will be very suited to discussion of them.

To the paper ~~of~~ of yours (and Feinberg's), which is going to be mimeographed, I am therefore looking forward with horror and I think, its argument ~~should~~ should be entirely put upside down!

√ I myself abandoned some ideas, which I had last spring: I don't see anylonger a sufficiant justification of applying the concept of isospin to leptons, which have no strong interactions. Moreover, the idea of a pure electromagnetic mass of an electron seems to lead to grave theoretical difficulties because of the γ_5-invariance, when one puts the mechanical mass $m = 0$.

I showed your letter also to Stapp and discussed it with him. He also sends regards. From the socalled "field theory", which seems to me more and more fictitious, I have enough for the time being (and I give a lecture on many body problems, which I hope slowly to learn.

√ In Geneva I had last months a discussion with

Ferretti, Ascoli, Glaser and Froissart. It was very discouraging. It is true, that the overdetermination of the conditions of unitarity was an error, but new difficulties arose: Glaser obtained in all with indefinite metric models), which he could discuss, for the physical particles (in particular Lorentz-invariant) (Lee theory) imaginary or complex additional self-masses (which are not easily to subtract). In other words: the interaction changed the "physical" particles into ghosts. There were other difficulties, too. There was a general inclination to abandon this whole idea of an indefinite metric. — It was also stated in this paper, that the conclusion $T \to P$ works in electrodynamics with no derivative couplings (including Pauli-terms).

I also was for 3 days in Hamburg last month, but Lehmann had nothing new. From Jost I can say myself that I „disapprove strongly of his approach to physics" which is getting more and more formalistic.

Please show this letter also to Lee and Yang with my kindest regards. I started to read their paper on superfluidity. In the moment I am unable, to do anything reasonable with field quantization.

All good wishes to both of you from Mrs. Pauli and myself and I am looking forward to have you here in spring. Please write to me about your plans.

Yours sincerely

W. Pauli

Regards to all friends at the Institute.