

BROOKHAVEN NATIONAL LABORATORY
ASSOCIATED UNIVERSITIES, INC.
UPTON, L. I., N. Y.
TEL. YAPHANK 4-6262

DEPARTMENT OF
PHYSICS

January 4, 1965

Dear FEA,

because of the Christmas vacation the mail has been rather slow and therefore only now I have finished the revision of your manuscript which will be typed & I hope, tomorrow submitted to P.R.L.

I enclose the correct version for you so that you may read it & if you do not agree, you may send a cable & I will stop the publication.

The main changes are the following

1. A few corrections, essentially formulas (10) & (19) and some rewording which in my belief eliminate some confusion in the use of the words 'state' & 'field'.
2. Elimination of p. 7, namely the Pauli-Ginney transformation. This I did after great

hesitation and only because of the urgency I finally decided to submit the paper without previously asking your 'blast'. The reasons are the following.

(i) This part is in the way it was worded rather obscure. Whereas the reader can verify the rest of the paper, I think he will find great difficulty to do the same here.

(ii) This is an entirely separate part giving a different interpretation of the little group. Since it stands apart, it is in itself important and I have contributed nothing to it, I think you should write a separate note with more details & publish it alone. I very earnestly urge you to do this.

(iii) It is the only part where the word field is really necessary since you use charge conjugation. In the rest of the paper we now only speak of states & representations.

I don't want to delay any further writing this letter. Please don't hesitate to cable.

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*if you disagree with this revision. I will
immediately stop the publication.*

*I hope to send you the typed copy tomorrow
with some more comments.*

*Yours ever
Lang*

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DEPARTMENT OF
PHYSICS

January 8, 1965

Stan Fisa,

I had written a long letter to you on January 5 but I didn't mail it & decided to send instead a telegram to avoid further delay. Let me first of all apologize for the mutilation which I have introduced in your paper. I spent a lot of time debating on the best thing to do & finally concluded that this was the best way to present the situation as I see it now. I sincerely hope that in so doing I haven't grossly misinterpreted your point of view.

Let me now explain my difficulties (I forgot to tell you that I have submitted the paper today; I will send you the final text next week)

1. Eq. (11) $[X_0^4, P_\mu] = 0$ shows that the X_0^4 are constant of the motion. There is no need according to me to move this separately for the Dirac field since this is true

in general. If however we want to do as we cannot do use your eqs (3.25) - (3.28) (of the long paper) since these are only valid for $\psi(x)|0\rangle$ & not for the operator $\psi(x)$. Indeed it seems to me there is there (in the big paper) a certain confusion since W_0, W_{λ} etc are operators, whereas δy_{λ} is a c-number in the second quantized theory which we obviously want to use if we now speak of Lagrangian, of mass term $m \bar{\psi} \psi$ etc. In the second quantization formalism we cannot say that

$$W_2 = \frac{1}{4} \epsilon_{\lambda\mu\nu\rho} \sigma_{\mu\nu} \partial_{\rho}$$

but only

$$W_2 \psi(x)|0\rangle = \frac{1}{4} \epsilon_{\lambda\mu\nu\rho} \sigma_{\mu\nu} \partial_{\rho} \psi(x)|0\rangle$$

(Eq. following Eq. (3.19) of the big paper)

Now whereas on projecting onto $|0\rangle$ we ~~get~~ have

$$[W_{\lambda}, \psi(x)]|0\rangle = \frac{1}{4} \epsilon_{\lambda\mu\nu\rho} \sigma_{\mu\nu} \partial_{\rho} \psi(x)|0\rangle$$

The general eq. is

$$[W_{\lambda}, \psi(x)] = -\frac{i}{2} \epsilon_{\lambda\mu\nu\rho} [i \tau_{\mu\nu} (\partial_{\rho} \psi) + \gamma_{\rho} (\frac{1}{2} \sigma_{\mu\nu} + \epsilon_{\mu\nu}) \psi + \frac{i}{2} \sigma_{\mu\nu} \partial_{\rho} \psi]$$

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with $L_{\mu\nu} = -i(x_\mu \partial_\nu - x_\nu \partial_\mu)$

This eq. is considerably more difficult to handle & requires a detailed & long calculation which I thought could not be squeezed into a paragraph. Besides this, as I have said before I don't think we need this since $[x_i, P_0] = 0$ gives us all the necessary information.

2. I don't think the free Dirac field is too exciting: there is only one mass ^{& only one spin} & no serious problems arise. If we want to give an example we must consider the free massless case corresponding to the 35-dimensional representation. Here we have the interesting problem of showing that a free Lagrangian for particles with different spins can be written in a form invariant under SU_6 . This might be interesting & should be done explicitly. I doubt I can do it now: if you can do it, please

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publish it as soon as possible before

someone else does it.

Let me tell you my point of view on this subject.

Since the operators X_k are such that

$[X_k, P_0]$ there is no way of breaking this symmetry,

P_0 being the total energy including every possible interaction. I don't think therefore that the terms

like X_k you mention in your letter can possibly break SU_6 .

The δ problem is to see whether the F_A which generates SU_3 are true constants. According to Gell-Mann, P. L. II, 258 (1964) $[P_\mu, F_A] \neq 0$ if we assume that no two particle pair of particles exists with the same mass except for the particle-antiparticle pairs. I believe the argument is

correct. Presumably there fore no intrinsic symmetry is exact: does the breaking come from the free

or the interacting part? I suppose from the interaction. If $[P_\mu, F_A] \neq 0$ I suppose also

the operators Y_k which transform like

$X_k F_A$ (They are not however equal to $X_k F_A$; this is only true for the fundamental representation)

are not exact constant of the motion

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$[P_{\mu}, Y_{ab}] \neq 0$. I think this shows that
the breaking of SU_3 is probably of the same
order as the breaking of SU_2 since the
non commutativity of the Y_{ab} is due to
the F_4 + not ^{to} the X_4 which are exactly
conserved.

If what I said is correct then assuming
 $P_0^{(0)}$ which corresponds to the free Hamiltonian is
such that $P_0^{(1)} [P_0^{(1)}, F_4] = 0$, then I think
the interesting terms Y_{ab} , which alone ~~cause~~
the have matrix elements between states with
different spin, are probably constant too
 $[P_0^{(1)}, Y_{ab}] = 0$. The free meson Lagrangian L_0
should be tested experimentally.

However as soon as you let the mesons
interact with quarks (you 1/2 add) the
interaction will destroy SU_3 and the
the self energy of the mesons will cause
an addition S_{int} such that $[S_{int}, F_4] \neq 0$

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I must now stop: maybe I will write more later if I understand things better.

And now a few words about logistics.

Unfortunately it now appears I won't be able to come to Ankara in the near future. Indeed my presence is required in Pisa for several things to which I have to attend. Please do try to come to Pisa as soon as possible at least for a short time if you can't get away for too long. I will arrive in Pisa on Feb 1: come any time after that date. I cannot tell you the exact financial terms I can offer: I hope something better than the other time though I cannot guarantee I have been out of contact with the administration for a while. Certainly at least not less. Let me know if you would like the plane ticket to be sent in advance in which case I'll write to Pisa to have it done in time. I could perhaps come to Ankara later, ^{after} the middle of May for example, if you think there is still use for a discussion at that time.

I very much hope to see you soon. Till then, best regards to Sukra & love to Susu from Luca & Alessandro

Yours
Luigi



Hotel New Yorker

THIRTY-FOURTH STREET AT EIGHTH AVENUE • NEW YORK, NEW YORK 10001

January 27, 1965

Dear Fiza,

I have delayed writing because I was waiting for a final decision about our paper. I have now heard from the referee that I will advise not to publish it in its present form (the referee is Steve Weinberg.) I had numerous telephone conversations with him & finally a long discussion today: he thinks our group does not have the S-matrix invariant. The main argument is 'we all know that there are other invariances besides those we already know'. At this point

I don't really think there is much sense
in arguing. We have to sit down
together and write a longer paper
which all the proofs.

I had a long conversation with Louis
Mackel who, ~~was~~ on the hand, was very
impressed by the existence of our operators
X's. He will think it over & tomorrow
morning we will continue the discussion.

I will write you again tomorrow from
Brookhaven -

We leave for Pisa Sunday 31 by plane,
and will be home the following Monday.

Please arrange to come as soon as
possible. I wrote to Susola Nordin
to have a plane ticket sent to you
with an open date. As soon as I get back
I'll make sure it has been done. Before
leaving, that is Friday, I will send an official
invitation to you for Pisa



Hotel New Yorker

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Here everybody is absolutely crazy
about groups. The latest thing is
S.U.M.E. : it was introduced by Paul
today in his talk at the N.Y. meeting
and is apparently similar to what
Sabam did. No one really understands
what it is but nevertheless there are
mass-conferences etc.

I will write more tomorrow.

Yours

W. J.

W. J.

L. A. Radicati
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VIA AIR MAIL

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FGA-SCI-02-020-06-CR-08



SCUOLA NORMALE SUPERIORE

PIAZZA DEI CAVALIERI

PISA

CLASSE DI SCIENZE

Pisa, li Feb 6, 1965

Dear Feza,

thank you for your letter of Feb 1. Yesterday, I have sent you by special delivery the reprint of our letter: I had sent from Brookhaven a few copies to the people you told me & one to you which presumably got lost or will arrive later.

I do not take such a pessimistic view of the rejection of our paper as you do. Indeed the fact that Weinberg couldn't understand it & that Bob Serber who read it had great difficulties (as he told me) in following our arguments shows that the paper was not after all too clear.

I had also given a copy to Wick & I have not heard any comment from him: if he had been greatly impressed he would no doubt have told me. Finally Michel after carefully reading

$\frac{1}{\sqrt{p^2}}$

$$\frac{1}{m^2} (X_1, X_2) \frac{1}{m^2} X_3^2$$

~~roughly~~ The paper was much less enthusiastic.

We had a long discussion in New York at the end of which we agreed that the operators X_i are indeed very interesting objects but their interpretation as generators of a group is somewhat unclear. The algebra of the X_i 's is not a closed algebra in the abstract sense: indeed the commutator $[X_i, X_j]$ contains in the R.H.S. a term $\frac{P^2}{m^2}$ which only in subspace $\frac{P^2}{m^2} = m^2$ cancels the m^2 in the denominator.

I believe that in view of the many papers published on the relativistic definition of $SO(10)$ one should not write one more letter but a complete paper which is understandable to every physicist. I don't care to compete with Pais & Salamee & I don't like to be lumped together with them as a bad writer or a confused thinker. This is the reason why I am not too distressed by the rejection of our letter.

What should we do next? I think simply work quietly & try to understand things completely. That is why I am looking forward to talk to you in Pisa & I hope this can be realized as soon as possible & I believe something will come out of it.

SCUOLA NORMALE SUPERIORE

PIAZZA DEI CAVALIERI

PISA

CLASSE DI SCIENZE

Pisa, li

As I mentioned before, Masket was very impressed by our operators Xi. He told me that he had been playing before with the W_{2m} but he had never had the idea of using $W_{2m} + \tilde{W}_{2m}$. He said he will think about this problem & will let me know his results, and his point of view. He also said he would like to talk to you & I therefore invited him to visit Pisa ^{for a few days} when you are here. I would however like to stress that he made quite clear he didn't want to intrude in our work as other people did & that his desire to discuss these problems was absolutely disinterested; his attitude ^{was} really friendly & gentlemanlike. I have insisted so much on these points because of our past sad experiences. I hope that my invitation to come to Pisa (to Masket, I mean) will not displease you: if you don't like the idea

shall tell me frankly & will take upon myself
to postpone his visit.

I must stop now to wait this letter in time -
I'm waiting for your arrival on the 10-th :
Please cable the exact day & hour so I can come
& fetch you at the airport (by the way: the
ticket has been sent to you through ALITALIA;
you should perhaps check with them to make
sure they received it. Finally your grant will
be Lit. 450,000 per month or a corresponding fraction
(greater or smaller than depending on your stay).

Regards to Susha & all yours

Yours
Luigi

Boğaziçi Üniversitesi

Arşiv ve Dokümantasyon Merkezi

Kişisel Arşivlerle İstanbul'da Bilim, Kültür ve Eğitim Tarihi

Feza Gürsey Arşivi



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