Dear Feza

(a) In writing εS_1, εS_2, εS_3

$$\varepsilon^2 = \varepsilon$$

note that $\varepsilon = \frac{1}{3} - \frac{2F_8}{\sqrt{3}}$

(as $F_8 = \frac{1}{2\sqrt{3}}\begin{pmatrix} 1 & & \\ & 1 & \\ & & -2 \end{pmatrix}$)

like $F_1 = \frac{1}{2}\begin{pmatrix} 0 & 1 & 0 \\ 1 & 0 & 0 \\ 0 & 0 & 0 \end{pmatrix}$

(b) I have the following suspicion. If one treats the tetrads properly, it will turn out that the coupling $\overline{B}\,B\,M$ which is in our draft actually contains the induced terms. I believe this because after having taken out the "center of mass motion" of the bi-tetrad, what remains is, in the low energy limit, a usual partial wave expansion. Take for example the vector meson coupling. The leading

Term in the bi-tetrad ⇒ the S-wave interaction.

The next term gives the Baryn $\bar{\text{Baryn}}$ pair in a ϕ-state ; interacting with a vector field this must be $g_{\mu\nu} g_V$ (for the $\bar{b}bV$)

↕ related to g_A

Likewise for a pscalar field a $\bar{b}b$ pair gives now the $\gamma_5 \phi_\mu$ term - related to g_V

I do not yet swear that this is so — but it seems plausible that it could be true. Then we are even more unique.

Bram

The higher partial waves are not determined due to the formfactor.

THE ROCKEFELLER INSTITUTE

A Graduate University and Research Center

NEW YORK 21, NEW YORK

Dec 4.

Dear Feza

Enclosed a little note on semi-leptonic interactions. I want to ask your guidance in regard to footnote 18 of this note, in preparation of which I sent you some days ago a copy of the Letter by Feynman et al.

It seems proper and fitting to use this occasion to say something about our own results on $U(6) \times U(6)$.

I want to ask you this. If you object in any way, please send a cable and I will amend, modify or delete in accordance with your wishes. If I do not hear from you I'll assume that you do not object.

Apart from this technical point, do let me hear from you. It's been a long time.

Many people are joining the game.* I just saw a preprint by Dyson and Thring on 2 nucleon systems in SU(6). I hope all this makes you content!

I have talked with Luigi by phone about this footnote and he does not object. I hope to see him and Jamna in a short time from now here in New York. He sends his warm wishes.

Come if you can!

All best wishes also from Bagi & Nick for you and Suka. as ever

Bram

* In fact, the only people who have not thrown their hat in the ring are Lee & Yang.

THE ROCKEFELLER UNIVERSITY
New York, N.Y. 10021

April 15

Dear Feza

At long last we have a new president at the university here, namely Seitz. This was announced a week ago. This has not yet solved some practical problems I want to settle, but the situation is much clearer now. These preoccupations have made me negligent in replying to you. For this, my apologies.

In addition, I had been waiting for further word on the Spitsbergen fields. Furthermore, a change in my own plans was necessary because it is indicated that I should take Josh to Holland to see my mother. This would do in the middle of May, coupled to the meeting at Göteborg which I plan to attend while Josh stays in Holland those days.

For all these combined reasons I want to ask you if I may still come to Ankara, but if it may be postponed just a bit. I also enclose a formal letter, just in case that may be of help to you.

From the list of participants at Göteborg I see

that you should be there too. I hope you'll actually go. That might give us a first opportunity to talk together, both about physics and about plans!

Meanwhile, I have busied myself with some work on dynamical asymmetries. I have found some model equations which have asymmetric solutions such that large SU(3) breaking as well as the formal equivalent of a Cabbibo angle appear as properties of these solutions, with an input that is SU(3) symmetric apart from electromagnetic and weak interactions.

Once again, I hope that my silence has not inconvenienced you. Till soon. All my best to Suha, Yousouf and you—

Brau.

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Meyrin-Genève, le 20 August

Dear Feza

I hope you have meanwhile received my letter and preprint. I am asking Prentki to take along the present letter which contains some amplifications.

I am now engaged to study the questions: 1) if you require parity clash to be the reason for $\Sigma^{\pm} \to n\pi^{\pm}$ being nearly P-pure is the coupling scheme described in my paper unique? 2) Can one weaken the conditions under which parity clash occurs so that the concept has stronger dynamical validity?

In this letter I state preliminary results on this questions. If you or someone else is going to talk about this work I would be grateful if these remarks could get a brief mention.

(1) Uniqueness. At present it appears that the following is true.

If the S-conserving currents are all isotopic vectors then the (J_S, J_t) scheme is unique. It is then unavoidable that the S-violating currents contain $\Delta T = 3/2$ terms.

In addition there is one further case to be considered and which goes beyond my paper.

If the S-conserving currents are allowed to be either isotopic vectors or isotopic scalars there is one further possibility namely the coupling J_S (Treiman interaction) + a coupling $\rho(S_0 + \bar{S}_0)$ where $S_0, \bar{S}_0$ are as in my paper and ρ is the isotopic scalar $\rho = \sum_{i=1}^{Y} \bar{N}_i N_i$. This case can be also dealed as follows.

All S-violating currents are isotopic spinors. They are coupled to S-conserving currents which are: one isotopic vector, one isotopic scalar.

This second case can be treated on the same lines as in my paper. The consequences are all the same except on one point. If one assumes that the same S-violating currents intervene in both leptonic and non-leptonic decays then the $\Delta T = 1/2$ rule is valid for all weak decays.

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Meyrin-Genève, le

In this case the S-violating currents are ~~are~~ therefore "pure" (in the absence of K coupling) both with respect to isotopic spin and with respect to space reflections.

In addition it is true that if the S-violating currents are of the V-type, then all such S-violating currents are conserved in the global symmetry approximation. The proof is trivial. Note that if the isotopic scalar current is also of the V-type*, then it is conserved in the presence of all interactions, as it is nothing but the baryon current.

(2) Weakening of conditions. I have already shown the following (~~the~~ true for either (J^S, J^t) or $(J^S, S(\bar{S}^t + S^0))$).

The parity conservation in

$$\Sigma^- \to n + \pi^-$$

is true even in the doublet approximation. Thus here the global symmetry is sufficient but not necessary.

---

* Which implies that $\Sigma^+ \to n + \pi^+$ is a P-wave reaction.

I am now studying $\Sigma^+ \to n\pi^+$ which, it appears, needs a more delicate analysis.

Finally the following. As I have tried to emphasize in my papers I do not know, obviously, if the "P-conservation" in the two Σ channels is accidental or not. I have rather asked, suppose it is not accidental how do you go about things. Clearly in this last case there is no other way of discussing than P-dash. As to which of the two solutions to P-dash mentioned in this letter could possibly deserve preference — the answer to this is experimentally flexible. Clearly it is once again a question of the hyperon β-decays.

I am most interested to hear your opinion on all this. Also I want to know what Gary says. Steinberger and I have discussed at length the Barshay-Schwartz model. We do not think it is very convincing as there is a hidden assumption about equality of S-wave and P-wave absorption. So einfach wie diese Herren glauben ist das leben nun einmal nicht.

Cordially

Bruce

Copy

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Meyrin-Genève, le

July 7

Dear Feza

About your recent paper
(Phys (8.7). I understand that
$J_{3/2}$ is "pure" (either V or A). But what
gives you the right to couple $J_{3/2}$ to J_1^A
only? Why not $J_{3/2}(J^A + J^V)$ and
if this is correct then what about
"Penguralon" is $\Sigma_+ \to n\pi_+$?
It is true that $J_{3/2} J^V \to$ 0 in lowest
order in strong interactions, but
what is not correct is that it
should also vanish in higher orders.

I am very confused Please
reply promptly

Cordially

Bram

Dear Foza ①

Hurrah - everything is clear.

The theory has three stages

(a) Neglect all meson masses. Then

a1 The V-current is conserved and unrenormalized

a2 The A-current is conserved and unrenormalized

(b) Turn on the vector meson masses alone (This is the right thing on physical grounds) Then SU_6 breaks down and

b1 The V-current is still conserved and still unrenormalized

b2 The A current is still conserved but is renormalized

(c) Turn the ps masses on. Then SU_6 breaks down and

as

C1. The V current is still conserved and still unrenormalized

C2. The A current is no longer conserved and gets other renormalizations

Stage (a) is a triumph because the baryon mass is still in. Now my mind is at peace.

Bram

BNL. Aug. 30

Dear Fegs

I hope that the three of ya have had a good crossing and also that the sea has been sufficiently serene so that you could recuperate from the madhouse of Brookhaven National Labratory. I must admit that I have envied you this trip, especially because we have again had some very hot and very humid days, just like the one on the Saturday of your return from Princeton. Even if I had been less tired than I am, it was not a period in which much thinking or work could be done.

I owe Youssouf a letter about TV programs. Please ask him to keep with me. The only thing I have watched was the acceptance by Johnson & Humphrey and that is not too interesting. (In an immoral move the NY Times declared itself next morning for the democratic ticket, in strong language).

There have been many discussions about our cosmological escapade. There general sense is of interest and — of course — of a wonderment what this is all about. But people listen and want to know. It seems a rather general opinion that the ultimate explanation of the effect cannot be trivial and ~~that~~ there is more reserve these days about plumbers jobs like $\Delta S = -\Delta Q$ and $\Delta T = 3/2$ than about frank speculation. This is, a propos, also the

Thinking progressing? Another piece of news will amuse you. Beg has of course the right central masses. With their help he finds $\frac{g_{PS}^2/4\pi = 13.}{}$. This value 1115 for f_{00} was not correct. Actually both Γ_{00} and μ_{00} are lower than what we fixed: 960 and 600. As a result the Γ_{33} also tends to a little value than we had. All good wishes to you three. Yours

Attitude of Frank. He and TT have written a very nice and useful paper about the various tests one wishes to make in consequence of the 2π-effect.

The hectic last 24 hrs of your stay made me realize insufficiently (at the time) that TD + Jeremy + Cathleen ≡ Bell. In particular, Bell also mentions the possibility of a small mass. On line 7, p. 1 of our note the word "massless" has been replaced by "new" in the copies I have since had produced.

I also enclose a copy of my "covering letter" to T.D.

I had some good talks with Gary who is quite interested. He has some ideas about screening which takes place in the vector model, due to the small but $\neq 0$ mass. We have also discussed the Γ-model and I have told him of attempts to couple a "current" to Γ_μ. We will discuss some more in the near future and I'll let you know the outcome, if at all interesting. By the way, it is believed at the lab that it will be known within a few months whether the main prediction of the vector model (Rate $\propto E_{lab}^2$) is true or not. — As even my stamina has its limits, I am sticking to my plan of July, not to go to Pitt. I shall go to the Düsseldorf meeting however and plan to give a talk there devoted entirely to the Fitch effect and its ramifications. It is very possible for me to make a visit to Ankara with this trip and I wonder what you think of that. It would not be before the last week in September and by that time I will be rested and will have thought a little more. For myself at least it would be a pleasure provided it would be no burden on anyone's time. (Financially there is no problem). Do let me know what you think of that (to Rat. Inst.).

I am of course eager for reactions to $\$16$ and to the 2π-problem. How is your own

P.S. Enrico has withdrawn in his enthusiastic attitude. As always, we have pleasure discussing things but not much has transpired. Jan leaves BNL in a few days and finally left to have a bit of rest. York is here for a few days which makes me happy. But BNL is no longer the shining

after the magic beyond

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Meyrin-Genève, le July 7.

Dear Fesa

I have trouble to understand one point in your recent paper. It concerns Eqn. (8.7). I understand your argument for saying that $J_{3/2}$ is "pure" (either V or A). Now however you couple this $J_{3/2}$ to J^A ($\Delta S=0$, axial current). What reason do you have for that? You must admit $J_{3/2}(J^A+J^V)$ mustn't you? And would it not be that, while the J^V term → nothing in lowest order in the strong interactions, it gives non-zero contributions in the higher orders? So would your

argument about $\Sigma_+ \to n\pi_+$ Correct
P incorrect?

I am very confused about this and
hope you will enlighten me as soon
as possible.

Best regards

Bram