~~Bring Fermi + Anderson paper~~ — ~~Feldman~~

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Dear Fermi,

Being thousands of miles away I have only heard by amateur radio from friends in the U.S. that you are doing experiments in meson scattering from protons. I don't know what your theoretical friends are saying, so I should like to make some comments at the risk of only saying what is obvious to everybody in the U.S.

To begin with I am of the opinion that Yukawa's meson theory with pseudoscalar mesons gradient coupling, is wrong, (or at least useless) in its present form--because at least perturbation theory is N.G. and otherwise divergences cloud the issue. But I think mesons are pseudoscalar, and I think the amplitude* that a nucleon emits just one may be proportional to $\sigma\cdot Q$ (where σ is the nuclear spin, Q the meson momentum) for Q small. (This is of course agreement with the Yukawa theory--to all orders in account, because for low Q one operator in the series $H_{fi} + \frac{H_{f\mu}H_{\mu i}}{E_i - E} + \frac{HHH}{E\,.\,.}$ etc is proportional to Q and the others, involving all the virtual mesons are not (the virtual moments are of order μ, the meson mass) so for Q low enough the sum will be proportional to Q, and further will be Q·times the sum with the σ operator in place of one of the H's--which means Q·times a spin 1/2 object which can only therefore be proportional to σ). Let us say then the coupling is $1/\mu\ G(Q)(\sigma\cdot Q)u$ for emission of one meson amplitude u, momentum Q, mass μ where G(Q) is a function of Q (and possibly the nucleon moments at higher Q??) and I expect G to have the properties of not varying much for Q small, just is a reasonable function of Q/μ. For Q = 0, call $G(0) = G_0$ (If pert theory were OK G_0 is just the usual g). Further this is most reasonable on nearly any theory--for the meson being pseudoscalar the coef to emit one (even if proton is a positron + 18 neutrinos + 4 neutral mesons) must be ps.scalar--which, if it doesn't involve the nucleon momenta (and I can't see how it easily can be galilean invariant--but Nature's imagination always has my respect) can hardly be other than $\sigma\cdot Q$. (According to Yukawa theory, standard form, the total series would give a G(Q) which, if g^2 were very small and

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* I make all analyses thinking of the theory non-relativistic in the <u>nucleons</u>.

integrals converged, would be nearly constant for all Q equal to g-- but if g^2 is larger, correction terms set in for Q of order μ).

I wish to appeal to experiment to try to establish, if possible, whether the above is correct and the coupling is like $\sigma \cdot Q$ for one meson absorption. You see tho I mean only to refer to low energy mesons--for $Q \sim \mu$ or higher I have no arguments about what to expect.

Yet it is impossible to measure the absorption of one meson by nucleon directly for the conservation of energy demands that another coupling enter to take out the energy. If we do it with a γ-ray, or a collision between nucleons new uncertainties arise, but if we do it by means of another meson (scattering) the situation would appear to be as simple as possible.

The "intermediate states" (if they mean anything) have, maybe, energy of order μ $\sim$ so that as long as Q remains small enough (non-rel. mesons) the intermediate states do not depend much on Q. Then, if we assume the coupling for two mesons is essentially like the double action of the 1st order coupling, we see that the matrix element for scattering ought to be proportional* bilinearly to Q_1 and Q_2. It must therefore have the form

$$M = X_1 Q_1 \cdot Q_2 + X_2 \, i\sigma \cdot (Q_1 x Q_2)$$

or if Q_1, Q_2 lie in x, y plane at angle θ one to other using c.g. system $Q_1 = Q_2 = Q$

$$M = Q^2 \, (x_1 \cos\theta + i\sigma_z \sin\theta \, X_2)$$

where X_1, X_2 are some functions of Q, insensitive to Q for small Q. But in principle knowledge of the coupling of one meson does not determine that for two. There could still be a term with arbitrary coefficient in the Hamiltonian of form $u_1 \, u_2$ which is scalar. Hence we might expect

$$M = Q^2 \, (X_1 \cos\theta \; i\sigma \sin\theta \, X_2) + X_3 \tag{0}$$

(For example, gradient and direct coupling theories agree on $\sigma \cdot Q$ for one meson,

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*Because, if you like, now in therpert series one of the H is prop. Q_1, other to Q_2 and otherwise nothing is sensitive to the value of Q_1, Q_2.

but for two X_3 is very different being very small for grad. and very large for direct-in pert. theory).

Naturally such a form is completely general--but what I want to verify is that

(1) X_3 is very small (maybe order μ^3/M smaller than X_1, X_2)
(could in principle depend on spin--I will assume it doesn't)

(2) X_1, X_2 are insensitive to Q^2 for Q^2 well below μ^2.

I am not in position to calculate X_1, X_2 in terms of G, nor to get a relation between them--for we have no good theory. (One possibility of course is that relations of the 1st order pert theory may be true, but let us first find out if (1), (2) are true and that being established go on from there.)

Comments: (1) is a pure guess--various evidence (such as γ emission competing favorably with π^0 emission in H capturing π^-) indicates it is so--all the evidence which is usually aduced to prefer the grad. to direct coupling is just a question of how big X_3 is. I assume for no excellent reason that X_3 does not depend on spins.

(2) could be wrong. It would be very interesting. For it probably would mean there exist important "intermediate states" at low (rel. to μ) energy--which would be a vital discovery. Hence I urge you to try to see whether the predictions of (1), (2) are satisfied.

Incidentally since M for the inverse reaction should be the complex conjugate I conclude all X's are real (but I am notoriously punk at such arguments--get a field theory or group theory expert).

Next, very interesting is the relation of the X's for different reactions (I mean mesons of different charges, neutral etc.). It would be very interesting if we could verify that the symmetric theory is valid. Let us look at the predictions of this theory for this problem and test it later experimentally. If $\vec{u}$, $\vec{v}$ are the vectors in isotropic spin space representing the mesons in and out, and τ is the operator for the nucleon M must be bilinear in u, and v and invariant in isotropic spin, or of the form

$$M = A(\vec{u}\cdot\vec{v}) + B\, i\, \vec{\tau}\cdot(\vec{u} \times \vec{v}) \qquad (3)$$

where A,B are matrices involving spin etc. (Which we later write in the form

$$A = A_1 + i\sigma A_2, \quad B = B_1 + i\sigma\ B_2 \qquad (4),(5)$$

and we expect nearly to write

$A_1 = Q^2 X_1 \cos\theta + X_3, \quad A_2 = Q^2 X_2 \sin\theta$

$B_1 = Q^2 Y_1 \cos\theta + Y_3, \quad B_2 = Q^2 Y_2 \sin\theta$

X_3, Y_3 small.
X,Y nearly constant
small Q^2. All real?

but form (3) does not depend on assumptions (3)(5) of course, just invariance.)

That is, getting down to cases, the matrix element for each process is given in the following table. Processes labeled with the same "TYPE" letter have equal probabilities—as would be expected from either reaction = inverse or the most naive use of the charge symmetry idea: π^+ is to p as π^- is to n and π^0 is impartial.

Now let us look at the X-sect for various cases. In complete generality A can be written in the form $A = A_1 + i\sigma/A_2$ where A_1 is scalar $/A_2$ is 3 quantities (complex)(vector) and $B = B_1 + i\sigma/B_2$. Summing over all spin directions of the nucleon then we obtain that the cross section is proportional in each case respectively to:

PROCESS	ELEMENT	TYPE
$\pi^+ + p \longrightarrow \pi^+ + p$	$A + B$	(a)
$\pi^0 + p \longrightarrow \pi^+ + n$	$-\sqrt{2}\,B$	(b)
$\longrightarrow \pi^0 + p$	A	(c)
$\pi^- + p \longrightarrow \pi^0 + n$	$+\sqrt{2}\,B$	(b)
$\longrightarrow \pi^- + p$	$A - B$	(d)
$\pi^+ + n \longrightarrow \pi^+ + n$	$A - B$	(d)
$\longrightarrow \pi^0 + p$	$-\sqrt{2}\,B$	(b)
$\pi^0 + n \longrightarrow \pi^0 + n$	A	(c)
$\longrightarrow \pi^- + p$	$+\sqrt{2}\,B$	(b)
$\pi^- + n \longrightarrow \pi^- + n$	$A + B$	(a)

(a) $|A_1 + B_1|^2 + |/A_2 + /B_2|^2$

(b) $2(|B_1|^2 + |/B_2|^2)$

(c) $|A_1|^2 + |/A_1|^2$

(d) $|A_1 - B_1|^2 + |/A_2 - B_2|^2$

(where $|A|^2$ means A^*A; $|/A|^2 = /A^* \cdot /A$). Hence the symmetric theory predicts $\sigma_a + \sigma_b = 2\sigma_c + \sigma_b$ which would be a wonderful thing to verify for it does involve the idea that neutral mesons have $1/\sqrt{2}$ times the coupling of charged. However, unfortunately σ_c is unmeasurable experimentally. (If someday we know sym. theory is OK we can use this to get σ_c which somebody might want to interpret π^0 production and subsequent escape in heavy nuclei.) So so far no test of subtle parts of sym. theory.

But now let us substitute (4),(5). I call X_3 zero for simplicity —you can put it in and see effects. Assume X,Y real—I hope it's true.

$$(a) = Q^4 \left[\cos^2\theta(X_1+Y_1)^2 + \sin^2\theta\,(X_2+Y_2)^2\right]$$
$$(b) = Q^4 \left[\cos^2\theta(2Y_1)^2 + \sin^2\theta\,(2Y_2)^2\right]$$
$$(d) = Q^4 \left[\cos^2\theta(X_1-Y_1)^2 + \sin^2\theta(X_2-Y_2)^2\right]$$

Hence (A) Cross sections should go as Q^4 (up until $Q^2 \approx \mu^2$)

(B) in angle should be of form $a + b\cos^2\theta$, or say $\alpha\cos^2\theta + \beta\sin^2\theta$. Effect of X_3 will be seen as a small residual constant x-sect. as σ vs. Q^4 is extrapolated to zero,--or more sensitively (?) a term in cos θ in the angular distribution (lack of front-back symmetry e.g.) for low Q.

(C) Symmetric theory predicts for α and for β; or for $\sigma_{(90°)}$ and for $\sigma_{(0)}$ one of the relations

$$\sqrt{(a)} - \sqrt{(d)} = \sqrt{(b)}$$
or
$$\sqrt{(a)} + \sqrt{(d)} = \sqrt{(b)}$$
or
$$\sqrt{(d)} - \sqrt{(a)} = \sqrt{(b)}$$

(these are not valid if my argument X,Y real is faulty)

which may serve as a test of that theory.

Could you tell me to what extent these foredictions (A),(B),(C) are verified by experiments? May I urge the importance of low energy meson experiments in establishing beyond doubt (if they agree) some of our basic premises today? Higher energy are interesting but in our ignorance we do not know how to interpret them--so it is well to study low energy as well. In particular there is hope to check the $\sqrt{2}$ of the symmetric theory with low energy data.

Sincerely,
/s/ Dick Feynman

P.S. I have already heard that x-sect rises rapidly with energy--stops rising about $Q = \mu$. so I am not entirely in the dark in Brazil.

P.P.S. Between us theorists (I imagined you as an experimenter above--hence the low remark about seeing a field theory expert to see if X_1, X_2 must be real) I'd like to make some remarks. I think now non-relativistically about nucleons, so errors of order $(P_{nuc}/\text{Mass proton})^2$ (c=1) can come in.

A coupling of one meson $\sigma\cdot Q$ is not Galilean invariant, for at additional velocity V, $Q' = Q + \omega V$ where ω = frequ. of meson. But nucleon changes mom by MV hence the Galilean invariant coupling must be (error now order V^2/c^2, not V/c), i.e. ($Q - \frac{\omega}{M}/P$ is invariant)

$$\sigma\cdot\left(\Delta u + \frac{1}{2M}\left(/P\,\underset{(b)}{\frac{\partial u}{\partial t}} + \frac{\partial u}{\partial t}\,/P\right)\right) \tag{6}$$

where P is the operator nucleon (b) momentum. Maybe you should use this in the x-sect analysis but it only makes factors of $1 + \omega/2M$ or $1 + \mu/2M$ to the accuracy we expect, so is just an unknown constant anyway.

The ps. theory grad coupling agrees, making for the non-rel. hamiltonian approx.

$$H = \frac{/P\cdot/P}{2M} + \underset{(b)}{\frac{1}{2M}\left((\sigma\cdot/P)\,\frac{g}{\mu}\frac{\partial u}{\partial t} + \frac{g}{\mu}\frac{\partial u}{\partial t}\,\sigma\cdot/P\right)} + \underset{(c)}{\frac{g^2}{\mu^2\cdot 2M}\left(\frac{\partial u}{\partial t}\right)^2} + \underset{(a)}{\frac{g}{\mu}\,\sigma\cdot\nabla u}$$

Now I argued above for the term (a) with a g renormalized to G as effect for absorption of 1 meson. Hence the galilean argument shows the g in (b) is the same as that in (a). Now the (c) looks like the type of $e^2/A\cdot/A$ term that comes in electrodynamics from $(/P - /A\,\frac{e}{c}/A)^2$. In that case renormalizing the charge must change the e in $e(/P\cdot/A + /A\cdot/P)$ and in $e^2/A\cdot/A$ by the same amount—by an argument of gauge invariance. Now is there some reason for a particular size (c)? Or, is there some principle which shows the renormalized g in (a) and (c) must be equal? Does anyone in U.S. know about this? It is very interesting because (c) of course is the origin of the X_3 term—so if X_3 is known in size it may tell us something.

Also when electric potential is present the $\sigma\cdot\Delta u$ gets another $\sigma\cdot/Au$. So one way G_0 might be got is from the cross sect for $\pi^- + p \longrightarrow p + \gamma$ capture from π^- at rest. We know this competes successfully with $\pi^- + p \longrightarrow p + \pi^0$ and the latter absolute x-sect can be got by extrapolating scattering cross sections down. (The latter is small either because it uses the Y_3 term, or if this is zero (as in 1st order pert. theory) by $\sigma\cdot Q$ of the out π^0 followed by the (b) term for the in π^-). Can we argue that the γ emission comes just from $(\sigma\cdot/A)u$? I think yes. If you imagine the pert. series again and try to get π^- in by $\sigma\cdot$ Q it can only go by (b) term and hence is so small that π^0 could compete. Hence π^- must go in via $G(\sigma\cdot/A)u$. Next should the G be G(0)? I am not clear on this. Probably not, for if the nucleon has a structure it would depend on the γ ray wave length --or otherwise put, in principle we cannot exclude additional terms of the kind $(\sigma\cdot(/A\ x/K))u$ etc. Any way it may be interesting when enough data is available to put in numbers and see how comparable are the G's obtained from X_3, from this reaction, from an attempt to get X_1, X_2 . . .from pert. theory, etc. Also interesting is to see if any electromagnetic properties can be got from the scattering based on the principle that any function of momentum for charged particles goes to $Q = e/c\ A$.

P^3S. I'm sorry to have to write by hand but secy's here have language trouble, and are slow, and are now on X-mas vacation, and I've delayed too long. If anything herein looks interesting enough to tell any other meson labs please tell them, I am not writing this to anyone else. (If you make copies please send me one.)

P^4S. Leite Lopes and I finished that test of the Yuk. theory potential I said I might try. The idea was take 1st order in g^2 potential from ps. grad. sym. meson theory. Assume OK for large r but not for small. $\therefore$ Integrate from outside in, but don't assume $r\Psi$ goes exactly to 0 at origin (because Ψ there is wrong). Starting with singlet, scattering length and effective range determine $g^2 = .18\ hc$. $r\Psi \longrightarrow 0$ at $-0.1/\mu$. But using these for triplet entirely too much D state results and no accord is got to experiment no matter what phase is chosen at origin. It is so bad that we can say the potential must be wrong by its own order of magnitude even as far out as at $\mu r = 0.7$. The next order (g^4) potential of Yuk theory makes changes of 200% at $\mu r = 1.0$, even for g^2 as large as 0.2 (the coefficients in the series are so large!), in directions which do not seem right to straighten things out. Hence we have no idea of what the potential should be even if the meson theory were OK.--perturbation expansions are inconsistent for such large g^2, and even if all is dropped arbitrarily but the first term the potential disagrees experiment. The terms (b) in (6) produce in 4th order (g^4) a quite strong spin orbit force between nucleons and a closed shell as well. It seems to be of the right

$\therefore$ Don't believe any calculation in meson theory which uses a Feynman diagram!

Eg. perturbation values of X,Y are $X_1 = 0$, $X_2 = +1$, $X_3 = u^3/2M$) Simple but
$Y_1 = -1$, $Y_2 = 0$, $Y_3 = 0$) false. How false??

sign and order of magnitude (it actually is too big, but . . .) for Mayer (order $\frac{PNUC}{M}$ main force). I am writing all particulars to Bethe, in detail. It is hard to believe in anything from the ps. theory because the perturbations are inconsistent. I have tried for 6 months and 100 closely written pages of formulas to work out intermediate coupling problems. I think I could succeed but the grad. meson theory diverges everywhere so I am disheartened to pick out any false model (without divergencies) and push it through, because it's so much work. I think I could do any special problem which didn't have divergencies (e.g. a cut-off theory) but I don't want to waste my time.

So I am, with this letter to you and one to Bethe, giving up Yuk. idea 1934 and am going to the Copacabana beach to see if I can get one of my own. I get lots of ideas at the beach.

Merry X-mas.