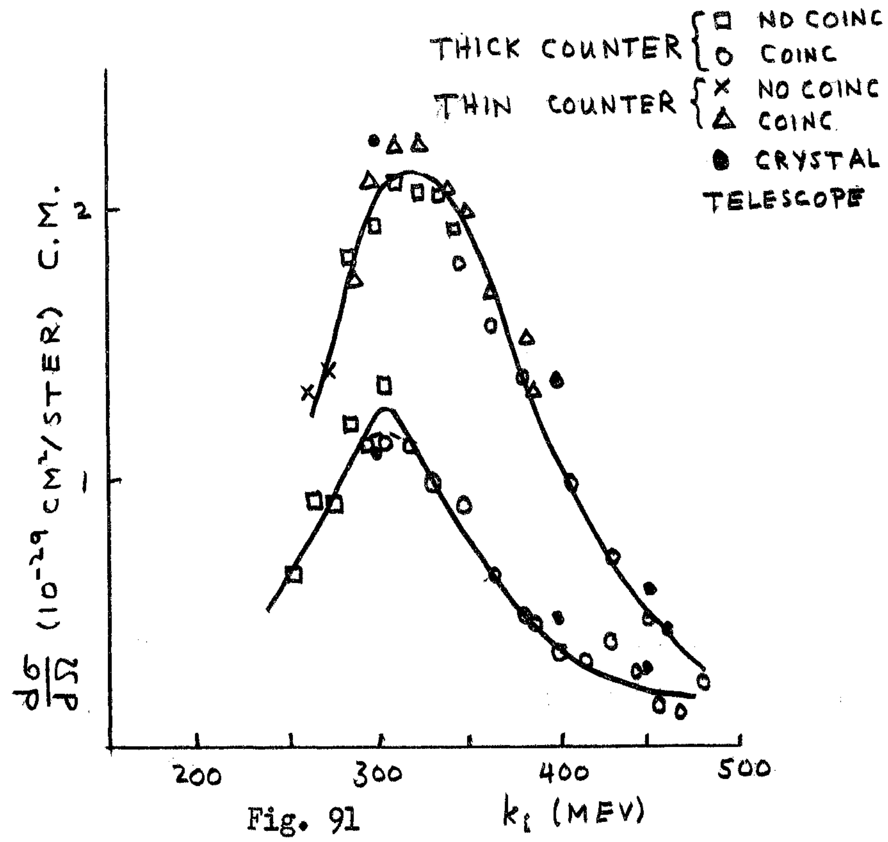


Fig. 91

in coincidence with one gamma ray, so that the good agreement between these and the other points indicate these results probably do have have something to do with π^0 production. (End of pion-scattering session.)

ANALYSIS OF PION SCATTERING: MULTIPLE MESON PRODUCTION

Wednesday morning, Professor Hans A. Bethe presiding.

Bethe opened the session by discussing the phase shift analysis of the pion scattering experiments. He first reminded the audience of the analysis made in the summer of 1953 by Fermi and Metropolis on the Los Alamos Maniac. The characteristic features of this analysis are indicated in Fig. 92. It

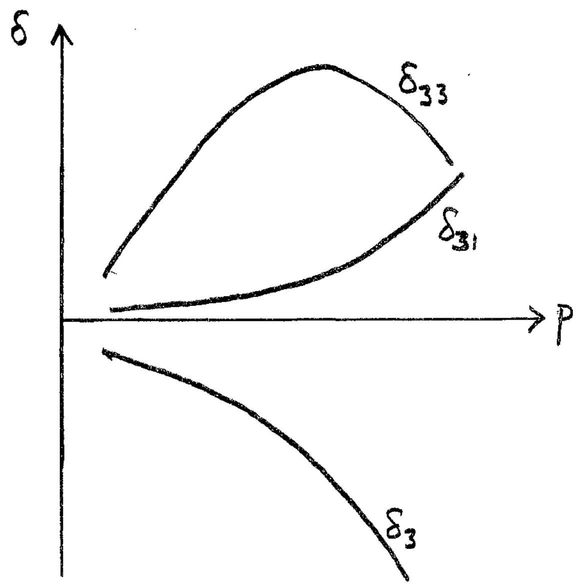


Fig. 92

seemed to Bethe that it was not likely for the phase shifts to go this way. His reasons for this feeling are (1) even in the summer the total cross section showed a maximum which looked very much like a resonance. (2) The photomeson production experiments at Cal. Tech. showed a sharp maximum of total cross section and a reversal of sign of the coefficient of cos θ at high energy. This is most easily explained if the $P_{3/2}$ phase shift goes through 90°. (3) Bethe doesn't believe the S phase shift can suddenly decrease sharply with energy. He believes there is enough truth in the theoretical assumptions to say that the S wave interaction is mainly (not necessarily exclusively) a repulsive potential of small range. If this is so, then the phase shift can at most be proportional to the momentum. (4) The $P_{1/2}$ phase shift was found to be small in all experiments at moderate energies. Again the Tamm-Dancoff approximation or other approximations to the theory indicate that this should be so. It is therefore unlikely that the P phase shift would suddenly become large at higher energy. Bethe therefore raised the question whether there could be another set of phase shifts which would satisfy more readily the requirements which theoretical intuition would like to put upon them.

The first indication that this might be the case was the experiment of Shutt, Fowler, Thorndike, and Whittemore at what was then called 260 Mev and is now believed to be 225 Mev. The angular distribution they obtained was very close to $1 + 3\cos^2\theta$ which is what one would expect from a $P_{3/2}$ state. Therefore, it is most natural to analyze the data mainly in terms of a $P_{3/2}$ state. Bethe's remarks set a few other people thinking, including Martin at Cornell and Glicksman at Chicago. Glicksman was the first to obtain a satisfactory set of phase shifts to meet the theoretical conditions. The question then arises to what extent this set is (a) unique, (b) compatible with the experiments, (c) what new experiments could be made to decide between this and other sets and (d) how generally can one obtain relations between different solutions to the phase shift problem. This was studied particularly by Metropolis and de Hoffmann at Los Alamos. By analyzing the π^- angular distribution obtained at Chicago at 217 Mev, they were able to get at least 6 solutions. For 4 of these, the predictions for the π^+ scattering are given in the following table.

TRACK	A_+	B_+	217 Mev C_+	$\sigma_T(\pi^+)$
I	.86	.96	3.46	160 mb
II	.98	.92	3.35	167 mb
III	.85	.93	3.51	161 mb
IV	.47	1.07	3.52	131 mb

It is seen that all four of the solutions listed give approximately the same total cross section for π^+ scattering at this energy and approximately the same angular distribution. At least two other solutions exist, but these have to be discarded since they give a considerably lower total π^+ cross section. On the basis of the recent measurements at Carnegie Tech., even solution IV should be discarded. These solutions were then followed to lower energy by an extrapolation procedure with the following results. The first solution gave

Track I

ENERGY (Mev)	α_3	α_1	α_{33}	α_{31}	α_{13}	α_{11}
217	-20	-4	107	-14	7	-7
194	-13	-14	90	-16	0	5
169	-4	7	64	3	-1	7
144	-13	14	46	5	3	-5
120	-12	8	30	6	2	-4

It is seen that this is essentially the solution which Glicksman obtained. There is a resonance at 194 Mev. α_3 behaves fairly regularly with energy and is small everywhere. It is extremely sensitive to the input data so the -4° at 169 Mev should not be worried about. α_1 probably does go negative as indicated at high energy. It certainly does not increase with energy. The indication that α_{31} also starts to go negative is probably correct. As Fermi had already noted at lower energies, α_{13} and α_{11} can vary all over the map and the numbers given here should not be taken seriously. Glicksman's solution was obtained by taking all three P shifts other than α_{33} as zero. The second solution is the one which goes over to the Yang solution at low energies and the energy variation of the phase shifts is given in the following table.

Track II

ENERGY (Mev)	α_3	α_1	α_{33}	α_{31}	α_{13}	α_{11}
217	21	2	50	117	1	-1
194	14	15	60	133	-4	-2
169	-4	7	32	96	0	6
144	-12	12	21	60	-6	9
120	12	-8	-14	143	3	-4

Note that both α_{33} and α_{31} go through a maximum at 194 Mev. In fact the maximum looks rather like a cusp which is something phase shifts should not have. The third solution is essentially the Fermi-Metropolis solution No. 1 and is given in the next table.

Track III

ENERGY (Mev)	α_3	α_1	α_{33}	α_{31}	α_{13}	α_1
217	-67	0	40	37	4	-11
194	-43	1	58	23	5	-19
169	-42	7	49	12	1	2

A plot of Glicksman's solution is given in Fig. 93.

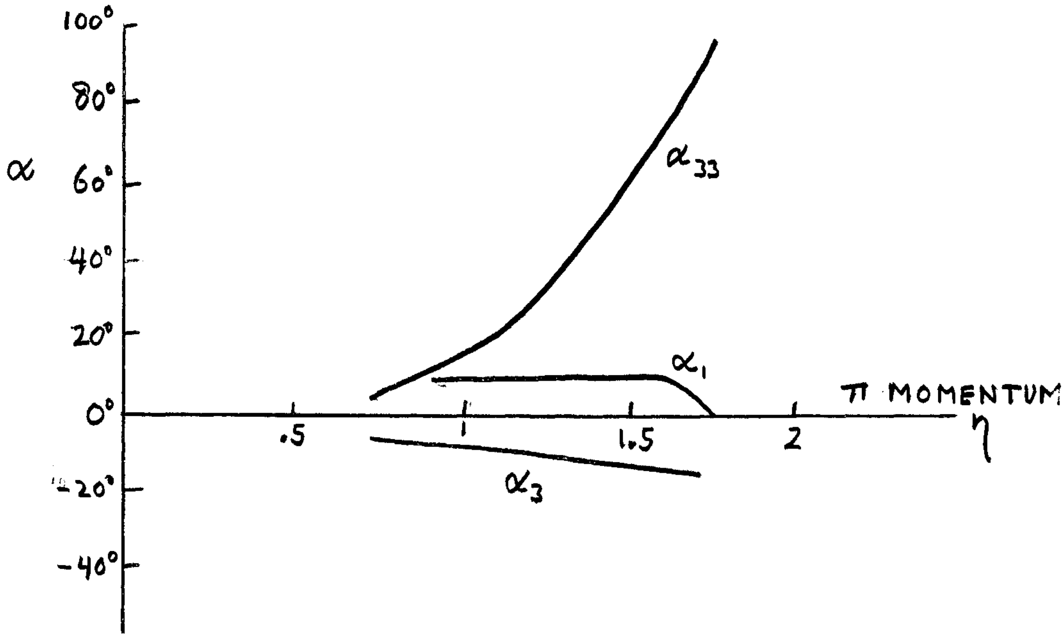


Fig. 93

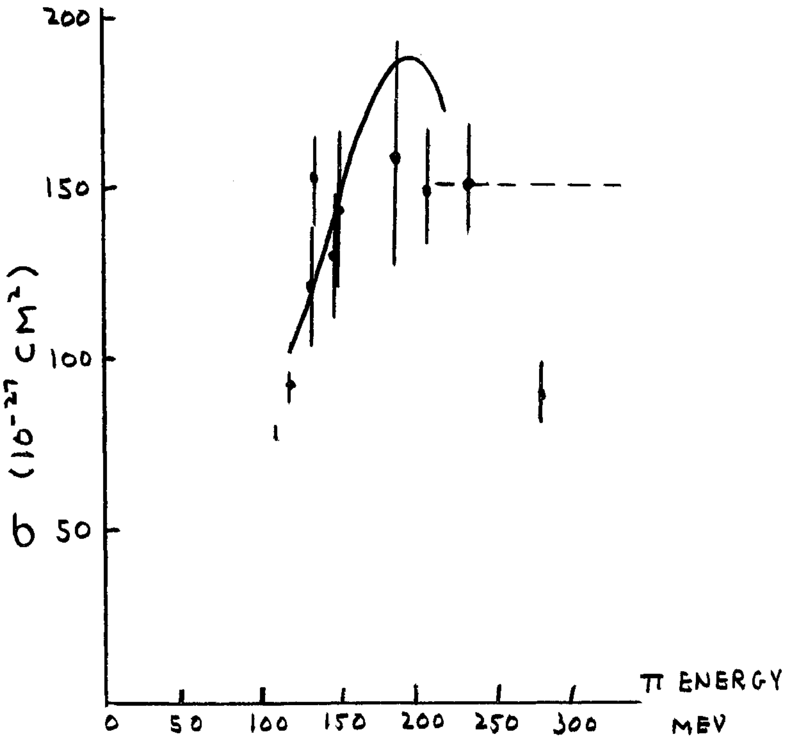


Fig. 94

This predicts the energy variation plotted in Fig. 94 for the total cross section. As we heard yesterday, Carnegie Tech. has now shown the π^+ total cross section to be considerably higher than the experimental points given in Fig. 94 so that the agreement between Glicksman's curve and the measured total cross section will be much improved.

The analysis given here shows that there are at least three different solutions for the phase shifts all of which fit the experiments at all energies. Of these only the first satisfies the demands of theoretical intuition. The question therefore arises as to how the three solutions are related to each other, and whether they can be distinguished experimentally. This question has to be discussed separately for solution 2 and solution 3.

Solution 2 is essentially the Yang solution. The discussion of its relation to solution 1 is most easily done in terms of a model. This model consists of taking as a working hypothesis that of Glicksman, that is, that α_{31} is 0 and

α_{33} changes monotonically with energy. If this is true, is there then any other solution? We shall use the experimental total cross section to the following extent. As was pointed out by Ashkin yesterday σ^+ is very nearly three times the σ^- cross section over this entire region; this remains true even somewhat beyond the maximum. This indicates that the isotopic spin 1/2 phase shifts are small, and consequently one can talk about π^+ scattering just as well as about π^- scattering. We assume there is some value for α_3 which does not have to be specified and that the isotopic spin 1/2 state is neglected. Then is there another solution? Yang showed already that there is; under these conditions one can obtain precisely the same fit to the experiments by taking

$$\tan \alpha'_{33} = \frac{1}{3} \tan \alpha_{33}$$

$$\alpha'_{31} = \alpha'_{33} + \alpha_{33}$$

From these two equations we see that for small α_{33} at small energies $\alpha'_{33} = \frac{1}{3} \quad \alpha_{33}$; but at the same point at which the Glicksman value of α_{33} goes through 90°, so does the α'_{33} of the corresponding Yang solution . Further, at the same point α'_{31} will go through 180°. If α_{33} varies from 0 to 180°, then the Yang solution will have one resonance in α'_{33} but two additional resonances in α'_{31} at 90° and 270°. The true situation is readily grasped by looking at Fig. 95.

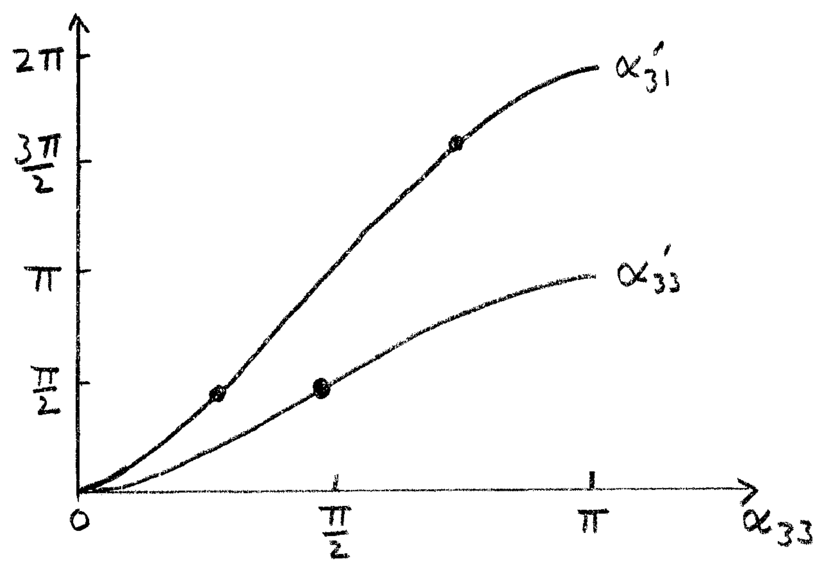


Fig. 95

Just on the basis of simplicity it would seem that the conventional solution is to be preferred. That is, the conventional solution has only one resonance where the Yang solution necessarily will have three. Under the assumptions made here, it would be impossible to distinguish between the the two solutions on an experimental basis.

Fermi said that this is fundamentally probably a good argument. However, he wished to point out that perhaps excessive emphasis has been put on identifying the resonance with the passage of the phase shift through 90°. To be a true resonance the scattering must contain other features. For instance, many potentials, such as the hard core potential, give a phase shift proportional to momentum which passes linearly through 90° and 180° and 270°, etc. These might be called resonances but would not exhibit the characteristic features. Bethe admitted that this is certainly true but that it is difficult to define resonance any other way.

Bethe then discussed solution 3 which is, as you recall, the same as the Fermi-Metropolis solution 1. In order to understand its properties it's necessary to look in some detail at the Ashkin diagrams. Martin has done this and finds that under certain conditions one obtains more than two solutions. If we write the scattering amplitude without spin flip as $A+B \cos\theta$ where A and B are complex numbers, then the experiment gives $|A+B|$ from the 0° differential scattering cross section and $|A - B|$ from the 180° differential cross section. This is true because the spin flip term is proportional to $\sin^2\theta$ and is therefore zero at these two points. Further, the total cross section gives the real part of $A+B$ which is always less than zero. If the real part of $A+B$ is approximately minus three-quarters of the absolute value of $A+B$, then there are more solutions than the Fermi-Yang set but these will disappear again when the real part becomes small. Therefore, when the total cross section becomes small, and consequently the real part of $A+B$ becomes small, one only has two solutions. But when the total cross section becomes big one can get four solutions. Therefore, in the neighborhood of the resonance(if there is a resonance) you are apt to get more solutions. This is the fundamental reason for the confusion at 200 Mev which is the region where more than two solutions are obtained. At low energy, solution 3 becomes indistinguishable from solution 1 if you make the passage from high to low energy reasonably. It is to be expected that beyond 220 Mev one will again find the third solution to disappear. Fermi: "If you assume that it is the wrong solution" (laughter).

Bethe has followed this through in some detail. Assuming solution 1 to be correct, he constructs angular distributions at different energies. It is then possible to see solution 3 appear at a certain point and disappear again at energies just a little higher than those at which experiments have been made up to the present. Fermi: If you assume that this hypothesis is correct and solution 3 should actually disappear around 250 Mev he feels that this would presumably kill it experimentally. But he wondered whether it does not disappear on the low energy side as well. Bethe explained that one has a situation such as is indicated in Fig. 96. One does get a solution which goes like the dotted line and which disappears on the low energy side, but to choose this is just silly. The alternative is to follow solution 3 to where it intersects solution 1 and then follow the latter to low energies.

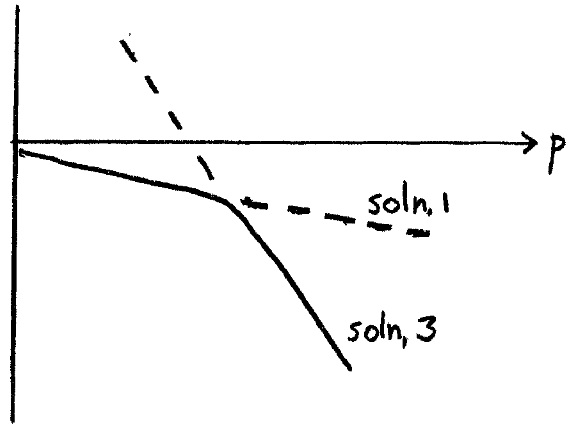
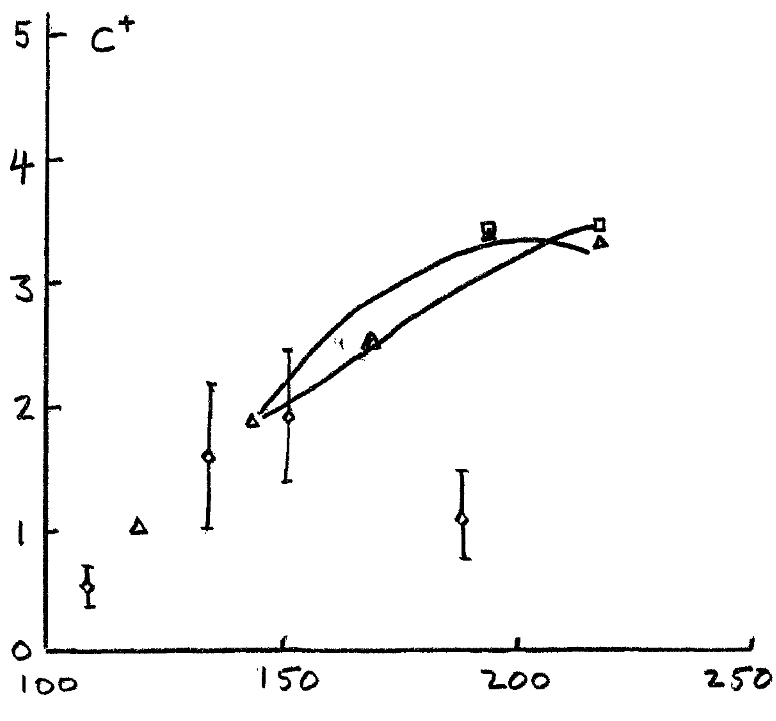


Fig. 96

The characteristic difference between solution 1 and 3 can be illustrated by assuming (which is not correct) that α_3 is zero. Then, solution 1 gives an angular distribution of $1+3\cos^2\theta$ and solution 3 gives a solution of $(1+\sqrt{3}\cos\theta)^2$ at high energy. Furthermore, one would predict something very near to a break in the derivative of the phase shift as a function of energy. This would occur somewhere around 230 Mev in π^+ scattering, so that Bethe predicts that a decision between solutions 1 and 3 should be possible if experiments are made on the angular distribution of the scattering at 260 to 300 Mev. He noted that at 217 Mev the Yang solution would give $\alpha'_{31} = 243^\circ$ and $\alpha'_{33} = 130^\circ$.

Bethe then discussed what happens at lower energies. He first gave slides for the prediction for certain of the coefficients in the angular distribution assuming the Glicksman solution to be correct. Fig. 97 gives the energy variation of C^+, that is, the coefficient of $\cos^2\theta$ in the π^+ angular distribution.



Meson Energy (Mev)

Fig. 97

The 240 Mev point is that obtained by Shutt and probably should be plotted at 225 rather than 240 Mev. The only experiment which seems to disagree very much with our analysis is that of Homa, et al. at 188 Mev. The coefficients of the angular distribution A, B, C, given by these authors are quite different from those deduced from π^- scattering; in particular, A is much larger and C much smaller. However, if the π^- scattering is analyzed and π^+ distribution deduced from the analysis, the latter is entirely compatible with the results of Homa, et al. The total cross section is also in good agreement with the value of Ashkin and others.

It is seen that the Glicksman solution gives a very reasonable fit to the data. Fig. 98 shows the coefficient B^- as a function of energy, that is, the coefficient of cos θ in the elastic scattering of π^-. The curve showing a deep dip is obtained by assuming that the phase shifts are linear in the energy of the pion from 140 to 217 Mev which of course is not correct.

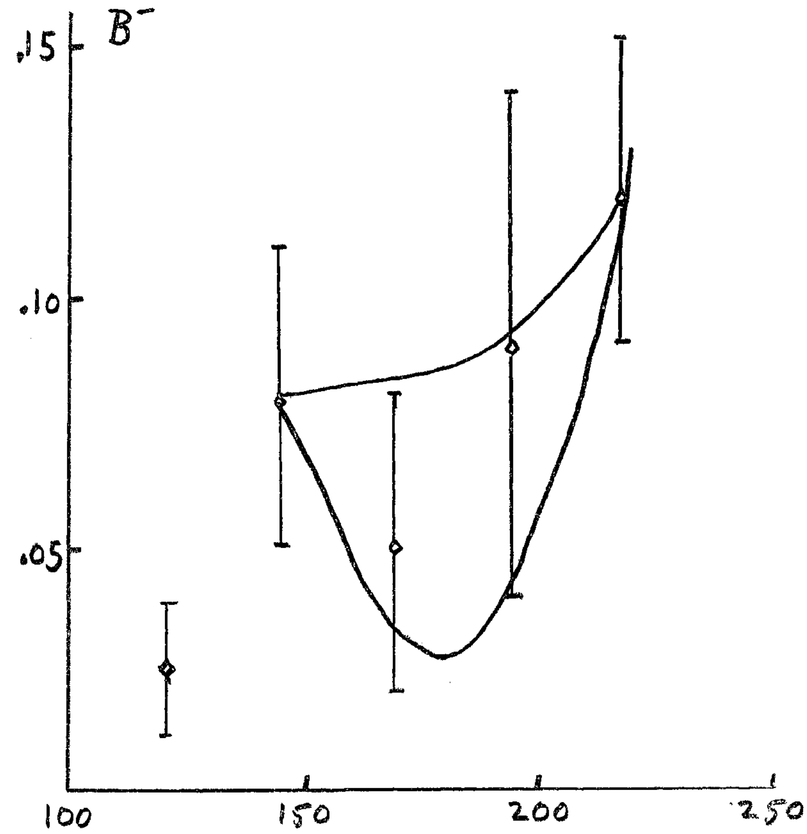


Fig. 98

However, this dip, Bethe feels, is independent of the particular assumptions made and is in fact simply due to α_{33} going nearly to 90°. He feels it would be very nice to have experiments accurate enough to show whether or not this step actually exists, which would require considerably greater accuracy than has been obtained so far. Probably the curve should exhibit a round maximum at 140 Mev and come down again; this region has not been plotted.

One other thing has been investigated at Los Alamos, namely, whether and to what extent the phase shifts are stable when the input data are varied. The experiments are limited in accuracy and one therefore doubts the accuracy of the phase shift analysis. At 217 Mev each of the coefficients ABC in the negative and charge exchange angular distributions were varied independently to the limit of the experimental error. Practically all the phase shifts are nearly unchanged, i.e. they do not change by more than 10°, when this is done. However α_{11} and α_{13} vary all over the map and are therefore not determined by the data. The sensitivity of α_{11} and α_{13} has been explained by Martin in terms of Ashkin diagrams. α_{33} and α_{31} can, however, be well determined from the experimental data. α_3 varies even less with changes in the input data than the other phase shifts. The same thing has been done at 140 Mev with the same results. However in the intermediate energies the phase shifts are much more sensitive to variations in the input data, that is, they are very sensitive in the region of the presumed resonance.

Fig. 99 indicates the situation at low energies. Here is plotted the difference between the S phase for isotopic spin 1/2 and S phase for isotopic spin 3/2 divided by the momentum of the pion. The highest energy point is that corresponding to 217 Mev. The 40 Mev circle point is obtained from the Roberts-Tinlot experiment. The three crosses are taken from the total charge exchange scattering cross section at 42, 30 and 20 Mev as measured by Spry, assuming a value for α_{33} from the 40 Mev angular distributions and extrapolating it proportional to the cube of the meson momentum down to 20 Mev. The point at zero energy has been calculated using the Panofsky experiment and the best data available on photomeson production at the time the curve was made. Probably in the light of Bernardini's results discussed yesterday this point will have to be lowered still more. From this curve Bethe and de Hoffmann have predicted the energy variation of the two S phases independently. This is given in Fig. 100. It is seen that any reasonable extrapolation of the curve for α_1, as has been done here with a French curve, gives quite a large slope at zero energy. Then, using the result of the Panofsky experiment, it is necessary that α_3 also have a posi-

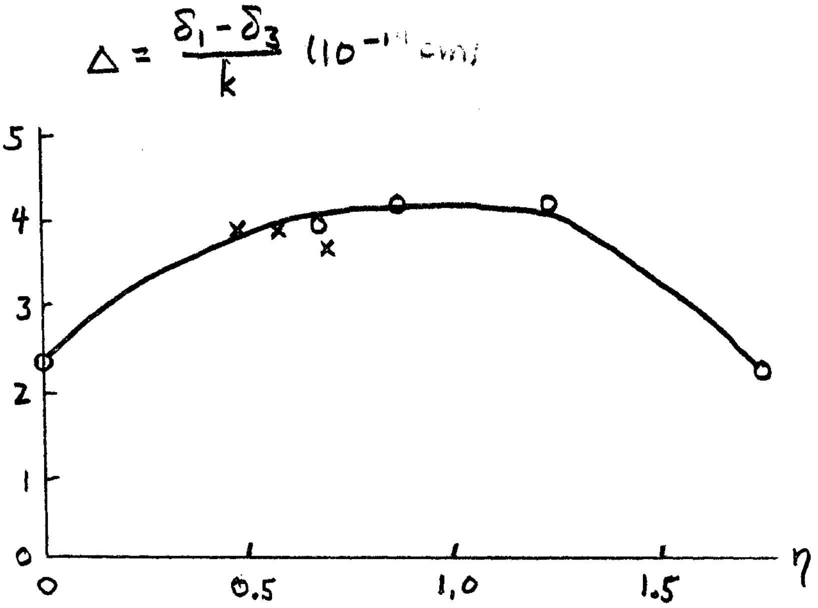


Fig. 99

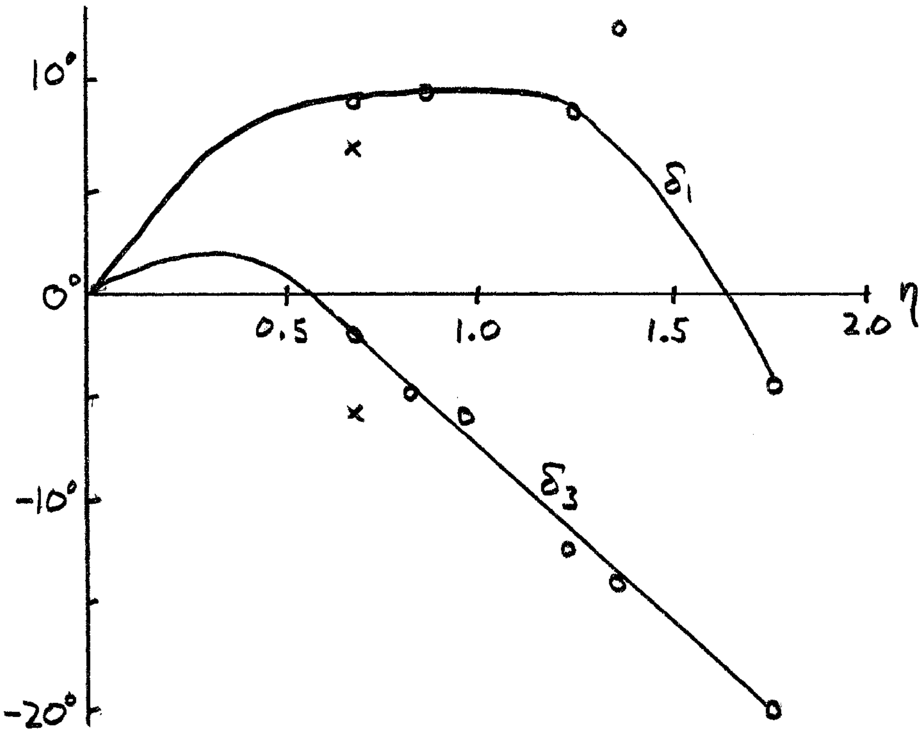


Fig. 100

tive slope Using these curves it is then also possible to extrapolate σ^- to energies below 40 Mev. This prediction was made before Lederman's data were available to Bethe and de Hoffmann but now that they are available, they tried to see what this extrapolation would predict for the π^- elastic scattering at 5 Mev. They predict a cross section of 22 mb which is in excellent agreement with Lederman, although his value is of course very uncertain. Therefore, since this was done before the Lederman experiment was known to them, they feel that the Lederman experiment is a confirmation of this extrapolation procedure. In fact, to get such a large cross section as Lederman finds, it is necessary to assume that both S phases have the same sign at low energy. It is clear from this that the energy variation of the S phases at low energy is extremely complicated though what the explanation for this will prove to be Bethe does not know.

Fermi summarized the situation as follows. There are two elements to this analysis, the phase shift's behavior around 200 Mev and the behavior at low energy. As far as the high energy experiments go Fermi's position is as follows. If one takes a purely experimental attitude, then no decision is possible with the accuracy of the present experiments, although Bethe has indicated that a decision may be possible if these were pushed to somewhat higher energy. Taking into account elements of simplicity, -- and of course we have no guarantee from nature that things are simple; in fact we have been slightly disappointed occasionally -- (laughter) Fermi considers the Glicksman solutions to be by far the simplest so he would hope that it turns out to be right. On the low energy side he has been surprised at the insistence especially of the π^- elastic scattering cross section to stay up so high. If Lederman's results are confirmed this is very interesting. Bethe proposes taking certain results quite literally. Fermi feels one should go easy on this and should not attempt to use complicated theories until the experiments are cleaned up further. He believes the theory which can be done easily is fine, but he wouldn't attempt anything else at this point. He noted that threshold information usually is extremely important and wished to urge everybody, including himself, to do more experiments in this region in order to firm up the experimental situation.

Lederman then quoted his results when asked to by the chairman (Bethe). He has not four but five events now which are π^- scatterings in hydrogen at 5 Mev so the cross section definitely is not zero and if you take the numbers literally, you get about 20 mb $\pm \frac{\sqrt{6}}{5}$. Bethe noted that the statement that the cross section is not zero is by no means trivial because if you extrapolated the phase shifts linearly from 100 Mev one would predict that the cross section should be zero.

In response to a question from Roberts as to why only S and P phase shifts have been used in the analysis and D waves have been ignored, Bethe replied that he has only a theoretical argument to justify this. In the Tamm-Dancoff theory α_{33} is large only by virtue of the fact that there is effectively an attractive potential and therefore a phase shift many times what the Born approximation would give, and the Born approximation is approximately 1/5th of the observed phase shifts. The Born approximation result agrees with the phase α_{31} at 220 Mev, which is of course not very well determined. Therefore, if you say that an honest P phase shift at this energy is about 10 or 20° it is possible to estimate how much the nuclear recoil would give for the D phase shift at this energy: it is smaller than P roughly by a factor of $\bar{p}/M$, with a factorial in the denominator. Bethe has had a graduate student calculate the D phases on this model and in Born approximation he finds a $D_{5/2}$ phase shift of the order of $+2^{\circ}$ and a $D_{3/2}$ phase shift of the order of -3°. Unfortunately, if the phase shifts have opposite sign, as it is indicated by the Born approximation, then the term in the angular distribution which would be observable, proportional to $\cos^3\theta$, has its coefficient essentially cancel and so would not show up, whereas there would be a modification of the coefficient of the $\cos\theta$ term which of course cannot be observed separately.

As to the experimental situation on this point, Glicksman's experiment at 217 Mev gives on its face value evidence for the presence of D waves, that is, the fit to the data without them is somewhat outside experimental error. However, Fermi noted that one should always be suspicious of an experiment which is consistently better than the quoted experimental error. H. Anderson gave a little more detail on this point. He does not take the evidence very seriously. In the angular distribution of the gamma rays one finds that a pretty good fit if obtained if only the first three spherical harmonics are used and little change in the fit is obtained by including Y_3, as shown in Fig. 101. However, in the elastic scattering, there is considerable change, as shown in Fig. 102. If terms up to $\cos^2\theta$ are included the two end points of the angular distribution are both missed outside experimental error. A much better fit is obtained if Y_3 is included. However, you always expect to obtain a better fit to a set of experimental points if an additional parameter is added so that it is hard to say whether this is a proof of the presence of a D wave. All Anderson would care to say is that this indicates some caution should be used in assuming that no D waves are present. Fermi noted that if the energy range could be extended by about another 50 Mev then the D phases whould blow up very rapidly and the effect should be unmistakable. Brueckner remarked that Shutt and his group last year in their experiment at what was then called 260 Mev and is now thought to be 230 Mev did seem to find a large D wave but the experimental errors were very large. Thorndike commented that the errors were in fact such that one did not have to assume a D wave.

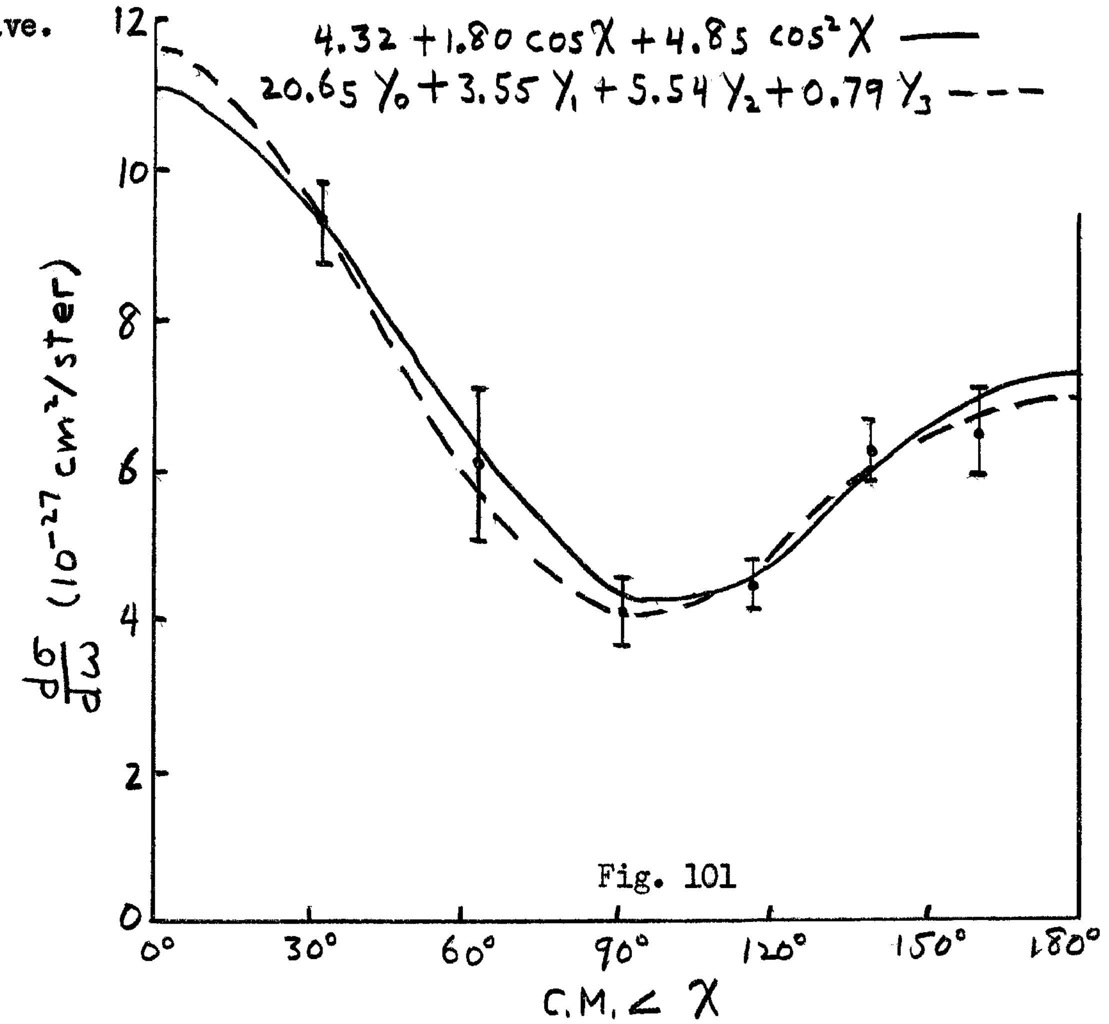


Fig. 101

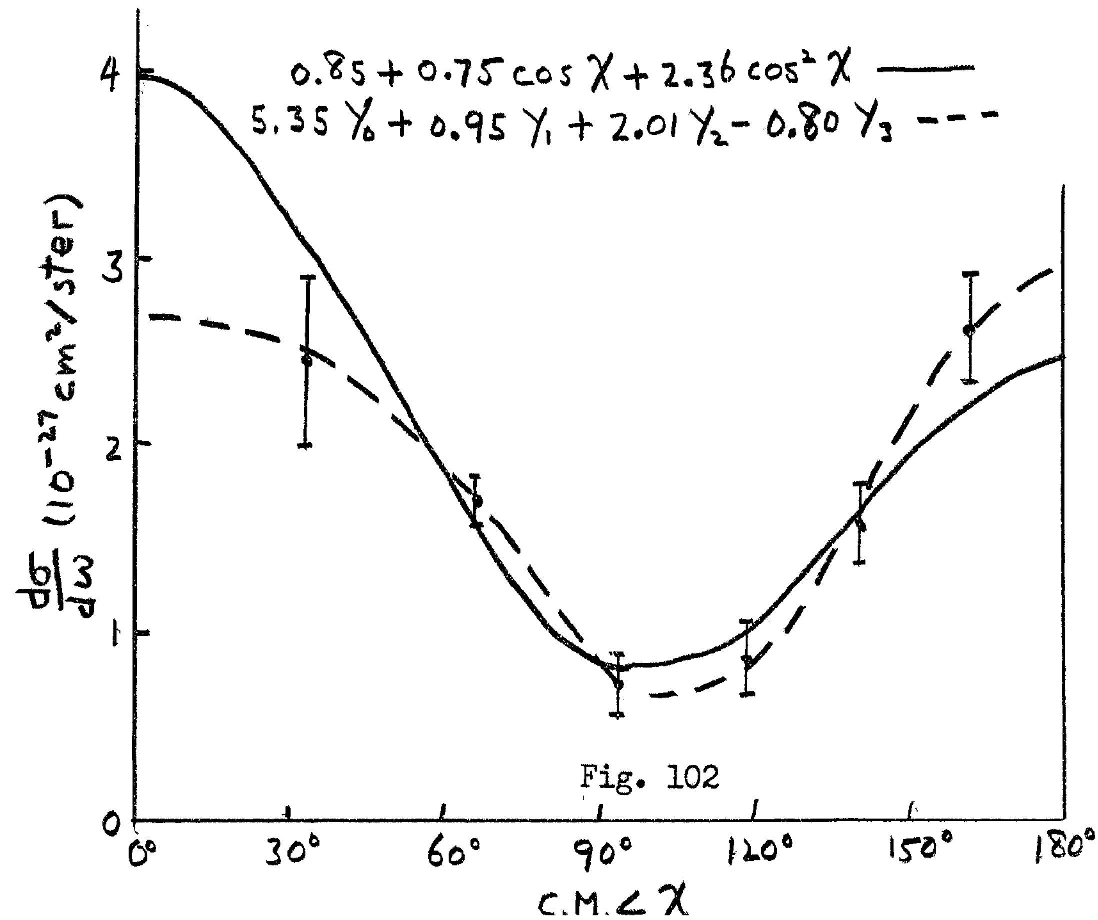


Fig. 102

Rarita then reported on some calculations he had made with Serber in an attempt to fit the pion-proton scattering using a Breit-Wigner formula. For the positive total cross section one has $\frac{\sigma^+}{4\pi\lambda^2} = 2\sin^2\alpha_{33} + \sin^2\alpha_{31} + \sin^2\alpha_3$. They assumed α_{33} to be dominant and to be given by the Breit-Wigner formula as

$$\sin^2\alpha_{33} = \frac{[\epsilon\sin\delta - \frac{\Gamma}{2}\cos\delta]^2}{\epsilon^2 + \Gamma^2/4}$$

where the first term in the numerator is the potential term and the second term is the resonance term. Assuming that both δ and Γ vary proportionately to the cube of the momentum, they were able to get a fit using the Brookhaven point at 260 Mev. This is curve 1 in Fig. 103.

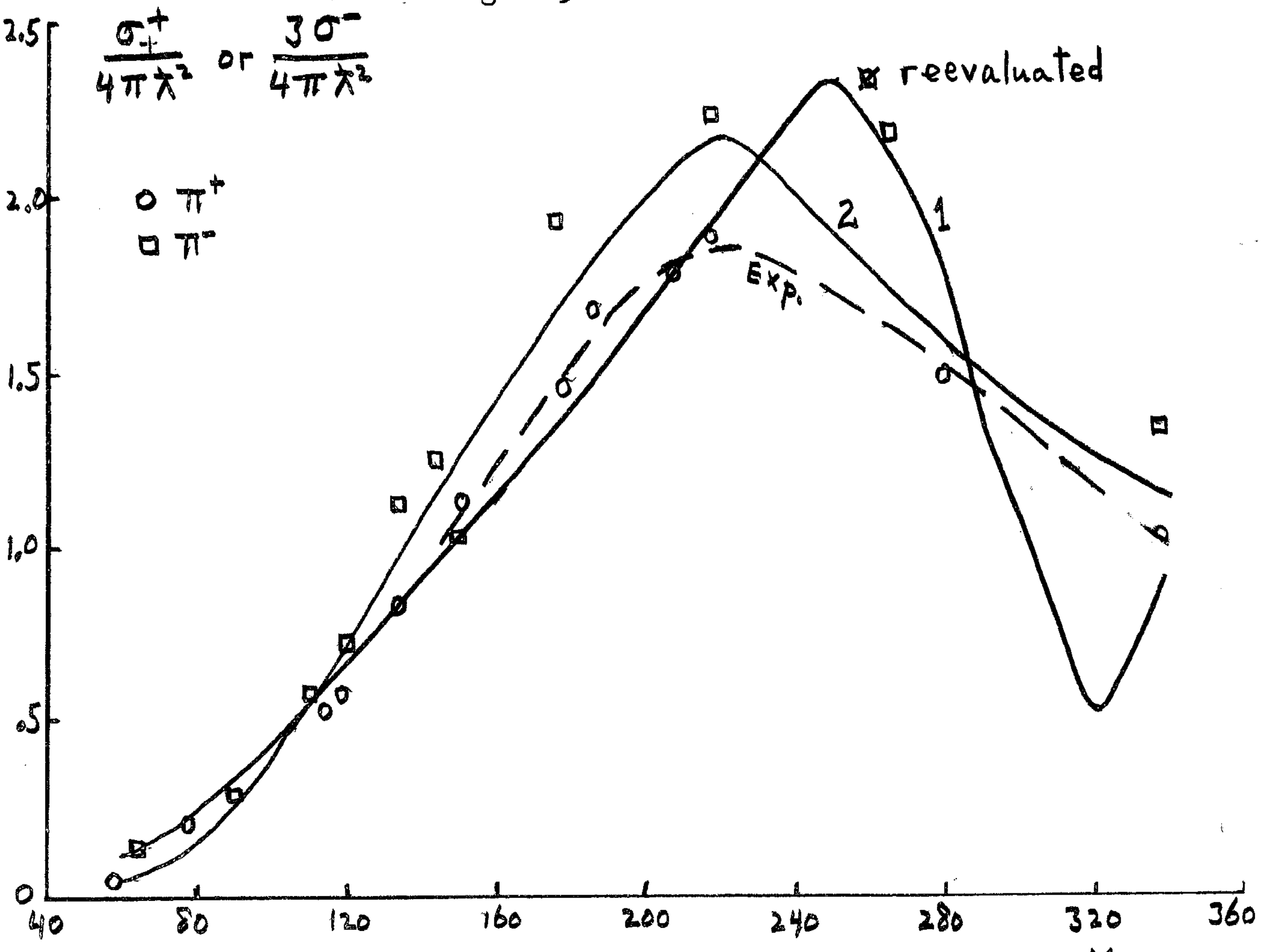
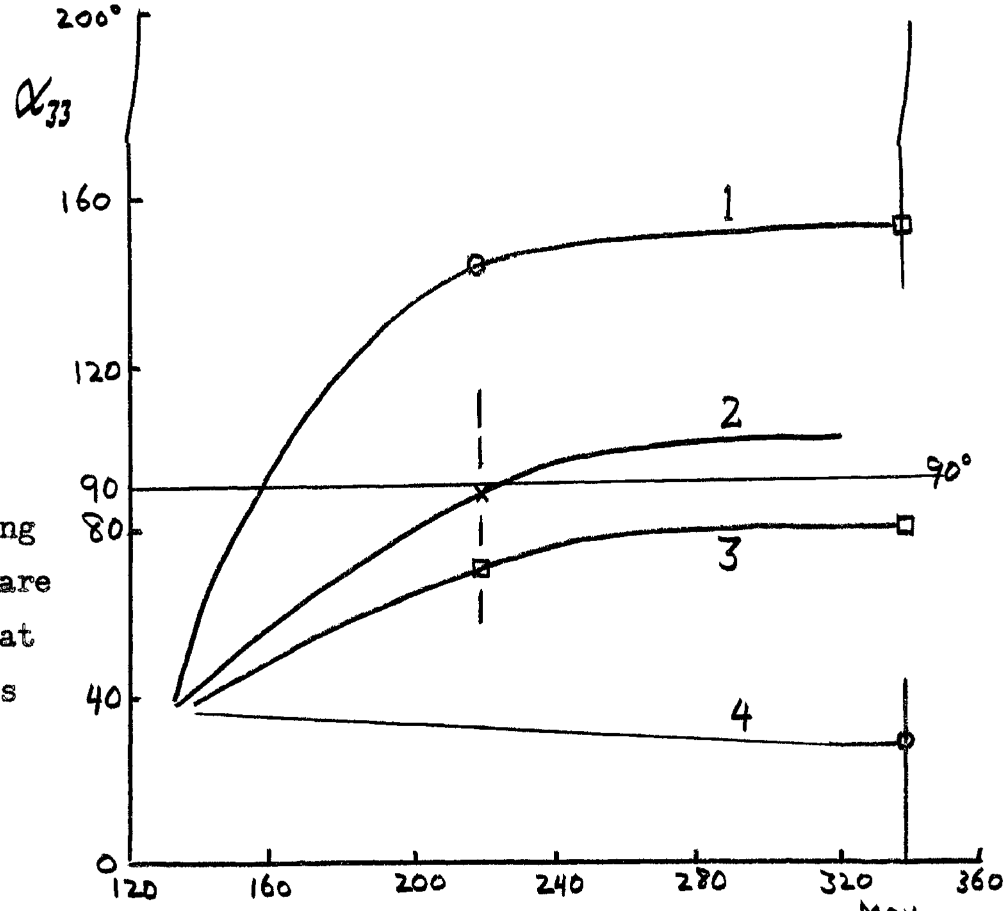


Fig. 103

α_{31} was simply assumed to vary proportionally to the cube of the momentum and α_3 proportionally to the momentum. It will be seen that there's a fairly pronounced antiresonance closely following the resonance. The angular distributions were also decently given by this fit. However, when the Brookhaven point was moved down 230 Mev the curve had to be recalculated given curve 2 in the diagram. It is seen that the antiresonance has disappeared. They also looked at the energy variation of α_{33} to be predicted from a square well. The results are given in Fig. 104.



The various curves are given with the following parameters for the square wells. It is noted that if the phase shift does go to 90° then this square well would also give a bound state.

Fig. 104

Curve	Depth (μc^2)	Range (λ_c)	Depth to Give Bound State (μc^2)
1	5.96	.4	4.17
2	1.93	.8	1.73
3	1.21	1	1.26
4	.26	2	.42

It is easy to obtain the π^- total cross sections under the same assumptions if one neglects the two P phases α_{11} and α_{13}. Then

$$b_+ = 3b_- = 3\,(b_{-0} + b_{--})$$
$$c_+ = 3c_- = 3\,(c_{-0} + c_{--})$$
$$\sin^2\alpha_1 = \frac{3a_- - a_+}{2\bar{\lambda}^2}$$

When three times the negative total cross section is plotted it again comes fairly close to the experimental points.

Some calculations were also made in an attempt to fit the 1.5 Bev scattering, which as we have seen is about 70% inelastic and 30% elastic, using an optical model. This can be done with the following

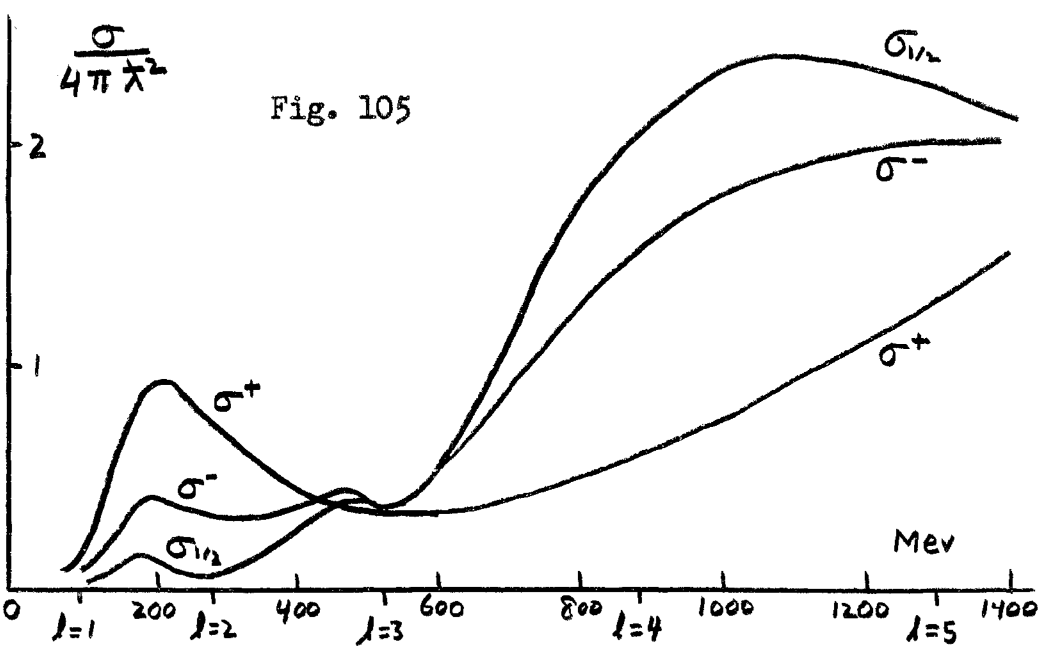


Fig. 105

parameters. The reciprocal absorption length K is .7R, the wavelength of this energy is .9R and the radius R is taken to ~~be~~ about 5/4 the Compton wavelength. The index of refraction divided by the reciprocal absorption length is k_1/K. Positive, negative and isotopic spin 1/2 total cross sections divided by $4\pi\lambda\!\!\!^-{}^2$ are plotted in Fig. 105.

Noyes then reported on some recent calculations made using the S wave potential model of the pion-nucleon S phase shifts originally proposed by Marshak. This model is not taken seriously above 120 Mev and is proposed only for the low energy region. Bethe's discussion of the probable energy variation of $\alpha_3 - \alpha_1$ has indicated their peculiar character. The important point in these calculations is that one can fit this sort of energy variation by a potential model. A potential model consists of a purely attractive force for the S phase of isotopic spin 1/2 and a Jastrow type repulsive core, attractive tail potential for the isotopic spin 3/2 state. The predictions of this model are given in Fig.106.

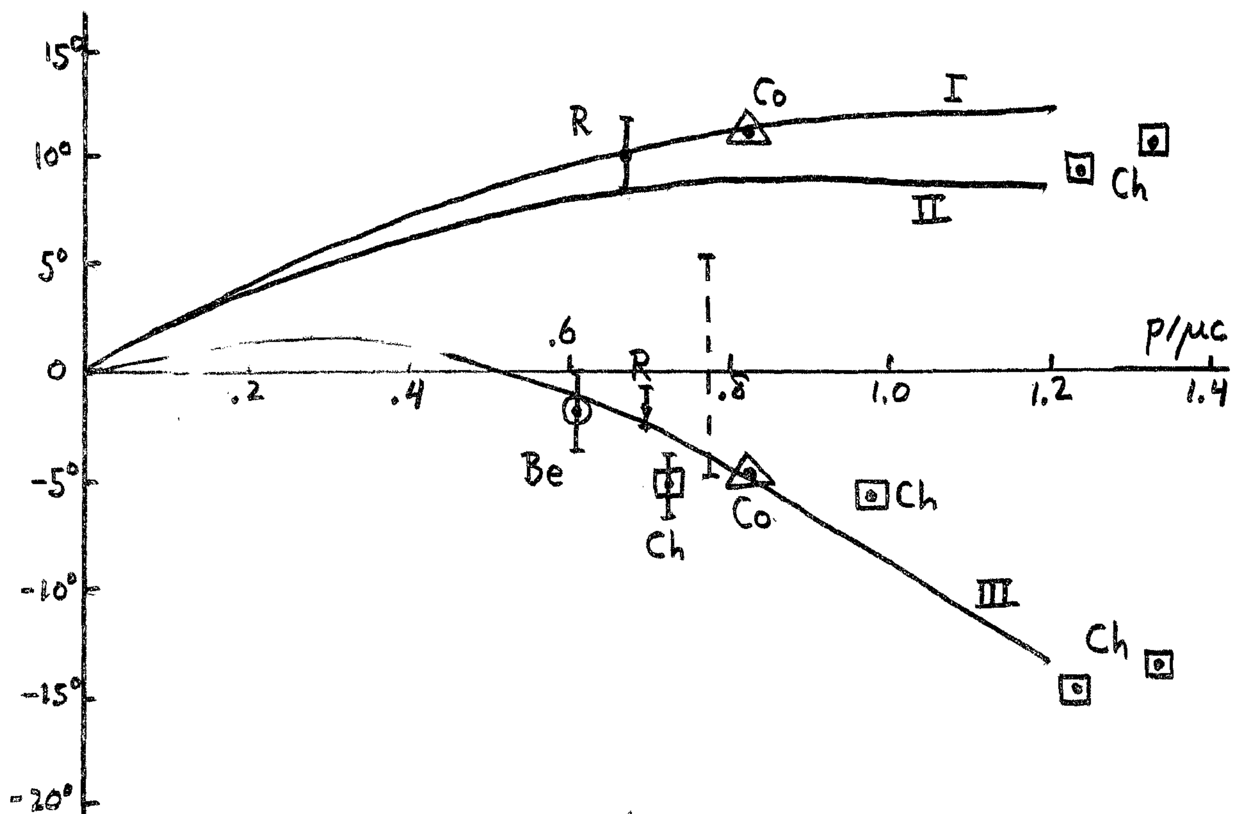


Fig. 106

The parameters corresponding to the various curves are given in the following table.

	Phase Shift	Range r_0	Depth V_0	Core r_c	$\lim_{E\to 0} \frac{\alpha}{k}$
I	α_1	$.47 \times 10^{-13}$ cm	-277 Mev	-	.34 p/μc
II	α_1	$.65 \times 10^{-13}$	-105 Mev	-	.31
III	α_3	$.60 \times 10^{-13}$	-187 Mev	$.54 \times 10^{-13}$	.11

$$V_1 = -V_0 e^{-r/r_0} \qquad V_3 = -V_0 e^{-\frac{r-r_c}{r_0}} \quad r > r_c$$

$$= \infty \quad r \leq r_c$$

It is to be noted that with these particular parameters α_1 has a quite normal behavior and α_3 reverses sign at about 20 Mev. I feel this model has particular relevance in relation to the photomesic data that Bernardini gave yesterday. As he noted then this data implies that $\alpha_1 - \alpha_3$ is equal to .16P/μc whereas a linear extrapolation from the Rochester and Columbia phase shifts would give .33 P/μc. This model gives for the upper curve .23 P/μc and for the lower curve .20 P/μc. It therefore has the correct qualitative features to explain the discrepancy. In fact, .20 P/μc is just at the upper limit allowed by Bernardini's quoted error of 25%.

I would not take this too seriously as a model. In particular the hard core makes it extremely sensitive to kinematic relativistic corrections as was also found with the Jastrow potential in the nucleon-nucleon case. Rather, I would consider it as serving as a qualitative indication that if you want to talk in terms of potential models or forces, these are the kind of forces that will explain the data. I don't think you can take the parameters too seriously or look for a much greater theoretical significance than this.

Bethe noted that if we have to believe the weird behavior of the low energy phase shifts, we have to believe that there is something like a long range potential. Dyson noted sometime in the past that maybe this has something to do with the meson-meson interaction with the virtual meson cloud surrounding the nucleon.

Sachs then presented another approach to the pion-nucleon scattering problem. He feels it clear that one needs some sort of intuition for the analysis of the pion-nucleon phase shifts. What he is attempting is not different in spirit from what has already been presented by Rarita and Noyes but it takes a model which is much more explicit and hence allows the procedure to be justified in much more detail. The model is complicated and involves several parameters, but since the phase shifts are turning out to be complicated, this should not be too surprising. He has not attempted to fit the phase shifts with this model yet. The model for the nucleon is complicated; that is, it consists of pions surrounding a nuclear core. This is somewhat similar to an atomic model except that the number of pions is not a good quantum number. The first assumption is that the ground state of the nucleon is describable by some normalizable state vector Ψ_0. The question then is what consequences does this assumption have for pion-nucleon scattering.

In this model one is talking about single pion orbits or states which may be multiply occupied. The bound pion functions should fall off exponentially. One assumes that the nucleon can be completely described by some such set of bound single pion states ϕ_b with b running over what may be an infinite number of values. The reason for assuming that there is more than one bound state is to allow for correlations between pion pairs. The conventional Tomonaga approximation assumes only one such orbit so that this is a more general description. In order to have a complete set of wave functions, it is necessary to assume that there is also a set ϕ_K which correspond to continuum wave functions of the pion having a momentum K at infinity. These have to be taken to be orthogonal to the ϕ_b and must be normalized by some suitable asymptotic convention. The philosophy is to attempt to specify the dynamics of the system as little as possible. It seems well established that the free behavior of the pion is that of a pseudoscalar Pauli-Weisskopf free particle. So, the Hamiltonian for the free particle should be written as $F_{free} = \int d^3k\, k_0\, a^*_k\, a_k$ where $k_0^2 = k^2 + \mu^2$ but one avoids in so far as possible any detailed description of the interaction of the pions with the core. Instead of using the usual momentum representation, one should set up his representation in terms of the orthogonal set of bound and unbound functions. Then the Hamiltonian breaks naturally into three parts: $H = H_B + F_{KK} + V$. Here H_B describes the properties of the bound states ϕ_b, F_{KK} of the free states and V is the interaction term which mixes these up.

The natural procedure is to first look for solutions of H_B. These will be given by $H_B\ \phi_\lambda = E_\lambda \phi_\lambda$ where E_λ are some set of energy levels. The lowest level is assumed to be E_0 and E_0 is taken equal to zero for convenience. This corresponds to the ground state or nucleon Ψ_0. The states other than Ψ_0 will presumably be unstabilized by the interaction V. H_B contains part of the interaction with the bound field, that is, one can write $H_B = F_B + I_B$. One can specify isotopic spin and angular momentum for each of these bound states. If now the free particle part of the Hamiltonian F_{KK} is introduced the system $H_0 = H_B + F_{KK}$ is separable. This determines the continuum solutions ϕ_K. If one has only one bound state, as is well known from Wentzel's work in strong coupling and the work of Tomonaga, one can solve the problem exactly. What perhaps is not so well known is that it can also be solved exactly for an infinite number of bound states. One finds those ϕ_K which are orthogonal to ϕ_b and which Sachs therefore calls the orthogonality functions. It turns out they have the property that at infinity they have a phase shift for a given angular momentum, that is

$$\phi_K \longrightarrow \frac{\sin(kr - \frac{l\pi}{2} + \eta_l)}{kr}$$

This phase shift has the property that

$$\tan \eta = \pi k k_0 \sum \frac{|\Psi_\beta(k)|^2}{q_\beta}$$

Terms entering into this expression are defined as follows. We form the matrix

$$Q_{bb'} = P \int d^3k \frac{\phi_b^*(k)\phi_{b'}(k)}{\chi_0 - k_0}$$

Here, $\phi_b(k)$ is the bound state function in momentum space. q_β are the characteristic values Q, that is, the diagonal elements of Q after it has been diagonalized. ψ_β are the transformed functions ϕ_b in that system for which Q is diagonal. That is, we have $U Q U^{-1} = q; \Psi' = U\varphi$. One can show quite generally that q_β has energy dependence shown in Fig. 107. There may of course be wiggles in this curve but it always starts out negative, crosses the axis, and goes positive. This means that the tangent of the orthogonality phase shift η goes to infinity or the orthogonality phase shift itself goes through $-\frac{\pi}{2}$. That is, it will always have the characteristic behavior given in Fig. 108. This will have an interesting connection with the behavior of α_3 as we shall see later.

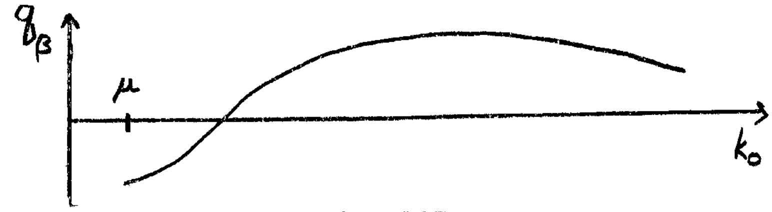


Fig. 107

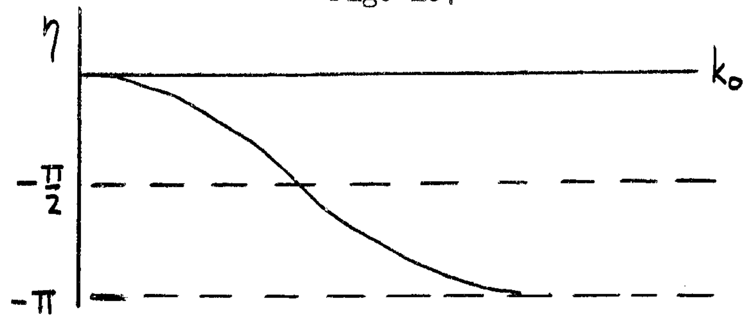


Fig. 108

If we now try to introduce scattering due to the interaction V we have to make somewhat more detailed assumptions in order to make any progress. We assume that V is linear in the creation and annihilation operators of the unbound field functions. That is, we write $V = P(a_b)\, a^*_k + P^*(a_b)\, a_k$. It is clear there is one property V must have, namely, $V\Psi_o = 0$. This is true because Ψ_o must be completely describable in terms of the bound functions ϕ_b. Therefore, we must have $P\Psi_o = 0$. If one makes these assumptions and calculates the scattering one finds that the total phase shift α is simply equal to the sum of the orthogonality phase shift and the phase shift δ due to the potential; i.e. we use the orthogonality functions as a basis for the calculation and so the additional phase shift simply superposes. Then we find

$$\tan\delta = -\pi k k_o \sum_n \frac{\gamma_n^2}{k_o - E'_n} \qquad \alpha = \delta + \eta$$

If the coupling is weak the constants E'_n are just the energy levels of the bound field Hamiltonian E_λ. These will get all scrambled up as the coupling increases in strength but the form of the expression for $\tan\delta$ will be unchanged. Again as we see δ has a typical resonance behavior. Comparing the two curves we have the situation in Fig. 109. The question then becomes where are the resonances and how strong are they, etc. If one

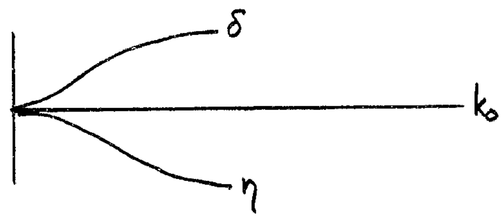


Fig. 109

uses the Tomonaga approximation, that is, one assumes only one bound function, then the crossover for P waves will occur at very high momenta, in fact close to the cutoff momentum in the theory. For S waves, however, under the most reasonable assumptions, the cross over comes much earlier. So the phase shift has a strong orthogonality component, that is, a strong negative phase shift. The combination seems just what is needed to give the isotopic spin 3/2 curve of the form given in Fig. 110 if one assumes that in this state one has predominantly orthogonality scattering and the resonances are high up. If we take this empirical energy variation to mean that α_3 is just the orthogonality scattering phase shift, then one can also reproduce the experimental energy variation of α_1 at least qualitatively by assuming that there is a resonance in the S state of isotopic spin 1/2 at 300 or 400 Mev. One could presumably also get an appropriate behavior for the $P_{3/2}$ wave by assuming a resonance at the correct energy.

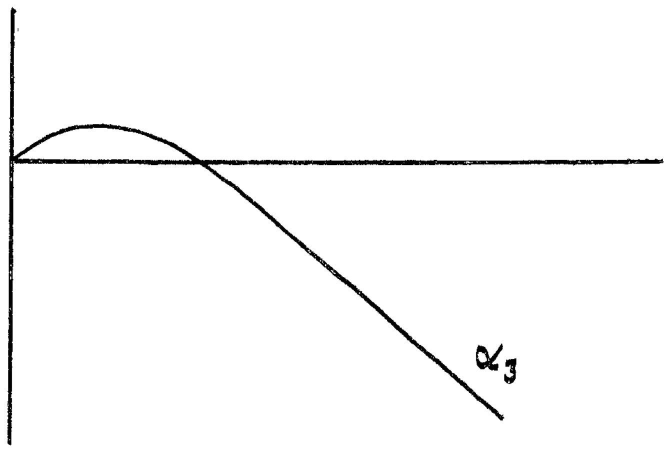


Fig. 110

Chew then reported on the results obtained by F. Salzman applying a specific model to the calculation of photomeson production. The model is old Yukawa theory with a cutoff. This has very often been used in strong and intermediate coupling calculations but Chew has found that if it is applied to calculate meson-nucleon scattering in the P state in weak coupling theory, the correct results are obtained. Not having time to go into the arguments in detail, Chew was merely able to say that he believes that the method of calculation is correct. The result so far is that it gives a successful description of P wave scattering in that α_{33} is big and the other P phase shifts are small. The scattering is fitted to the resonance by fixing the two parameters in the theory, namely the coupling constant and the cutoff at high momenta. He would like just to note that the prediction beyond the resonance from this theory almost certainly is that the phase shift does not go on to 180° but rather comes back through 90°, although this will not give a secondary maximum. He feels that this is not in obvious contradiction to anything which has been observed so far. His reason for hoping that this theory will apply to photomeson production has partly to do with the fact that it does already seem to give a resonable description of the anomolous magnetic momenta with the same cutoff and coupling constant as are used to fit the scattering data. It, therefore (if this is not coincidental), should be able to do a decent job for the photomeson production.

The cross section for charged positive photomeson production is given by the expression

$$\left(\frac{d\sigma}{d\Omega}\right)_+ = \frac{2e^2f^2}{\mu^2}\frac{k}{\nu}\left\{1-\frac{\mu^2}{2\nu^2}\frac{v^2\sin^2\theta}{(1-v\cos\theta)^2}+\sin^2\theta\left[2|M_1|^2+\tfrac{1}{2}|M_1+E_2|^2 + v\,\mathrm{Re}(M_1+E_2)\right]+|M_1-E_2|^2\cos^2\theta-2\,\mathrm{Re}(M_1-E_2)\left[1-\frac{\frac{v^2}{2}\sin^2\theta}{1-v\cos\theta}\right]\cos\theta\right\}$$

Here, $e^2 = 1/137$, $\hbar$ and c have been taken equal to one and the parameters of the theory are $f = .258$ and $W\,max = 5.6\mu$. The result as given is for infinite nucleon mass. The v/c corrections can be computed and do not change the qualitative features of the result significantly. The 1 inside the brackets is simply the electric dipole term which is closely connected with the gauge invariance and which everyone believes should be there. The second is small and has been often discussed; in rough language it can be described as arising from the absorption of the photon by the circulating pion without subsequent interaction with the nucleon. The remaining terms are associated with the secondary scattering of the pion by the nucleon. Only the term for the state of angular momentum 3/2 and isotopic spin 3/2 is given here. The other terms have been calculated and are small. M_1 is essentially a magnetic dipole term and E_2 an electric quadrupole term. They can be explicitly given in terms of well-defined quantities in the theory as

$$M_1 = \frac{1}{9\pi}\nu^{3/2}\int_0^{k_{max}} dk'\,\frac{k'^3}{\sqrt{\omega'}\,(\omega'^2+\nu^2)}\,\frac{T_k(k')}{\nu-\omega'+i\epsilon}$$

and $E_2 = 3M_1 - D$ where $D = \frac{4}{15\pi}\nu^{3/2}\int_0^{k_{max}} dk'\,\frac{k'^5}{\sqrt{\omega'}\,(\omega'^2+\nu^2)^2}\,\frac{T_k(k')}{\nu-\omega'+i\epsilon}$

$$T_k(k) = -\frac{1}{k\omega}\,e^{i\delta_{33}}\sin\delta_{33}$$

The corresponding cross section for neutral meson production is given by the expression

$$\frac{2e^2f^2}{\mu^2}\frac{k}{\nu}\left\{\sin^2\theta\left[4|M_1|^2+|M_1+E_2|^2\right]+\cos^2\theta\left[2|M_1-E_2|^2\right]\right\}$$

The ratio of neutral to charged meson production seems quite natural. The difficulty so far has been that although M_1 has not been accurately evaluated as yet, the rough estimates made so far all turn out to be small. (Chew noted later that in addition to M, there is a substantial contribution from the proton Dirac moment. When fourth order corrections are also made, the values of the matrix element are sufficiently large.) M_1 is greater than zero and E_2 is greater than zero and smaller than M_1. Consequently the interference term has the correct sign and all the qualitative features of the photomeson production cross sections are correctly given.

Bethe noted that the cutoff has increased over the previously given values until it is closer to the nucleon mass, which is where the relativistic people would like to have it. He also noted that this calculation bears some relation to a similar calculation by Marc Ross of the same phenomenon which was done somewhat more phenomenologically. The results obtained were rather similar. Ross also finds some charged meson production in the absence of the resonance and this helps to give agreement between the neutral and charged ratios. As to the positive to negative ratio Chew's opinion, with which Bethe concurs, is that in the classical region the ratio should be the same as was calculated long ago by Brueckner and Goldberger. The resonant term is extremely important and in this region the ratio should be approximately one. In the region where the experiments have been done one does obtain a ratio of approximately one. He feels, however, the situation is complicated and that further analysis would be very desirable.

Yuan then reported on experiments done by himself and S. Lindenbaum at Brookhaven National Laboratory on multiple meson production. The meson beam used is the same 32° beam coming through the cosmotron shielding that was described yesterday in connection with pion scattering experiments in hydrogen. The beam was analyzed by momentum and the time of flight as before. A sketch of the relative, positive and negative production cross sections is given in Fig. 111 plotted against momentum of the pion in the laboratory system.

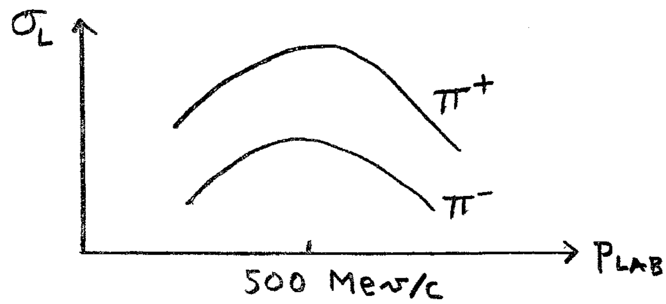


Fig. 111

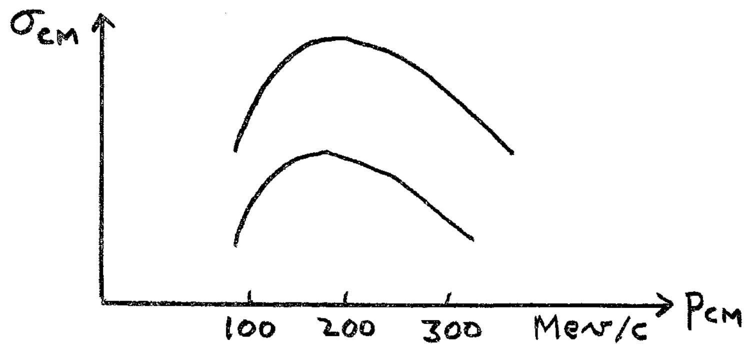


Fig. 112

This result has been corrected for μ decay. Momentum of the pions was obtained by wire measurements of the magnetic field and also by the range curve. If one assumed that the beryllium is light enough so that you are really seeing p-n interactions then it possible to transform these curves to the center of mass system and one then obtains the result in Fig. 112. The pions are emitted at approximately 90° in the center of mass system. The available energy in the center of mass system is 930 Mev. If these spectra are integrated to obtain the mean total energy it is found that this is approximately 300 Mev. Naively, one might assume that this is the result of 3 pion production. However, the mean energy goes down as the multiplicity goes up so instead a comparison is made with calculations done by Yang using the Fermi statistical theory. According to this theory the spectrum for two meson production would have the shape of the observed spectrum whereas that calculated for one meson production is much broader and the peak lies at much higher energies, which is completely inconsistent with the data. If the analysis is done on this basis, it would indicate 90% two meson production and 10% three meson production. Also the cloud chamber results on n-p meson production reported Monday at 1.7 Bev indicated a very large amount of double as opposed to single meson production. An argument against this interpretation, however, is that if the resonant state found in the low energy nucleon-meson scattering is extremely important, it could enhance the low energy end of the meson spectrum in a way that would tend to reproduce the observed curves. The positive to negative ratio as a function of momentum rise slightly with momentum of the pion and is at approximately 1.8 over most of the range. Having obtained these results it was thought that experiments at a lower energy would be likely to show more predominance of single meson production so that the same experiment was done with 1 Bev protons incident on the beryllium target. The resulting spectra in the laboratory are sketched in Fig. 113. These results are very preliminary and one has to be careful about proton contamination in the positive pion spectrum at the high energy end. It is seen that the positive to negative ratio is much higher than for the 2.2 Bev production reaching a value of about 4 at the peak of the spectrum. Again if these are transformed to the center of mass system where 440 Mev is available the spectra look approximately as in Fig. 114.

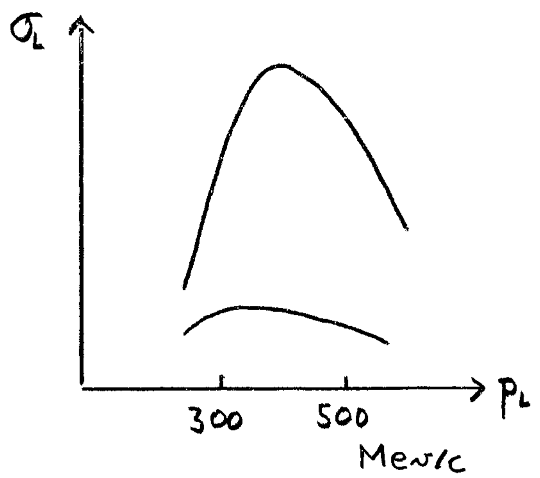


Fig. 113

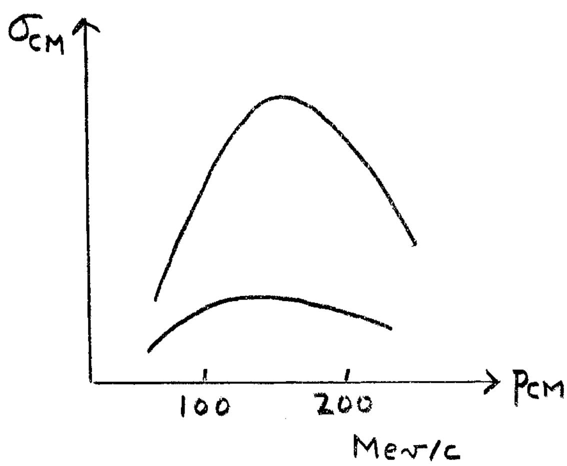


Fig. 114

The mean energy of the spectrum is 267 Mev which again favors production of low energy mesons. Although the cloud chamber data were obtained with a neutron beam whose mean energy was 1.7 Bev the energy spread of the neutrons was very great and this can be broken up into an approximately 2 Bev part and 1.46 Bev part. For the 2 Bev part one obtains a doubles to singles ratio of 4 to 1 as compared to a doubles to singles ratio of 3 to 1 at 1.46 Bev. This data also therefore supports the assumption of large double meson production at 2 Bev. The π^+/π^- ratio for the 1 Bev produced pions as a function of pion energy increases from a value of 4 to approximately a value of 7 as one ranges over the pion energy spectrum. Preliminary measurements have also been made of the π production spectrum at 1.8, 1.4, 1, .8 and .5 Bev proton energies. The relative cross sections as a function of pion momentum are indicated in Fig. 115. From this one obtains a preliminary excitation function which looks roughly as in Fig. 116. Fermi noted that it is somewhat risky to compare these results directly with the predictions of the statistical theory which applies to free nucleons when the experiments are actually made on beryllium. In the latter case there are lots of particles present to share the energy and, while this might not be important, if it did happen it would tend to lower the energy of the produced π mesons. Yuan said they were worried about this and were getting set to run a carbon polyethylene difference. The cloud chamber data is not yet sufficiently extensive to allow the plotting of an energy spectrum of the reduced pions.

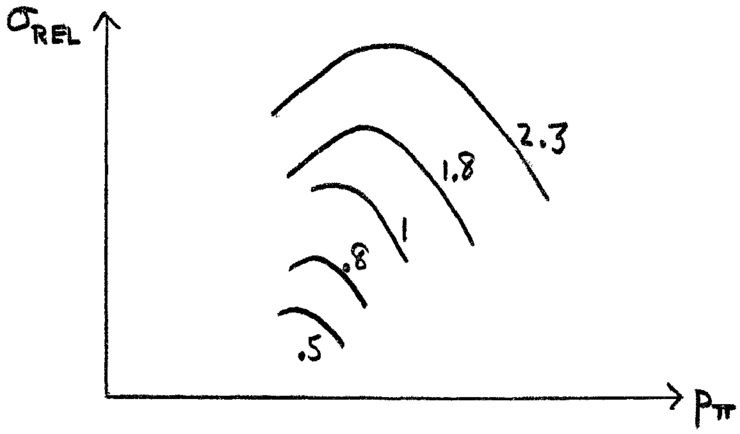


Fig. 115

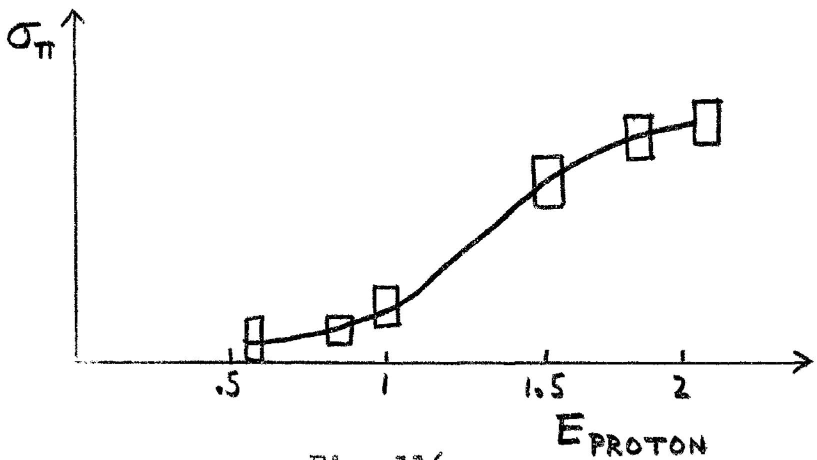
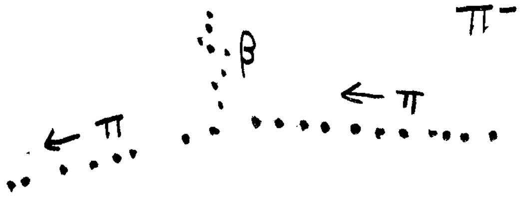


Fig. 116

W.D. Walker then reported on measurements that he, Crussard and Koshiba have made of interaction of 1.5 Bev pions in nuclear emulsion. They scan along the track and select "hydrogen interactions". At this high energy it is difficult to be sure how many of these are free proton interactions since most of the interactions turn out to be inelastic. Walker believes at least 50% of the interactions below are due to striking a proton on the edge of the nucleus, but feels that at this high an energy the Fermi energy of the protons in the nucleus probably does not distort things much. So far they have scanned 250 meters of track and obtained 60 "$\pi + p$" interactions. These fall into the following classes.

$\pi^- + p \longrightarrow$	$\pi^- + p$	14
	$\pi^- + \pi^0 + p$	33
	$\pi^- + \pi^+ + n$	22
	? - ends $\pi^0 + n$, $\pi^0 + \pi^0 + n$, $\Lambda^0 + \Theta^0$	24
	$\pi^+ + 2\pi^- + p$	2

They also find what are presumably $\pi^- + n$ collisions. These look like



$\pi^- + n \longrightarrow$	$\pi^- + n$, $\pi^- + \pi^0 + n$	25
	$\pi^- + \pi^- + p$	8

where the dotted line is a beta ray emitted by the residual nucleus. These fall in the classes given in the table so in these cases also it seems that π^0 production again predominates. The angular distribution of the protons in the elastic $\pi^- + p$ collisions are given in Fig. 117.

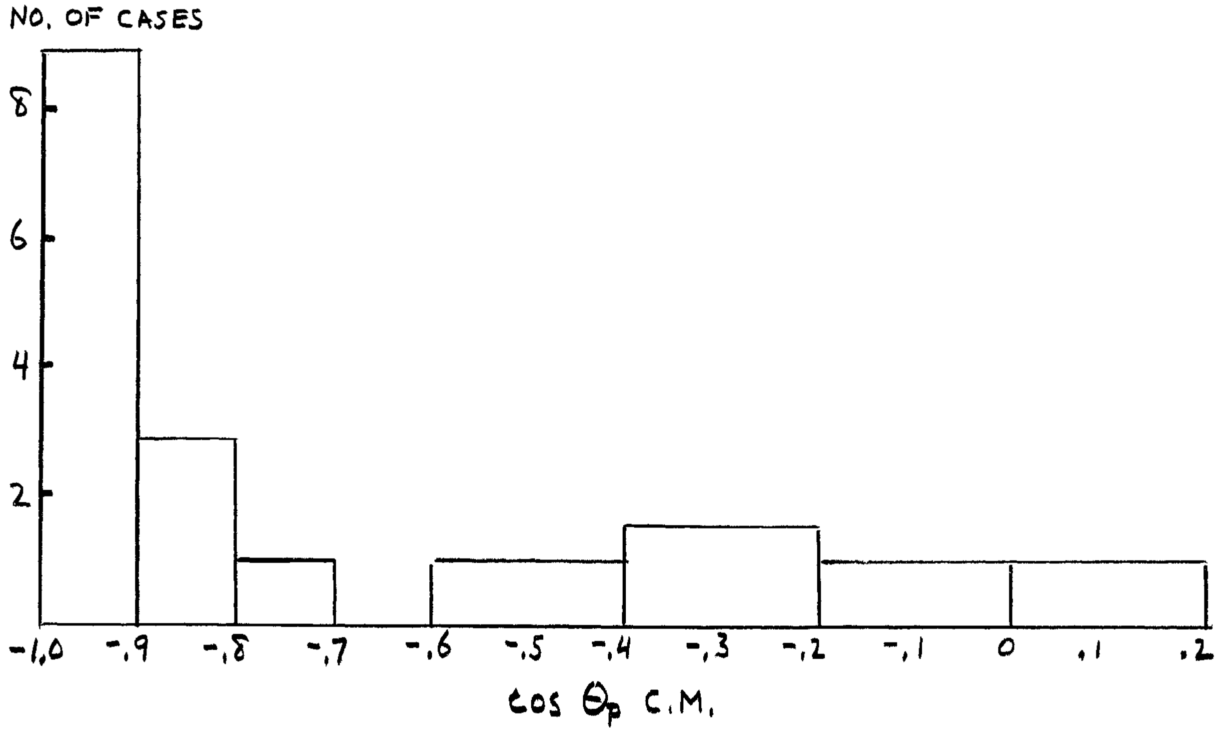


Fig. 117

Clearly, the tendency of the proton is very much to go backward in the center of mass system. That is, there is relatively little momentum transfer to the proton, the average scattering angle being only about 35°. The angular distribution of the protons when a π^0 is also produced is given in Fig. 118.

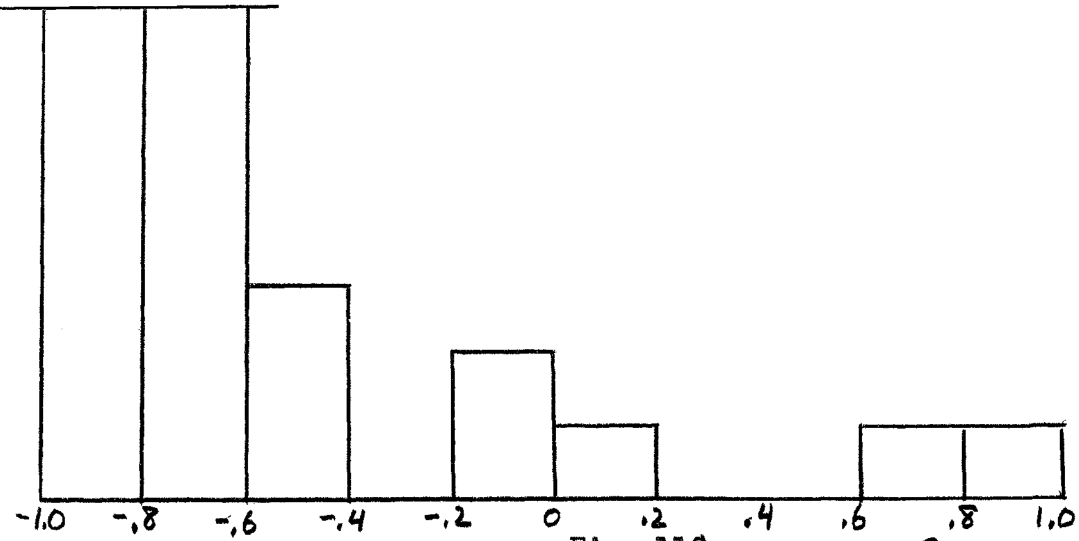
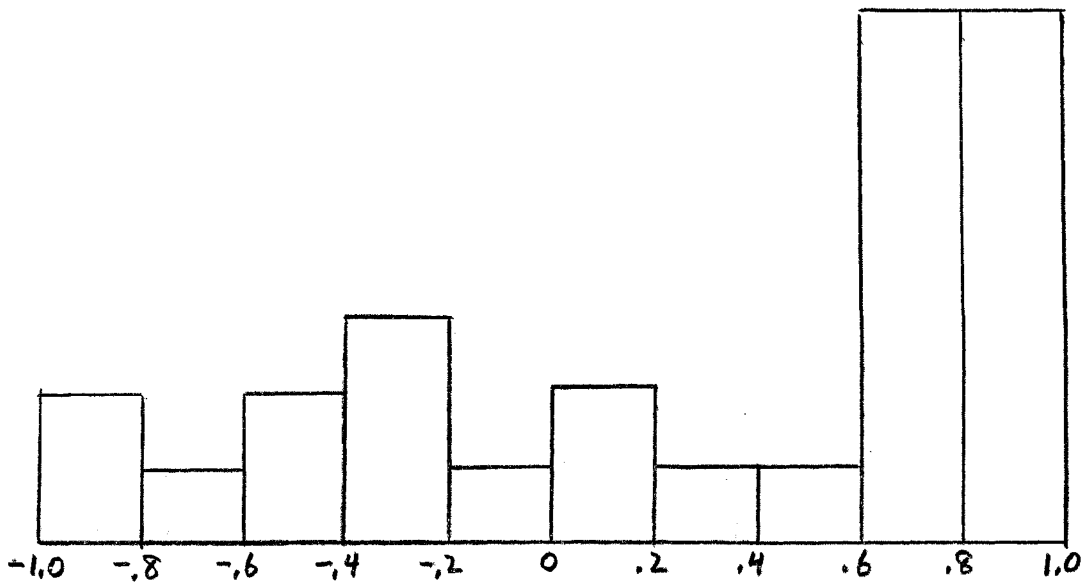


Fig. 118 cos θ C.M.

Again the protons tend to go into the backward hemisphere although the scattering angles are somewhat larger. The angular distribution of the π^- mesons in the same case is in Fig. 119.



cos θ C.M.

Fig. 119

The tendency is clearly to go into the forward hemisphere, that is, the π^- mesons also seem to remember where they came from. The π^0 angular distribution, however, as indicated in Fig. 120, is fairly uniform in the center of mass system.

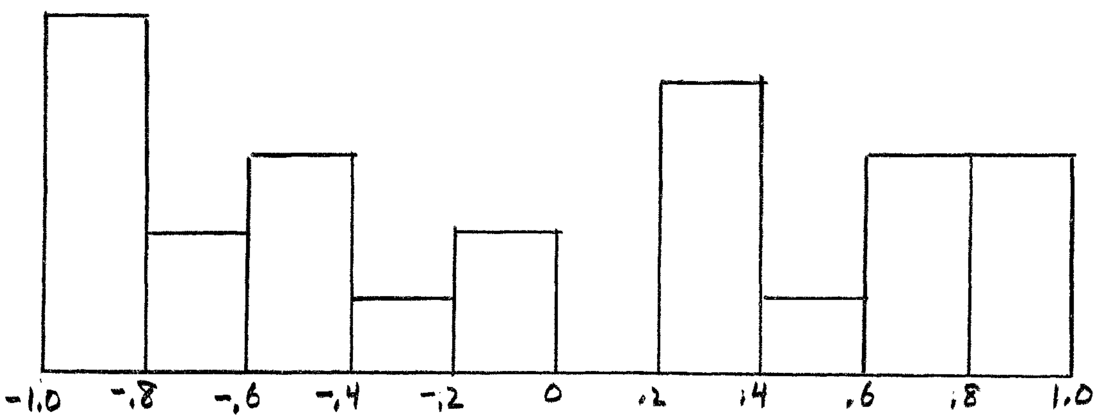


Fig. 120 cos θ_{π^0} C.M.

Fig. 121 gives the separation between the π^0 and the π^- directions.

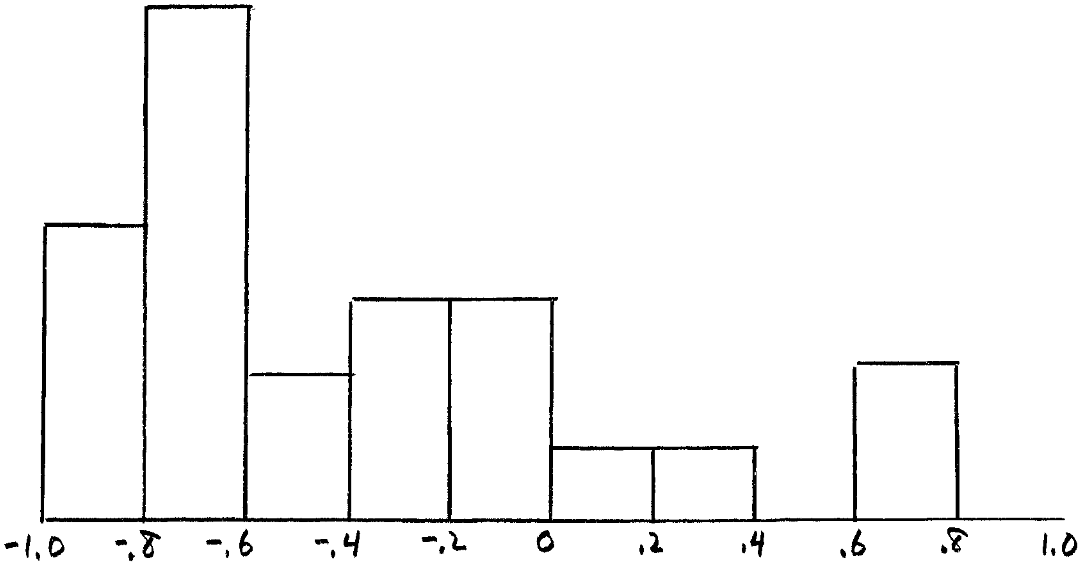


Fig. 121 $\cos\theta_{1-2}$

The tendency is for them to come out in opposite directions. Therefore, a typical π^0 production process would look like Fig. 122.

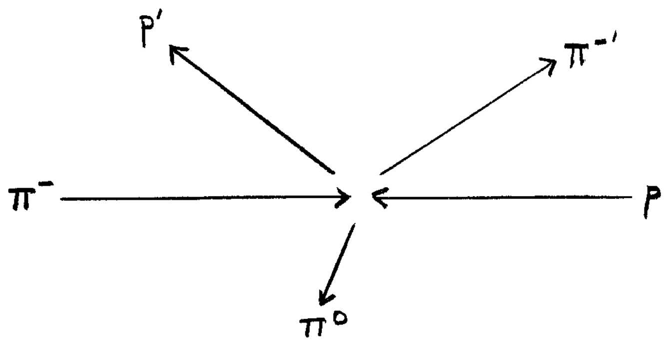


Fig. 122

The average momentum of the scattered proton is around 500 Mev/c, of the scattered π^- and of the produced π^0 around 450 Mev/c. A phase space calculation would seem to indicate that the average angle between π^- and π^0 would tend to be about 120° which is somewhat smaller than actually observed, though not very much. It would be necessary probably to go to lower energy in order to show up clearly whether or not this correlation is really strong. The mean free path and emulsion obtained from these experiments is 4 meters whereas from Piccioni's experiments one would expect a mean free path of about 9 meters. Bethe noted that the π^- energy is rather larger than the value which Yuan's spectrum would indicate.

Brueckner then talked briefly about the modifications of Fermi's statistical theory of meson production that one would expect if the particles after leaving the interaction region where the energy density and the temperature are high, interact with some long range force such as the ordinary nuclear force outside this volume during the separation. This effect is important if the wave function of the scattering of the particles at the edge of the volume has a large amplitude compared to its asymptotic value. One can easily make this correction by putting in just this factor and one finds that near threshold for n meson production the ratio of the probability of no deuteron formation to deuteron formation is given by

$$\frac{\text{Prob (no d)}}{\text{Prob (d)}} = \frac{4\pi\mu^{3/2}}{(2\pi)^3|\psi_D(0)|^2}\ \frac{1\cdot3\cdot5\cdots\cdot 2n-1}{2\cdot4\cdot6\cdots\cdot 2n+2}\ (E-n\mu)^{3/2}$$

If one calculates this as a function of the number of mesons one obtains

n =	1	2	3	4	5	6
$\frac{\mu^{3/2}}{(E-n\mu)^{3/2}}\frac{P(d)}{P(\text{no } d)}$ =	2.6	5.2	8.4	12.0	16.2	20.2

If one wishes to go further and assume the characteristic momentum dependence of the matrix element which the pseudoscalar theory gives for the meson production matrix element, then one obtains numbers which are three to four times larger for the small multiplicities of mesons and five to six times larger for the large multiplicities. Furthermore, two nucleons at low energy will again have a much larger wave function near the volume of interaction than they will have asymptotically which will also give effects of the same order of magnitude or larger. Thus it is easy to modify the weighting factors that appear in the statistical theory of Fermi by something like two to four orders of magnitude. (This closed the Wednesday session and the Conference.)

Appendix I: V-PARTICLE EXPERIMENTS AT IMPERIAL COLLEGE, LONDON AND AT MANCHESTER, C.C. Butler, Imperial College, London.

I wish to present a short summary of the V-particle experiments at London and Manchester under the general direction of Professor P.M.S. Blackett. Two small cloud chambers, one with a magnetic field of 7500 gauss and one with two lead plates but no magnetic field, have been in regular operation at the Pic-du-Midi in the French Pyrenees. A large cloud chamber in a magnetic field of 6000 gauss has been operating continuously on the Jungfraujoch in Switzerland; the group responsible for this work is led by Mr. J.A. Newth.

The principal results of the experiments which have been obtained during the last year are as follows:

1. Neutral V-particles

The Pic-du-Midi group have made a detailed study of the decays of V^0-particles using the magnet cloud chamber. Two main groups of particles are found, namely $\Lambda^0 \longrightarrow p^+ + \pi^- + (42^{+3}_{-2})$ Mev (c.f. Armenteros et al. 1953)
and $\theta^0 \longrightarrow \pi^+ + \pi^- + \sim 210$ Mev (c.f. Barker 1954)

The average Q-value of the Λ^0-particles is somewhat higher than the value of 37 Mev obtained by other groups who have made accurate measurements; the small difference, which is hardly significant, is probably due to residual systematic errors. Approximately 15 per cent of the decays of V^0-particles cannot be interpreted by the decay schemes of Λ^0- and θ^0-particles. Barker (1954) has