HIGH ENERGY

NUCLEAR PHYSICS

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FOREWORD

The Third Annual Conference on High Energy Nuclear Physics was held at the University of Rochester on December 18-20, 1952. For the first time, the National Science Foundation was a co-sponsor of the Annual Conference, joining with the group of Rochester industries which had furnished support for the first two conferences. The purpose of each year's conference is to assemble a representative group of active workers in the field of high energy physics for an informal discussion of the latest experimental and theoretical results. The Third Annual Conference was attended by well over one hundred physicists representing approximately fifty American and Foreign universities and research laboratories.

In previous years, a record of the conference deliberations was sent to participants and to a limited number of research workers in the high energy field. In view of the great number of topics discussed at the Third Annual Conference and the interest shown in earlier Proceedings, it was decided to make this year's Proceedings generally available at a nominal cost. Drs. Noyes, Camac and Walker are responsible respectively for the theoretical, "accelerator" and cosmic ray portions of the Proceedings, with Dr. Noyes also acting as general editor. Thanks are due the Gray Audograph Company and the IBM Company for supplying the recording equipment and typewriter respectively, Mrs. Helen Woodruff and Mrs. Bernice Skelly for typing the "pagemasters", Mr. Robert Trumeter for the printing job, and finally the Atomic Energy Commission and the Air Research and Development Command for their cooperation.

R. E. Marshak
Conference Chairman

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CHARGE INDEPENDENCE AND SATURATION OF NUCLEAR FORCES

Thursday morning, Professor E. P. Wigner presiding.

Marshak opened the conference by welcoming the conferees and stressing the informality of the sessions. Wigner started off by remarking that the purpose of the first session was to serve as an introduction to high energy physics and to make those of us who know only about low energy physics not to feel badly. He then gave a short historical introduction stating that the charge independence hypothesis originated in 1936 with the experimental work of Tuve et al. on proton-proton scattering followed by the analysis of Breit and Feenberg who showed that p-p scattering was very similar to n-p scattering in the singlet state. The consequences of these analyses for nuclear structure were first pointed out by Wigner through the first approximation which neglected the spin dependence the forces and any difference between the heavy particle interactions. This supermultiplet theory was improved in the second approximation by introducing the spin dependence, that is, the known difference between singlet and triplet scattering, since tensor forces were not yet known. It is now known that the second approximation possesses a substantial validity.

The extension of the charge independence hypothesis to the meson theory of nuclear forces was first carried out by Heitler and by Kemmer, but very little was done after the beginning. When the situation was reviewed by Wigner in 1942, he showed that the then existing experimental evidence was still inadequate to make any definite statements about the validity of the charge independence hypothesis. This situation persisted until new data on n-p and p-p scattering were available and a new method of analysis was developed by Breit, Landau and Smorodinsky, Bethe, and Blatt. Recently, there has been work on the inherent limitations of the theory; that is, even if the nuclear forces are in fact charge independent, the electrostatic forces which are also known to exist will influence the selection rules which are derived on the basis of charge independence.

Wigner then proposed four general topics for discussion: (1) What is the role in physics at large of such regularities as charge independence? He remarked that this is a very general subject, but is likely to come up again and again. Mainly, what should it mean that we have a kind of symmetry which is not complete? We have in fact another interaction which is similar in that the symmetry is also not complete, namely, the electrostatic interaction. Thus the exact equality of proton -proton and positron-positron forces which holds at large distances fails at short distances. This can be reformulated by stating that the proton-proton and positron-positron interaction are exactly alike insofar as they are transmitted by the electromagnetic field. Similarly, the hypothesis of charge independence for heavy particle interactions can be formulated by stating that they are exactly alike insofar as they are transmitted by the meson field. It is tempting to speculate what this means more generally. In this connection Wigner remarked that the term charge independence is most unfortunate since in fact it has nothing to do with charge. The proper name for this phenomenon is invariance with respect to rotations in isotopic spin space.

(2) Consequences for low energy nuclear phenomena; selection and intensity rules. Just as symmetry with respect to ordinary rotation has selection rule consequences for practically every process, for example, scattering, light emission, etc., similarly, invariance with respect to rotations in isotopic spin space has consequences for nearly every process. Some of these selection rules have been known for a long time, but others have been pointed out only relatively recently. Evidence for these selection and intensity rules comes from (a) nuclear reactions and alpha decay, (b) beta decay, (c) electromagnetic radiation, (d) stable states of nuclei, and (e) meson transitions. The last is a much larger subject than all the others put together and will be discussed in other sessions of the conference.

(3) Inherent limitations of the theory. There are two possible origins for such limitations. (a) The electrostatic interaction will introduce deviations. This is largely a theoretical subject, but to some extent practical in that in some cases the electrostatic interaction distorts the results to such degree as to give gross apparent contradictions to the basic hypothesis. These effects have been investigated by Thomas with regard to mirror nuclei, somewhat more theoretically by Tibarri and Radicati, and by the group at Princeton. (b) Complications in matrix elements on account of mesons. All selection and intensity rules are based on the assumption that we are calculating the matrix elements of an operator. The role of mesons is less simply described than that of electromagnetic radiation in atoms where, for example, dipole radiation is given by the matrix elements of x, y, and z, and higher multipoles by more complicated expressions. However, Jacobson and Wick have shown that this limitation is not relevant and that the selection rules are given correctly in spite of the complication of the matrix elements.

(4) Question of potential. That is, to what degree can low energy phenomena be described by a potential and by two particle interactions? In this connection we should discuss (a) Lévy's work, (b) general questions of saturation, and (c) "new fangled methods" of derivation of all of these rules, for example, as given by Van Hove. Wigner then called upon Christy to discuss topic 2 (a) that is, selection and intensity rules in nuclear reactions and alpha decay.

Christy started by stating that, as is well known, charge independence can be described in terms of isotopic spin wave functions for the neutron and the proton and the operators associated with the isotopic spin. Because of the fact that the isotopic spin matrices have identical commutation relations with the Pauli spin matrices, the selection rules for isotopic spin can be identified as being essentially the same as those one obtains for angular momentum. If the operator τ_z has eigenvalues -1 for a proton and 1 for a neutron, then the charge on the proton is described by the operator $e(1-\tau_z)/2$ and the charge on the neutron by the same operator. The x and y components of together with τ_z form a vector in isotopic spin space, but only the z component of this vector has a direct, simple physical interpretation in terms of total charge.

The first step in deriving selection rules for the isotopic spin is to identify the total isotopic spin T for various nuclear states. Just as the total angular momentum J can be determined by counting the number of levels into which a given state splits under an applied magnetic field, the coulomb field automatically splits states of different T. Therefore, we have to identify the number of different charge projections rather than the components of J along the z axis; that is, the number of different isobars in which a given nuclear state manifests itself is simply $(2T+1)$. This identification can be made with some assurance for the low energy levels of some light nuclei. For example, in alpha particle nuclei such as carbon and oxygen there are no corresponding isobars at low energies of excitation; therefore, since the multiplicity of all low energy levels of carbon and oxygen is 1, these levels must have isotopic spin $T=0$. In the case of $A=10$, that is, Be^{10}, B^{10}, and C^{10}, the difference between the ground states is only a few Mev. Again, the ground state of B^{10} has no counterpart and must have $T=0$, but the ground states of Be^{10} and C^{10} and an excited state of B^{10} at 174 Mev form a triplet with apparently corresponding properties and hence with $T=1$. The correspondence can readily be seen in any energy level diagram for the three nuclei where the coulomb corrections have been removed. For nuclei with half integral spin, for example Li^7 and Be^7, there are two nuclei with corresponding ground states and corresponding first excited states when coulomb energy corrections are made; there is also evidence for correspondences between states of higher excitation energy. The next isobars occur at 15 or 20 Mev excitation so that if there is charge independence one can say that all the low states have $T=1/2$. It is not always easy to make this sort of identification in all cases (e.g. when the levels are dense) without detailed measurements of the nuclear properties of the levels.

As we have seen, the selection rules we expect, follow in direct analogy with those for J. Thus, in any nuclear reaction between two particles, $T=T_1$ and $T=T_2$, the compound state will have $|T_1 - T_2| \leq T \leq T_1 + T_2$, and if this state breaks up into two nuclei of definite T, the same selection rules would apply. Unfortunately, in most cases this selection rule does not obviously exclude anything. This is true because in the cases where the levels are identified, that is, in light elements, the isotopic spins are 1/2, 0, 1, and if one of the reacting particles (e.g. a proton) has $T=1/2$, then all possibilities can exist. For example, a proton on Li^7 can give states of isotopic spin either 0 or 1 and there are no obvious selection rules. But it is possible to get exclusive rules in special reactions where $T=0$; for example, there are no corresponding n-n or p-p states to the deuteron which therefore has $T=0$, and the alpha particle also has $T=0$, so that when either is used the isotopic spin of the nucleus cannot change. Hence in the reaction $O^{16}(d,\alpha)N^{14}$, strict selection rules may appear. One must be careful because the simple fact that a reaction does not happen is not evidence for a particular selection rule unless it is known certainly that there is no other reason for the reaction not occuring. In this reaction O^{16}, the deuteron, and the alpha particle all have $T=0$ so that we conclude that only $T=0$ states of N^{14} can be formed. It is possible to test this prediction since both 0 and I states of N^{14} are known. Most of the excited states of N^{14} have $T=0$ with an occasional state of $T=1$; hence, the working of this selection rule is sufficient to explain the fact that N^{14} can be made in its ground state and certain other states but is not made in a T = 1 state. That is, particle groups corresponding to the first T = 1 level are weak by at least a factor of 100, and how much more is not known. At this point, Serber commented that this reaction can be explained in terms of a weaker selection rule than the full charge independence hypothesis (cf. discussion by Kroll below). Christy went on to remark that also in the inelastic scattering of deuterons by B^{10} the first isotopic spin state 1 is not formed, which again is plausibly explained in terms of the constancy of the isotopic spin.

Christy noted that selection rules can also appear in the emission and absorption of electromagnetic radiation, if one assumes that the coupling between nuclear particles and the electromagnetic field is by virtue of the charge on the proton, i.e. represented by the operator $(1 - \tau_z)$. The selection rules for this operator are the selection rules for the component of a vector, that is, in strict analogy to the well known selection rules for electric dipole radiation, namely, $\Delta T = \pm 1$, or 0. The proviso that the coupling is only to the charge of the proton ignores all complications due to the electromagnetic properties of the virtual meson clouds surrounding the nucleon. For electric dipole radiation there is a further restriction. Ordinarily an operator which is a component of a vector allows no zero-zero transition but the charge operator $e(1 - \tau_z)/2$ has a constant term as well as the component of a vector. However, in the special case of electric dipole radiation this term contributes a matrix element proportional to the summation over the nucleons of $r_i/2$, which is the position of the center of mass of the nucleons, which is fixed and does not radiate; hence $T = 0 \rightarrow T = 0$ transitions are forbidden for electric dipole radiation. Unfortunately, there is one well known exception to this selection rule, namely, the gamma ray transitions in O^{16}. The levels have been definitely identified by angular correlation experiments, and there is in fact a gamma ray transition from the state of J = 1 and negative parity to a state of J = 0; this must be an electric dipole transition and the low states in oxygen are presumably T = 0. However, the relevant fact that needs to be shown is whether or not this transition is anomalously long for electric dipole radiation; this has not been measured but is conceivably observable. Evidence might also be obtainable from the competition with transitions to other states, but this evidence is at present unavailable. In most cases, the low states of a nucleus involve a change in isotopic spin so that the above selection rule does not operate. Alpha decay clearly gives no change in isotopic spin for an allowed transition. In the case of beta decay, the Fermi selection rules are given by the operators τ_+ or τ_- which convert a neutron into a proton or a proton into a neutron. These are linear combinations of components of the vector τ, but the summation of τ_+ or τ_- over all the nucleons commutes with the isotopic spin operator; hence, for Fermi's selection rules $\Delta T = 0$. However, for Gamow-Teller selection rules, the operator is a summation over the nucleons of $\sigma_i \tau_i$. Since this weights the various nucleons differently, one has only the selection rules for the component of a vector, namely, $\Delta T = \pm 1$, 0. Usually, this gives no check because when T changes by one unit, as for example in the transition from He^6 to Li^6 the spin also changes by one unit and we know that we must use Gamow-Teller selection rules. Conversely, in the cases where the spin does not change, for example, in the transition from Be^7 to Li^7, the isotopic spin also does not change.

Wigner commented that it seems pretty far fetched to talk about the connection between p-p and p-n forces and then talk about rotation in isotopic spin space. In this connection it is well to recall Slater's work on atomic spectra, where it became apparent that if there was no spin it would be a good thing to invent it in order to express the Pauli principle; there is no isotopic spin, but it is a good thing to invent it in order to express in a mathematical way the regularities that have been mentioned. He would also like to state that it would be very helpful to have a new Condon and Shortley written on the subject. Of course, the theoretical physicist says "I know the selection rules for isotopic spin operators because I know them for the spin operators". But it would be nice to have rules written up, and we are very far from this. For example, electric dipole [illegible]nsitions have the same matrix element as first forbidden beta transitions; hence one can calculate the matrix element of one from the other and so on. There is a whole slew of such regularities, and it would be very valuable to see if they can be checked. Finally, Wigner remarked that there never has been as much theoretical thinking done on a subject the experimental foundation of which was as inadequate as this one. (Laughter)

At this point, Breit raised the question of what experimental evidence there is that T is a good quantum number and where he would find calculations showing what wou[illegible] wrong if it were not. Christy's statement that the best experimental evidence still comes from the elementary particle scattering was questioned (cf. discussion primarily by Blatt and Bethe below). Wigner emphasized the intensities of beta transitions, while Serber stressed the equality of energy levels. However, Serber said that one should not overstate the case from low energy experiments because of possible interpretation in terms of weaker selection rules (cf. Kroll's remarks below.) In response to a question from Wick as to what is meant by "low energy", Wigner attempted to say "where the interpretation is reasonably unambiguous" which provoked considerable laughter; he therefore qualified to regions where only S wave scattering occurs, namely, below 4.5 Mev.

Blatt objected that even 4.5 Mev may be too high, and described the situation with regard to scattering as follows: In the region below 2 Mev the scattering can be described by two parameters, namely, the scattering length and effective range. The scattering length is charge dependent so that any correspondence between scattering lengths can be stated only very roughly; further, there is no corresponding state in the proton-proton system to the triplet S state of the neutron-proton system. Hence there is really only one parameter to check charge independence, namely, whether or not the singlet effective ranges for the two systems are equal. In response to objections from the floor he countered that the charge corrections to the scattering length are not easy to make accurately but perhaps one might say that there is a second parameter. The situation with respect to the singlet effective range seemed dubious three or four years ago but by now the value for the proton-proton effective range is about 2.7 x 10^{-13} cm. as compared with the neutron-proton effective range of 2.5±0.3. The possible disagreement indicated by the Brookhaven data at 4 - 4.5 Mev is uncertain because at this energy the next term P in the expansion $k \operatorname{ctn}\delta = -1/a + r_0 k^2/2 + Pk^4 + \ldots$ comes in, and the P coefficient depends on the shape of the well. Blatt, therefore, concludes that the low energy evidence for charge independence is inadequate except for the corresponding levels in light nuclei. Breit's comment that Snow had obtained agreement with the Brookhaven experiment by using a repulsive core was restated by Blatt as equivalent to stating that such a model gives P 0, while P 0.15 does not give nearly as good agreement. However, the introduction of tensor forces without a repulsive core would reduce P to zero for Yukawa potentials. In response from a question from Jastrow as to how the situation differed from the analysis given by Salpeter, R. G. Sachs commented that Salpeter's analysis depended upon the neutron-proton capture cross section which really is not well enough known even theoretically to be used.

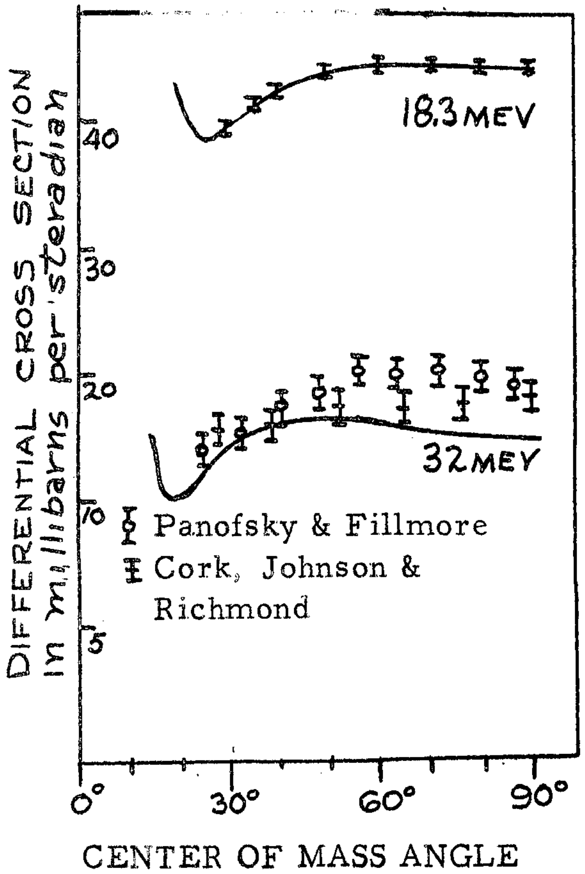
Bethe commented that the new experiments on scattering <u>are</u> more reliable than the capture cross section and made a positive and a negative remark. The positive remark was that it is still remarkable that the scattering lengths indicate potentials of equal strength to about 1%. The negative remark, made at the request of Salpeter, was that Schwinger has pointed out that the magnetic interaction is different in the neutron-proton and proton-proton system, and that this difference can account for the difference in scattering length. This, however, depends on the shape of the well, and works with the Yukawa potential essentially because two nucleons like to be close together in that case and the magnetic interaction for an S state, which is essentially a contact interaction, is therefore enhanced. It does not work for a square well because the wave function does not become so large at short distances, and it was found that Levy's repulsion at short distances will also depress the magnetic interaction. Oppenheimer commented that there are inherent limitations on charge symmetry and soon we will have to worry about the different electrical properties, dissociation of nucleons, and all the rest of it. That these effects can be big enough for some purposes we know from Schwinger's work. That we should be able to calculate them today, he would find very surprising.

At this point Pais offered to present a new calculation with Lévy's potential for the proton-proton system by two of his students, Martin and Verlet. However, Oppenheimer thought that a review of Lévy's work would be in order as "it is not completely clear from his papers, it is not completely clear to him, and not completely clear to anyone".

Pais, therefore, summarized Lévy's work as follows: With incredible faith Lévy says that he will investigate the symmetric pseudoscalar meson theory with pseudoscalar coupling, that is, the interaction $G\bar{\Psi}\gamma_5\tau_\alpha\Psi\phi_\alpha$. If you begin to play with this interaction and to orient yourself with regard to the constant G, you find that $G^2/4$ hc is of the order of magnitude 10. Then come the well known hesitations, since this orientation is obtained by calculating in a very low order. Then you say "what the hell, if I have a power series expansion and expand with respect to a parameter which is as large as this one, what can I believe of all this?" Lévy, in essence, looked at the nuclear forces following from the PS (PS) interaction taking the G^2 and G^4 terms into account. In this approximation one already finds a strong repulsive contact-like interaction which is smeared out by relativistic effects but is still very singular. He then makes a kind of guess, but it turns out to be very fruitful to follow up the consequences of this guess. He says, at small distances I have a very eminent history which tells me that I don't know what I'm talking about, and I have this very strong interaction which seems to be very dominant there. So I divide the distance into an inner and an outer region. I shall believe the specific shape given by the theory in the outside region and assume that I have a hard core inside. It is immediately obvious that this approach can only work at low energies, since at higher energies the more detailed structure of the interaction at small distances must be quite vital (Oppenheimer - "This is an understatement"). Lévy's claim for dropping terms higher than G^4 is that these terms will be important only in the inside region. Oppenheimer not[illegible]at this is not true of all terms since there are terms of an arbitrarily high order in G which occur as a multiplicative constant times Lévy's potential V_4, which he did not find out until after the calculation was completed. Therefore, his theory contains in fact three parameters rather than two, one of which is arbitrarily set equal to 1. Wentzel in fact has an argument to show that this constant should be considerably smaller than 1. At any rate, since the precise forms for V_2 and V_4 do not tell one much, Pais did not write[illegible]n down, but instead listed the parameters of the theory which are $G^2/4\pi = 9.7 \pm 1.3$ and $r_c = (0.38 \pm 0.03)\hbar/\mu c$. There are only two parameters since the meson mass is equal to the experimentally observed π meson mass in this theory. From these two parameters, the deuteron binding energy and the singlet scattering length, Lévy then fits the six numbers: the triplet effective range, the singlet effective range, the triplet scattering length, the singlet scattering length, the percentage of D state, and the quadrupole moment of the deuteron approximately. R. G. Sachs objected that two of these parameters are already essentially included by assuming the [illegible]ding energy of the deuteron and the zero energy singlet scattering length; further, the quadrupole moment is out by 20%, while a change of strength of the tensor force by a factor of 100 would only change the quadrupole moment by 10%, and the percentage D state is hardly known. Further, the[illegible]umption of the π experimental rest mass means that he is only working on a small correction to the effective ranges. Oppenheimer objected to the last statement because the large V_4 leads one to expect no a priori magnitude for the effective ranges. Bethe finds it remarkable t[illegible] a repulsive core which really corresponds to two mesons and has half the desired range still gives the right scattering. Blatt objected that it was a little unfair to say that the percentage D state was not at all a check, since when you change the tensor force, although you do not change the quadrupole moment very much you do get completely unreasonable D state admixtures, and one can argue that this quantity is known within the range of 1 to 8%, although not precisely.

Oppenheimer summarized the si[illegible]ation as follows: we could argue a great deal about the right percentage of D state. But, starting with a not unreasonable theoretical program and making only a finite number of mistakes, Lévy has obtained a better overall charge symmetric description over a wide range of energies than people who have been treating the problem empirically. He thinks that this is not without interest.

Pais then reported on the calculations of Martin and Verlet on the proton-proton scattering to be expected from the Lévy potential at 18.3 and 32 Mev. They calculate S, P and D phase shifts and obtain the agreement with experiment indicated in figure below. To obtain this agreement, they find that the original latitude in the coupling constant given by Lévy is too large and that in fact it must be chosen as 10.36 ± 0.02. The agreement at 18 Mev appears perfect although there are discrepancies of about 10% at 32 Mev, which is the order of magnitude of the discrepancies Lévy found in calculating the n-p scattering at 40 Mev. The potential and phase shifts are given in the table. (It should be stressed that the P and D phases are born approximatio ases obtained with Coulomb wave functions and therefore may well be completely misleading; cf. discussion by Wick, below.



P-P scattering from Levy potential as calculated by Martin and Verlet
N. B. P&D waves calculated in born approximation

Calculations of Martin and Ver

$$V(r) = \infty, \quad r < r_c$$

$$V(r) = V_c + S_{12} V_t \qquad r > r_c$$

where

$$V_c(r) = \frac{G^2}{\pi}\left(\frac{\mu}{2M}\right)^2 \left\{ \frac{(\vec{\tau}_1\cdot\vec{\tau}_2)(\vec{\sigma}_1\cdot\vec{\sigma}_2)}{3} \quad \frac{e^{-\mu r}}{r} \right\} - 3\left(\frac{G^2}{4\pi}\right)^2 \frac{1}{\mu r^2}$$

$$\times \left\{ \frac{2}{\pi} K_1(2\mu r) + \frac{\mu}{2M}\left[\frac{2}{\pi} K_1(\mu r)\right]^2 \right\}$$

$$V_t(r) = \frac{G^2}{4\pi}\left(\frac{\mu}{2M}\right)^2 \frac{\vec{\tau}_1\cdot\vec{\tau}_2}{3} \left[1 + \frac{3}{\mu r} + \frac{3}{(\mu r)^2}\right]\frac{e^{-\mu r}}{r}$$

$$\frac{G^2}{4\pi} = 10.36 \pm .02 \qquad r_c = 0.38\,\hbar/\mu c$$

Phase Shift	18 Mev	32 Mev
$^1S_0$	52.8°	44.85°
$^3P_0$	6.82°	11.79°
^3P	-1.45°	-1.62°
$^3P_2$	1.85°	3.79°
$^1D_2$	0.35°	0.97°

Certain objections were raised. In particular, Sachs wanted to know why the P wave at 4 Mev as measured at Wisconsin is so low and whether this is in agreement with Lévy's potential. Oppenheimer remarked that this is in fact a beautiful feature of Lévy's model. Thus the bucking of the core and the attractive potential tends to reduce the odd state phases, which is a gross effect that does not follow from a charge symmetric theory but does follow in this particular case. However, Jastrow admitted that this particular feature, which is characteristic of his model, also, although it is energy independent over wide regions, does not fail at very low or very high energies and hence that the low observed P phases at 4.5 Mev might prove to be a difficulty with the Lévy potential. At this point Wick questioned how precisely the phase shifts were calculated at 32 Mev. This brought out the point that in fact they were calculated from coulomb wave functions in born approximation. Wick considers this procedure extremely questionable since at 32 Mev he is almost certain that the $^3P_0$ phase shift is greater than 30°. He went on to add that this is in fact a typical feature of Lévy's potential, namely, the enormous attraction in the $^3P_0$ state, and that since the $^3P_0$ gives a very small front to back asymmetry, it may indeed be the qualitative reason for the flat angular dependence of the proton-proton scattering and at the same time of the symmetry about 90° of the neutron-proton scattering.

Breit remarked that R. M. Thaler and J. Bengston at Yale have made an analysis of n-p and p-p high energy scattering data which succeeds in giving good fits to experiment entirely without D waves but with S and three different ^3P waves. These fits have been made consistently with the hypothesis of charge independence. The existence of the fits shows that there are other ways of reconciling the hypothesis of charge independence with observation than those discussed in terms of potentials so far. Also, in connection with the discussion of the repulsive core potential he stated that approximate corrections for retardation to the nucleon-nucleon interaction have been worked out on the pseudoscalar theory. The effect increases slowly with energy at low energies but at 300 Mev the preliminary calculations indicate large corrections to the static values. It was suggested that the slowness of the increase of the corrections may be related to the success in fitting 30 Mev data by the Lévy-Jastrow potential which has been reported by Jastrow.

Kroll was asked at this point to explain his and Foldy's weaker selection rules which had been mentioned earlier. These selection rules follow from charge symmetry and do not require charge independence. From them, one finds that, for instance, the $0^{16}(d,\alpha)N^{14}$ reaction for which certain states are apparently forbidden is equally explicable assuming only charge symmetry and not charge independence; also the dipole transition in 0^{16}, ... is not in fact forbidden, would be just as strong evidence against charge symmetry as it ...d be against charge independence. This can be shown by considering any reaction of the type which, expressed in isotopic spin language, consists of the transition of two particles each with isotopic spin 0 to a set of two other particles one of which has isotopic spin 0 and the other possesses states of both isotopic spin 0 and isotopic spin 1. Since isotopic spin 0 implies equal numbers of neutrons and protons, it is clear that the initial state is self-conjugate with respect to an interchange of neutrons and protons. Consequently, the initial state can be characterized as symmetric or anti-symmetric with respect to such an interchange and if there should prove to be charge independence, the symmetric states have even isotopic spin while the anti-symmetric states have odd isotopic spin. However, even if T is not a good quantum number, transitions from symmetric to anti-symmetric states are still prohibited. Hence, the selection rules for all such reactions are the same whether one assumes charge symmetry or charge independence. Therefore, the only good experimental evidence from low energy region for charge independence is the existance of isotopic spin multiplets, that is, corresponding energy levels. Similarly the electric dipole operator is odd with respect to neutron-proton interchange and hence can only connect states of opposite charge parity. Since $T=0$ states all have even parity, again zero-zero transitions are forbidden.

Feenberg commented that the selection rule against dipole transitions is removed by taking into account the neutron-proton mass difference; hence, the selection rule merely reduces the probability of electrical dipole transitions by a factor of 10^6, which is not such a large factor for such transitions. Feynman asked whether the second order effect of the distortion of the wave functions due to coulomb forces was not a much bigger effect. Wigner replied that this has been calculated by Radicati and also at Princeton and in particular for 0^{16} this only gave a 10^{-3} effect in the transition probability.

R. G. Sachs: a comment on Christy's discussion made to him, but not to general meeting. It concerns the apparent violation of the isotopic spin selection rule $T=0 \rightarrow T=0$ forbidden for an electric dipole transition in 0^{16}. Feenberg remarked on the possible importance of the neutron-proton mass difference. There is an effect which seems to be of far greater importance. The selection rule arises as a direct consequence of the fact that the dipole moment can be expressed rather directly in terms of the pos... of the center of mass of the nucleons. ...ever, at an energy as high as that (7 Mev) associated with the O^{16} transition in question, the contribution of the magnetic quadrupole moment (sometimes referred to as a retardation term in the electric dipole moment) must be included, and this is not simply related to the coordinate of the center of mass. One can estimate (see Phys. Rev. 88, 824 (1952)) that the lifetime for the forbidden transition is of the order of $(Mc^2/Kw)^2$ times that of the allowed dipole transition, hence only some (2×10^4) times slower. It can be concluded that a lifetime measurement is essential for a test of the selection rule.

Fermi added that Telegdi experimentally finds in the photo-disintegration of C^{12} into three alpha particles that the 17 Mev level is relatively sharp, indicating a rather strong selection rule. Gell-Mann, arguing like Christy in terms of dipole transitions being forbidden for T=1 states, ties this fact into the isotopic spin multiplets of neighboring elements. It remains to investigate whether the intensity of this reaction bears out this interpretation. Wigner commented that although individual mirror nuclei beta transitions are evidence for charge symmetry only and not charge independence, the systematic trend of the ft values for such transitions would fail by a factor of 4 to agree with the experimental values if only charge symmetry and not charge independence was operative.

Feldman briefly presented the following implications of charge independence for high energy nucleon-nucleon scattering. His results are obtained in the scattering matrix formalism and hence are completely independent of any hypotheses about the nature of the interaction. There are p+p→p+p, n+p→n+p, and n+p→p+n (since the momenta and spins are specified) and two complex amplitudes (singlet and triplet) to describe them. Hence one gets in general restrictive inequalities only and not equalities relating the cross sections. There are three such inequalities; in the center of mass system they are: $\left[\sigma_{np}(\theta)\right]^{1/2} + \left[\sigma_{np}(\pi-\theta)\right]^{1/2} \geq \left[\sigma_{pp}(\theta)\right]^{1/2}$; $\left[\sigma_{pp}(\theta)\right]^{1/2} + \left[\sigma_{np}(\theta)\right]^{1/2} \geq \left[\sigma_{np}(\pi-\theta)\right]^{1/2}$;

and $\left[\sigma_{pp}(\theta)\right]^{1/2} + \left[\sigma_{np}(\pi-\theta)\right]^{1/2} \geq \left[\sigma_{np}(\theta)\right]^{1/2}$; which are also applicable if the incident nucleons are unpolarized. The second and third relations are not interesting because of the symmetry of the neutron-proton scattering about 90°; however, the first relation is of interest since it could be violated depending on whose experimental data you believe. This test is most critical, clearly at 90° where one must have $\sigma_{np}(90^\circ) \geq 1/4\, \sigma_{pp}(90^\circ)$. Thus, the Berkeley scattering data at 260 Mev gives $\sigma_{np}(90^\circ) = 1.3 \pm 0.2$ mb and $\sigma_{pp}(90^\circ) = 3.8 \pm 0.2$ mb, or a ratio $\left[\sigma_{pp}/\sigma_{np}\right](90^\circ) = 2.8 \pm 0.5$ which agrees with the charge independence inequality. However, if one takes $\sigma_{pp}(90^\circ) = 4.9 \pm 0.4$ mb as measured by Rochester or Harwell, then the ratio becomes 3.8 ± 0.6, which could violate the charge independence hypothesis. This emphasized the importance of precise measurements of $\sigma_{np}(90^\circ)$ and $\sigma_{pp}(90^\circ)$, particularly at high energies.

Weisskopf then presented a brief account of a preliminary investigation of the saturation problem of nuclear forces carried out by Drell and Huang, using Lévy's potential and Lévy's optimism. He expressed Lévy's potential as $V^{(2)} = V_2^{(2)} + V_4^{(2)}$. Here the superscript (2) denotes a two body force and i introduced because of the generalization to n body forces given below. V_2 is the exchange tensor force while V_4 is the ordinary repulsive force. Weisskopf remarked parenthetically that this potential is very nice since it throws light on a point which had always been puzzling until now. It had been n[illegible]ed that the effective range of the tensor force is greater than that of the central force, while the singularity given by meson theory always indicated a shorter effective range; this is now understood since V_4 contributes to the central force and its (repulsive) singularity cuts down the effective central force range. In Lévy's spirit, there are only two unknowns in this theory, namely, the core radius and the coupling constant; as in Lévy, radiative corrections are essentially dropped in higher order (by setting the unknown coefficient of V_4 equal to one.) It would be very difficult to derive the general $V^{(n)}$ but one can deduce the leading terms in analogy to Lévy's $V_4^{(2)}$. Both the form and the multiplicative constant of these potentials are given exactly within the framework of this program. They are of the form:

$$V_4^{(2)} = \lambda^2 \frac{K_1 (x_{12} + x_{21})}{x_{12}\, x_{21}}$$

$$V^{(3)} = \xi \lambda^2 \frac{K_1 (x_{12} + x_{23} + x_{31})}{x_{12}\, x_{23}\, x_{31}} \qquad \lambda = \frac{G^2}{4\pi} \frac{\mu}{2M}$$

The generalization to n body forces is obvious. (Wentzel remarked that he had given precisely this formula in a paper written ten years ago; Weisskopf granted this but added that they had merely calculated the constant ξ in front of this expression as given by the pseudoscalar theory). As had also been shown by Wentzel, the sign of these forces alternate as the number of particles increases. It is noted that the Lévy two-body force alone is even worse than the Serber exchange force with respect to saturation because of the large central force; the repulsive core is of such small volume as not to help, since if nuclei collapsed to this core, the densities would be very much greater than the observed nuclear densities. However, the repulsive three body force is sufficient to give saturation if the higher order forces, that is, the 4, 5 and body forces are neglected. Drell and Huang indicate that there is some reason to hope for the convergence of the series of multibody forces.

The saturation calculation is carried out in a primitive way just as it would have been done by Wigner in 1936 ("You permit me [illegible] :all this primitive?"). That is, the average values of the potentials are found using free particle wave functions averaged over the position of the particles taking account of the regions excluded by the cores. To escape all surface and electrostatic effects, the calculation is carried out for infinite nuclear matter and the resulting density found; a density of $\rho=1$ corresponds to the observed nuclear density. Only the two and three body forces are included, and of course the kinetic energy, with the hope that these will give a minimum at $\rho=1$. The probable convergence of the series of n body forces is due to the fact that it is very unlikely to find several particles within one another's ranges, because of the pauli principle, even if the cores are neglected. Exchange effects due to the exclusion principle are included but not exchange effects due to the exchange character of the forces. Since this calculation uses Lévy's constants and Levy'[illegible] optimism, there is nothing free and all is given. The result is shown in the figure below.

It is seen that a minimum does occur at ρ=1.1 and corresponds to an energy of 12 Mev as compared to the experimental value 14. This is too encouraging, as a great deal has been left out. It should be stressed that the core is not important for the many-particle problem, since, if two particles cannot get together, then neither can three. There are three points to be considered if one wishes to improve upon the above calculation: (1) Levy's optimism may not be justified, (2) we are suffering from an illusion if we say that we know the constants that have been inserted here because we do not know about convergence, and (3) imagine that everything goes fine. It is still possible that we may be just lucky. But there is still trouble with regard to the shell model. That is, although the cores are unimportant for the saturation problem once collapse due [illegible]o the 2 body potential is prevented, they are large enough to prevent the particles from moving freely in this infinite nuclear matter as would be required by the independent particle model. Therefore, one still has to investigate the problem of whether there is some mechanism that reduces the effect of the repulsive cores to zero in nuclear matter.

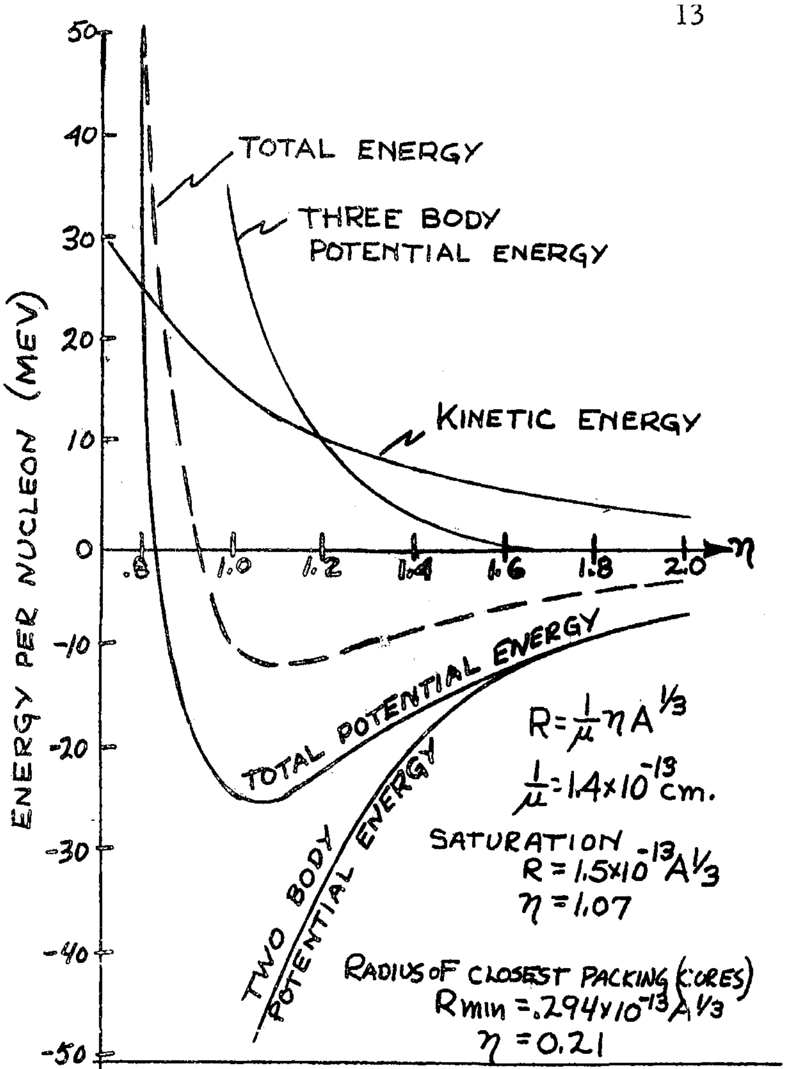
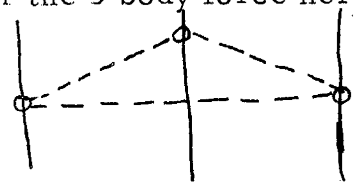


FIGURE 2
SATURATION of NUCLEAR FORCES
DRELL & HAUNG

Serber asked whether it had been investigated if such nuclear matter were [illegible] against the lining up of all the spins parallel due to the tensor forces. Weisskopf admitted that this has not been done [illegible]gh he thought it likely to be unimportant. Wentzel commented that the non[illegible]ange part of the problem can be done rigourously and has been done by him in a recent paper in Helvetica Physica Acta using calculations based on pair theory. By "exchange" is meant exchange terms associated with the energy, not exchange forces. Weisskopf commented that he was not yet sure, but it seemed [illegible] present that the exchange terms might be very important for the convergence of this procedure; that is, they subtract 15% for the 2 body forces, 35% for the 3 body force and about 65% for the 4 body force. The basic Feynman diagram for the 3 body force here considered [illegible] given below. The generalization to 4 and 5 particles has all sorts of combinations which must be summed over. Their contributions to the potential energy are presently being calculated.



Wigner closed the session by thanking everyone who had made a contribution and also those who had just listened.

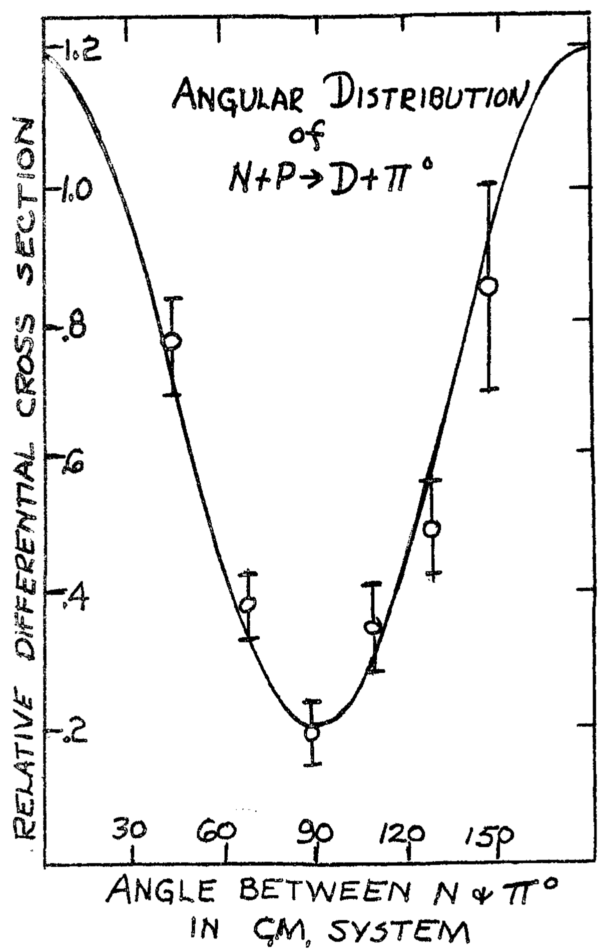
PION PRODUCTION AND PION-NUCLEON SCATTERING

Thursday afternoon, Professor Enrico Fermi presiding.

Fermi opened the session by indicating that the discussion would be divided into three parts which are indicated schematically by the following relations:

1) $\underline{N} + \underline{N} \rightarrow \pi + D$
2) $\gamma + P \rightarrow \pi + \underline{N}$
3) $\pi + P \rightarrow \pi + \underline{N}$, where $\underline{N}$ indicates a nucleon.

H. Anderson reported on the reaction $N+P \rightarrow \pi^0 + D$ as measured by Hildebrand at Chicago. At last year's Rochester conference considerable interest was expressed in the conservation of isotopic spin I, as a principle governing pion production and pion-nucleon scattering. It was pointed out by Yang and others that the reactions $P+P \rightarrow \pi^+ + D$ and $N+P \rightarrow \pi^0 + D$ are complementary. Note that on the right hand side of these equations the isotopic spin of the deuteron is 0 and the isotopic spin of the π meson is 1; thus, the total isotopic spin is 1. On the left hand side the PP system has only isotopic spin 1, while the NP system possesses both isotopic spin 1 and 0. The neutron-proton isotopic spin wave functions are $1/\sqrt{2}$ (NP + PN) for I=1 and $1/\sqrt{2}$ (NP − PN) for I=0. Assuming conservation of isotopic spin, then, in a reaction resulting in a pion and a deuteron only the states for which I=1 are possible. The relation between the cross sections of these two reactions is therefore $\sigma(PP, \pi^+) = 2\ \sigma(NP, \pi^0)$ at a given energy for any angle.



The π^+ production was measured by Richman and Wilcox and later others at Berkeley. The inverse process $\pi^+ + D \rightarrow P+P$ was measured at Columbia by Durbin, Loar and Steinberger. The latter obtained the angular distribution $d\sigma/d\Omega = A\,(\cos^2\theta + 0.2)$ where the π meson energy in the CM system was 55 Mev. Hildebrand has measured the differential cross section for the reaction $N+P \rightarrow \pi^0 + D$. The experimental arrangement was as follows: a neutron beam with energy centering around 400 Mev struck a target. Carbon-hydrogen differences were taken. Both the deuteron and the π meson were observed. The deuteron was detected by scintillation detectors and the π meson was detected by a scheme first used by Panofsky, Steinberger and others. Two γ-ray detectors, each consisting of a lead radiator, a scintillation detector, and a Cerenkov counter define the direction and the energy of the π^0. Since we are dealing with a 2 body process, the neutron energy was determined completely for given angles of the π^0 and the deuteron detectors. The experiment was set up so that the π^0 energy in the center of mass system was 55 Mev. The results are shown in the above graph. The curve represents the Columbia results and the experimental points are Hildebrand's results. It is seen that the agreement is very good. Thus, we can conclude that the angular distribution for the π^+ and π^0 production are the same. At present the absolute cross section has not been measured but the results should be forthcoming soon.

H. Anderson then discussed the general relations imposed on pion production processes by the constancy of the isotopic spin. The relations for a given meson energy and angle integrating over all nucleon coordinates are:

1) $\sigma(PP,\pi^+) = T_{11}^2 + S_{11}^2$
2) $\sigma(PP,\pi^0) = T_{11}^2$
3) $\sigma(PN,\pi^+) = T_{11}^2 + T_{00}^2$
4) $\sigma(PN,\pi^0) = 1/2\,T_{00}^2 + 1/2\,S_{11}^2$
5) $\sigma(PN,\pi^-) = T_{11}^2 + T_{00}^2$
6) $\sigma(NN,\pi^0) = T_{11}^2$
7) $\sigma(NN,\pi^-) = T_{11}^2 + S_{11}^2$

The terms on the right hand sides of these equations are the isotopic spin matrix elements. The letters S and T refer to the charge of the two nucleons in the final state, and denote charge singlet and charge triplet states, respectively. The subscripts are the total isotopic spins of the initial and final states. [illegible]er the assumption of conservation of isotopic spin the two subscripts cannot be different. To see how these equations were derived let us consider Eq. 1 in more detail. It refers to the cross section for the reaction $P + P \rightarrow P + N + \pi^+$. The initial two-proton system has I=1 and the final state has I=2, 1, or 0 for the neutron-proton system in a charged triplet T state, and I=1 in a charged singlet S state; the latter includes the bound state of the deuteron. Applying the conservation of isotopic spin, only final states with I=1 are permitted; thus we get the two matrix elements T_{11} and S_{11}. Note that there are only three different matrix elements. There is also the advantage that T_{11}^2 which appears at several places is quite small near the threshold, while the others are large. In fact, at 350 Mev experiments at Berkeley show that this is so and give an indication what the size of these matrix elements should be. Equations 1, 4 and 5 are most accessible to experimental verification.

Feld inquired whether the similar angular distribution for the reactions $P + P \rightarrow \pi^+ + D$ and $P + N \rightarrow \pi^0 + D$ only indicates conservation of the ordinary angular momentum, and whether information on the conservation of total isotopic spin would only come from the factor 2 in the ratio of the absolute values of the cross sections. Anderson explained by considering the reaction $N + P \rightarrow \pi^0 + D$ in more detail. The initial NP system is a mixture of isotopic spins 1 and 0 while the final state has isotopic spin 1. Since the deuteron is in a singlet charge state, only the S_{11} matrix elements exist. If, however, there were no conservation of isotopic spin then the "S_{01}" matrix element would also exist. Since the initial isotopic spin states are different, we would expect different angular distributions. Because of the rather peculiar shape of the angular distribution, the fact that the two cross sections [illegible]gree strongly supports the concept of conservation of isotopic spin. But one experiment in itself is not a proof.

Blatt then asked to what extent could one obtain relations among the cross sections by just assuming charge symmetry and not charge independence. Charge symmetry assumes that π^0 goes into π^0, P goes into N, π^+ goes into π^- and conversely. For example, Eqs. 1 and 7 have the same matrix elements because of charge symmetry. However, the matrix elements for the reactions $N + P \rightarrow \pi^0 + D$ and $P + P \rightarrow \pi^+ + D$ are the same because of charge independence.

Van Hove then gave a further discussion of the relations between the cross sections for pion production in nucleon-nucleon collisions. Let us consider the reactions just treated by Anderson and the relations between their cross sections assuming conservation of isotopic spin. It should be noted that when differential cross sections are considered, in many cases interference terms arise. Assume that two nucleons, indicated by the numbers 1 and 2, collide and produce two nucleons, indicated by 1' and 2', and a pion. The case in which a deuteron is formed in the final state will not be considered. Assume that we have observed the directions and the spins of all these particles. We will obtain expressions for the cross sections for various charges for the initial and final particles, assuming that conservation of isotopic spin I is taken into account. The initial and final states are subdivided into states of given total isotopic spin and only states of given total I can be connected in the production experiments. For the initial state we enumerate the various isotopic spin wave functions corresponding to I=1 and 0 for the two nucleons. A similar procedure is used for the outgoing particles. There are three complex scattering amplitudes compatible with conservation of isotopic spin that connect the initial and final states. These will be the S_{11}, T_{00} and T_{11} just given by Anderson. An important point is that the states of the particles are completely specified by their directions and spins. Let us assume for example that two protons collide and produce a π^0. Thus the initial state is 1 = P and 2 = P and the final state is 1' = P, 2' = P and a π^0 meson. There is only one scheme for π^0 production. But if two colliding protons produce a π^+, then there are two ways to have an outgoing proton and an outgoing neutron. Namely, 1' = P and 2' = N, and 1' = N and 2' = P. For arbitrary directions in space, these two processes are completly different, and have different cross sections, e.g. see Eqs. 2 and 3 below. The difference is due to interference terms that have to be added to Anderson's formulae in order for them to apply to differential cross sections.

The following are the explicit expressions for the differential cross sections:

1) $\sigma(PP \rightarrow PP\pi^0) = 1/2 \left| C'_1 \right|^2$

2) $\sigma(PP \rightarrow NP\pi^+) = \left| \frac{-C_1}{\sqrt{2}} + \frac{C'_1}{2} \right|^2$

3) $\sigma(PP \rightarrow PN\pi^+) = \left| \frac{C_1}{\sqrt{2}} + \frac{C'_1}{2} \right|^2$

4) $\sigma(PN \rightarrow NN\pi^+) = \sigma(NP \rightarrow PP\pi^-) = \left| \frac{1}{\sqrt{6}} \; C_0 - 1/2 \, C'_1 \right|^2$

5) $\sigma(PN \rightarrow PP\pi^-) = \sigma(NP \rightarrow NN\pi^+) = \left| \frac{1}{\sqrt{6}} \; C_0 + 1/2 \, C'_1 \right|^2$

6) $\sigma(PN \rightarrow NP\pi^0) = 1/4 \left| \frac{1}{\sqrt{3}} \; C_0 - C_1 \right|^2$

7) $\sigma(PN \rightarrow PN\pi^0) = 1/4 \left| \frac{1}{\sqrt{3}} \; C_0 + C_1 \right|^2$

The production amplitudes used here compare to Anderson's notation as follows: $C_1 = S_{11}$, $C_1' = \sqrt{2}\, T_{11}$, and $C_0 = \sqrt{3}\, T$.

Note that Eqs. 2 and 3 differ only in the permutation of the P and N in the final state, which gives rise to a difference in sign of the interference term. It is the sum of such two cross sections which have to be integrated in order to obtain the total cross section as given by Anderson. The equality of the two cross sections both in Eqs. 4 and 5 is due to charge symmetry.

The three collision matrices representing the allowable isotopic spin transitions are functions of the spins and directions of the particles. In general, for a given set of conditions there are no further relations.

The problem is now reduced to the purely algebraic question of eliminating the complex numbers C_0, C_1, C_1' between the seven cross sections obtained. The cross sections are seen to depend on five real parameters: C_0, C_1, C_1', $\theta = \arg \frac{C_1}{C_0}$, and $\theta' = \arg \frac{C_1'}{C_0}$. Therefore, at least two relationships must exist between them as a consequence of charge independence. A convenient way of showing the two relations is with the following notation. Let the sum and difference of Eqs. 3 and 2 be σ_3 and $2\sigma_3'$, respectively; the sum and difference of Eqs. 5 and 4 be σ_1 and $2\sigma_1'$, respectively; and the sum and difference of Eqs. 7 and 6 be σ_2 and $2\sigma_2'$, respectively. Let Eq. 1 be denoted by σ_4. The two relations are of different types. One refers to the cross section averaged over all the different states and gives a relation involving the matrix elements for the total cross section. We get

$$\sigma_1 + \sigma_3 = 2(\sigma_2 + \sigma_4), \tag{8}$$

The other relation is obtained by taking the interference terms into account. This gives a relation between the phase differences of the complex conjugate quantities.

Let θ and θ' denote phase angles as defined by the following equations:

$$A_1 = \frac{\sigma_1'}{\sqrt{(\sigma_1 - \sigma_4)\,\sigma_4}} = \cos \theta' \tag{9}$$

$$A_2 = \frac{2\sigma_2'}{\sqrt{(\sigma_1 - \sigma_4)(\sigma_3 - \sigma_4)}} = \cos \theta \tag{10}$$

$$A_3 = \frac{\sigma_3'}{\sqrt{(\sigma_3 - \sigma_4)\,\sigma_4}} = \cos(\theta - \theta') \tag{11}$$

Eqs. 9, 10 and 11 can be written as the single relation

$$\cos^{-1} A_3 + \cos^{-1} A_1 = \cos^{-1} A_2 \tag{12}$$

The relation Eq. (8) has a clear meaning: if in an experiment having the same number of p-p and p-n collisions the total number of π^+, π^- and π^0 mesons production, say N_+, N_- and N_0, respectively, are measured at a fixed angle, then Eq. (8) means

$$2 N_0 = N_- + N_+. \tag{13}$$

This relation was given by Watson for more general processes. It is remarkable that when multiple meson production occurs the number of phase relations increase rapidly with the number of mesons produced, whereas no relation independent of Watson's seems ever to appear between the total cross sections. So far no simple interpretation has been given to the phase relations.

N. B. However, as Feldman has pointed out, it is important to realize that if unpolarized nucleon beams and/or targets are used, the phase relations are lost and only Watson's relationship remains.

Stevenson presented recent measurements on the reaction $p+p \rightarrow \pi^{+}+D$ by F. Crawford and M. Stevenson at Berkeley. A liquid H_2 target was bombarded with the external 335 Mev proton beam and an event was determined by a time coincidence between the resulting π^{+} meson and the deuteron. Counter telescopes were used for the particle detection and the particles were identified by their momentum and range. The two measured angles of the outgoing particles and the known momentum of the incident beam determine the momenta of both particles. The masses of the particles are then identified by their ranges. The measurements were extended to a minimum angle of 30° in the center of mass system corresponding to 2.5° for the deuteron counters and 15° for the meson counters in the laboratory system. The range curve of the deuterons, within the statistics, was flat. This is important since protons from the free neutron and proton final state can also register coincidence counts. For the case in which the neutron and proton have zero relative momentum the proton has 1/2 the range of the deuteron. Thus, most of the particles detected were deuterons. Fermi pointed out that the reason that no protons appear is probably due to the fact that they are produced in the three body final state and thus have little angular correlation with the meson.

The ...s section was determined as follows: the deuteron detector was moved over an angular interval of a few degrees and the integrated number of counts observed gave the cross section for that particular angle of the meson counter. The analyzed data up until the present time is shown in the following table:

Center of Mass Angle	$P+P \rightarrow \pi^{+} + D$ differential cross section
30°	$36.4 \pm 2.9 \times 10^{-30}$ cm²/ ster.
60°	$16.4 \pm 2.6 \times 10^{-30}$ cm²/ ster.
90°	$10.7 \pm 1.1 \times 10^{-30}$ cm²/ ster.

A least ...uare fit to the data gives the differential cross section in the C.M. syste...

$$\frac{d\sigma}{d\Omega} = 33 \pm 10\% \, (0.32 \pm 12\% + \cos^2 \theta) \times 10^{-30} \text{cm}^2/\text{ster.}$$

The π^{+} energy was 18 Mev in the center of mass system. The total ...oss section obtained is 2.7×10^{-28} cm².

Using detailed balancing one can determine the cross section for π^{+} absorption in deuterium. The differential cross section in the center of mass system for the reaction $\pi^{+}+D \rightarrow P+P$ is $\frac{d\sigma}{d\Omega} = 12.6 \pm 10\% \left(0.32 \pm 12\% + \cos^2\theta \right) \times 10^{-28}$ cm²/ ster.

The existing measurements of the cross section for reaction $\pi^+ + D \rightarrow P + P$ are listed in the following table.

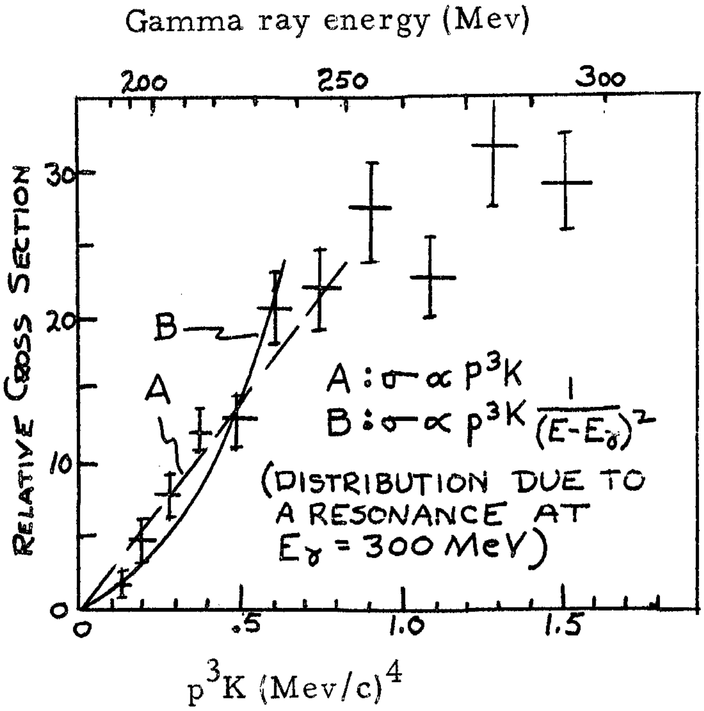
Laboratory	π^+ energy in C. M.	$\frac{d\sigma}{d\Omega}$ in 10^{-28} cm^2/ster.	Total σ in 10^{-27} cm^2
Berkeley	18 Mev	$12.6 \pm 10\% (0.32 \pm 12\% + \cos^2\theta)$	$5.5 \pm 10\%$
Columbia	25 Mev	$9 \; (0.22 + \cos^2\theta)$	$3.1 \pm 10\%$
Rochester	21 Mev		$4.5 \pm 18\%$ (assuming $0.4 \cos^2$)
Berkeley	21 Mev	$10.8 \pm 36\% (o.11 \pm 0.06 + \cos^2\theta)$	3.5 ± 1.2

- Marion Whitehead)

The laborious calculation of the correction for π-μ decay in flight has not been carried out. Note that the disagreement between the first mentioned Berkeley result and the Columbia result is outside the stated errors. Since the cross section is known to increase with energy, the Berkeley results would be even higher for the same meson energy as used at Columbia.

Chew then discussed the theoretical angular distribution of the reaction $p + p \rightarrow \pi^+ + D$ using the Lévy potential. The central force part of the Lévy potential is the same in both the singlet and triplet case. This has not always been true for all the phenomenological potentials that have been proposed. For the Lévy potential the short range attractive part is spin independent and thus gives the same central force for both states. The tensor force appears in the triplet but not in the singlet state. For this special kind of potential if the angular distribution for the reaction $p + p \rightarrow \pi^+ + D$ is calculated in the most naive way, that is, if it is assumed (1) that the meson is absorbed by either one or the other of the two nucleons but not in a three body process, and (2) that the matrix element is independent of the nucleon coordinates but depends only on the meson momentum, then you find that the angular distribution is uniquely predicted to be $(1 + 3 \cos^2\theta)$. The absolute cross section has not been calculated because it was too difficult. Brueckner said that the calculation does give the right order of magnitude for the cross section with a coupling constant of the usual magnitude.

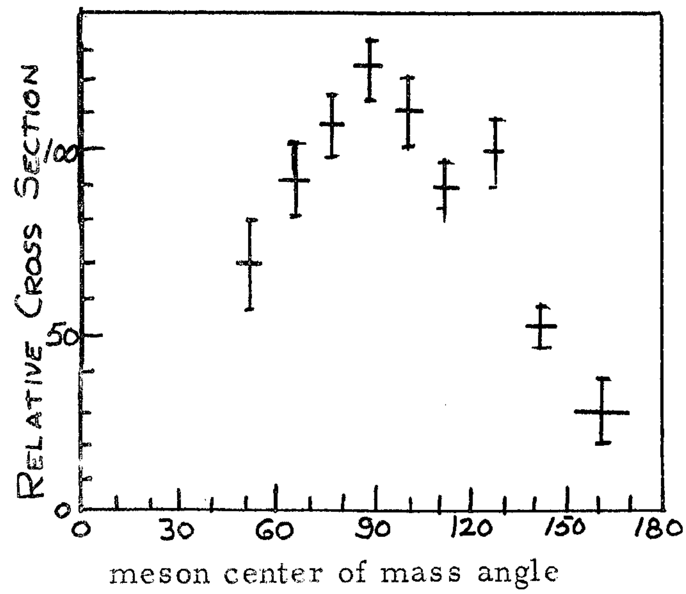
Goldschmidt-Clermont next presented experimental results on the reaction $\gamma + p \rightarrow \pi^0 + p$ obtained at M. I. T. The bremsstrahlung beam of the M. I. T. synchrotron produced π^0 mesons in an H_2 gas target. Both the range and angle of the recoil protons were measured in photographic emulsions. Since the reaction is a two-body process, these measurements give all the information required to determine the energy of the incident γ-ray and the energy and angle of the outgoing π^0. The experimental arrangement was as follows: The beam was collimated through two Pb collimators. It then passed through a long cylindrical tank containing H_2 gas at high pressure and at -60° C. Thin Al windo~~ were placed at the ends of the tank to minimize background. The photographic plates were placed in the middle of the tank at some distance from the beam and shielded from the main shower of electrons. Most of the protons observed came from the π^0 production process. Background of photo-protons from impurities in the gas was observed and subtracted out. The following graph is the excitation curve for the π^0 production. The cross section (ordinate) is in relative units because a good measurement of the γ-ray beam was not obtained. The



$\gamma + p \rightarrow p + \pi^{\circ}$
Excitation function

abscissa is in units of P^3K where P is the momentum of the π^0 and K is the energy of the γ-ray, both in the center of mass system. If the meson is emitted in a P state then this curve should be a straight line, at least near threshold. This would not be completely true if, according to Breuckner and Watson, a resonance existed at higher energy; then the curve should be concave up (Cf. discussion by Feld, Friday afternoon). To obtain the π^0 momentum in the C. M. system the rest mass of the π^0 must be assumed. The M. I. T. group assumed a series of masses and demanded that the excitation curve pass through the origin. This gave a measurement of the mass of the meson as 130 $\pm$ 10 Mev, in agreement with the better measurements of Panofsky and others as 135 $\pm$ 3 Mev.

The angular distribution is shown in the next graph. The curve is taken over all the meson energies. The M. I. T. group tried to separate the data for different γ-ray energies but within the statistics there was no difference.



$\gamma + P \rightarrow$ [illegible] ular Distribution

Assuming that the meson is emitted in a P state and taking the conservation of angular momentum and parity into account then the interaction of γ-ray with the nucleon has to be a magnetic dipole type. If, however, an intermediate nucleonic state is permitted, then this intermediate state can have total angular momentum 1/2 or 3/2. If it is 3/2, the angular distribution should be $(1 + 1.5\sin^2\theta)$. If it is 1/2, the angular distribution should be 1. There can also be interference terms between these two types. It is noted from the graph that the experimental points have symmetry about 90° in the center of mass system and can be fitted by the curve $A + B\sin^2\theta$. The best fit gives $B/A = 5 \pm 3$ which is consistent with the 3/2 state; however, it is not incompatible with a mixture. It should be noted that the weak coupling approximation gives the angular distribution of

$(1 + \cos^2\theta)$ (assuming the nucleon is at rest) which is excluded by the data. The measured angular distribution does not fit too well with the Berkeley data which was peaked forward. It is in agreement with the data of Silverman and Stearns and the recent work by Cocconi and Silverman.

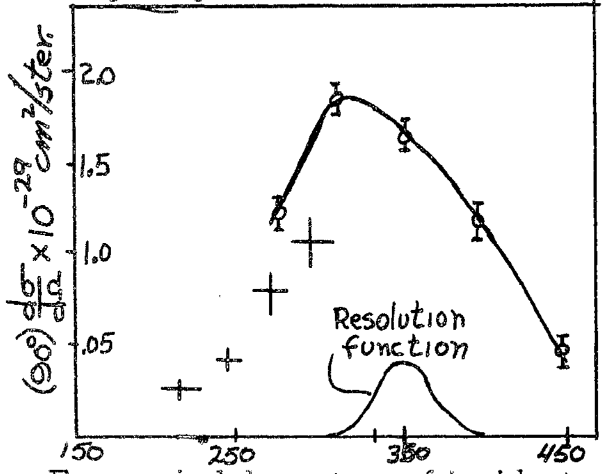
Breit asked if the M. I. T. data can or cannot be interpreted as evidence for the existence of a resonance. Goldschmidt-Clermont replied that Brueckner and Watson pointed out that from the scattering experiments one should expect a resonance. The resonance should be at about 310 to 320 Mev which is a little higher than the M. I. T. data goes. Brueckner remarked that if the scattering is strong in the angular momentum 3/2 state, one might expect the photo-effect to be explained without assuming a resonance.

Silverman reported on measurements of the reaction $\gamma + P \rightarrow P + \pi^0$ by Stearns, Cocconi and Silverman at Cornell. The measurements were carried out by observing one of the quanta. The incident γ-ray energies were essentially 280 Mev. The angular distribution agreed best with $(2 + 3\sin^2\theta)$, which is in agreement with the M. I. T. work. It is incompatible with the isotropic distribution or a $\sin^2\theta$ distribution. The excitation function was measured by measuring a time coincidence between the recoil proton and one of the π^0 quanta. Up to a laboratory γ-ray energy of 310 Mev or about 265 Mev in the C. M. system, there is no evidence of a flattening of the excitation curve. The cross section was expressed differently from the M. I. T. group. The excitation function varied as $(E_\gamma - E_{\pi^0})^{2 \pm 0.5}$ where E_γ is the γ-ray energy and E_{π^0} is the π^0 rest mass energy. It appears to rise faster than the P^3 found by the M. I. T. group.

Silverman also presented measurements of the reaction $\gamma + D \rightarrow N + P + \pi^0$ The ratio of the cross section for the π^0 photoproduction from deuterons and protons is 2.0 ± 0.2 at all energies and all angles measured. The cross section for deuterium includes both of the reactions $\gamma + D \rightarrow N + P + \pi^0$ and $\gamma + D \rightarrow D + \pi^0$. It appears that the neutron is contributing as much as the proton to the π^0 production.

Bacher reported on the measurement of the cross section for the reaction $\gamma + P \rightarrow P + \pi^0$ by R. Walker at Cal. Tech. The bremsstrahlung beam from the Cal. Tech. synchrotron was used. π^0 production from hydrogen was obtained by taking a difference measurement on C and CH_2 targets. Both the recoil proton and one of the π^0 quanta were observed with scintillation counters. The proton counter was at 32° and the quantum counter at 90° for all the measurements. The range of the proton was also measured. Thus the energy of the incident γ-ray and the outgoing π^0 were calculable. The process was pinned down by a time coincidence between the photon and the proton counters. The energy resolution was not too good in these preliminary measurements, being about 40 Mev full width at half maximum (see the resolution function on the graph below). The excitation curve is shown in the following graph. The differential cross section at 90° is plotted as the ordinate. The crosses are the data of Stearns and Silverman, while the other points are the Cal. Tech. results. There is essential

Differential cross section at 90^{o} for photoproduction of π^{0} mesons in H



Energy in lab system of incident photon

agreement at the overlap since the beam calibration was only roughly determined. The maximum of the curve is at about 315 Mev and by about 450 Mev the cross section drops by a factor of 4. At the 315 Mev peak the cross section is 2×10^{-29} cm^2/ster. There were several runs at two different synchrotron beam energies in order to check the effect of the bremsstrahlung spectrum, but no irregularities showed up. Each measurement was repeated 4 or 5 times.

There was some question as to the validity of the assumption that the π^0 C. M. angle would be the same for all the incident quantum energies since the positions of the counters were fixed. Christy pointed out that the π^0 C. M. angle had a small variation of about 10 or 15^{o} over the energy range of incident photons used. The efficiencies of the detectors as a function of energy was also taken into account. In answer to a question Bacher said that the $2\pi^0$ meson production was not looked for, but it should be possible to detect this process. In this preliminary work only the excitation curve for the one meson production was obtained.

Bethe discussed the theoretical angular distribution for the reaction $\gamma + P \rightarrow P + \pi^0$. It has been mentioned that the angular distribution for the $P_{3/2}$ state is $(\sin^2\theta + 2/3)$. If both p states contribute equally, the distribution is $\sin^2\theta$. Bethe has made a calculation using the phase shifts measured at Chicago and assuming that both p states contributed equally, i. e., the electromagnetic transition matrix elements are the same for both p states but differ in the phase shifts. In this case, at 135 Mev meson energy, he obtained the angular distribution $(\sin^2\theta + 1/4)$. Fermi asked Bethe what sign he assumed for the phase shifts. Bethe replied the theoretical sign, namely, positive phase shifts were used for the p state, but it does not make any difference here. Blatt asked does one expect a sum rule to exist for the photomesic production and if so can one estimate how much of the function has been used up by the Cal. Tech. data. Bethe replied that he does not know of a sum rule.

Goldwasser next presented experimental results obtained on the reaction $\gamma + p \rightarrow n + \pi^+$ by Bernardini and Goldwasser at Illinois. The experimental apparatus was as follows: the 200 Mev betatron beam was collimated to 1/2" diameter and struck a 1 1/2" diameter liquid H_2 target. G-5 emulsions were placed at various angles around the target 10" away and π^+ tracks were observed in the emulsion. Measurements were made of the relative yield for several angles from 30^{o} to 150^{o} in the laboratory system. This method is good down to 5 Mev meson energy in the laboratory system. The mesons were separated from the protons by grain counting and plural scattering. The energies of the mesons were obtained by grain counting. The calibration in each plate was made by measuring the range and scattering of mesons stopping in the emulsion. The results were analyzed only two days ago so they are very preliminary. The data are shown in the following table; observations were made at 5 C.M. angles.

Center of Mass Angles	39°	58°	92°	148°	158°
178-200 Mev	3.1	6.5	20.2	24.2	36.3
176-200	6.1	16	32.8	27.7	
$163\frac{1}{2}$ - 200	30.9	60.2	72.8		
165 - 175	19.7	39.8	36		

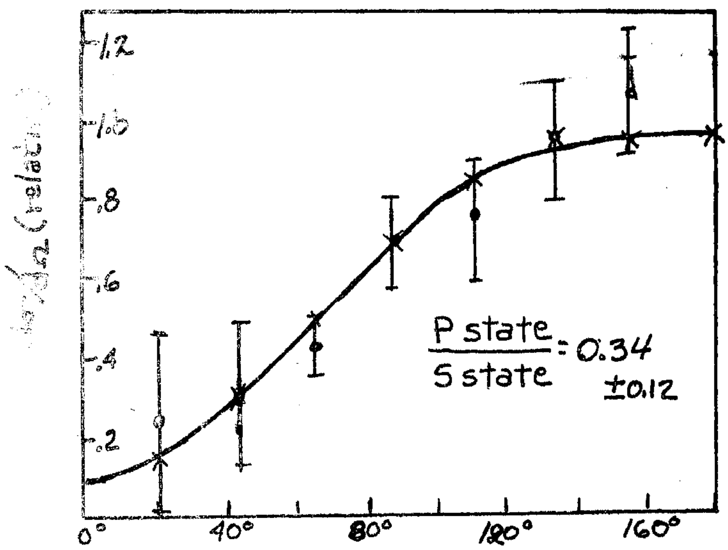
The data are expressed in energy "bins" for the various center of mass angles observed. Several normalizations had to be used but roughly the numbers in the bins are equal to the numbers of mesons observed. Mesons with less than 5 Mev energy cannot be detected with 100% efficiency so that there is a cut for mesons going the backward direction in the low energy bins. The plates were heavily loaded with electron background giving minimum ionization tracks It was thus difficult to see lightly ionizing fast mesons passing through the emulsion. Until better checks are made there is some uncertainty in the measurement of meson tracks with 2.0 or less times minimum ionization. It is possible, because of this uncertainty, that mesons are missed in the forward angles at 39° and 58° and in the 178 to 200 and 176 to 200 Mev bins. The other bins include, for the most part, mesons of greater than 2.0 minimum ionization and the detection efficiency is essentially 100%. The number in each bin has been normalized to the counting rate per unit solid angle in the C.M. system and has been corrected for the decay probability. However, the rows were not intercalibrated.

A few remarks about the angular distribution can be made. For the 178 to 200 Mev energy range, it appears that the cross section is increasing in the backward direction. For the 165 to 175 Mev row the distribution is flat around 90° with some drop at about 40°. The differential cross section at 90° for a photon energy in the lab system of 165 to 183 Mev was $d\sigma/d\Omega = 0.72 \times 10^{-29}$ cm²/ster. This cross section is based on 64 mesons observed in the emulsion and has a 13% statistical error. This result can be compared to the work of Steinberger. His data do not go down to these low energies but a straight line extrapolation of his data to the meson threshold gives a cross section 0.55×10^{-29} cm²/ster. The Cornell laboratory has checked the beam calibrations used at various other laboratories and Wilson reports that the Berkeley beam calibration differs from that of Illinois and Cornell by 25%. If the Steinberger-Bishop data is normalized to the Illinois-Cornell beam calibration, the extrapolation of their differential cross section at 90° and 175 Mev becomes 0.69×10^{-29} cm²/steradian.

There were several requests for conversion factors to express the numbers in the bins in terms of cross section per photon but Goldwasser said that they had not been determined yet. Fermi asked for information on the energy variation of the cross section. This was calculated by splitting the 64 mesons used for the 90° cross section into 2 bins from 165 to 175 Mev and from 175 to 183 Mev. The cross sections were 0.718 and 0.723, respectively; there is essentially no difference but the statistics are very poor. Fermi remarked that knowledge of the excitation function and angular distribution in the vicinity of the threshold might give a great deal of information concerning the structure of this phenomenon.

Osborne presented recent measurements on the reaction $\gamma + p \rightarrow N + \pi^+$ at M.I.T. The experiment is still in its preliminary stages. A characteristic feature of the synchrotron is the very strong electromagnetic background produced in the forward direction when the bremsstrahlung beam strikes the target. Special precautions have to be taken to make measurements at small angles with respect to the beam. For this particular experiment of π^+ meson production, the following procedure was used: a coil was wrapped around the synchrotron donut. A current pulse in the coil shifted the orbit of the electrons into the target, so that the X-ray beam came out in one microsecond. The counters were off during the beam pulse and turned on after the electromagnetic radiation had passed. The delayed μ^+-e decay from π^+ mesons that stopped in the detector were observed. The μ-e decay was checked by the characteristic lifetime and other tests. The π^+ production in hydrogen was obtained by a polyethelene-carbon subtraction. The detector consisted of a brass absorber and a scintillation detector which were sandwiched in front and back by anti-coincidence counters. The meson must stop in the sensitive counter and then produce a delayed electron pulse in this counter. Also, the electron must not cross an anti-coincidence counter. Since the π^+ stops in the detector, its range is measured and, thus, its energy can be calculated.

Angular distribution of π^+ mesons from gamma rays on protons ($h\nu = 270$ Mev)



Center of Mass angle

The results are shown below: the incident photon energy was 270 Mev. The angular distribution is in relative units because of the difficulty of determining the efficiency of the detector. Note that with this method a large range of angles are covered. All corrections except for nuclear absorption in the brass absorber were taken into account. The latter was only 15% at the worst angle and less at other angles. To interpret these results we can make use of the technique of partial wave analysis suggested by Brueckner. More generally, if we assume two possible electromagnetic interactions, electric dipole and magnetic dipole and a pseudoscalar meson, then by conservation of parity and angular momentum an outgoing meson will be in an S state and P state, respectively. The S state meson gives a spherically symmetrical angular distribution. Assuming that the P state is a pure $J = 3/2$ state, then there is one parameter for the ratio of the P state to the S state. Using the best fit to the data the ratio is 0.34 ± 0.12. The line drawn on the graph is for this ratio. Since the P state does not contribute very much, the $\cos\theta$ interference term is the main non-isotropic term This does not depend very much on whether the P state has $J = 1/2$ or $J = 3/2$.

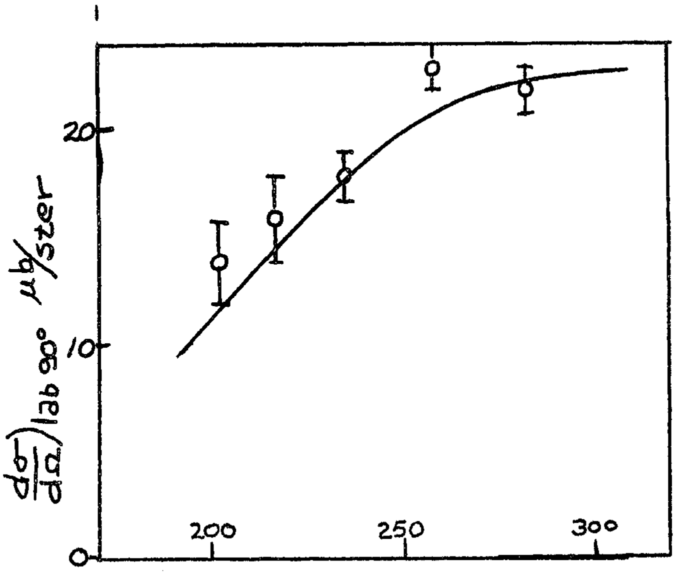
Breit asked what normalization this implies for the P state. Osborne replied that if one integrates over all angles one obtains $(1 + a^2)$ and $a^2 = 0.34 \pm 0.12$. The 1 is for the S state and the a^2 is the P state contribution; a $P_{3/2}$ state was assumed. The $\cos\theta$ coefficient changes somewhat if we assume the $P_{1/2}$ state. Bethe pointed out that there should be another relation if one assumes charge independence. Namely, for ... the P state part, the contribution to the π^+ cross section should be one half the contribution to the π^0 cross section. Osborne replied that this is verified experimentally. Using Steinberger and Bishop's measurements of π^+ production and Silverman and Stearns' π^0 production data, both at about 270 Mev, we find that within the relative beam calibration $\sigma(\gamma P, \pi^+) = 2\sigma(\gamma P, \pi^-)$. If the π^0 production goes exclusively through a photomagnetic $J = 3/2$ state, then as Bethe just pointed out the amount of the $J = 3/2$ state contribution to the π^+ cross section should be one half of that for the π^0, i.e., just looking at the $J = 3/2$ part, $\sigma_{3/2}(\gamma P, \pi^+) = 1/2\ \sigma_{3/2}(\gamma P, \pi^0)$. Since the total cross section measurements give $\sigma(\gamma P, \pi^+) = 2\sigma(\gamma P, \pi^0)$, the ratio of the P to S state must be of the order of 1/3 assuming that the S state makes up the difference and increases the π^+ yield over the π^0. They obtained a ratio of P to S of 0.34 in this experiment.

Brueckner pointed out that the above ratio follows only if you assume that the isotopic spin 3/2 state dominates. Osborne replied that he assumed the isotopic spin formalism which identifies the $T = 3/2$ with the P state. Brueckner said that if the $T = 1/2$ state dominates, there is no such simple ratio; you get additional factors. You have to make the assumption suggested by scattering that only the isotopic spin 3/2 is important, then the P state has to be assumed to be isotopic spin 3/2 in order to get the ratio 2 to 1. Feld said that if you turn this around and assume that it is pure isotopic spin 1/2, then the factor 2 would go the other way. As a matter of fact, this is just another bit of weight for the argument that the $J = 3/2$ state we are dealing with is a state of isotopic spin 3/2. Brueckner said that the same thing happens in ... on -nucleon scattering as Fermi and Yang have pointed out. For scattering in the $T = 3/2$ isotopic spin state, if you mix the $P_{1/2}$ and $P_{3/2}$ states you fit the angular distribution very well. The same is true for meson production. Osborne pointed out that his results are insensitive to the mixing. The square term is very insensitive to the mixture and the interference term always goes as $\cos\theta$. Brueckner said: "Actually, there is an additional argument which was proposed originally by Bethe to the effect that for the assignment of the phases of the electric dipole and the magnetic dipole transitions which comes directly from the form of the electromagnetic Hamiltonian in meson theory, the signs of the terms are of such a nature as to give the right result only if the $P_{3/2}$ phase shift is the large one and positive relative to the $P_{1/2}$." Bethe then said: "I am somewhat confused about my whole argument by now. (Laughter) I think now that the interference term indicates just the same thing that the scattering ought to indicate, namely, the weighted average between the $P_{3/2}$ and the $P_{1/2}$ phase shifts. As far as I can see now, we cannot decide between Yang's and Fermi's phase shifts from the photomesic production." Brueckner said that at any rate the meson theory does give the negative interference in the forward direction without any further assumptions about the process. In connection with the ratio of 2 to 1, it is interesting that if this ratio holds, the γ production of π^0 from neutrons and protons should be identical. This seems to be bourne out by experiments on photomesic production from deuterium.

Osbourne then pointed out that his results are not in bad agreement with Steinberger and Bishop; however, his errors are rather large. Steinberger and Bishop's data have a tendency to go down in the backward direction. The future plans at M. I. T. are to investigate the S and P contribution at lower energies.

Wilson reported on the measurements of the reaction $\gamma + p \rightarrow n + \pi^+$ by Palfrey, Luckey, Jenkins and Wilson at Cornell. To get a better check on the π^+ production an alternate method to the photographic plate and pulse beam techniques just described has been developed. A magnetic field was used to separate the mesons and determine their momentum. This method has the great advantage that the absolute cross section can be measured. The solid angle calibration was made with the familiar wire technique. The solid angle and energy resolution are known to 5%. The magnetic field can be flipped over so that the π^+ to π^- ratio can be measured directly. The system can be used also for all angles of emission of the π^+, that is, from 0^o to 180^o. For the forward direction the pulsed beam technique of the M. I. T. group has to be used in which the delayed μ electrons are counted. For the backward direction the γ-ray beam went through the apparatus. The π^+ mesons go back and are bent out of the beam. The magnet was of a double focusing type having a $\frac{1}{R^{1/2}}$ field which bent the mesons through an angle of 90^o. The mesons were detected by a time coincidence with proportional counters. Protons with the proper momentum would not have the range to get through the air and electrons were not observed at angles greater than 30^o. Electrons must have an energy of 170 Mev to have the same momentum as 50 Mev mesons. It is difficult for electrons of such energies to scatter through such large angles. This conclusion was checked with a cloud chamber and lead plates behind a similar magnetic system. It was concluded that only pions or their decay products come through the magnet.

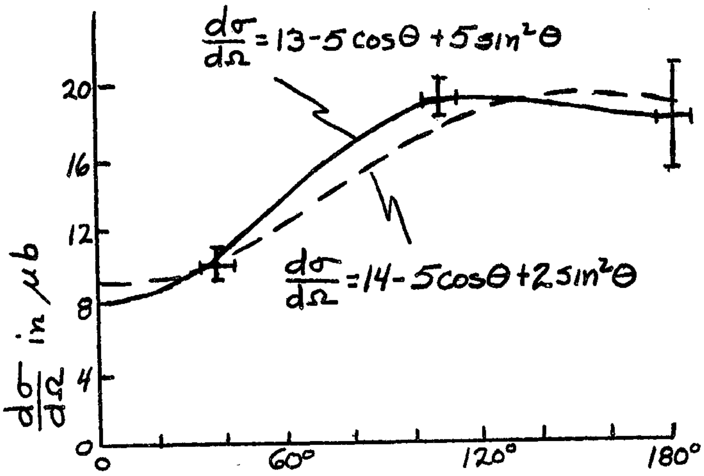
Since absolute cross sections were being measured, considerable attention was paid to the calibration of the beam, i. e., to the measurement of the number of quanta in the bremsstrahlung beam. The measurements of the pair cross section and the energy distribution of the γ-rays were made with a pair spectrometer. A standard ionization chamber with a thick copper absorber in front of it was calibrated in terms of the pair spectrometer measurements. At Cornell the absolute calibration of the beam is believed known to 5%. This standard chamber was sent around the country last summer to be intercalibrated with the other laboratories. Berkeley differs by about 25%; M. I. T. differs by about 30% in the opposite direction; Illinois agrees to about 1%; and Cal. Tech. uses the Cornell calibration. Now, even if all the labs are wrong, there is at least an intercalibration between them.



Incident Photon Lab Energy

The measurements were made as follows: the bremsstrahlung beam struck a 1" cylindrical target. The hydrogen cross section was obtained by a difference measurement between polyethylene and graphite targets. The beam was measured with the standard ionization chamber. The mesons were detected by proportional counters and their energy was determined by the magnetic field. The angle and energy of the meson completely determined the process. Measurements were made for targets of H, D, Be and higher Z. Only the H data is reported. The preliminary results on the π^+ production from H are shown by the experimental points on the above graph. The curve represents the results of Steinberger and Bishop. The agreement is good; however, there is a 25% discrepancy in the beam calibration which would push the curve up.

The angular distribution for 234 Mev γ-rays is shown in the next graph.



Center of Mass Angle

Measurements were made at three angles, 30°, 90° and 180° in the lab system corresponding to 36, 106 and 180° in the C.M. system. Preliminary results for the absolute differential cross sections at these angles are 10, 19 and 18 microbars per steradian. The last two points are essentially the same. Assuming only s and p states and that the p states go through the isotopic spin state 3/2, the cross section has the form $d\sigma/d\Omega = (a^2 + b^2) + 2ab\cos\theta\cos\delta + 3/2\, b^2 \sin^2\theta$. This will look more familiar if we remark that if there were no s state, as in neutral meson production, then the cross section would have the form $d\sigma/d\Omega = b^2 + 3/2\, b^2 \sin^2\theta$. The second term in the first expression is due to interference between a, the amplitude of the $s_{1/2}$ wave, and b cos Θ, the amplitude of the $p_{1/2}$ wave, δ being the phase between a and b. Fitting the formula to the three points we get $d\sigma/d\Omega = (13 \pm 2) - (5 \pm 2)\cos\theta + (5 \pm 3)\sin^2\theta$. This equation is shown by the solid curve in the accompanying figure. Alternatively we can use neutral meson production as measured by Silverman at Cornell, and evaluate b. Then for the last term we get $2\sin^2\theta$ instead of $(5 \pm 3)\sin^2\theta$, which is within the probable error. Assuming the neutral meson cross section is correct and using the three experimental points, a, b, and the phase angle δ can be computed. δ turns out to be -135°. The broken curve in the figure represents $d\sigma/d\Omega = 14 - 5\cos\theta + 2\sin^2\theta$ obtained by the above procedure, and we see that the fit is good.

The Cornell group also looked for negative mesons from H but did not see any. The maximum beam energy was 312 Mev. The π^- to π^+ ratio at 90° in the lab system and for 34 Mev mesons was 1 ± 4%. The γ beam energy was too low for meson pair production. However, if an isobar of a doubly charged proton was made, then a π^- might be produced. A considerable effort was made to look for this isobar.

Measurements on deuterium were also made. The ratio of π^- to π^+ cross section was the same at all angles and all energies observed. The ratio of π^- to π^+ was 1.3 ± 0.2. This ratio indicates that the meson production from the neutron is exactly the same as from the proton. Comparing the π^+ production from H and D, the yield from D is smaller by 20% ± 10%. This might be due to reabsorption of the mesons in the deuterium.

Fermi turned to a report on pion scattering from hydrogen by Anderson, Nagel and Fermi at Chicago. There are three types of pion scattering from hydrogen: (1) $\pi^+ + P \rightarrow \pi^+ + P$, (2) $\pi^- + P \rightarrow N + \pi^0$, and (3) $\pi^- + P \rightarrow \pi^- + P$, where the second phenomenon is measured by observing one of the two π^0 decay photons. Process (1) has the largest cross section, then (2), and (3) has the smallest. From the experimental point of view this order has very unpleasant practical consequences for the measurement of reaction (3). The trouble arises since the photon background going in the direction of the counter is in many cases of the order of ten times the number of π^-. The observed cross sections for reaction (3) determined by previous measurements were somewhat in error.

The experimental setup consisted of a π^- beam going into a liquid H target about 6" in diameter; leaving the target were a mixture of photons and π^- having an intensity ratio of about 10 to 1 or 10 to 2 depending upon the angle of emission. The usual measurements consisted of the detection of the π^- meson with two scintillation counters without any material interposed. To detect the π^0 photons, a lead converter was placed in front of the first counter. Unfortunately, even without the lead interposed there is some photon conversion, mainly from the walls of the hydrogen Dewar. A new set of measurements are being made which are an improvement over the old ones, primarily because the Dewar walls have been made thinner. Expressed in radiation units the Dewar walls are now one half as thick. Fermi pointed out another disturbing fact about the old hydrogen Dewar that still cannot be explained. When calibrating the equipment by a Panofsky-type experiment where the π^- mesons are stopped in hydrogen, they found a pair conversion at birth coming from the region of the Dewar of about 4% of the photons after the calculated effect of the Dewar walls were subtracted off. This is a few times larger than both the theoretical value and also Steinberger's measurements using a thin-walled Dewar. With the new Dewar the same experiment gives an understandable yield much lower than the 4%. So the new data on the π^- interaction with hydrogen looks more convincing. The cross section for the π^0 exchange scattering has not changed, but there is a difference in the scattering results (see table below).

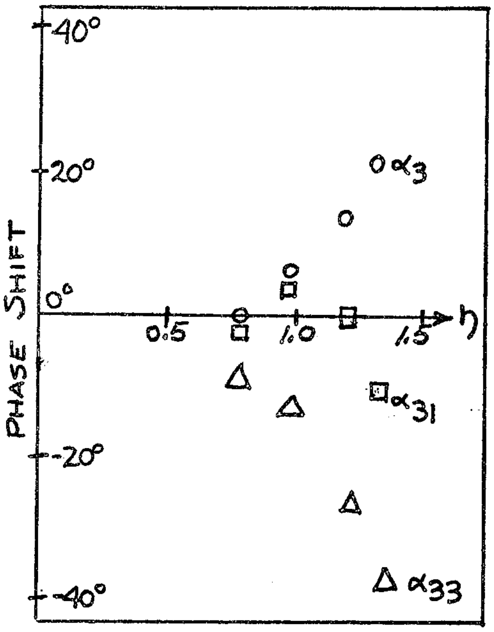
A characteristic feature to the π^+ scattering and the π^- exchange scattering is that they have a larger cross section in the backwards direction. However, the π^- elastic scattering cross section goes appreciably forward although not as much as the other reactions go backwards. This behavior can clearly be seen in the following table consisting of all the worthwhile measurements to date.

CHICAGO PION SCATTERING DATA

	$++$		$--$		$-\gamma$
78 Mev	$\eta = .973$				
54°	$1.96 \pm .33$				
102°	$2.26 \pm .31$				
143°	$3.09 \pm .34$				
110 Mev	$\eta = 1.18$				
55°	$3.3 \pm .7$				
103°	$5.1 \pm$				
144°	12.3 ± 1.0				
120 Mev	$\eta = 1.24$				
		55°	$1.02 \pm .15$	53°	$3.0 \pm .3$
		104°	$.42 \pm .15$	100°	$4.3 \pm .3$
		144°	$.88 \pm .20$	142°	$8.0 \pm .5$
135 Mev	$\eta = 1.325$				
56°	5.7 ± 2.2				
104°	6.8 ± 2.2				
145°	21.6 ± 3.6				
144 Mev	$\eta = 1.375$				
		56°	$1.63 \pm .15$	54°	$5.0 \pm .4$
		105°	$.64 \pm .15$	102°	$6.1 \pm .4$
		145°	$1.10 \pm .25$	143°	$10.5 \pm .7$

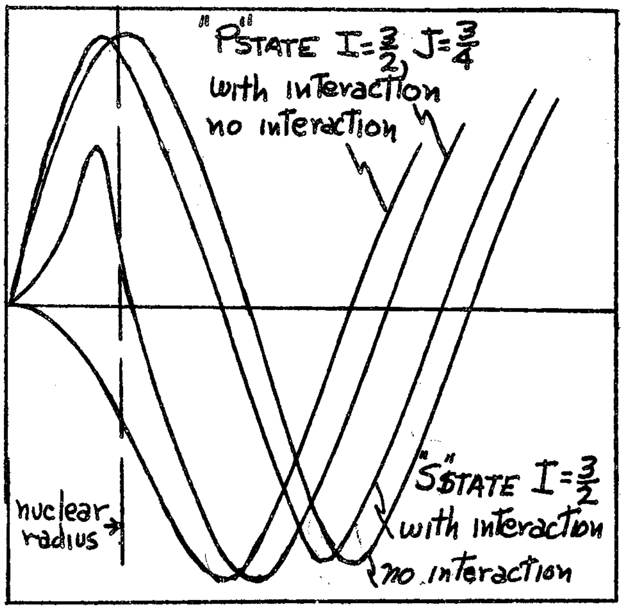
The data are expressed in millibarns per steradian in the C. M. system. The scattering angles are in the C. M. system. η is the meson momentum in units of μc. The heading $++$ stands for π^+ elastic scattering; notice the strong backward scattering. Also notice the similar behavior in the third column under the heading $-\gamma$ which stands for π^- charge exchange scattering. The new data is on the π^- elastic scattering and shows an increase in the forward direction.

These data can be correlated with the isotopic spin phase shifts. With the use of an electronic computer the phase shifts can be computed in five minutes, since there is one code for all calculations. With each calculation only taking about five minutes, one can learn something of the mathematics of the problem by varying the conditions a little. In particular, using the $++$ and $-\gamma$ measurements at three angles to compute the isotopic spin phase shifts, the phase shifts are then used to calculate the $--$ cross section. The results invariably want the $--$ cross section to look as they do experimentally. In this calculation only the S and P phase shifts are used. The following curves show how certain of the phase shifts depend upon energy. All the data are for the phase shifts calculated at Chicago as distinct from those of Yang.



Phase shift angles for I=3/2 in meson scattering from hydrogen

The points labelled α_3 correspont to I=3/2 and $S_{1/2}$. The ordinate scale is the meson momentum in the C.M. system and is expressed in units of μc. The points seem to fall on a curve. The meson energies are 135, 113, 78 and 55 Mev. The last point represents the Brookhaven data and has poor statistics. The curve through the points was theoretically suggested by Marshak. The points for I =3/2 and J=3/2 are denoted by α_3. They have a regular behavior and fit a curve varying with the cube of the momentum. This is the simplest law that one can expect for P level phase shifts. The points for I=3/2 and J=1/2 are denoted by α_{31} and do not show a regular beh vior. The data indicate that these points are small and slightly negative, but the experimental errors are too large.



Meson wave functions (energy 135 Mev)

The next figure shows a schematic representation of the meson wave function for 135 Mev incident energy. S waves with and without a perturbation of the nucleus are rather similar except for the phase shift which is of the order of 20°. The nucleon radius is conventially set at $\hbar/\mu c$ and the wave function goes to the origin by bending down sharper than a sine curve. The situation is different for the case of I=3/2 and J=3/2. The curves with and without nuclear interaction are two sine curves outside of the nucleon radius that have a separation of the measured phase shift. Inside the nucleon, however, the difference between the wave functions is very large even for a phase shift that is similar to that of the S wave.

Fermi pointed out that Yang's phase shifts agree with the data as well as the phase shifts computed at Chicago. These are compared in more detail in the Saturday morning session. There is hardly any difference for the S levels. The major difference is in the P states. There are probably no other solutions than these two. This limitation was suggested by the following calculation: 30 random samples of data were assumed and the phase shifts were computed, each set taking about 5 minutes with the electronic computer. The results fall into two minima corresponding to the phase shift analyses of Chicago and Yang.

There was another minimum with a very large value, so that it is meaningless experimentally. The + or - signs of the phases have not been determined. Brueckner asked whether the correct set of phases could be determined by a more accurate measurement. Fermi replied that this is probably so but if more accuracy is available then there is the added complication of the D and higher order phases. Anderson pointed out that the fact that there are two sets of phase shifts is due to the -γ process which can go either with or without spin flip for the proton. The uncertainty in the choice of phase shifts is due to the small contribution to the cross section from the spin flip part. Fermi pointed out that the two sets of phase shifts are distinctly different but in some respects quite close. For example, the S phases are roughly equal. Both sets of phase shifts appear to behave properly with energy. Probably a way to decide which set of phase shifts is correct is by interference methods.

Barnes reported on the π^+ and π^- transmission experiments in hydrogen C. Angell and J. Perry at Rochester. The energy of the π^+ and π^- meson beams was 37 ± 6 Mev. The targets were CH_2 and graphite. The hydrogen cross section was obtained by the usual subtraction technique. The data is shown in the following table.

Total cross sections for 37 Mev π^+ and π^- interactions with protons.

Reaction	Uncorrected data	Corrected data
π^+ transmission	16.2 ± 1 mb	21.5 mb
π^- transmission	17.2 ± 2	21.8
$\pi^- + P \rightarrow N + \pi^0$		5 ± 2
$\pi^- + P \rightarrow \pi^- + P$		16.8
$\pi^+ + P \rightarrow \pi^+ + P$		21.5

The first column lists the uncorrected data, that is, the raw data before correction on the basis of the efficiency curve. The second column lists the final data corrected by the efficiency curve and assuming an isotropic distribution. The first two lines represent the direct measurements of π^+ and π^- transmission in hydrogen. The third line is the data taken from Roberts, Spry and Tinlot on π^- charge exchange scattering at roughly the same energy (see next report). The cross section for π^- elastic scattering is given in the fourth line and was obtained by subtracting line three from line two. Finally, the last line contains the π^+ elastic scattering cross section and is simply the first line rewritten. The efficiency curve was obtained as follows: in any attenuation experiment there is an uncertainty in the maximum angle through which mesons can scatter and not be detected by the final crystal. For the angular region from 60° to 180° all scattered mesons miss the last detector. There is an angular region less than 60° where some mesons would miss and some would be detected depending upon the geometry of the apparatus. The overall average angular acceptance of the final crystal is determined by the efficiency curve. The final column containing corrected data assumes an isotropic distribution. However, even assuming a $\sin^2\theta$ or a $\cos^2\theta$ distribution, the cross section does not change by more than a couple of millibarns. It is surprising that the π^- elastic scattering cross section is so high at 37 Mev when it is about 3 millibarns at about 57 Mev.

Tinlot then reported on the π^- charge exchange scattering from hydrogen by Roberts, Spry and Tinlot at Rochester. The incident π^- beam had an energy from 35 to 45 Mev. This is slightly higher than in the pion transmission experiment just described by Barnes. The data were obtained by measuring the incident meson flux and one of the outgoing π^0 photons. Measurements were made on both H and D giving information on the reactions (1) $\pi^- + P \rightarrow \pi^0 + N$, (2) $\pi^+ + D \rightarrow \pi^0 + 2N$ and (3) $\pi^+ + D \rightarrow \pi^0 + 2P$. The experimental arrangement for the hydrogen measurement consisted of three scintillation counters to define the meson beam. The usual subtraction technique was used with carbon and polyethylene targets. A γ-ray telescope with Pb in front of it was placed at 90° with respect to the pion beam. An anti-coincident crystal was placed in front of the lead. Assuming that all the γ-rays observed were from π^0 decay, then the total charge exchange cross section is 5 ± 1.5 millibarns where the statistical inaccuracy alone was about 15%. In order to calculate the cross section, the γ-ray detector efficiency must be determined. This means using shower theory for incident γ-rays with an energy spread from 35 to 139 Mev. The theory of R. R. Wilson was used and the errors in this theory can only be estimated. We believe that this error plus errors due to beam contamination, etc., gives an overall uncertainty of 35%. It should be noted that for the meson energies used in this experiment, the γ-rays from the π^0 are essentially isotropic for even quite an anisotropic π^0 distribution. For example, for a $\cos^2\theta$ π^0 distribution, the isotropic part of the γ-ray distribution is 3/4. Thus, it is felt that this is a good measurement of the total cross section even though the γ-rays were measured at only one angle.

In order to detect both π^0 photons another counter was placed at 110° with respect to the first counter and still at 90° with respect to the incident beam. Unfortunately, the efficiency for 2 photon detection is very small. Thus the statistics are too poor to give a good measurement of the cross section. However, the results are compatible with the assumption of an isotropic distribution. Charge exchange scattering from deuterium with both π^+ and π^- mesons was also measured by detecting both the single and the double γ-rays. It is probably the first time that π^+ exchange scattering has been detected. The resulting cross section is about 1.5 ± 1 millibarns for the π^- and 1.5 ± 0.5 millibarns for the π^+ mesons.

The main interest in the results of this and the previous work reported by Barnes rests in the large variation of the ratio of the charge exchange to the elastic π^- scattering cross sections with respect to energy. It is suggested that the S phase shift α_3, as indicated by Marshak, does reverse its sign at about 40 Mev which brings the π^- elastic cross section up. The Brookhaven result is about 3 millibarns at 57 Mev while the Rochester result at 37 Mev is 17 millibarns (see previous report by Barnes). Fermi said that at Chicago a program has started to investigate the region from 20 to 30 Mev with photographic plates, but it will be some time before there are any results.

V PARTICLES

Friday morning, Professor C. D. Anderson presiding.

Anderson gave a brief summary of the present state of knowledge about V particles. There seems to be uniform agreement among the laboratories on the existance of a neutral particle which decays as follows: $V_1^o \rightarrow p + \pi^-$, with a $Q \approx 35$ Mev and a lifetime of 2.5×10^{-10} sec. However, there is also evidence for neutral particles with the above decay scheme which decay with a higher Q value (~ 75 Mev). In some cases the errors are large but there are one or two cases which are very good that give high Q values. Q values can be computed by measuring the angle of decay and the momenta of the decay products. These give Q values of about 75 Mev. A limit on the Q value can also be obtained by measuring the specific ionization and again it is very difficult to reconcile these exceptional cases with a Q value as low as 35 Mev. However, the great majority of the cases show a Q value of about 35 Mev. In most of the cases in which measurements are possible the decay products seem to be coplanar with the point of origin of the V_1^o. These results indicate that in the majority of the cases the decay is two body. One cannot exclude the possibility that in a few cases the decay is a three body one. There seems to be general agreement that the lifetime for decay of the V_1^o in its center of mass system is close to 2.5×10^{-10}sec.

There also seems to be agreement that other types of neutral unstable particles exist, namely $V_2^o \rightarrow \pi^+ + \pi^- + ($?). The upper limit on the masses of the decay products is less than 500 m_e and the decay products are presumed to be π mesons. There is agreement that the number of V_2^o's produced is considerably less than the number of V_1^o's. There is as yet little evidence on the coplanarity or lack of coplanarity of the decay products of the V_2^o. If one assumes a two body process, the Q value of V_2^o seems to be about 120 Mev. Not enough cases have been obtained to make a good determination of the lifetime of the V_2^o. However, the lifetime could be of the same order of magnitude as the lifetime of the V_1^o.

There are cases in which the decay products have momenta too high to measure and in which the measurement of specific ionization is too low to give information. Some of these cases give Q values as high as 200 Mev regardless of what the decay products are. There is also some evidence that there is a particle $V_3^o \rightarrow X^- + \pi^+$ where the mass of X^- seems to be greater than the mass of the π meson.

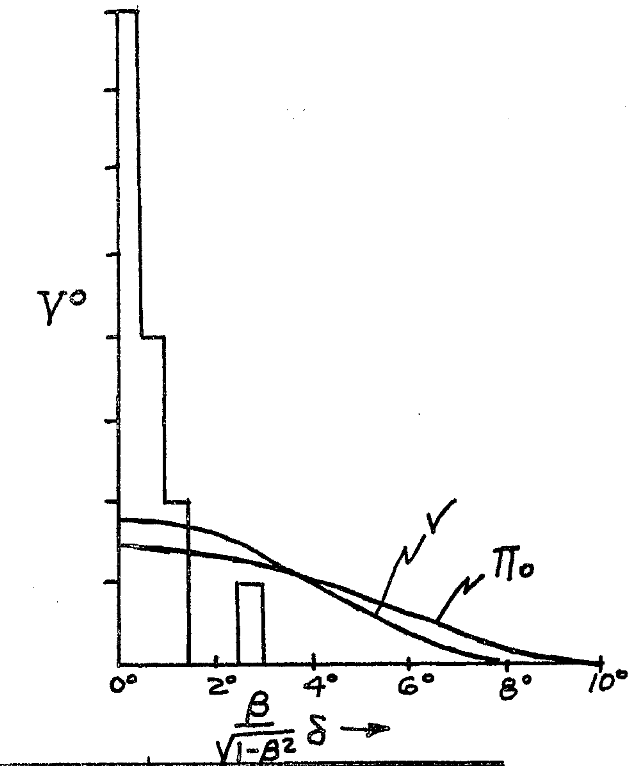
Anderson then called on Peyrou to report on the [illegible]cent work at M. I. T. Peyrou reported on work done with the M. I. T. multiple plate c[illegible] chamber by Bridge, Safford and himself. The chamber contained eleven 1/4" lead plates. V^o particles which originated from interactions occurring in the chamber were examined. In these cases the origin of the V^o particle can be seen. Out of 60 or 70 V^o_0particles, 23 V_1^o, 5 V_2^o, and 3 $V_?^o$ originate inside the chamber. For the 23 V_1's a measurement of the angle of noncoplanarity was made. δ is defined as the angle between the plane of the product particles and the line of flight from the point of origin. The following results were found:

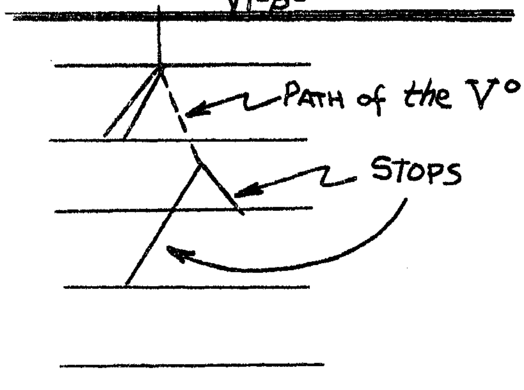
No. of Cases	Range of
2	$\delta = 5^\circ$
5	$2^\circ < \delta < 5^\circ$
16	$\delta \leq 2^\circ$

Peyrou discussed next the problem of bias in measuring the coplanarity of V^o particles. If the V^o were at some distance from an interaction and were not coplanar then it is possible that the V^o might be ascribed to a different origin. In this measurement only the V^o's which decay in the gas immediately below the plate of origin were used to test coplanarity. In these cases it is "obvious" where the origin of the V^o is. A histogram has been drawn using 16 cases in which the point of origin was obvious. The average angle δ for the 16 cases was consistent with the geometric errors expected from such a measurement. An error of the order of 0.7 mm can be made in the location of the origin of the nuclear event. These data were compared with the calculation of Thompson and Brueckner who assumed that the third body was a or a neutrino. In testing coplanarity, the important quantity is $\beta\gamma\delta$ for the decaying V^o where

$$\gamma = \frac{1}{\sqrt{1-\beta^2}} \qquad \beta = \frac{v_v}{c}$$

The following histograms show the experimental results compared with the calculation. The curves have been normalized to give equal area with the histogram. Peyrou concluded that the distribution was much too sharp to be consistent with three body decay.

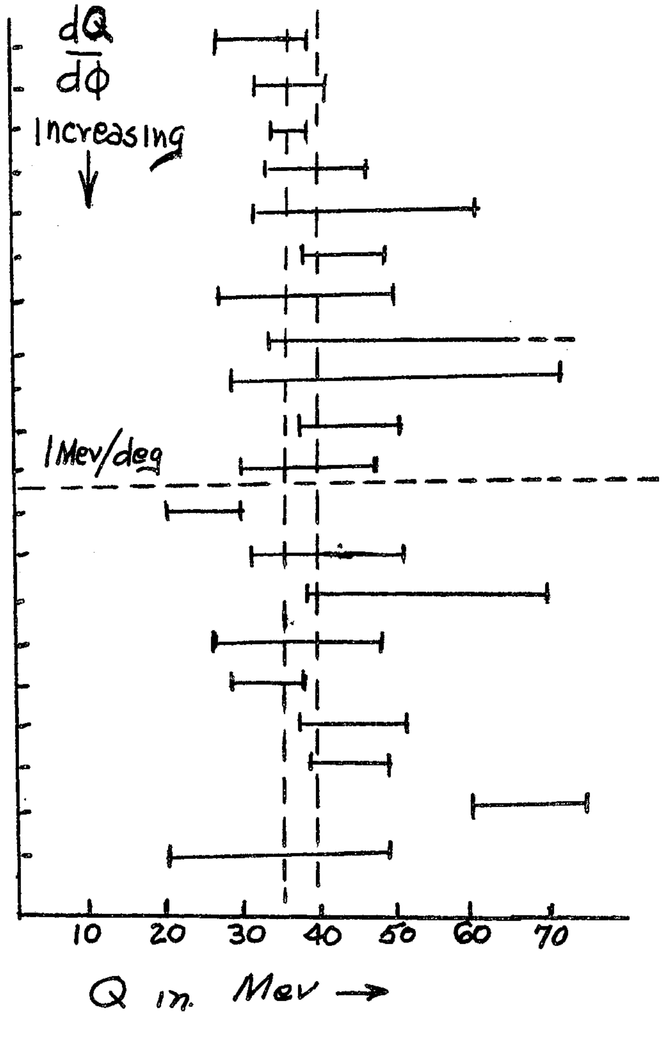




Feynman wanted to know how much momentum the third body was assumed to carry off. Peyrou replied that if the third body were a neutrino the average momentum that the neutrino would take off would be about the same as that carried by the π.

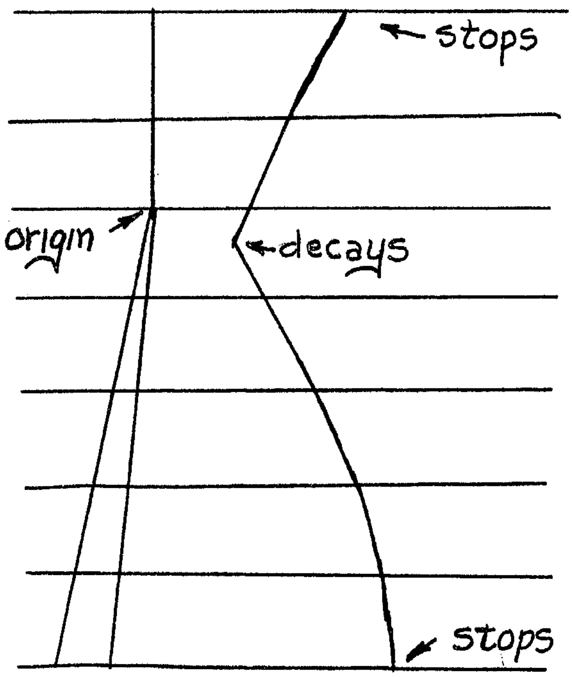
Since the results indicate strongly a two body decay, the rest of the analysis was made under this assumption. A picture of a V^o particle in which both secondary particles stopped in the chamber is shown below. The momentum of the meson was determined on the basis of its range. From the momentum of the light particle and the line of flight of the V^o the momentum of the heavily ionizing particle ...rmined to be 320 - 330 Mev/c. The ionization of the heavy particle is continuous and for this chamber this means an ionization of greater than 5. Ionization between 5 and 10 times minimum cannot be determined. Assuming an ionization of 5 times minimum, the mass of the heavy particle cannot be less than 1600 electron masses. There is another case in which both particles traverse lead plates and stop. Then the momentum of both particles is determined by range. It is found that the mass of the heavy particle is between 1500 and 2200 electron masses if the light particle is assumed to be a π. No decay particles are found to originate at the stopping point of the heavy particles. One π-μ decay in flight of a V^o secondary was seen. Two nuclear interactions were seen and consequently most of the light particles would seem to be mesons. In order to measure the Q value, the range of the stopped particle was used with the momentum balance. The range of the π particles gives a good determination of the momentum. The ionization of the stopping particle was used to determine the range with better precision than just the thickness of the lead plate.

Ranges were set on the momentum and from this a range of Q values were determined. The determination of the Q value also depended on the angle measurements. For each measurement a quantity $\frac{\partial Q}{\partial \phi}$ was calculated to determine how much an error in ϕ would change the measured Q value. Sometimes the $\frac{\partial Q}{\partial \phi}$ is as large as 9 Mev/deg, but can be as low as 0.1 Mev/deg. The result of the Q measurements are given below. The Q value can be well determined only if the particles are stopped by ionization. Some of the stoppings are the result of nuclear interactions. In these cases only lower limits for the Q value can be set. The results are all consistent with a unique Q value of 35-40 Mev. The best value of Q is 37 Mev.



The lifetime is determined by measuring the mean time between when the particle leaves a lead plate and when it decays. There are corrections due to the fact that the particles are observable only for a finite time. With corrections, the experiment gives $\tau = 3.5 \pm 1.5 \times 10^{-10}$ sec.

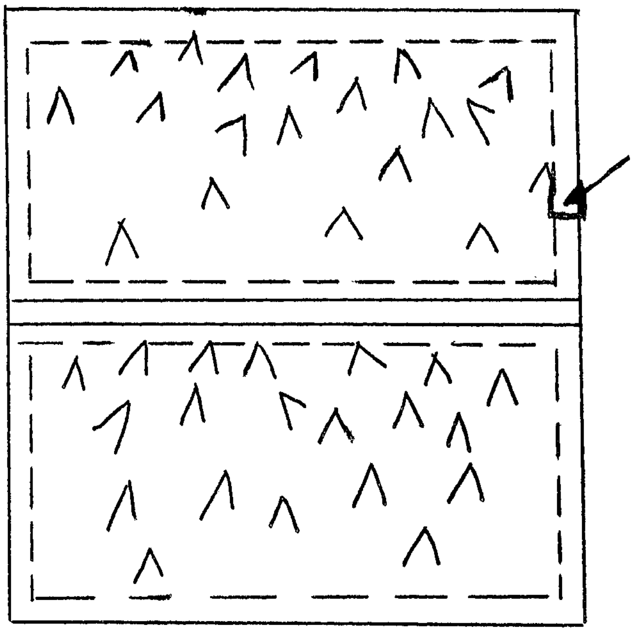
Five cases of V_2^o have been found. One of these was recently obtained and analyzed by B. Dayton. The coplanarity angles were good for the five cases and were consistent with the results on the V_1^o. The results probably indicate a two body decay. The case obtained by Dayton is shown below.



The downward moving particle was scattered through a large angle in the next to the last plate and stops in the last plate. The scattering was probably nuclear. The upward moving particle was apparently stopped by ionization. The upward moving particle had a momentum of 145-158 Mev/c. The momentum of the second particle is determined to be 272-296 Mev/c by momentum balance. The coulomb scattering of particle 2 agreed with the momentum balance considerations. From the momentum and ionization the mass of particle 2 was determined to be less than 500 m_e. Assuming that both particles are π mesons the Q must be about 195$\pm$15 Mev. The coplanarity was good. In the other 4 cases the proton was eliminated as a decay product by momentum balance. In two of these cases a mass of 1000 for one of the decay products was eliminated by momentum balance. In the other two cases it seemed unlikely that the mass of either of the decay products could be as high as 1000 m_e. If the particles are V_2^o then the Q values are consistent with a unique value. If however these particles are V_3^o then the Q values are spread. In reply to a question Peyrou said that perhaps the life-time is a little shorter than that of the V_1^o. Peyrou showed a picture in which a V_2^o and V_1^o were simultaneously produced in an interaction. Of the 5 V_2^o's observed two were produced in an interaction in which V_1^o's were also produced.

Feynman asked what sort of limit for the fraction of V_2^o's particles that are produced in pairs could be set from the experimental data. Peyrou replied that if one takes all types of V^o particles into consideration then the fraction of particles produced in pairs must be small. If, however, one restricted the types in the pair (e. g. $V_1^o + V_2^o$) then the results indicate the fractions of V_2^o's produced in such pairs could be large. Rossi pointed out that the V_1^o might have an alternate mode of decay into a neutron$+\pi^o$ which would be missed. Thus it might be possible that a large fraction of V_2^o's could be produced in pairs with V_1^o's. Brode stated that Fretter has three cases in which pairs of V^o particles are produced. In one of these cases the particles can be identified as V_1^o and V_2^o respectively.

A report on a measurement of the V^o lifetime was then given by Leighton. Leighton stated that care had been taken to eliminate sources of bias in the lifetime determination. The following diagram gives an idea of the point of origin of all of the V_1^o particles observed at Cal. Tech. Qualitatively, more seem to decay close to the top of the chambers than to the bottom. There were 134 cases presented. It was evident from the diagram that in some portions of the chamber the detection efficiency was less high, namely, around the sides of the chamber, and possibly near the top and bottom of each section. Fiducial surfaces were put around the edges of the chamber such that if a V^o particle



decayed inside these surfaces it would have been detected with high probability. Then for each V^o observed inside of the surfaces a measurement was made of the time elapsing in the V^o's frame between passing the fiducial surface and decaying. In order to make a reasonable estimate of the lifetime it is necessary that $t/T \leq 0.5$ where t, time spent inside fiducial surfaces before decay, T, time available inside the fiducial surfaces. Experimentally $\overline{t/T} = 0.30 \pm 0.05$, where $t = x/\beta\gamma$, x distance inside the fiducial sur[illegible]ce, $\beta = v/c$, $\gamma = 1/(1 - \beta^2)^{1/2}$, and τ [illegible] $N \sum_i \frac{T_i}{(e^{t_i/\tau} - 1)}$ For particles of high momentum it is difficult to measure momentum unless the particle decayed close to the top of the chamber. Consequently, in order to prevent bias errors on the high momentum pa[illegible]icles an unbiased method of eliminating high momentum particles should be used. In order to do this only V^o particles whose opening angles were greater than 10^o were used in the measurement. The opening angle measurement does not depend on the position in the chamber and yet the opening angle is closely related to the momentum of the particle. By using these criteria about 60 of the 134 cases of V^o decay were eliminated from the data. It was assumed that the V_1^o particles were a homogeneous group. The result of the calculations gave $\bar{t} = 1.6 \times 10^{-10}$ sec. and $\tau = 2.5 \pm 0.7 \times 10^{-10}$ sec. The stated errors were calculated by assuming that the experimentally determined values of τ are distributed Gaussianly about the true value, although this is not strictly true.

The V_1^o's were d[illegible]ided according to their measured Q values. Taking those with $Q \leq 50$ Mev and those with $50 \leq Q \leq 150$ Mev, the lifetimes calculated from these two groups were as follows.

$\tau_H = 1.6 \pm 0.5 \times 10^{-10}$ sec.
$\tau_L = 2.9 \pm 0.8 \times 10^{-10}$ sec.

There were also 20 cases in which the Q value was unknown. In order to keep from biasing the results by requiring long tracks to determine high Q cases, the twenty unknown cases were divided in proportion to the number of particles in the two classes. Depending on just which cases are put in which group the results are $\tau_H = (1.3 - 2.3) \times 10^{-10}$ sec,
$\tau_L = (2.4 - 3.5) \times 10^{-10}$ sec.

There appears to be little difference between the two groups of particles. Peyrou stated that they used a similar method to compute the lifetime although the method was a little more complicated because of the multiple plate cloud chamber.

Sard asked about the distribution of transverse momentum for the high Q cases. Leighton showed a curve of the number of cases with a given P_- and $\sin\theta_-$ versus $P_- \sin\theta_-$. The expected distribution should resemble an arctangent curve with the peak of the peak of the distribution occurring at about 100

Mev/c for $Q \simeq 37$ Mev. The experimental curve is in qualitative agreement with the prediction; however, there are some cases with values of $P^- \sin\theta_-$ which are beyond the limit for $Q = 35$-40 Mev. Some of these cases were of sufficiently high quality as to be strong evidence for a higher Q value.

Rossi stated "I would like to know what you think about the existence of the high Q value as this is an extremely important question. The Cal. Tech. group seems to be the only one which has evidence for its existence." Leighton answered by saying that it might be that some experimental arrangements such as the one at Manchester might bias against seeing the high Q cases if the V^0's with high Q have a shorter lifetime, as seems possible. The M. I. T. group should however have found some of these particles. Rossi noted that they had one possible case of a high Q decay. Leighton showed an example of a V^0 with high Q and stated that 2 or 3 other good examples existed. The case shown had the following momenta and angular openings.

$P_- = 77 \pm 7$ Mev/c $\qquad \dfrac{\partial Q}{\partial P_+}$ .1 Mev/(Mev/c)

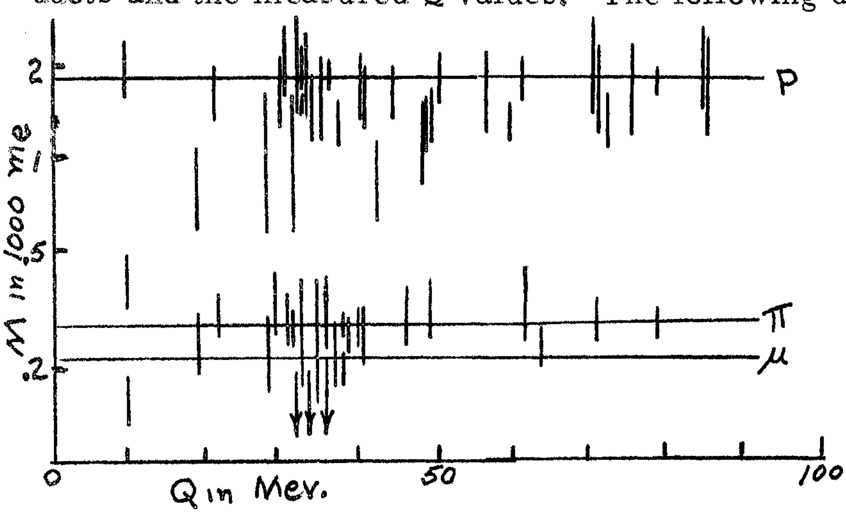
$P_+ = 800 \pm 150$ Mev/c $\qquad \dfrac{\partial Q}{\partial P_-} = 0.77$ Mev/(Mev/c)

$\theta = 114^o$

$Q = 79 \pm 15$ Mev $\qquad \dfrac{\partial Q}{\partial \theta}$ 0.82 Mev/degree

In this case the negative particle (π^-) was thrown almost directly backwards in the C. M. system. Greisen pointed out that the interpretation as a high Q case depended on the identification of a V_1^o. Leighton agreed, saying that the ionization of the + particle makes this case almost certainly a V_1^o. The people who had looked at the track agreed that the ionization was greater than minimum but probably less than twice minimum. The large image sizes of the pictures used made such a distinction possible.

Oppenheimer: "Would you think that the high Q objects could be some V_3^o's and some poor measurements?" In replying Leighton showed a slide on which the estimated masses of the product particles were plotted as a function of the measured Q value of the V^o. If Oppenheimer's suggestions were correct, one would expect some correlation between the estimated mass for the decay products and the measured Q values. The following diagram gives the results.



There appears to be no apparent correlation. However, they could not guarantee that some decay products were not heavy mesons. Anderson noted that in the M. I. T. experiment decay particles were not observed to come from the stopping heavy particles.

Messel: "Just what is the evidence for the V^o ?" Leighton said that they had three such cases in the V^o class. There were cases in which the negative particle was a π^- and in these cases it would be very difficult to distinguish a heavy meson from a proton. However, there were cases in which the positive particle was consistent with a π^+ and the negative particle was definitely heavier than a π^-. An example of such a case was shown. In the particular case the positive particle had a momentum and ionization (slightly above minimum) which were quite consistent with its being a π^+. The negative particle had both momentum and ionization higher than the positive particle and consequently could not have been π^-. Unfortunately, the negative particle's track was very short. Le Prince-Ringuet questioned whether the case presented might be an example of a back-projected K meson decaying. Leighton replied that if this were the case then the π meson would have to have a momentum of ejection of 200 Mev in the C. M. system which was higher than had ever been observed.

Messel wondered how many cases of $V_1^o \rightarrow X^+ + \pi^-$ there might have been in their data. Leighton replied that there could have been any number because of the difficulty of differentiating from a proton. Shapiro: "How many cases of are there in which you can exclude the possibility of their being V_1^o or V_2^o's. Leighton: "You cannot exclude any of the V_3^o's from being V_1^o's if you are willing in each case to assume the negative particle to be a negative proton. We are not willing to make this assumption, however." Anderson: "There are of the order of three to six cases which could definitely not be V_2^o's."

Anderson then called on Thompson to report on his recent work (with A. V. Buskirk, L. R. Etter, C. J. Karzmark, and R. H. Rediker). Thompson reported that a new magnetic cloud chamber had been placed in operation. The magnetic field has a strength of 7000 gauss and the illuminated volume of the chamber is 22" high, 11" wide, and 5" deep. The height of the chamber makes long tracks available for momentum measurement. No-field tracks of μ-mesons taken with the magnet coils in opposition indicate that the maximum detectable momentum is in the neighborhood of 5×10^{10} ev/c, however, this figure refers to tracks which traverse the entire chamber. In most of the pictures the tracks are close to minimum ionization which makes identification of particles difficult. In those cases where mass measurements are possible, masses are obtained which are compatible either with a proton mass or a π mass.

Thompson then gave a table of decays in which the mass of the positive particle was definitely less than protonic.

P_+ $\frac{Bev}{c}$	M_+	P_- $\frac{Bev}{c}$	M_-
0.27	$< 660m_e$	1.3	
0.38	$< 930m_e$		
0.53	$< 1300m_e$		
0.25	$< 640m_e$	0.39	$< 1000m_e$
0.50	$< 1200m_e$	1.52	

Thompson then described a new method of plotting the data by using the Manchester parameter α and the transverse momentum.

In the C. M. system, we have $P_x'^2 + P_y'^2 = P'^2$. P_y' is invariant so P_y' P_T. P_x' may be expressed in terms of

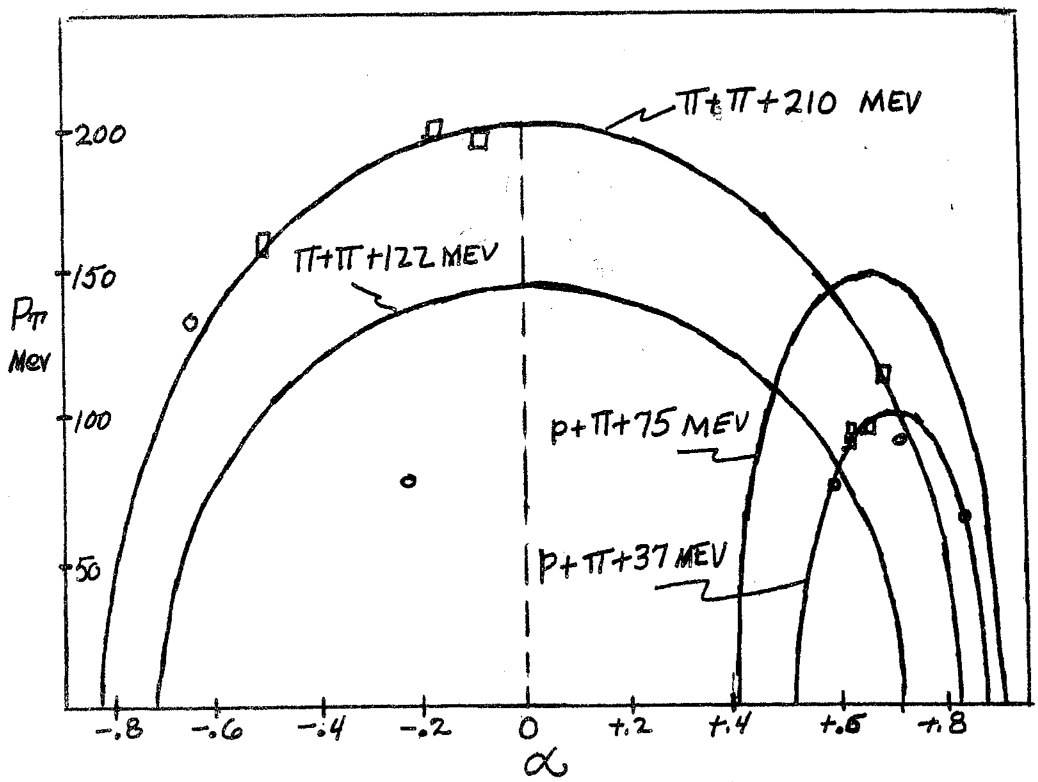
$$P_x' = \frac{(\alpha - \bar{\alpha})}{(2/\beta M)},$$

where $\alpha = \frac{P_+^2 - P_-^2}{P^2}$; $\bar{\alpha} = \frac{M_+^2 - M_-^2}{M^2}$

Thus

$$\frac{(\alpha - \bar{\alpha})^2}{(2P'/\beta M)^2} + \frac{P_T^2}{P'^2} = 1$$

For a given type of two body decay and a constant value of β, a plot of the experimentally determined P_T's versus α should lie on an ellipse. This ellipse has very simple physical significance in terms of the sphere on which lie the terminal points of P' in the C. M. system. If the decay were a three body decay then all of the experimental points should lie on the inside of an ellipse. Thompson then showed such a plot, referred to the plane $\beta = 1$.

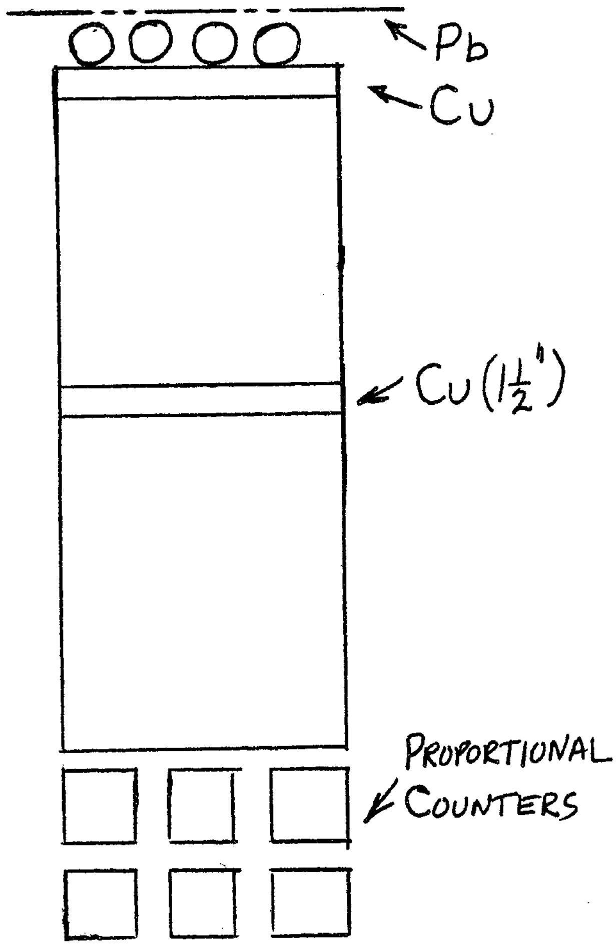


The data suggests a fit by a pair of ellipses corresponding to $p + \pi + 37$ Mev and $\pi^+ + \pi^-$ 210 Mev although Thompson emphasized that the latter decay scheme is suggested for purposes of comparison only. Present evidence cannot exclude other possibilities or a three body decay, etc. For example, the fit with (π, μ) is almost as good.

The calculated Q-values from the events observed with the new magnet which lie near the new $\pi^+ + \pi^-$ curve lay between 205 and 216 Mev. This is higher than the 120 Mev reported by the Manchester group. There was one point which fell a long way from the $\pi^+ + \pi^-$ 210 Mev curve.

Rossi: "What is the meaning of the point lying outside of all the curves?" Thompson: "Well, its not outside of all reasonable curves. If this represents the same type process as the others on the arch then this would indicate a three body decay." C. Anderson: "Suppose it were a proton plus π^- case?" In reply Thompson said that in this case low upper limits can be set for the masses of both particles; the positive particle had mass less than 600 m_e. The Q of this decay was about 50 or 60 Mev if it were a $\pi^+ + \pi^-$. Thompson also noted that this case could be a, decay into $\mu^{+} + \mu^{-}$. Sard said that Manchester had revised their estimate of the Q value for the V_2^o upward.

Rau then described a new magnet-cloud chamber setup ($H \approx 5400$ gauss) which was recently placed in operation by the Princeton group consisting of J. Ballam, R. R. Harris, A. L. Hodson, R. R. Rau, G. T. Reynolds, and M. Vidale. The chamber and associated equipment was ar anged as shown in the diagram. In order to trigger the chamber 3x minimum ionization was required in the proportional counters. At least one ionizing particle was required to strike the Geiger counters above the chamber. Two examples of V particle production were shown. The rate at which V's are obtained is three per running day. In the 5000 photographs obtained so far 49 V^o's and 12 $V^{\pm}$'s have been observed. There have been two cases showing pairs of V's produced, and one case showing two neutral V's and a charged V.



SUPER HEAVY MESONS

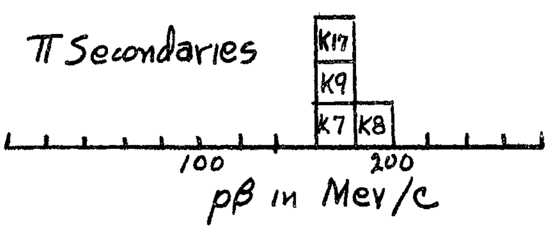
Friday Afternoon, Professor J. R. Oppenheimer presiding.

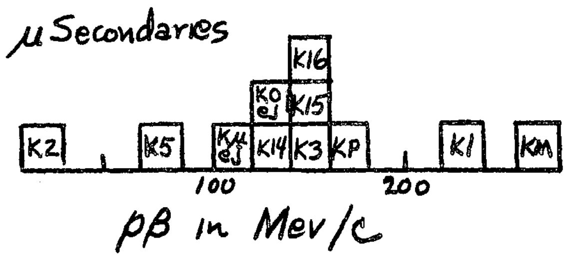
Perkins opened his talk by saying that most of his esults were tentative and should be treated with reserve. The first topic that Perkins discussed was the modes of decay, production and lifetime of the heavy mesons with mass of about 1,000 m_e. This is the work of Menon and O'Ceallaigh.

$$\tau \rightarrow 3\pi \qquad M_\tau \simeq 975\, m_e$$
$$\kappa \rightarrow \mu + ? + ?$$
$$\chi \rightarrow \pi + ?$$

(κ and χ will be collectively denoted by K)

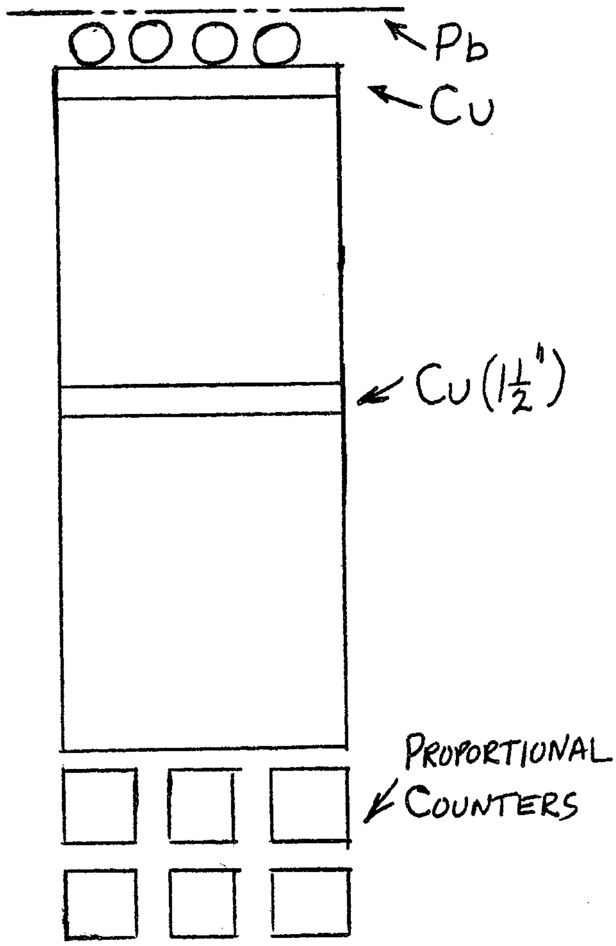
Perkins showed a picture of a κ meson which decayed into a μ meson which in turn decayed. A plot of the distribution in $p\beta$'s of the secondaries of the K particles is shown below.





decay into $\mu^+ + \mu^-$. Sard said that Manchester had revised their estimate of the Q value for the V_2^o upward.

Rau then described a new magnet-cloud chamber setup ($H \approx 5400$ gauss) which was recently placed in operation by the Princeton group consisting of J. Ballam, R. R. Harris, A. L. Hodson, R. R. Rau, G. T. Reynolds, and M. Vidale. The chamber and associated equipment was ar anged as shown in the diagram. In order to trigger the chamber 3x minimum ionization was required in the proportional counters. At least one ionizing particle was required to strike the Geiger counters above the chamber. Two examples of V particle production were shown. The rate at which V's are obtained is three per running day. In the 5000 photographs obtained so far 49 V^o's and 12 $V^{\pm}$'s have been observed. There have been two cases showing pairs of V's produced, and one case showing two neutral V's and a charged V.



SUPER HEAVY MESONS

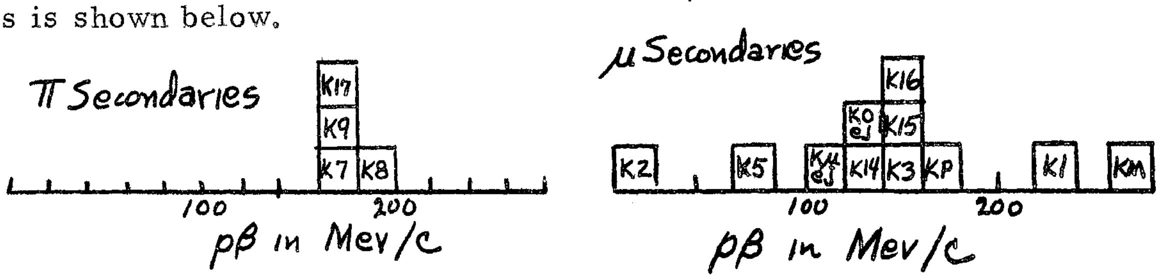
Friday Afternoon, Professor J. R. Oppenheimer presiding.

Perkins opened his talk by saying that most of his esults were tentative and should be treated with reserve. The first topic that Perkins discussed was the modes of decay, production and lifetime of the heavy mesons with mass of about 1,000 m_e. This is the work of Menon and O'Ceallaigh.

$$\tau \to 3\pi \qquad M_\tau \simeq 975\, m_e$$
$$\kappa \to \mu + ? + ?$$
$$\chi \to \pi + ?$$

(κ and χ will be collectively denoted by K)

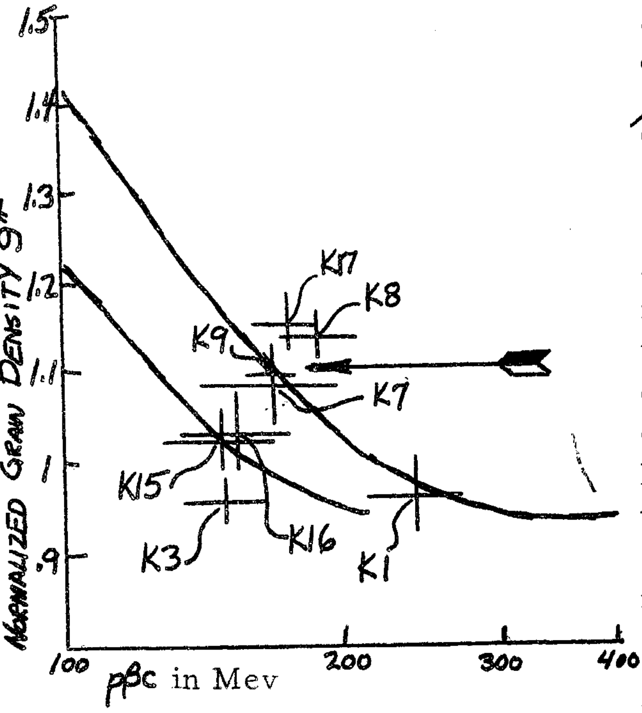
Perkins showed a picture of a κ meson which decayed into a μ meson which in turn decayed. A plot of the distribution in $p\beta$'s of the secondaries of the K particles is shown below.



If the μ mesons are the decay products of the same type of particle then from the fact that there is a spread in energy of the secondary μ's, the decay must be into three or more secondaries. The π meson secondaries could have a single energy at emission according to the data presented.

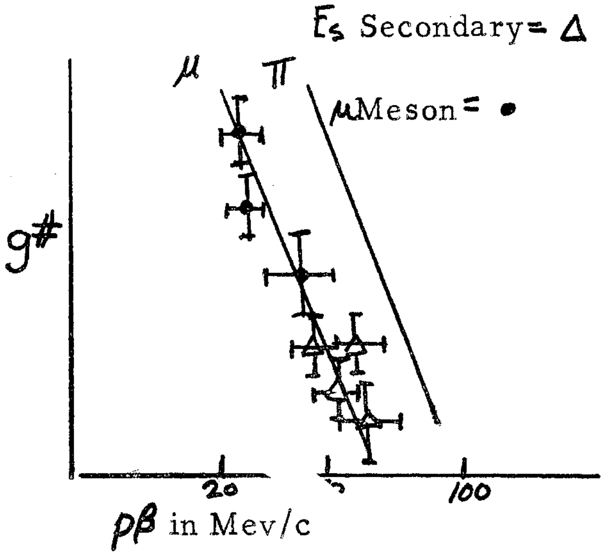
Plots of $P\beta$ versus the normalized ionization for decay products are shown below.

Secondaries of K particles



The belief in a π-meson secondary is based largely on the K9 point indicated by the arrow. The next diagram shows the results of grain density and $P\beta$ measurements on a μ-meson and on the secondary from K 5. The π-meson line on the diagram was drawn as the result of measurements on 35 π-mesons in the emulsion. To date there are 4 π secondaries and 7 μ secondaries identified. Some of the secondary tracks were minimum ionization and consequently could be electrons. However, in the course of traversal of 3.5 cms of emulsion there has been no evidence of bremsstrahlung, so that it is assumed that the unidentified particles are μ's or π's. The mass of the K may be computed from its assumed decay scheme using the limiting momentum of the μ. Such a computation gives:

$$K \rightarrow \mu + \bar{\nu} + \nu \qquad 1100\text{-}1200\ m_e$$
$$K \rightarrow \mu + \pi^0 + \nu \qquad M_K \sim 1400\ m_e$$



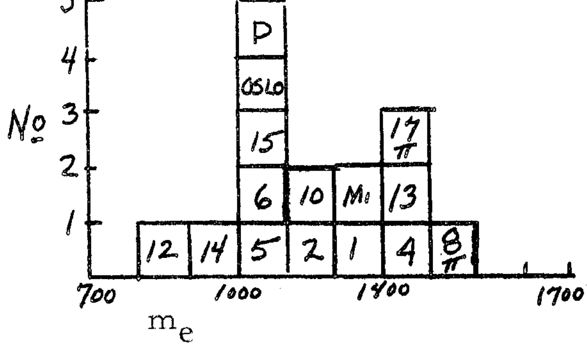
For the χ particles experimentally:

$$\chi \rightarrow \pi + Neutral \qquad E_\pi = 115 \pm 10\ Mev.$$

Assuming different particles for the neutral particle it is found that:

$$\chi \rightarrow \pi + \pi^0 \qquad 970\ m_e$$
$$\chi \rightarrow \pi + \gamma \qquad 920\ m_e$$
$$\chi \rightarrow \pi + V_2^0 \qquad 1400\ m_e \text{ If } M_{V_2^0} \; 800\ m_e$$

Next, the calculated mass values are compared to the experimentally observed mass values of the unstable particles. The mass is obtained by observing the scattering of the particle as a function of its residual range. The results are given in the following diagram. The errors on the mass values are each about 200 m_e. The two particles which decay into identified π mesons have a mean mass of 1400 $\pm$ 200 m_e. The measured mass of the primaries which decay into

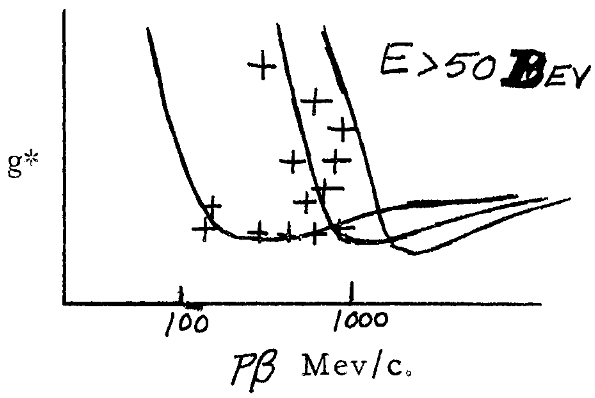


identifiable μ mesons have a mean mass of about 1100 ± 150 m_e. For the χ meson, the mass 1400 corresponds to the decay into a $\pi + V_2^o$; however, a decay into $\pi + \pi_o$ cannot be excluded.

To date, at Bristol, 7 identified K and 4 identified χ have been found, so that they have approximately equal frequencies. Calling $K = K + \chi$, then the rate at which these particles are found is $N_K/N_\pi \simeq$ relative number of K's and π's stopping $\simeq 1/70$. (Previously a figure of 1/150 had been given; however, this did not take into account the fact that the K particles had to have considerable track length in the emulsion in order to be identified whereas the π's are identified by their decay or star production.) $N_K/N_p \simeq 1/3000$; $N_K/N_\tau \gtrsim 10$ since only one τ has been found in lloon flights so far. These data were computed from balloon flights at 80,000 feet in which there was about 20 g/cm^2 of local matter (glass, emulsion, etc.) These abundance numbers apply to the relative numbers coming to rest and decaying in the emulsion.

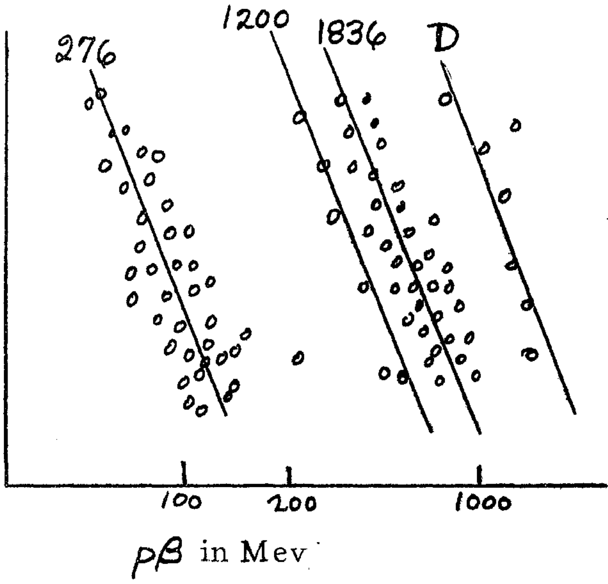
(Daniel and Perkins)

When the K particles were discovered by O'Ceallaigh in 1951, a study was begun on the origin of these particles. The jets (nuclear interactions of primary energy greater than 50 Bev) were searched for evidence of K particle production because the ratio of protons to π mesons is low. The next diagram shows a plot of g* versus $p\beta$ for jet particles. From these data an estimate of the relative numbers of π's and K's may be made, namely; $R = K/\pi = 0.5$; however, some might say that all of the particles in the K curve are protons and $R \simeq 0.3 \pm 0.3$. Consider a second argument then; for the lower energy showers the ratio $N_{\pi^o}/N_{\pi^\pm}$ was determined by looking for the number of electron pairs originating near the shower origin from the decay of the π^o. A ratio of 0.56 ± 0.1 was found in agreement



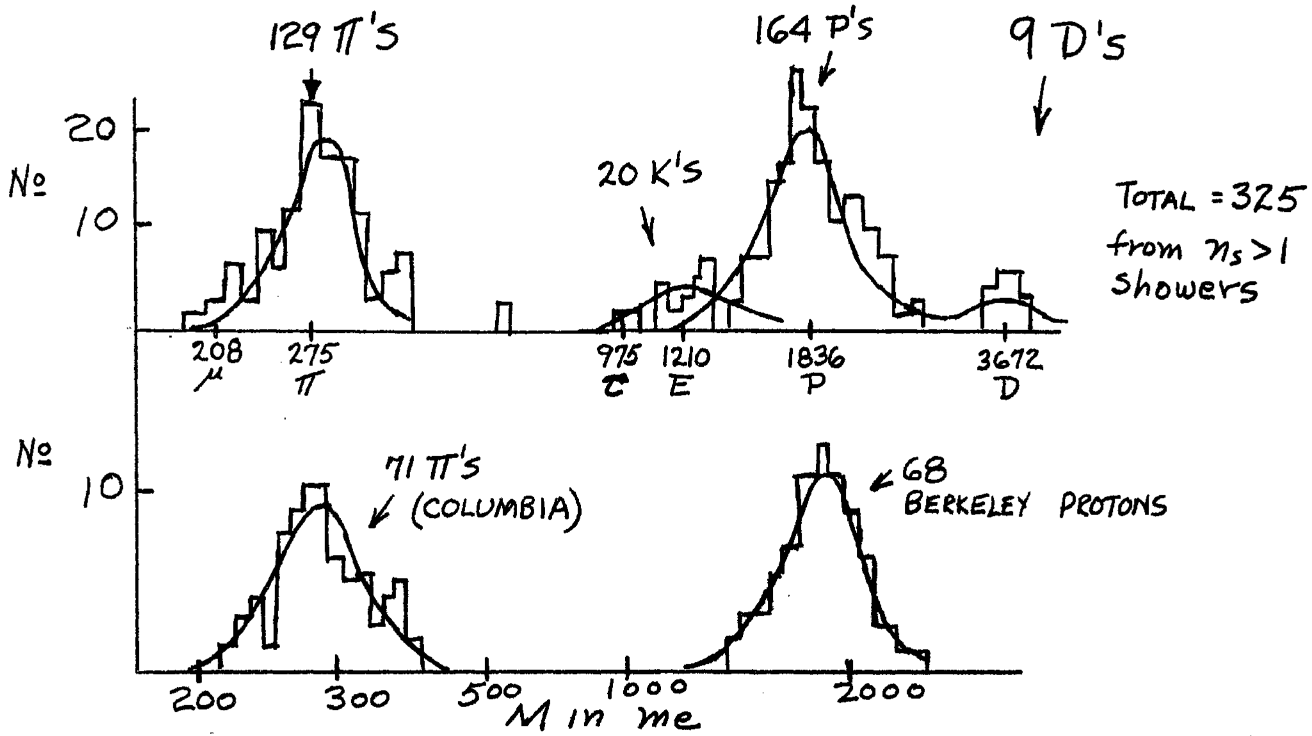
with cloud chamber work. In jets, however, the ratio $N_{\pi^o}/(N_K + N_{\pi^\pm})$ $0.33 \pm 0.$ which appears to substantiate the conclusion that there are K particles in the jets. If it is assumed that $N_{\pi^o}/N_{\pi^\pm}$ 0.5 then it is found that $N_K/N_{\pi^\pm} = 0.6 \pm 0.5$. There are not many primaries of energy greater than 50 Bev. and consequently these primaries cannot account for all the K's observed to stop in the emulsion. Some K's must therefore originate in the lower energy showers.

By requiring longer tracks and doing more accurate grain ting and scattering measurements it was considered possible to separate the K particles from a 90% proton background. The measurement was calibrated by using plates exposed at Columbia to the π meson beam and at Berkeley 340 Mev protons. The next graph shows the results of measurements on 3 shower particles from showers of multiplicity greater than 1. Only tracks whose ionization was between 1.07 and 2.0 times the r u value were used. The line through the π meson



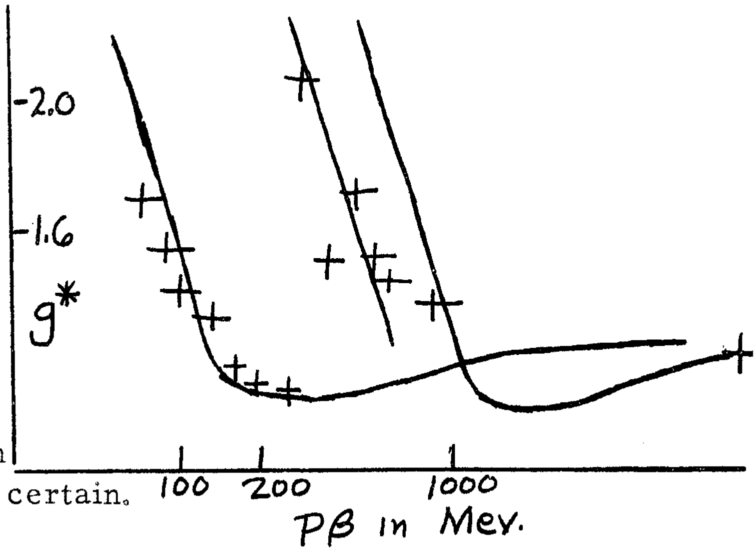
points was the best straight line through the points and the P and D lines were placed according to the mass ratios. The K line was drawn at 1200 electrons masses.

The curves below show the mass spectrum obtained from measurements on artificially accelerated and produced particles. The distributions are fitted with skew gaussians with two adjustable parameters, A and B. The r.m.s. errors in the mass measurements due to statistical fluctuations in the scattering of the particle is equal to $A/\sqrt{n}$ where n is the number of scattering cells. The error in grain counting is equal to $B/\sqrt{m}$. Where m is the number of grains counted. The constants A and B are determined from the mass spectrum obtained from artificially produced mesons and protons. The mass spectra obtained from the measurements and the fitted curve are shown below.



The mass spectrum obtained from cosmic ray showers with $n_s \geq 1$ is shown above. In order to guard against local variations in the sensitivity of the emulsion a plot of g^* versus $p\beta$ was made for cases in which other tracks besides the supposed K in the same star were measurable. When other tracks were measurable they appeared to fit well on the π or P curves. This diagram is given below. (see p.45)

A comparison was made of the mass spectrum of the K particles coming to rest and those created in showers. From the comparison it was concluded that the two curves are the same within the experimental errors. It is not possible to tell whether the particles that are ejected are κ or χ particles. There have been a few cases observed in which the K particle is observed to come from a shower and decay. Sorenson (Oslo) has observed such a case. The K particle comes from a star produced by a proton with 16 heavy prongs and 3 shower particles. The K meson has a range of 14 mm in the emulsion. The mass measurement gives 1080 ± 100 m_e. The secondary particles travels 2.5 mm. in the emulsion and consequently the identification as a μ meson is fairly certain; $p\beta$ = 125 ±20 Mev/c, g* = 1.05 ± .03. In reply to a question by LePrince-Ringuet concerning the identification of the Perkins showed a plot of g* versus $p\beta$ which indicated that the identification was 95% certain.



Levi-Setti in Milan also has found two cases of K particle production. One of these showers produced by a proton of E > 30 Bev has 23 heavy prongs and 15 shower particles. The K particle emerges from the shower, stops and decays but unfortunately the path length of the secondary particle is too short for identification. The mass of the K particle was found to be 1040 ± 90 m_e. In the second case a K particle emerges from a shower of 28 heavy prongs and 11 shower particles produced by a proton. The track of the K is 7 mm. long and gives a mass value 1380 ± 250 m_e. The $p\beta$ of the secondary was 100^{+15}_{-} Mev/c. This is lower than the $p\beta$ of a π produced in the decay of a τ meson. Shapiro asked whether the grain density of the secondary particle had been established. Perkins replied that the secondary particle was almost at minimum ionization having a grain density of 1.1 ± 0.1 times minimum. The track length was unfortunately too short to establish the identity of the secondary particle. Perkins concluded that this was more direct evidence that χ particles are projected from stars. This evidence, however, does not exclude the possibility of the direct production of τ mesons.

Perkins next presented evidence as to how the fraction of K particles might depend on the energy of the primary particle. In almost all of the cases presented the primary energy was deduced by indirect means (multiplicity of the shower particles.) The number of π and K particles in the same velocity range ($0.5 \leq \beta \leq 0.8$) were compared.

For	R = K/π	$\bar{E}$
		5 Bev
$n_s < 4$	0.13 ± 0.05	(1.5 Bev < E_p < 8 Bev)
$n_s \geq 4$	0.28 ± 0.08	20 Bev (Bev < E < 40 Bev)
jets ⟶	0.30 ± 0.30	200 Bev

Next the number K-particles and of π-particles in the momentum range 300 $\frac{\text{Mev}}{c} \leq p \leq 950 \frac{\text{Mev}}{c}$ were compared.

R $\frac{N_K}{N_\pi}$	$\bar{E}$	R' = $\frac{\text{Energy into K's}}{\text{Energy into } \pi\text{'s}}$	$\bar{E}_p$
0.10 ± 0.04	5 Bev	0.17 ± 0.06	5 Bev
0.20 ± 0.06	20 Bev	0.36 ± 0.10	20 Bev
0.30 ± 0.30	200 Bev	1.00 ± 0.03	200 Bev

The yield of K particles is rather high even at low energies. If the relative amounts of energy going into K particles and π particles is compared, the ratio R' is given in the second table. These numbers are what is to be expected on the basis of Fermi theory if the K's have spin 0.

In an extremely high energy interaction of $E \sim 10^{13-14}$ ev, the rest masses of the product particles are small compared to the energy available so that one should expect equal numbers of produced π's and K's. For a shower of 10^{14}ev energy, the relative numbers of π^0's and shower particles was determined. On the basis of five high energy pair conversions; (cf. insert),

$$\frac{N_{\pi^0}}{N_s} = 0.22 \pm 0.1 \text{ if } \tau_{\pi^0} = 0$$

$$\frac{N_{\pi^0}}{N_s} = 0.26 \pm 0.1 \text{ if } \tau_{\pi^0} = 10^{-14} \text{ sec.}$$

this is consistent with half π and half K particles. It was noted that Kaplon and Ritson had obtained a ratio of 0.5 for showers in this energy range. Shapiro remarked that Peters had obtained an even higher ratio of π^0's to shower particles. Oppenheimer said that there had apparently been some misinterpretation in this case.

The nuclear plates are not well suited for a determination of lifetimes of decaying particles; however, an estimate of lifetime may be made from the relative number of K's produced and stopping in the emulsion. The relative numbers of K's and π's brought to rest in the emulsion is as follows: $N_{K/(N_\pi)\text{stopping}} = 0.015$. The relative numbers produced in showers of all energies: $N_{K/(N_\pi)\text{ production}} = 0.05 \pm 0.05$. This figure is somewhat different from those quoted before because of the contribution of the low energy showers. Actually, the K's which come to rest in the emulsion are created in a lower part of the momentum sprectum than the 330-950 $\frac{\text{Mev}}{c}$ interval for which the 0.05 ratio has been determined. From the ratios as quoted, a lifetime of 3×10^{-10}sec. is deduced. If the 0.05 were high by a factor of 2.5 then no upper limit could be set on the lifetime. The lifetime could very easily be as long as 10^{-8} sec. Oppenheimer commented that a lifetime an order of magnitude shorter would give an inconsistency. A total K particle track length of 15 cm. has been observed without observing any decays in flight. This result gives a lower limit of 3×10^{-10}sec. for the lifetime. The group at Manchester working on charged V's (Astbury et al) have found negative particles of about protonic mass emerging from penetrating showers. If they assume equal numbers of positive and negative unstable particles then they deduce a lifetime of 1.6×10^{-8} sec. from the number of charged V decays that they observe.

So far 16 cm track length of identified K particles have been observed without seeing any nuclear interaction. The mean free path in nuclear emulsion corresponding to geometric cross section is 26 cm. If one believes that K particles are produced abundantly at extremely high energies (10^{13} ev.), then there is evidence that the K's do interact with geometric cross section. The following data indicate a geometric mean free path for the secondary particles from extremely high energy interactions.

Bristol shower	-10^{13} ev/nucleon:	4 secondary showers	total track length $\simeq$ 440 cms
Peters et al	-10^{13} ev/nucleon:	8 secondary showers	
Jets ($>$ 50 Be.)		5 secondary showers	

From these data a mean-free path of 27 ± 6 cm is deduced for the secondary particles.

If the K particles have strong interaction, then the negative K particles should produce stars on coming to rest. So far 20 K particles have been observed to come to rest and decay. Bristol has found no examples of K particles producing stars. Schein and his co-workers (Fry and Lord) have found one such example. The ratio of the number K's decaying to those producing stars must be greater than 10 to 1.

A slide showed the mass sprectum obtained by measuring scattering versus residual range of the particles producing σ stars. All of the mass values seemed consistent with π mass. Perkins then suggested that the K particles might not fall down to the inner shells of the material before decaying in non-metallic materials. A short discussion followed on the subject of trapping. The concensus seemed to be that trapping effects could not explain the lack of stars by K particles.

Dr. Oppenheimer next called on LePrince-Ringuet for some comments on K particles in French, "so lucid, that all will know it."

LePrince-Ringuet: Je voudrais reprendre simplement quelques uns des passages du superbe exposé de Perkins. En Europe, il y a pour les émulsions, Bristol, le grand soleil, et puis un tout petit nombre de petits satellites dont la dimension, même en faisant la somme, reste très inferieure a celle de Bristol. On est toujours un peu timide pour parler après Bristol de problèmes dans lesquels Bristol a obtenu 75%-80% des résultats. Je voudrais simplement rappeler dabord deux ou trois points, et puis venir à certaines questions tout à fait précises sur le faite de savoir si il y a deux particules K et χ, si il n'y a pas d'évidence tout à fait sûre sur ces deux particules, et comment on peut les distinguées. Parce que ce sont sûrement des particules très differentes, et c'est très important d'avoir une vision peut-être très critique, et peut-être trop critique. Dabord, il y a un an, au congrès de Bristol, il y avait quatres, peut-être cinq. mesons kappa, et il n'y avait pas encore de mésons chi. Et après, au congrès de Copenhagen, il y avait une dizaine de mésons kappa, et on c'est apperçu que, grâce à l'abondance des résultats de Bristol, que L'on trouvait l'ensemble des autres compatible avec quelque chose qui ce situait autour de 180-190 $\frac{Mev}{c}$ pour le $p\beta$. Alors, on a décidé qu'on examinerait tous les résultats avec beaucoup de soin; et, que Perkins vient de dire, c'est dans ce domaine là le résultat de ces observations. Naturellement, les mesures de trajectoires et $p\beta$ sont difficiles, et on sait très bien que les méthodes des différents laboratoires ne sont pas absolument comparables; et que même des formules donnant les erreurs ne sont pas comparable, parce qu'il y a le "spurious scattering", parce qu'il faut utilizer plusieurs cellules successives, etc. Cela donne des difficultés pour comparer les mesures et les erreurs.

Alors, la première question à laquelle je voudrais tirer attention avec un esprit spécialement critique, c'est premièrement; quelle est la fin du spectre? Dont le spectre actuel, il y a deux résultats qui sont au-dela de 270 Mev/c ($p\beta$), c'est à dire, 200 Mev. d'énergie. Il y a deux mesures qui ont été indiquées: (1) le kappa un de Bristol, (2) les résultats italiens. C'est très important, la fin du spectre, parce que la masse de la particule primaire dépend de la fin du spectre. Or, je pense que les deux secondaires correspondant ne sont pas très bons pour les mesures. Le secondaire de Levi-Setti est court et on ne peux pas être sûr. Le secondaire du kappa a une longeur de 2200 microns seulement, et c'est à peu près 200 Mev, mais ca peut être 150 Mev; on n'est pas sûr d la fin du spectre. C'est mon impression (tout a fait personelle) d'experimentateur.

Deuxième point: Est-ce qu'il y a des differences, est-ce qu'il y a certainement des secondaires pi et des secondaires mu? Pour cela, il y a deux cas de secondaires mu qui sont sûrs, ce sont les deux de Bristol qui donnent, l'un un mu-electron, et L'autre, dont l'energie est faible; par conséquent la differentiation est tout à fait certaine entre le mu et le pi. Il y en a d'autres qui sont extrêmement probables, et, en particulier, l'intéret du méson de Paris (qui a été étudié par le groupe de mon laboratoire avec Crussard, Trembley, Mabboux, Jauneau et Morellet) est que le secondaire est très long (plus de 20,000 microns.) Et l'autre intéret est que cette longueur peut etre doublée, parce que nous avons à Paris deux méthodes de "scattering" indépendente, qui sont, l'une, le "scattering" latéral, et l'autre le "scattering" en profondeur, qui a été mis au point par Mabboux. Et ça aide beaucoup à avoir de la précision sur les mesures. Aussi bien, puisque cette particule se trouvait dans la bande correspondant au chi, il était intéressant d'avoir des informations plus certaines sur cette particule; et, nous savons maintenant que c'est une particule mu (comme Perkins l'a montré). Mais c'est une particule mu qui est obtenu avec une bonne certitude. Par conséquent, il y a dans ce domaine la, qui correspond a environ 120 Mev d'énergie, une particule mu qui est très probable. Çela veut dire, il y a une chance sur 20 que ça soit un pi. Ensuite, il y a le kappa 3 de Bristol qui donne aussi un mu qui est également très probable. Ce kappa 3 a une longueur de 6000 microns qui est suffisant pour avoir une bonne mesure, et la mesure donne un mu. À mon avis, si on veut etre extrêmement critique, il y a 4 secondaires qui sont presque certainement des mu; par conséquent, 4 kappa-mu surs.

Est-ce qu'il y a des kappa-pi (ou des chi-pi) sûrs? Il y a, à mon avis, deux kappa-pi sûrs. Il n'est pas la sécurité complète, mais c'est une très grande probabilité. Ce sont les numéros kappa 8 de Bristol, qui a 7,800 microns, et le kappa 9 de Bristol, qui a 19,500 microns. On a la une bonne certitude qu'il existe des mésons pi. Si l'on avait encore 4 ou 5 cas semblables, on aurait une certitude presque absolue.

En dehors de ces résultats, est-ce que l'on peut séparer la particule kappa de la particule chi par quelques propriétés. Si l'on prend les résultats sûr quelle est la masse du primaire? La masse du primaire dans aucun de ces cas n'est bien determinée. Dans le cas du kappa de Paris (1600 microns) et de Bristol, la masse du primaire n'est pas bien determinée. Il n'y a donc pas de possibilité, à present, par des valeurs individuelles, de dire que le primaire de ceci (mu) est différent du primaire de cela (pi) par les mesures de masses. Par consequent, ceci est aussi un point qui est intéressant.

Enfin je fais encore une remarque. Il y a 3 kappa lents qui sortent de les étoiles. Il y a le kappa de Bristol que Sorens a éxaminé, et il est très long. Dans ce cas, le primaire est bien mesuré, et c'est toujours le même ordre de grandeur de la masse qui a été indiqué par Perkins: entre 1000 et 1100 m_e. L'énergie du secondaire est, je crois $p\beta = 125 \pm 20$ Mev/c. Pour les autres kappa de Levi-Setti et Tomasini, l'un d'eux est long([illegible] microns) et donne 1040 ± 90 m_e, l'autre est très court. Tout est consistent avec une masse que l'on a bien mesurée. Est-ce que ces particules (les secondaires) sont pi ou mu? Ce cas n'est pas, à mon avis, certain. Les deux mésons kappa italiens ont des secondaires trop courts pour que l'on puisse dire; et je crois que je suis un peu plus pessimiste que Perkins pour le troisième méson-celui de Sorenson. La longueur du secondaire de celui de Sorenson est de l'ordre de 2000 microns; ce n'est pas très considerable pour avoir une différentiation certaine entre un pi et un mu. Par consequent ceci reste, pour mon avis, avec un point d'intérrogation, tant qu'il n'y aura pas d'autres mesures de ce côté-là.

Un dernier mot maintenant, sur un problème que Perkins a évoqué tout à l'heure. C'est le problème des mésons lourds, négatifs, s'arrêtant dans l'émulsion. Nous avons un phénomène qui peut s'interpreter comme cela, qui est un phénomène réel, et qui correspond a un étoile sigma, a un branche seulement, mais avec des characteristiques différents. Dans cette étoile, la particule est très longue (elle va dans deux plaques); elle a une ionization de 3, environs--c'est très facile a mesurer. On sait que c'est un proton--il n'y a aucun doute--on peut faire des mesures de masse par plusieurs méthodes. Ces méthodes donnent une énergie kinétique (si c'est un proton) de 130 ± 20 Mev. C'est une grande énergie kinétique; dans les étoiles sigma, on a observé seulement trois cas en 3000 pour un proton de 90 Mev, ou plus. Ceci sont les résultat obtenus avec les mésons artificielles, par Menon et autres, par Cheston et autres, et Adelman et autres.

Mais d'autre part, la mesure de la masse du primaire; nous l'avons faite par plusieurs facon, et nous avons même aussi envoyé la plaque à Bristol. La longueur n'est pas très grande--2300 microns--mais elle permet une évaluation. Les moyenne des observations donne 570 ± 200 m_e, environs. C'est à dire, c'est peut-être un méson pi; on n'est pas sûr, mais la probabilité est de l'order de 1%-2% pour avoir un méson pi. Si l'on prend l'ensemble de ce phénomè[illegible], et si l'on pense que l'émission par un méson pi d'un proton avec une telle énergie est très improbable, (parce qu'il faudrait qu'il y ait tout un groupe de [illegible]s--10 neutrons--qui sortent en même temps dans l'autre sens--et rien d'autre); la probabilité totale d'avoir une étoile sigma est de l'ordre de un sur 100,000. Mais tant qu'il y a une lecture de 100,000 phénomènes, il est possible qu'on trouve le phénomène rare. Je ne veux rien dire de plus ladessus; simplement, si d'autres physiciens ont d'autres phénomènes analogues, les mettre simplement dans la balance. Ce n'est pas un résultat très sûr en faveur d'intéraction nucléaire d'un méson lourd.

Oppenheimer next called on Amaldi for a report on the τ meson. Amaldi wrote down the Q values calculated from the observations on the mesons observed in photographic emulsions.

	Q	
$Bristol_1$	65 ± 6 Mev.	(nly case in which one of the three products given rise to a star).
$Bristol_2$	75 ± 4	
$London_1$ (Harding et al)	76 ± 15	
$London_2$	69 ± 8	
$London_3$	73.5 ± 7	
$Padua_1$ (Ceccarelli, Dallaporta, Merlin, Rostagni)	86.5 ± 5	
$Rome_1$ (Baroni, Castagnoli, Cortini, Franzinetti, Manfredini)	75 ± 4	

These Q values were obtained under the assumption that all three particles are mesons. Using this assumption ($\bar{Q} = 74 \pm 2.5$ Mev; $m_\pi = 277.4 \pm 1.1\ m_e$) a mass of $979 \pm 4\ m_e$ is obtained by averaging the above results.

Next Amaldi spoke of the nature of the secondary particles. In the original case found at Bristol one of the decay particles stops and gives rise to a σ star. This particle is presumably a π meson. From the τ observed at Rome recently, one of the decay particles stops in the emulsion and gives rise to a secondary particle. The secondary particle is not very well situated for measurement; however, it is possible to exclude the possibility that the particle is a proton and it is probably a μ meson from a π-μ decay. It was not possible to establish with certainty by grain counting and scattering measurements whether the other two particles were π or μ mesons, but one can recognize from a detailed discussion of the experimental data that they are better fitted with a three π decay.

The decay found by the group at Padua is also very favorable for identifying the secondary particles. The secondaries had lengths in the emulsion of 2,600, 2,000 and 100 μ respectively. The angles of emission can of course be measured rather well. By assuming the nature of two of the particles the momentum of the third can be calculated by momentum balance and compared with the experimentally determined value. As a result of the analysis it was concluded that the experimental data could be fitted by assuming that all three of the particles are π mesons; however, the possibility could not be excluded that two of the particles were μ's and one of them a π. Under this latter assumption the energy of the π should be 27

Mev and experimentally it was found to be 37 Mev, but since the distance traveled in the emulsion was only 100μ the possibility of the π's having 27 Mev cannot be excluded.

Next, the production of the τ's was discussed. Amaldi noted that all except one of the particles observed were produced at mountain altitudes under considerable amounts of absorber. The Bristol τ's were observed under 10 and 30 cm of lead respectively. The London τ's were found in plates exposed under 1 - 3 meters of ice. The Padua τ was observed under aluminium. The only exception known until now is the Rome τ which was observed in a plate flown at 25,000 meters with very little material surrounding the plate.

THEORETICAL DISCUSSION OF PHOTOMESIC PRODUCTION AND PION-NUCLEON SCATTERING

Friday afternoon (Part II), Prof. J. R. Oppenheimer presiding.

The session was opened by Oppenheimer who requested comments from Feld, Chew and Dyson. Feld discussed the interpretation of the photoproduction of mesons, that is, the processes $\gamma + p \rightarrow \pi^+ + n$ and $\gamma + p \rightarrow \pi^0 + p$. The point of view adopted is that of Brueckner and Watson, namely, to correlate photoproduction to pion-nucleon scattering. The most important processes are those involving three states of the pion and nucleon characterized by the total angular momentum J and the parity of the pion with respect to the proton, as summarized in the table.

Transition caused by:	J	Parity	Amplitude
electric di	1/2	-	a
magnetic di	1/2	+	b
magnetic dipole (or electric quadrupole)	3/2	+	c

The possibility of electric quadrupole pion production in a 3/2 state is neglected because the observed angular distribution corresponds to magnetic dipole transitions. The most general angular distribution possible for these three states is given by

$$\frac{d\sigma}{d\Omega} = |a|^2 + |b|^2 + |c|^2 (1+1.5 \sin^2\theta)$$
$$+ 2 \text{ Re } \left[a (b-c)^*\right] \cos\theta$$
$$-2 \text{ Re } bc^* (3/2 \cos^2\theta - 1/2)$$

Which terms are the most important? In the production of π^0 mesons we can drop the electric dipole term since close to threshold the matrix element depends on the cube of the momentum of the π^0 meson. Hence, the π^0 meson is being emitted into a p state and since it is pseudoscalar, it can only come from a positive parity state. Therefore, the only remaining question is the ratio of the $p_{1/2}$ and $p_{3/2}$ contributions. It turns out that because of the interference term, if we describe the cross section as $(A + B \sin^2\theta)$, then the ratio B/A gives a sensitive test of this mixture. Actually, the ratio B/A depends on three constants: b, c, and the relative phase between them; for simplicity we neglect the phase, that is, assume it to be 0 or 180°. The resulting dependence of the ratio B/A on the

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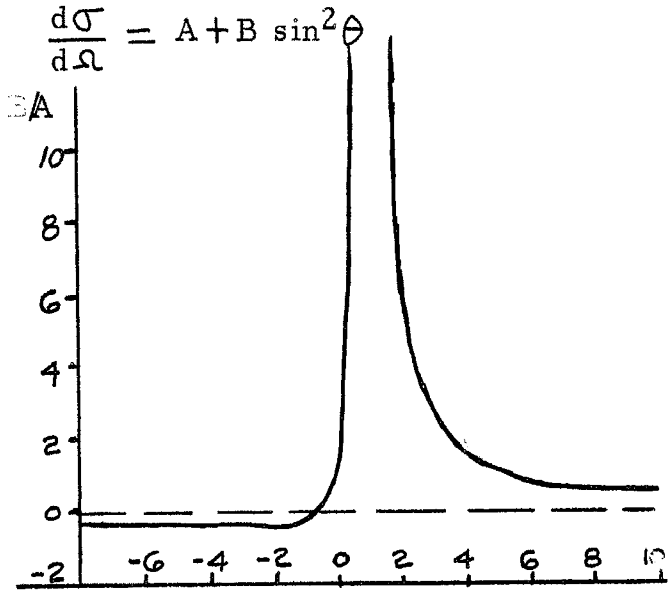


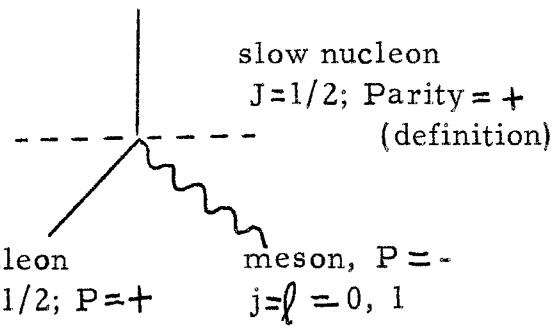
Ratio of $p_{1/2}$ to $p_{3/2}$ contributions = b/c

The second point Feld made had to do with the resonance in the neutral photomeson production so strongly indicated by the Cal. Tech. data. If there is a true resonance, it will in fact make itself felt even at the threshold, that is, in distorting the p^3 dependence of the meson matrix element. Thus even if the resonance were of zero width and occured with a peak at 300 Mev, the resonance factor $\frac{1}{(E-E_r)^2}$ would contribute at threshold. The effect of this term is indicated on page in comparison with Goldschmidt-Clermont's excitation curve. The data is not yet good enough to say whether or not the resonance manifests itself near threshold, but it is conceivable that a considerable improvement in experimental accuracy could settle this point. Finally, the resonance may not be as strong as one would think at first sight. Thus, the Cal. Tech. measurement has been made at 90°; if the magnetic dipole dependence $(1+1.5\sin^2\theta)$ is most important, this gives a maximum at 90 However, the possibility which we neglected at lower energy of electric quadrupole production in the 3/2 positive parity state could occur at the higher energies with its angular dependence of $(1+\cos^2\theta)$ which has a minimum at 90°. Further, the interference effects have not been calculated. Hence, part of the reduction in the cross section beyond the maximum could be due to the reduced contribution at 90° due to the increasing importance of the electric quadrupole term. This might perhaps explain a factor 2 but not the observed decrease of a factor 4.

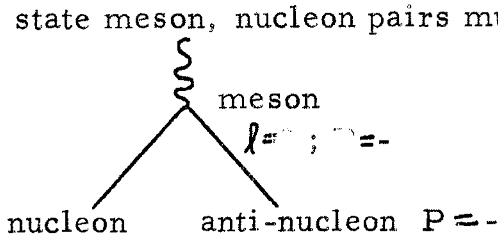
Marshak injected one word of warning about the photoproduction resonance. The rise in the cross section comes only from dropping the recoil terms in the usual perturbation theory calculation and even a weak coupling calculation would predict a drop in cross section beyond a certain energy. Oppenheimer added that the drop is a pretty major thing and thought that though it might in part have to do with the instrumentation and in part with the importance of recoil and in part with the shifting importance of quadrupole and dipole terms, it also does suggest that there is a maximum at a rather special energy for the system. Bethe commented with regard to the double valuedness of the $p_{1/2}$ to $p_{3/2}$ ratio, that if you replace Fermi's phase shifts by Yang's phase shifts, you do get exacly the same angular distribution in scattering.

Brueckner commented that the charge independence arguments which were discussed yesterday also show that if S wave is not active for neutral mesons, then the S wave photoproduction for charged mesons would be the same for neutrons and protons, and that these are very intimately connected together. Hence, if one could actually show the absence of that term in the neutral photoproduction for both neutrons and protons then one could conclude that the equality of the charged meson production is not at all surprising.

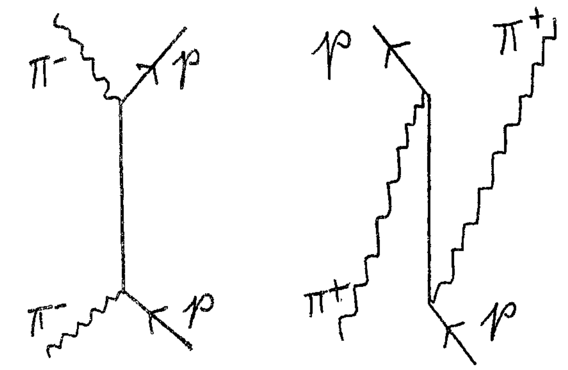
The discussion now shifted to the pion-nucleon scattering problem. Chew began this discussion with what he characterized as a simple-minded theoretical attempt to understand the problem on the basis of Yukawa's fundamental idea. He had agreed to make the following rather glib statements only with the understanding that Dyson and Bethe would not contradict him at this session, but would take up these points in the technical theoretical session. The main feature of the Yukawa theory is that the fundamental process consists of the emission or absorption of a single pion. If we assume that the motion of the nucleon is unimportant compared to the motion of the pion, that is, that nucleon pairs are not important, then the large interaction between the nucleon and pion must be in p states. This can be seen by considering the following diagram: The slow nucleon has angular momentum 1/2 and we arbitrarily define its intrinsic parity P as positive. Then the emitted nucleon continues to have j=1/2 and positive parity, and we can ask what must be the angular momentum and parity of the emitted meson. Clearly it can only have angular momentum equal to either 0 or 1, and since its intrinsic parity is negative with respect to the proton, the parity of the angular momentum state must be negative. Therefore parity restricts us to $\ell=1$, that is, to p states.



It is clear that in order to absorb an s state meson, nucleon pairs must be employed as indicated in the second diagram. This is clear since the parity of the initial state is odd and the intrinsic parity of a nucleon pair is odd for Dirac particles; therefore, the same argument applies. Consequently, if we ignore nucleon pairs we need discuss only p wave interactions. Chew has reason to believe that the coupling is in fact intrinsically weak, although these arguments are certainly controversial; however, one can start out by being optimistic and see where this leads.



The problem is, therefore, to discuss the basic meson-nucleon interaction using only these fundamental ideas from the Yukawa theory. The two basic processes are then schematized in the following diagram. The first diagram indicates



the absorption of a negative meson by a proton followed subsequently by the reemission of the negative meson. The second diagram consists of the emission of the final positive meson prior to the absorption of the initial positive meson by a proton. The second process is less controversial since it does not involve the difficulties with the self energy of the nucleon. Therefore, the discussion will be limited to the process in which the final pion is emitted before the absorption. We characterize the p wave coupling by a symbol f and ask what phase shift it will give rise to if it is small. By using a straightforward perturbation theory and charge independence, it is then possible to break down the p wave scattering into four non-interacting states characterized by angular momentum 3/2 and 1/2 and isotopic spin 3/2 and 1/2. The corresponding phase shifts have been called by Fermi α_{33}, α_{31}, α_{13}, and α_{11}, where the first index is twice the isotopic spin and the second index is twice the angular momentum. The result of the calculation is given in the table below:

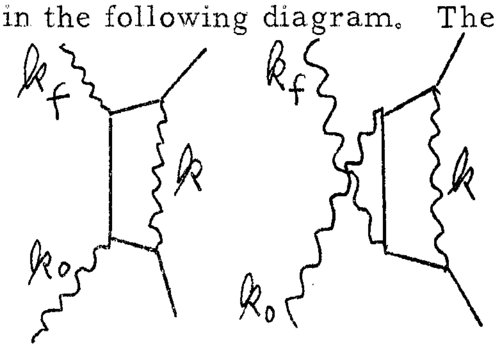
$$\alpha_{33} = 2x \qquad \text{where } x = \frac{2\, f^2\, k_o^3}{3\mu^2\, w_o} \qquad \text{and} \quad k_o = \text{meson momentum}$$

$$\alpha_{31} = -x = \alpha_{13} \qquad\qquad \mu = \text{meson rest mass}$$

$$\alpha_{11} = -4x \qquad\qquad w_o = \text{meson energy } (\mu^2 + k_o^2)^{1/2}$$

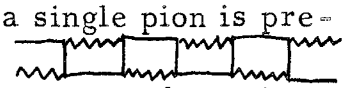
These are the well known weak coupling results for the p wave Yukawa scattering and are in disagreement with experiment; for example, they predict that the sc er-ing of positive or negative mesons have the same cross section while the charge exchange scattering is smaller; further, that the angular distributions are isot for the ordinary scattering and $\cos^2\theta$ for the charge exchange scattering. So it was formerly thought that the weak coupling approach could not possibly explain the experimental results.

However, Chew was more optimistic because of the indications that the interaction is in fact essentially weak and calculated the fourth order non-relativistic corrections, which are relatively simple. He wishes to emphasize that this should not be characterized as a pseudovector meson theory calculation since it is based only on momentum and parity arguments and not on a statement about the basic nature of the coupling. Two of the basic higher order processes can be schematized in the following diagram. The first describes the emission of a virtual meson of



momentum k, the absorption of the initial meson with momentum k_o, the emission of the final meson with the momentum k_f, and finally the reabsorption of the irtual meson with momentum k; clearly this diagram must be summed over all virtual momenta k. The second diagram indicates a similar process in which the final meson is emitted before the initial meson is absorbed. Both are proportional to f^4 and both contain an integration over intermediate momenta. It soon became apparent that there is a very basic difference in the size of the contribution from each of these two diagrams. In the first case only one meson is present at a time, while in the second case there are two additional mesons present. That means that in a perturbation calculation the energy denomenator which is associated with the intermediate state in the first case can become very much smaller than it can in the second case, because one can have an intermediate meson with an energy quite close to the energy of the initial meson. Therefore, when this energy approaches the energy of the incident meson, one will get an unusually large contribution to the scattering. This is well known from ordinary scattering calculations. For example, if you try to calculate the nuclear force according to meson theory and find the matrix element for nucleon-nucleon scattering, and calculate the scattering with it you obtain a very poor answer; but if the matrix element is used to derive a potential and this potential is then used to calculate the scattering [illegible] answer obtained is much better. The reason is simply that the second procedure takes into account higher order states which can have energies quite close to the initial state.

(Discussion was choked off at this point by Oppenheimer with the comment that Bethe and Dyson have renounced all our rights to make any comment.)

The procedure is therefore to take into account a sequence of higher order terms characterized by intermediate states in which only a single pion is present. This can be schematized by the following diagram: 
The first thing we find is that we should be calculating the tangent of the phase shift instead of the phase shift. This well known result is due to Heitler and corresponds to picking out just the intermediate state with the value of the momentum of the intermediate meson equal to the initial momentum. In our approach we propose to keep in addition those values of k in the same neighborhood as the initial momentum and not just that value which is precisely equal to it. The result is that the original formulae are damped in the following way:

$$\tan \alpha_{33} = \frac{2x}{1 - 2\Delta}$$

$$\tan \alpha_{31} = -\frac{x}{1 + \Delta} = \tan \alpha_{13}$$

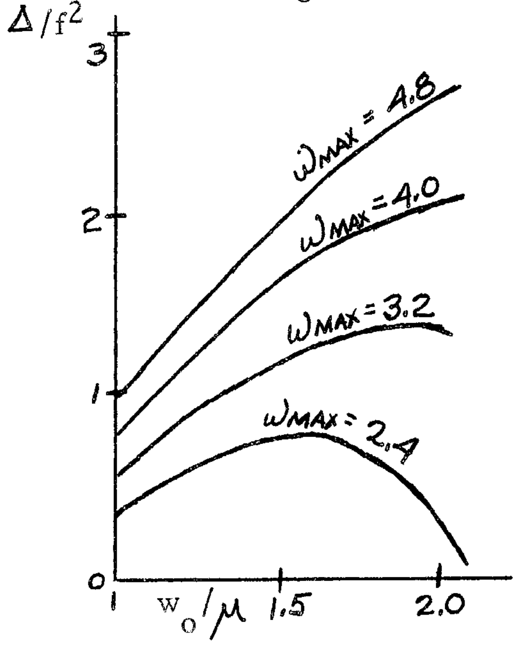
$$\tan \alpha_{11} = -\frac{4x}{1 + 4\Delta}$$

Here Δ is the integration over intermediate meson momenta or, as Chew calls it, the reaction term in the scattering, given by the formula:

$$\Delta = \frac{2}{3} \frac{f^2}{\mu^2} \int_0^{k_m} \frac{d^3k}{(2\pi)^2} \frac{k^2}{w^3} \left[\frac{w_o}{w - w_o} \right]$$

This integral is divergent as it stands and so it is necessary to give a maximum value to k_m to cut-off the integral; the divergence is due to the omission of the recoil energy of the nucleon. For the energies now under investigation, [illegible] is intrinsically positive [illegible] reasonably high values of the cut-off momentum; this, of course, adds twice as many parameters to the theory as we had initially. It is seen that all but $_{33}$ are decreased by the reaction. There is a simple correlation which explains this, namely, positive phase shifts are increased by the reaction while negative phase shifts are decreased by the reaction, the change being proportional to the original size of the phase shift. The above formulae are based on one of Schwinger's variational principles. They are valid so long as the non-relativistic cut-off approach is valid, and even if the cut-off approximation fails they will indicate correctly the direction and order of magnitude of the reaction effects. As Dyson will point out, if the high frequency pions cannot be eliminated, then the variational formula is quantitatively inadequate for the 33 state when a resonance occurs. The values of the parameters, f^2 and k_m, which are used here to fit the data as shown correspond to the resonance being approached but not reached.

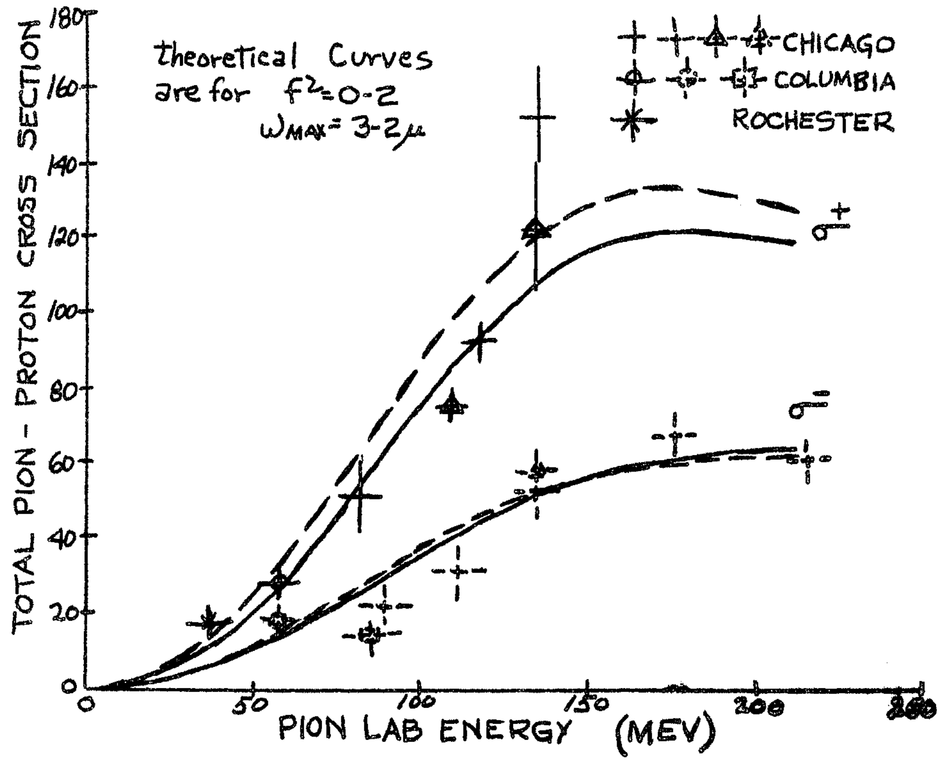
Physically, our method corresponds to saying that there exists a potential between the meson and the nucleon which is giv[illegible] by the first order matrix element of the interaction. The iteration of this potential then gives [illegible]ive corrections. The sign of the matrix element gives the sign of the potential, a positive sign corresponding to a repulsive potential. An attractive potential gives a positive phase shift, in this case α_{33}, and as usual the reactive effects for an attractive potential are larger than for a repulsive potential. It is clear that the scattering will have a resonance in the 3/2-3/2 state if the reaction Δ is equal to 1/2. If the cut-off is fixed, the resulting behavior of Δ is indicated below. In the present experimental region Δ is increasing and we are approaching a resonance in a certain sense. It is still necessary to see if an appropriate choice of the cut make the higher order terms negligible. First, it is shown that the two parameters can be chosen to obtain agreement with experiment for $f^2 = 0.2$ and $k_m = 3.2\,\mu$. The agreement obtained is not a critical test of the parameters, since if α_{33} is given correctly by some combination of f^2 and k_m, another combination which also fits α_{33} will produce little change in the results. The cross sections and phase shifts calculated for these parameters are given in the table below, in comparison with the experimental values given by Anderson, Fermi, et al, Phys. Rev. 86, 793 (1952).



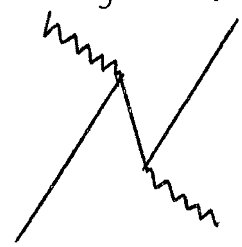
$$\frac{d\sigma}{d\Omega} = a + b\cos\theta + c\cos^2\theta \quad \text{(millibarns per steradian)}$$

Process	a	b	c
$\pi^+ \rightarrow \pi^+$	3.8 ± 2.2 (6.3)	-6.8 ± 2.7 (0)	17.5 ± 6.6 (9.4)
$\pi^- \rightarrow \pi^-$	1.2 ± 0.2 (1.2)	-0.1 ± .3 (0	0.3 ± 0.7 (0.4)
$\pi^- \rightarrow \pi^0$	1.1 ± 0.6 (1.0)	-2.5 ± 0.5 (	6.3 ± 1.9 (5.1)

Theoretical values for $f^2 = 0.2$, $w_{max} = 3.2\mu$ are given within the parentheses. Note that the old bad feature of $\sigma(\pi^+) = \sigma(\pi^-)$ has been overcome. If the S wave is small, it shows up only in the interference so that the correctness of this theory for the p wave is checked approximately by comparing with a and c alone. Hence the gross disagreement of the theoretical predictions with experiment has been eliminated; further, the approach is consistant since the terms which have been dropped are (with the above choice of parameters) only about 10% of those retained. Graphical comparison with experiment is given in the accompanying figure. The connection of Chew's approach to a more sophisticated theory will concentrate on the discussion of k_m. Chew used to be skeptical that the conventional γ_5 theory would give a sufficient cut-off but he is no longer so skeptical of this point. However, heavy mesons may well spoil complete agreement, so that Chew feels that it is sensible to work with a cut-off theory until a complete relativistic calculation is available.



So far the s wave terms have been omitted in this theory. If we are to believe the pseudoscalar γ_5 theory with nucleon pairs, then the basic diagram is sketched below:

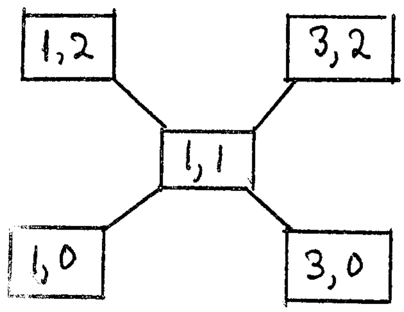


The s wave scattering gives a repulsive potential for both states and the result $\tan \delta_0 = G^2(k_0/M) \;/\; (1 + 0.35\, G^2)$ where M is the nucleon mass. The numerator is the weak coupling result with the nucleon mass appearing due to the pair creation. The denominator due to reactive effects has been estimated from the fourth order calculation of Ashkin, Simon and Marshak. If the value of G^2 is as large as proposed by Lévy, then $\tan\delta_0$ is essentially independent of G^2 and is given by $\sim 3\, k_0/M$. Chew assumed that the damping would be the same in both isotopic spin states since the potential is the same for both, but this may well be incorrect. Damping does cut down the s wave scattering (which is in fact that due to a repulsive potential of short range) in spite of the large coupling constant, although it does not reproduce the difference between the two isotopic spin states or the rapid energy dependence. The discussion of these points will be left to Dyson. Chew's own feeling is that only the p wave calculation is believable since it does not involve the relativistic properties of the nucleon.

Dyson then described the investigation of the pion-nucleon scattering problem by the theoretical group at Cornell, which was started as a direct consequence of hearing what Chew had done. The people working on this are Bethe, Dyson, Salpeter, Ross, Sundaresan, Schweber, Mitra, and Visscher. We attempt to carry out the calculation of the meson-nucleon interaction using the full blooded relativistic theory, and in particular take seriously the relativistic properties of the nucleons. The method adopted is due to Tamm and Dancoff and will be explained briefly here. If we use the relativistic meson theory we find that a complete description of the meson-nucleon system cannot be expressed in terms of a single particle or a two particle wave function; one needs a wave function which represents a mixture of states with all kinds of numbers of particles present. That is,

$$\begin{aligned} & \psi_{1,1} + \psi_{1,2} + \psi_{1,0} + \cdots\cdots\cdots \\ \psi = & + \psi_{3,1} + \psi_{3,2} + \psi_{3,0} + \cdots\cdots\cdots \\ & + \psi_{5,1} + \cdots\cdots\cdots \end{aligned}$$

Here the first subscript refers to the number of nucleons present and the second to the number of mesons present, so that the first line corresponds to a single nucleon with 1, 2, 0 mesons, etc. present while the second line corresponds to three nucleons present with 1, 2 or 0 mesons, etc. The fundamental equation of the theory is the simple Schroedinger equation $H\psi = E\psi$ where $H = H_0 + H_1$ and H_0 corresponds to the non-interacting particle Hamiltonian, that is, H_1 corresponds to the energy of interaction. When this equation is expressed in terms of the components of the wave function, it becomes a complicated infinite array of coupled integral equations, and there is no chance of obtaining an exact solution. The basic idea is to restrict all considerations to a certain portion of the wave function, but to calculate the matrix elements exactly within this subspace. If we draw a coupling scheme, the states directly coupled to the initial state can be schematized as follows:



The approximation therefore consists in throwing away all other states. When this is done we find, for example, an equation for the one meson part of the wave function $\psi_1(k)$ defined by the equation $(H_0 - E_k - E)\psi_1 = \int dk' \; H_1(k,k')\psi_2(k,k')$. Here $\psi_2(k,k')$ is a wave function for two mesons of momenta k and k'. There will then be a second equation which defines ψ in terms of ψ, and in general other wave functions as well, which are however dropped by our fundamental approximation. It is therefore possible to substitute the expression for ψ into the original equation and obtain an equation for ψ_1 alone. This is precisely Lévy's procedure in the neutron-proton system, only he has carried it much further. However, we stopped here as it was not clear how to go any further. The main complication in the pion scattering problem is that the relativistic behavior of the nucleons is taken seriously. In Lévy's analysis of the neutron-proton system, he could make a consistent non-relativistic approximation, that is, he assumed that the nucleon wave function only contained low momenta and his final result confirmed this assumption. This is by no means the case for pion-nucleon scattering.

We were able to write down an integral equation for the ψ_1 part of the wave function alone, which restates the Schroedinger equation in our approximation. We were able to derive individual equations for each scattering state much as Chew has done. The phase shifts have been computed from these equations by means of a variational principle used by Chew. They confirm Chew's results very well. The relativistic properties do give cut-off at momenta comparable to the nucleon rest mass. Therefore we find the same qualitative behavior as Chew for α_{33}, namely a strong attractive potential close to resonance which is sensitive to the strength of the interaction. The other phase shifts are insensitive functions of the energy.

We have also tried to solve the isotopic spin 3/2 integral equations numerically in order to get a check on the general behavior of the space wave function and to estimate the accuracy of the variational principles used. It turns out that the estimates made by the Chew method for the α_{33} phase shift are very bad. For example, for $G^2 = 10$ at 110 Mev, the Born approximation result (2x) is approximately 5°. The estimate by Chew's method gives a denominator of approximately 1/5 and hence a phase shift of about 25°. But the exact solution yields 9°. The reason is simply that the wave function is far from correctly given by the Born approximation. The reactive terms are greatly overestimated by such a variational principle. Therefore, it is difficult to get solutions for the α_{33} phase shift, which are accurate enough to be useful, even if we ignore the inaccuracy of the starting equations.

Actually, Bethe has performed a rather complete calculation which will be discussed in the theoretical session tomorrow. However, the results will be reported here to the full group. He used a coupling constant $G^2 \approx 14$ and obtained phase shifts approximately as a function of en gy. The difficulty is that the actual energy of scattering occurs as a parameter and hence one has to solve the integral equation for each energy, which makes the amount of work very great. However, by approximate methods, Bethe finds that with this value of the coupling constant, the experimental values of the α_{33} phase shifts are fairly well represented; they go through a resonance at about 200 Mev and come down very sharply on the high energy side. This fall is much more rapid than a single term resonance formula would give, and is apparently in cated experimentally in the photoproduction, although the exact connection to the photoproduction is not at all clear.

Salpeter has also obtained a solution for the s states which confirm the results of Drell and Henley. That is, he finds a spin independent short range repulsion. Th one has the scattering by a small hard sphere of radius roughly twice the Compton wave length of the nucleon, and the results are in good agreement with theirs. The results are, however, not in good agreement with the experiment. As Chew has said, the s phase shift is proportional to the momentum and the order of magnitude is correctly given as 15 to 20° at 135 Mev. But the energy dependence is quite wrong. Furthermore, isotopic spin 1/2 phase shift is not calculated consist ntly. Experimentally it should be small; theoretically we don't know what it is. But the theory is unambiguous for the isotopic spin 3/2 s phase and gives a variation linear with the momentum. Therefore, if the phase shift is large enough at 135 Mev, it is much too large at 80 Mev and this is not by any means a consequence of our way of doing things. The point is, it does not matter to what order of perturbation theory you go, it does not matter how you set up your equations, as long as the meson must come in and interact with the proton, then the interaction has a range which is of the order of the proton Compton wave length, and a repulsive interaction of this range cannot give you phase shifts which are essentially different from hard sphere phase shifts. So the experiments re are certainly very interesting because they show unmistakably that there is a force of some kind of longer range than that. is is not included in our theory and if you went to better approximations would still not be included. Therefore, in S states at least we have something in the nature of a long range force acting in addition to the direct interaction of meson and proton. This is understandable only as a direct interaction of the incident meson with the meson in the meson cloud, which extends out to 10^{-13}cm from the proton. This would possibly explain the rapid energy variation of the s phase shift but this calculation has not been made exact as yet. So we cannot expect quantitative agreement without the inclusion of the long range term.

Oppenheimer asked Dyson to comment on the one parameter character of his theory and the problem of renormalization. Dyson said that one parameter is certainly an advantage. With regard to renormalization, we have to pay the penalty for a relativistic theory, with the result that so far this difficulty has not been overcome in the isotopic spin 1/2 state. The value of the coupling constant will certainly be strongly influenced by what is done about the renormali :ion and it is already known that the α_{33} phase shift is extremely sensitive to the value of the coupling constant; for example, the resonance at 200 Mev which is obtained with a coupling constant of 14 is reduced to zero energy if the cou· ling constant is increased to only 14. 6.

DISCUSSION OF FERMI'S NEW PHASE SHIFTS; FURTHER INFORMATION ABOUT MEGALOMORPHS.

Saturday morning, Professor Oppenheimer presiding.

Oppenheimer opened the session by remarking that he thinks it is hardly necessary to say in behalf of everyone who has spoken on nuclear forces and π-mesons that it is not of course a question of getting a complete description of what goes on from the pseudoscalar meson theory. No one has any notion, for instance, of how one could in this way understand the masses of proton and neutron, or the magnetic moments of proton and neutron, or the difference between them, or the electrical properties of neutron, and no one understands how one will in detail get the small deviations from charge symmetry, but there have been big changes in the last few years. These are, that instead of on the one hand using manifestly inadequate mathematical tools to find out what this theory predicts, and on the other hand waving generally in the direction of the unknown, one has now found some way of getting a little closer to what the theory predicts. I think no one is sure that one can even read the theory with arbitrary accuracy; that is, that something better than a ough solution which cannot be impro d upon exists. This is an open question and I have no wisdom to add to it. But the point now is that one can recognize in the consequences of the theory some things which bear a remote resemblance to what is found in real life. So the comparison is instructive, and a good example is just the deviations from charge symmetry. If one had not thought of charge symmetry, one would not have noticed the 1% deviation. That is a problem for the future. In the same way if the p ogram outlined by Lévy, or the program outlined by Dyson should be successful, the success would be in indicating what was wrong. You couldn't do that before, ice there was no similarity between what seemed to be implied by these equations and anything anyone ever found, and it is only in that very general sense that it seems to me that an immense progress may have been started.

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Fermi then presented the new data for which the conference had been waiting, first remarking that the courier when he got here handed him a small piece of paper on which there were written, in a cryptic fashion as is proper for something that comes from Las Alamos, certain numbers which then had to be decoded. Fermi then presented to the conference certain essential results which he had converted from duo to decimal notation. He has since supplied us with the full and correct data, and these are given in the table below:

Mev	Type	η	α_3	α_1	α_{33}	α_{31}	α_{13}	α_{11}		
			Phase Angles (degrees)							
53	Conv.	0.78	0		- 9	- 2			Brookhaven	
78	Conv.	0.97	6		-13	3				
113	Conv.	1.20	13	- 7	-27	- 1	12	14	Old Data	
113	Yang	1.20	13	- 7	-10	-36	14	10	Old Data	
135	Conv.	1.325	21	- 3	-38	-11	17	4	Old Data	
135	Yang	1.325	20	- 2	-21	-49	15	9	Old Data	
					New Data					Least Squares Sum
120	Conv.	1.24	17.8	-10.2	-31.6	- 4.1	0.3	3.1		1.44
120	Yang	1.24	30.1	4.6	-13.1	-29.5	6.3	10.5		6.05
135	Conv.	1.325	16.1	-11.1	-41.8	- 6.1	1.2	5.1		1.25
135	Yang	1.325	40.5	5.9	-19.6	-33.9	7.8	13.8		4.75

It will be noted that the calculated cross section represents the observed cross section very well. The phase shifts have no business to represent the observations so well. That is, for the nine measurements this set is inconsistent statistically with the errors given. The most striking difference from the previous results is in the α_{13} and α_{11} phase shifts. Fermi had noted more or less empirically the extreme sensitivity of these angles to a change in cross section. They have never changed sign, but they have varied all over the map. He was sorry to report that Yang's solution is much worse with the new cross sections although this is really a trick of arithmetic and, cheer-up, maybe they are very good. The result can be expressed by giving the least square constant for the two solutions. For Fermi's solution the least square constant is approximately 1.44, which is a value that is very much too small. That is, the six variables are adjusted to minimize the least squares constant and if the errors were correctly given one would obtain the value 9; Yang's solution is much worse in the sense that it corresponds to a constant of 6.05 but this is still well within the experimental error. The change in Yang's solution due to the new data is much more striking than that in Fermi's but it still has the feature of α_{31} being large and α_{33} being small, so that in this sense it is still recognizable. A second point about the new data is that the α_3 phase shift now is lower for the higher energy. This is probably a trick of the errors, and the cross section may easily still be rising with energy in this region, although the smoother dependence given originally no longer appears so convincing.

Bethe made the following comment on the two sets of phase shifts, considering the simplest case of a single isotopic spin state, that is, the elastic scattering of positive π mesons. In this case, only α_3, α_{33} and α_{31} enter, if there are no d waves present. That is, the experiments are completely described by the s phase shift, the p scattering amplitude without spin flip, and the p scattering intensity with spin flip. The latter intensity is given by $\sin^2(\alpha_{33} - \alpha_{31})$. Therefore, if the sign of the difference is changed one still obtains the same intensity. The <u>average</u> p amplitude is given by

$$A = 2e^{2i\alpha_{33}} + e^{2i\alpha_{31}} - 3 \qquad (1)$$

Bethe now proves that, given a solution that yields a certain value for A, there always exists a second solution that yields the same A and hence identical p scattering and an identical interference term with the s state. The s wave scattering amplitude is regarded as definitely given except possibly for sign. Consider the quantity A' defined by

$$A' = A + 3 = e^{2i\alpha_{33}}(2 + e^{2i\Delta}) \qquad (2)$$

If there exists one solution α_{33}, Δ, then clearly

$$|A'| = |2 + e^{2i\Delta}| \qquad (3)$$

Now we have seen that, as far as spin-flip is concerned, we are permitted to replace Δ by $-\Delta$, and if α'_{33} is the corresponding value of α_{33}, we must have

$$A' = e^{2i\alpha'_{33}}(2 + e^{-2i\Delta}) \qquad (4)$$

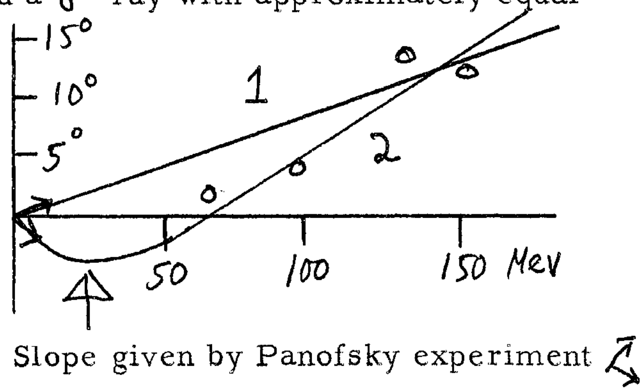
But by (3), the absolute values of the two sides of Eq. (4) are equal; therefore it must be possible to find a solution α_{33} such that also the complex phases are equal, Q. E. D.

Fermi remarked that if Bethe's point were true in general, then a representation in terms of a given set of phase shifts and a second set of the Yang type could never differ in any respect. In particular, simply as a matter of formal mathematics, they could not give appreciably different least squares constants, as is actually the case. It is true that when one starts with a different set of approximate angles for the machine to minimize one does get a scatter of about 0.01 in the least squares constant for the final solution, but the difference between the least squares constants for the two types of solutions is much larger than this. Then Yang remarked that if $(\alpha_{33} - \alpha_{31})$ is of the same order of magnitude as $(\alpha_{13} - \alpha_{11})$, then the equivalence of the two solutions is exact, but not otherwise. Thus the scattering of positive mesons can always be fitted as Bethe's remarked, and the scattering of negative mesons at 0 and 180° but not in general at 90°. H. L. Anderson made two remarks: (1) Yang's transformation is designed to maintain the same cross section of any one isotopic spin state separately, but does not control the relative phase; (2) The transformation itself would keep α_3 and α_1 the same; however, the machine starts at this point and finds a better solution than the strict Yang transformation would give. Marshak remarked that, independently, the Rochester group has also found a second set of phase shifts by starting from the T = 3/2 case and finding the alternate exact solution (α_3 staying the same); it was then found possible to fit the full data within the errors by a second set of phase shifts.

Bethe then made the following remarks on the sign of the phase shifts. Every theorist who does meson theory gives opposite signs to those listed by Fermi. There is, in fact, experimental evidence which tends to show that the signs should be turned around; this comes from the scattering of mesons by carbon. We heard last year (cf. Byfield, Kessler, and Lederman, <u>Phys. Rev. 86</u>, 17(1952)) that there is interference between coulomb and nuclear scattering showing that e nuclear scattering is attractive at 60 Mev. The question then is which partial wave is responsible for this scattering. Bethe believes that this is mainly p wave scattering; firstly because at these energies the p phase shift is the largest and secondly because it has a weight factor of 2 as compared to 1 for the s phase shift so that it will predominate where cos θ is large, that is, precisely in the region where the interference with the coulomb scattering takes place. Further, the Brookhaven data at 60 Mev indicates very small s phase shifts which might even be zero, and the experiments on carbon were done at 60 Mev. Further, the analysis of the carbon experiments themselves carried out by Peaslee gives evidence that the p wave scattering does predominate. That is, he showed that the angular distribution looks like p wave scattering as modified by the nuclear form factor to be expected from carbon with a given nuclear radius. Therefore, there are good arguments to believe (a) that the nuclear scattering is p scattering and (b) that the nuclear scattering is attractive, which means that the signs should be turned around.

the added the third remark that it might prove possible to decide on the sign of $(\alpha_{33} - \alpha_{31})$ from other experiments. This is possible if one uses a model such as that of Brueckner and Watson or of Feld for the photomeson production. Recall that the photomesic interference term had as its coefficient $a^*(b-c)$ wher a, b, and c represent the s, $p_{1/2}$, and $p_{3/2}$ scattering amplitudes respectively. If the phase shifts are small, then these amplitudes are real and negative if the potential is repulsive, or positive if the potential is attractive. From Fermi's analysis, s and p phase shifts have opposite signs. Therefore, one can decide whether b - c is positive or negative from the experiments, and if one elieves this analysis and the signs have been inserted correctly, the backward maximum of photoproduction shows that the $p_{3/2}$ phase shift is larger than the $p_{1/2}$ phase shift which decides for Fermi's set rather than Yang's. Bethe is somewhat uncertain of this conclusion because it depends on the model used for the photo-effect; but it is clear that there exists a possibility of deciding the question.

Fermi remarked that the recent results on the photo-effect permit one to anchor the energy dependence of the very low energy phase shifts, unfortunately still with a plus or minus sign. There is some not inconsiderable evidence for the low energy s state production of pions. Panofsky found that when a π^- meson is captured from a Bohr orbit in hydrogen, it gives rise to a neutron and a π^0 meson, or a neutron and a γ- ray with approximately equal probabilty. The first reaction is essentially a form of charge exchange scattering at very low energy, while the second is simply the inverse of the photo-effect at very low energy. Therefore, since we know the photoeffect at very low energy, it is possible to calculate from it the charge exchange scattering at low energy.



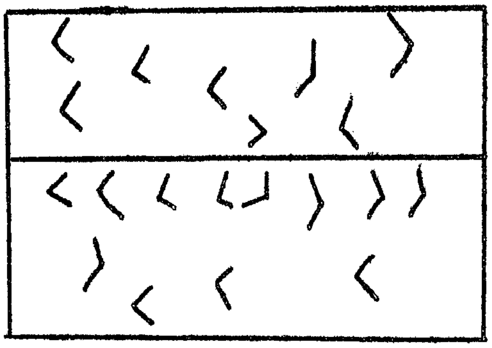
Slope given by Panofsky experiment

..gives the slope of the α_3 phase shift plotted against energy as indicated above. Unfortunately, the high energy data are still so inaccurate that either ... polation indicated by curve 1 or curve 2 is possible. However, these can be distinguished experimentally in the region indicated by the arrow on the diagram by looking at the s-p interference, that is, at whether there is a backward or forward maximum in the scattering. If the backward scattering observed at high energy were to shift to forward scattering at low energy, it would be evidence for the extrapolation given by curve 2 rather than by curve 1. Marshak commented that he had tried to fit the old high energy data and the slope at zero energy phenomenologically by using a monotonic potential for the s wave scattering (in the 3/2 isotopic spin state) and found this to be impossible. However, by using a repulsive core potential surrounded by an attractive tail, it was possible to obtain a fit of the type indicated by an inverted curve 2. Fermi warned that there is a weakness in the argument since α_1 is not zero and introduces another parameter into the theory. Marshak went on to point out that Dyson's model including a meson-meson interaction term would seem to indicate the type of isotopic spin 3/2 s wave potential with a repulsive core and attractive tail that he had arrived at phenomenologically. A second remark was that Van Hove has calculated coulomb interference with nuclear scattering in hydrogen at 40 Mev and finds a factor of 2 difference between the two signs for the s phase shift, using the 17 mb cross section as measured by Barnes. This cross section is large and the effect would be even greater if the cross section were smaller. Therefore, it is in fact possible to settle the sign of the phase shift unambiguously by experiment. Fermi agreed with this remark.

Chew remarked that all theoretical calculations agree that only the α_{33} phase shift is attractive and the $\alpha_{13} = \alpha_{31}$ very closely. He, therefore, feels that the attempt should be made to try to analyze the data under these restrictions and to see if they still can be fitted. If this should prove to be impossible it will then become imperative to look at the terms which have been omitted in the analysis.

Wentzel remarked that looking at the new figures in a quite unbiased way one can say that the 3/2 resonance is even better shown. Schiff noted in connection with the repulsive core remark by Marshak that Lelevier some time ago had attempted to fit the meson scattering in carbon with such a model. He found that the attractive region gave the observed coulomb interference at small angles while the core gave the observed large angle scattering. Brueckner warned that one characteristic feature of perturbation theory is the appearance of d with s waves at high energy. Therefore, the energy dependence of the s wave phase shift should perhaps not be taken too seriously as the d wave could easily make possible a rapid energy change. Bethe stated that he did not agree with this at all. It is true only for the pseudovector theory which he is sure is not right. In the pseudoscalar interaction the d wave is truly a quite small perturbation and the s wave is something all its own.

Oppenheimer asked Leighton to report on the recent Cal. Tech. work on the charged V particles. Leighton announced that they believed that they had evidence of particles that appeared to be the charged counterpart of the neutral V_1^0, that is, an unstable charged particle more massive than a proton. Recently, Manchester published a paper which indicated doubt concerning the existence of any charged V's heavier than a proton. All measurements of the mass of the decaying particle indicated a mass less than that of the proton and all of the decay products appeared to have masses consistent with the π mass. The first indication that there might be two types of charged V's came as a result of the study of the lifetime of the charged V. The decay points of all the charged V's are indicated in the diagram below. Qualitatively there appear to be many decays very close to the plate between the two chambers. The decays occurring in the upper chamber appeared to be spread more uniformly throughout the chamber. Next the ratio of positive to negative V's formed above the chambers and in the plate between the chambers was measured. The results are given in the following table. The probability that such a distribution of + and - particles should occur by chance is about 0.001 (assuming equal numbers of V^+'s and V^-'s with equal lifetimes.)

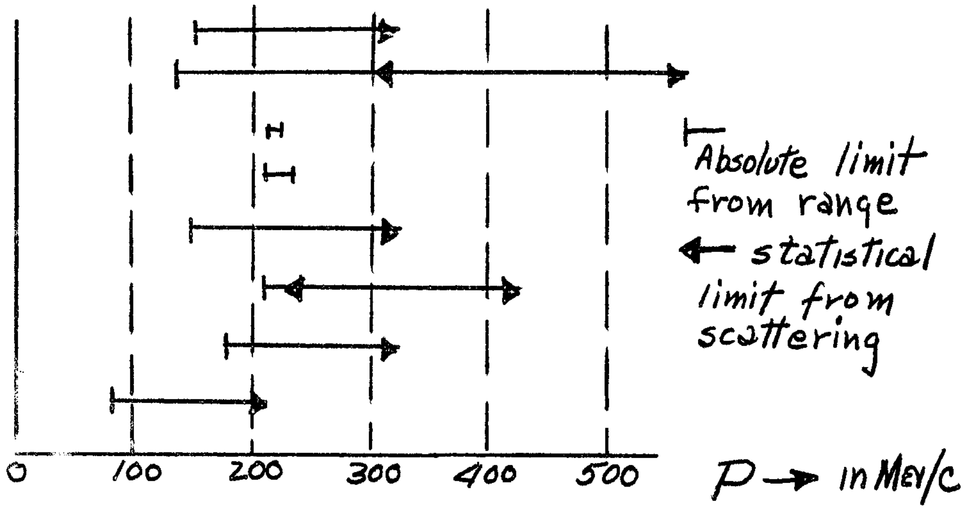


	+	-
Formed above	4	15
Formed in Pb between chambers	14	6

Here Leighton showed a slide of V^+ decaying in which the decay particle was very probably a proton. Leighton said that such a decay might be simulated by a scattering of a proton in the gas. The momentum change in this case was 90 Mev/c so that some blob of ionization from the recoiling argon nucleus should be visible. Rossi wondered whether the case shown might be a neutral V. Leighton replied that the parent particle appeared to originate at the point of interaction and that the primary particle appeared to have higher ionization than the secondary particle. Thompson asked whether this event might be a neutron-induced star in the gas. Leighton thought that the fact that the primary particle seemed to originate at the interaction point made his own explanation more likely. Reynolds said that from their experience with stars originating in argon that a recoil nucleus should have been visible if this were an elastic scattering. Peyrou questioned Leighton's calculation of the probability of the distribution of the relative numbers of positive and negative V's in the upper and lower chamber as the <u>a-priori</u> assumptions were not clear.

Oppenheimer then asked Rossi to give a report of the investigation on S-particles and charged V's at M.I.T. Rossi reported the work by Bridge, Safford, Courant, Annis, Peyrou and himself. Eight examples of unstable particles stopping and decaying (S particles) and six examples of $V^{\pm}$'s decaying in flight have been found. Most of the observations were made in the course of a series of 22,000 cloud chamber pictures. There seems to be a continuous transition from $V^{\pm}$ to S particles, so that calling them by different names is perhaps unnecessary. (This was illustrated by a picture of an extremely slow $V^{\pm}$ decaying in the gas.) However, the $V^{\pm}$ and also the S group might be made up of several kinds of particles in different proportions.

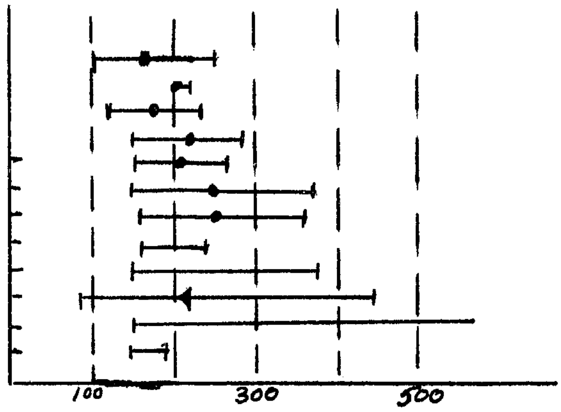
From the measurements of multiple scattering versus range of the S particles an estimate of their mass was made. The mass determined was $1470^{+410}_{-360} m_e$. In the calculation all possible mass values were given equal statistical weights. The + and - values were determined by using the $(1/e)^{1/2}$ maximum values of the mass on the probability distribution curve. These results indicate that S particles could be as heavy as protons but not as light as mesons. The S particles were assumed to be all positive since negative ones would be expected to suffer nuclear absorption. There were two cases in which the secondaries stop inside of the chamber. From scattering and ionization it was clear that the particles are probably π or μ mesons. The range of the secondary particles in the two cases was 65.6 - 67.7 g/cm^2 Pb and 64.4 - 74.3 g/cm^2 Pb. The range estimates were made to thicknesses smaller than the plate thickness by using estimates of the ionization of the particle. If the secondary particle was assumed to be a π meson the momentum imparted in decay is 213 $\pm$ 2 Mev/c, if a μ, 184 $\pm$ 2 Mev/c. In the other 6 cases the secondary particle leaves the chamber before stopping so that a minimum value of the range was obtained. In all cases the minimum value is less than the values given above. Also an estimate of the momentum was made from scattering. In all cases the measured momenta were consistent with a unique value of the momentum. The limits of momenta of the secondary particles are given in the diagram below. In reply to a question by Shapiro, Rossi said that the kinetic energy of the π secondary is about 100 Mev. In reply to another question Rossi noted that the chance of seeing an electron from a $\pi \rightarrow \mu \rightarrow e$ decay is very small. None has been observed in relation to a stopped secondary particle. No nuclear interactions of the secondary particles were observed. From the momentum of the secondary particles these particles are similar to the χ particles of Bristol.



There is a continuous transtion from V's to S's so that it is felt that there is the same type of particle among the V$^\pm$'s and the S's. There is the possibility that either the V's or the S's have components of particles not present in the other group. If the κ has a shorter mean life than the χ then we would expect to find more among the V's than the S's. There are 6 cases of V$^\pm$'s which have been analyzed. The first of these was the case obtained by Bridge and Annis. This V was seen to emerge from a star and to decay in the gas. The secondary particle undergoes a nuclear scattering in one of the cloud chamber plates. If the nuclear interaction was an elastic scattering then from the range measurement the momentum of the secondary particle was between 172 and 225 Mev/c. It is assumed in the calculation that the particle is a π meson which appears well established by the nuclear interaction. The β of the V particle is estimated from its ionization. In 4 of the remaining 5 cases one may make rough estimates of the momenta in the center of mass system. With no effort at all the measured momenta are all consistent with 212 Mev/c. The errors are large so that a spread is possible.

In reply to question by Shapiro, Rossi said that 4 V's came from outside and 2 were produced in the chamber. In one of the cases of V$^\pm$'s produced inside the cloud chamber the momentum of the secondary appeared to be less than 212 Mev/c; however, it was possible that the particle underwent an inelastic scattering in traversing one of the lead plates.

Rossi next discussed the Manchester results on the analysis of about 20 $V^{\pm}$'s. Manchester has one case in which the β (from ionization) of the primary and the P of the secondary are known. In this case $P_{sec} = 225 \pm 20$ Mev/c in the C. M. system. In 6 other cases the momenta of the primary and secondary were known. If one assumes a mass of 1500 m_e then the C. M. momenta of the secondaries were all consistent with 213 Mev/c. If one assumes a mass of 1, the agreement is not so good but is not ruled out. Rossi then showed the combined results of Manchester and M. I. T. on the P_{sec}. All of the cases appeared to be consistent with the momentum of 213 Mev/c. Rossi then discussed the Manchester argument for a three body decay through their distribution of transverse momentum. Their distribution of P_T seems to favor the three body decay. Their arguments were based on only 14 cases. Some of the transverse momenta were larger than the maximum allowable for a two-body decay; however, the errors were large. Rossi said that he and Putler were agreed that most but perhaps not all of the Manchester $V^{\pm}$'s could be χ's. Rossi concluded that probably all of the S's and many and perhaps all of the $V^{\pm}$'s are χ's.



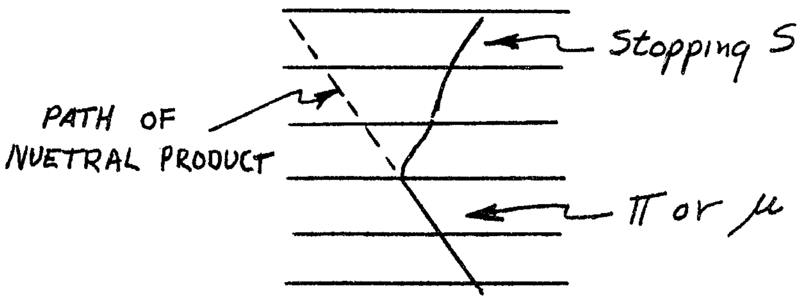
P in Mev/c
Manchester Data-

The S particles require a time in their own frame of reference of the order of 10^{-9} sec. for stopping. Thus their mean life cannot be much shorter than this. An estimate of mean life can be made from the relative number decaying in flight and at rest. Under the following assumptions an estimate of τ was made.

1. V's and S's are the same.
2. Only S particles decay after stopping.
3. How many of the $V^{\pm}$'s would have stopped had they not decayed. The last was determined from specific ionization. Using the above assumptions and allowing generously for possible errors in the ratio of the numbers of V's and S's the following results were obtained: $2 \times 10^{-8} \geq \tau \geq 2 \times 10^{-9}$ sec.

On the other hand, if it is supposed that all the V's are different from the S's then a lower limit of 2×10^{-8} sec. for τ is obtained.

The next question is that of the nature of neutral decay product. Assuming a mass of the $V^{\pm}$ of 1400-1500 m_e. This is suggestive of a V_2^o and in one case the Manchester group has seen a charged V apparently giving rise to a neutral V. This might be a chance coincidence. By looking along the path that the neutral decay product must have followed a total time of flight of 5×10^{-9} sec. has been so far observed without any signs of decay or interaction. Including the charged V's as well, the total time spent in the chamber by the neutral decay products must have



been of the order of 10^{-8} sec. Thus we must conclude that either the neutral product has a long life or else decays in an invisible manner. It is possible that the V_2^0 decays into two π^0's in which case the γ rays from the π^0 mi t easily be missed. Unless this type of decay predominates over the decay into charged π's by a considerable amount, it is extremely difficult to reconcile these data with $V^{\pm} \rightarrow \pi^{\pm} + V_2^0$ unless the lifetime of the V_2^0 is longer than now appears likely. If the mass were as low as 1,000 m_e, then it is possible that $V^{\pm} \rightarrow \pi^{\pm} + \pi^0$ and the γ rays from the π^0 might have been missed. Fermi: "If it were a neutral pion, do you not have a fairly high probability of seeing a shower?" Rossi: "We must look into this matter again but we are not very confident because the momentum of the π^0 is not very high, the γ 's will go at wide angles and will not be energetic, the plates are fairly thick, etc. Hence the π^0 could possibly be missed." "My impression is that we would have seen it but we need more data to be certain." Cocconi: "Might you not have discriminated against the cases in which the converts in the same plate that the decay occurs in?" Rossi thought that it was unlikely that such an event would be missed because one looks for the penetrating particle arising from the stopping particle. Rossi said that because of the close agreement between the best Manchester and M.I.T. measurements of the secondary momentum, that there was good evidence for a two body decay. Uhlenbeck: "Is it the conclusion that $\pi^{\pm} + \pi^0$ decay is possible or not?" Rossi: "The conclusion is that it is not very likely but possible." Marshak pointed out that if $V \rightarrow \tau^0 + \pi^{\pm}$ and $\tau^0 \rightarrow 2\gamma$ then the angle between the γ's would be quite large and the γ's might have been missed. Leighton: "Couldn't a lower limit be set on the mass of the $V^{\pm}$?" Rossi: "No, not from our measurements. We can not say that the mass of the V is greater than 1,000 m_e." Shapiro: "I think Perkins would agree that the photographic plate evidence would tend to exclude the neutral pion and tend to favor something of the order of the neutral V_2^0." Oppenheimer: "What does Perkins say? Perkins is here." Perkins: "We have only two χ mesons and the errors in the mass are rather large." Perkins then gave the mass values reported below.

P secondary		
225 ± 20 Mev/c	Manchester	charged V
204-235 Mev/c	M.I.T.	S
212-215 Mev/c	M.I.T.	S

1: $M \simeq 1450 \pm 300$
2: $M \simeq 1380 \pm 350$

Shapiro: "I made my statement on the basis of previously published errors of ± 100 m_e." Perkins: "This error is certainly unrealistic. You cannot exclude the possibility that the mass of the χ is as low as 1,000."

Rossi then wrote down the table below in order to give some idea of the abu ance of the charged unstable particles. Alvarez wondered whether any correction was made for charged V's that decayed in the lead plates in the chamber. Rossi said that this did not matter very much in their analysis; if the charged V were heavily ionizing when it entered the plate in which it decayed it might be misclassified as an S.

	Produced Inside	Produced Outside
V_1^0	21	
V_2^0	4	28
$V_?^0$	3	
S	2	4
$V^{\pm}$		3
π stopping		500

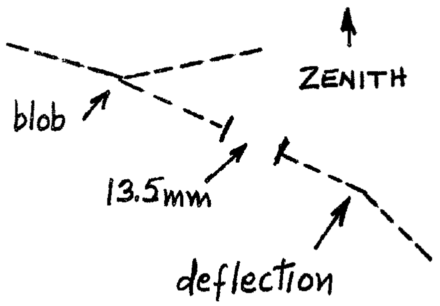
Perkins noted that since the five particles were presumably +, one should see the decay electrons from the stopping π^+ mesons. Rossi said that in the course of many stoppings of mesons in the cloud chamber that very few decay electrons were observed.

Heavy Mesons

Shapiro started with a comment on the report given by Rossi. "If we define the χ as Bristol does and the way that Rossi is willing, as a two-body decay, one of the products of which is a pion of unique energy about 100 Mev, then I think that it is significant that Rossi has apparently given us evidence for the production in nuclear collisions of these χ's. It is worth noting that there is no evidence from photographic emulsions for the direct production of χ's although there appears to be some evidence for direct production of K's.

Shapiro then proceeded with his report. The investigation described here was carried out in collaboration with D. T. King and N. Seeman. The tracks of lightly ionizing particles generated in energetic nuclear collisions have been studied. Four events involving the production of particles heavier than π mesons have been found so far. The tracks occur in 400μ emulsions exposed at 10 g/cm^2 depth above Minnesota. The plates were exposed vertically under 30 g/cm^2 of carbon or no absorber. Two of the particles seem to have a mass of 525 m_e. The latter particles resemble closely the $\rho^{\pm}$ reported by Powell at the Copenhagen conference but which Bristol seems much less sure of now. All of these particles which were described apparently arose from collisions of moderate energy--10 Bev or possibly much less. In none of the cases of production are there any black evaporation tracks. In each case of production there are three thin or grey tracks involved in the event.

In order to attain reasonable precision in the mass measurements, measurements were confined to long tracks of particles with relatively low velocity ($.5 \leq \beta \leq .8$). A lower limit of several thousand μ length gives an adequate number of independent cells if the particle is not too fast. An upper limit of 0.8 on β insures that the mean angle of scattering will not be too small, with cell lengths less than 400μ, for a reliable determination of $p\beta$, which is directly derived from multiple scattering. For reliable results on velocity measurement, the grain count must lie in a certain range of values. The grain count must be higher than the Fermi plateau value, otherwise g is too insensitive a function of β. On the other hand, if the grain count is too high, difficulty is encountered from overlapping grains. Grain densities between 1.3 and 2.5 times the minimum value permit good measurements of ionization and multiple scattering. Tracks which satisfy these conditions were examined for phenomenological evidence of unusual processes of generation or decay. About 25 interactions of fundamental type have been found. "Fundamental" means that all the charged particles are fast (thin or grey tracks). Most of the particles from these interactions are protons or pions within the experimental errors of about 10%. For the calibration P and π tracks of 2 or 3 centimeters length were used, (20,000-30,000μ !). A diagram of one of the four tracks is given below. The ionization was 2.2 times the minimum. The mass is determined to be 1270 ± 140 m_e. In the measurement, 54 independent cells 250μ long were used. For this cell length the scattering was about 5 times noise level for the system. The mean scattering angle is $\bar{\alpha} = 0.105 \pm 0.010$ deg. $/(100\mu)^{1/2}$. The grain density after the deflection was nearly 40% higher. The track deviates through 43°, almost entirely in a plane perpendicular to the emulsion. The secondary track leaves the emulsion after 460μ which does permit a fair estimate of β but not the mass. There was no recoil visible at the point of deflection. We therefore suppose that this is a decay. The time between production and decay in the particle's own rest system is 6.4×10^{-11} sec. It is worth considering whether this might be the decay of a $\chi^{\pm} \rightarrow \pi^{\pm} + N^{0} + Q$ where the $\pi^{\pm}$ has an energy of about 110 Mev. If this were the case the decay particle should have $\beta = 0.9$. The measured β is only about 0.4. This seems to rule out the possibility that this might be a decay of the χ type meson.

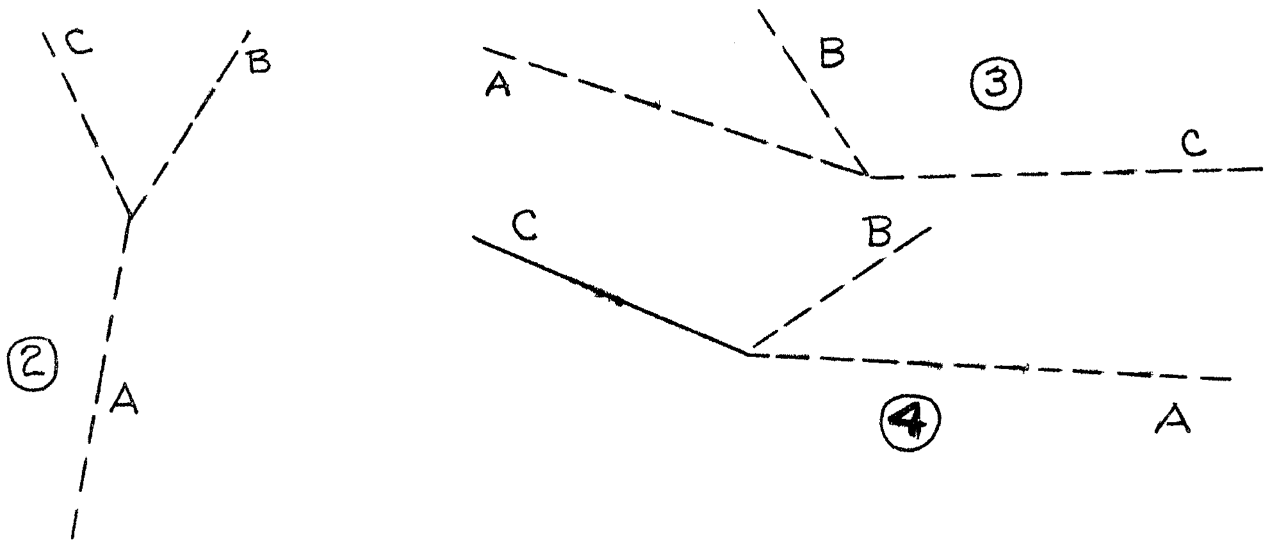


There is no evidence for the direct production of K's at Bristol. In the cases discussed yesterday of the direct production of K's, the one by Sorenson seems the best. The track length in that case of the decay product was 2500μ and the grain density was 1.1 plateau value. Perkins considered that this was well established as a muon; however, Leprince-Ringuet took exception with this. With that grain density and length it would be extremely difficult to distinguish between a pion and a muon. Since there seems no clear cut evidence for the direct production of K's it seems that our example is of special interest because it is possible to rule out the production of a χ.

If the daughter particle is assumed to be a pion, then a two body decay would fix its velocity in the center of mass system as 0.39 c, and the mass of the neutral decay product ~ 955 m_e. On the other hand, if the decay product were a muon, then the mass of the neutral particle is 1030 m_e. Both of these mass values are close to that of the charged τ meson or a V_2^0. If, on the other hand, the charged decay product is assumed to be a τ, then the neutral decay product must have zero mass and would presumably be a neutrino.

In order to avoid invoking new decay schemes, it is reasonable, provisionally, to regard this meson as a K meson. This case is unique in that both production and decay are observed and the particle is not a χ meson. Also, the production was associated with a particularly simple star, involving only two other tracks, both due to fast particles.

On the next page are schematic sketches of the other three heavy meson events. These events like the first case contain three thin or gray tracks. Track 2 has a length of 5,000μ before leaving the emulsion; no decay is observed. The velocity is 0.57c, as for particle 1. The mass estimate is 1240 ± 215 m_e where the magnitude of error is due mainly to the shortness of the track. As in example 1, there is a small blob at the origin.

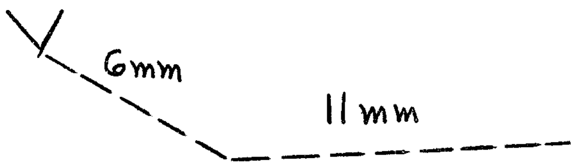


Track 3 goes 3,000μ and the mass is measured as 525$\pm$ 105 m_e. At the end of its travel it appears to suffer a deflection of 0.9°, and then it runs for another 15,000μ. The velocity after the point of deflection is the same as before to within experimental error of 3 or 4%, but its mean angle of scattering suddenly jumps by a factor of 2 and stays at this higher level for the remaining 1.5 cm. of travel. This is attributed to a decay in flight. The secondary mass is 265$\pm$30 m_e. The time between production and decay is 1.1 x 10^{-11} sec. The significant fact is not so much the deflection at 3,000μ but the change in the mean scattering by a factor of 2$\pm$0.3. The secondary particle is a well-behaved pion in showing just the right energy loss in the course of the 15,000μ as manifested by the change of grain density and the change in multiple scattering.

Particle 4 presents less favorable conditions for observation, mainly because its velocity is high, as in the Bristol examples, but it resembles particle 3 closely. Its track runs for 7,300μ, then undergoes deflection (through 0.7°), and an abrupt change in the mean scattering angle. The deflected track continues in the emulsion for another 4,000μ. The parent's mass is estimated as 457$\pm$100 m_e. The daughter particle's mass is estimated as 260$\pm$35m_e. The time of flight before the decay is 2.0 x 10^{-11} sec. The ratio of masses obtained directly from the scattering measurements is 1.8$\pm$0.3 (g remains constant).

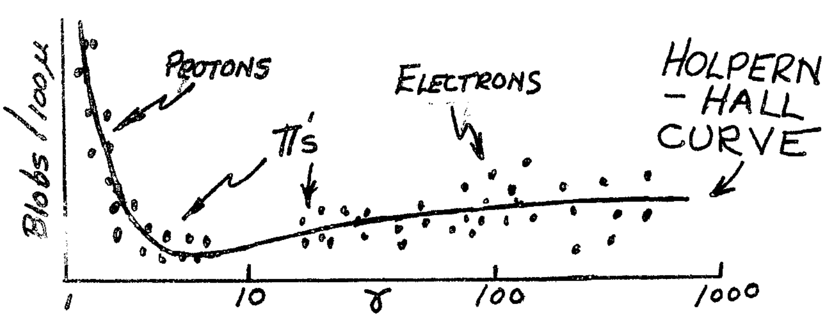
In response to a question Shapiro said that they had not seen evidence of any γ rays associated with the decay of the $\rho^{\pm}$; however, the chances of seeing any is pretty small. Shapiro said that there is nothing inconsistent in their data with the two body decay as proposed by Bristol. Fermi asked what Q value the decay of the $\rho^{\pm}$ shows. Shapiro: "The Q value is difficult to determine with any precision at all but I am sure that it is less than 6 Mev and it is likely that it is less than 1 Mev. If I use the transverse momentum which is not apt to give an answer wrong by more than a factor 2, I come out with the surprising answer of 40 Kev." Leighton commented that the transverse momentum can give a good lower limit on Q if the transverse momentum is accurately known.

Oppenheimer asked Perkins to give what evidence he had on the $\zeta^{\pm}$. Perkins showed a diagram of $P\beta$ vs. g for several hundred shower tracks. Four tracks were in the intermediate mass range between 276 and 1000 m_e. Three of these tracks had rather short lengths. Two of the tracks which gave an apparent mass of about 530 ± 60 m_e are rather long and are retained. The track which showed the decay looked as follows. The total track length is 17 mm; 6 mm from the beginning at the star there occurs an angular deflection. The first 6 mm of track gives a mass value of 530 ± 80 m_e. The grain density is 1.19 times minimum for the whole track. The next 6 mm of track gives a mass value of 265 ± 30 m_e. If the whole track is used for a mass determination assuming that no decay occurs a mass value of 303 ± 20 m_e is obtained. There is another track of 12 mm length which gives a mass value of 520 ± 60 m_e. This track gives no indication of a decay in flight. Bristol would consider as proof of the existence of the $\zeta^{\pm}$ the following type of evidence. Suppose a mass spectrum is made from long tracks of shower particles from many interactions. If $\zeta^{\pm}$s exist, some of them will live long enough to leave the emulsion before decaying and others will decay in the emulsion. The mass spectrum would show such a state of affairs as a continuous smear in the mass spectrum from the π mass up to a mass of about 500 m_e. This would be better evidence than giving a few isolated examples.



Shapiro said that he thought Perkins' remarks were well taken but still thought that it was remarkable that in both cases the scattering should change by a factor of 2. Perkins: "Have you split all your tracks in two and found the distribution in the ratio of the apparent masses given by the two halves?" Shapiro: "No, but it is a good idea." Perkins: "Occasionally you should find particles which give an apparent ratio of two." Oppenheimer: "I think it is clear to everyone that the fact that you get a large mass for the first part of the track and a smaller mass for the second part, these are not entirely independent things." In reply to a question Perkins said that if all of the tracks used in the determination of a mass spectrum were halved that the resulting mass distribution would be $\sqrt{2}$ times wider. Perkins: "Regarding the direct production of mesons, we have never asserted that there was evidence here (from the mass spectrum of shower particles) for the direct production of χ mesons. One obviously cannot see from the curve what the average mass is and I think the words χ were brought in by those who reported the conference. It is in the proceedings, but we didn't say it." (Laughter)

Shapiro then showed the table given on the following page with the data on the unstable particles. A plot of grain density [illegible] for relativistic velocities, by B. Stiller and M. Shapiro was given next. This curve differs from the curve obtained at Bristol last year. The above curve agrees very well with the Halpern-Hall theory (5 Kev $\nmid$ 2 Kev)



using restricted energy loss. This is very important in measuring masses. It may be noticed that the rise from the minimum to saturation comes gradually between $\gamma = 4$ and $\gamma = 100$ and the total rise is of the order of 14%. This may be compared with data obtained at Bristol by Voyvodic which shows saturation of $\gamma = 20$ and a rise of 8 or 9%. (See Appendix I)

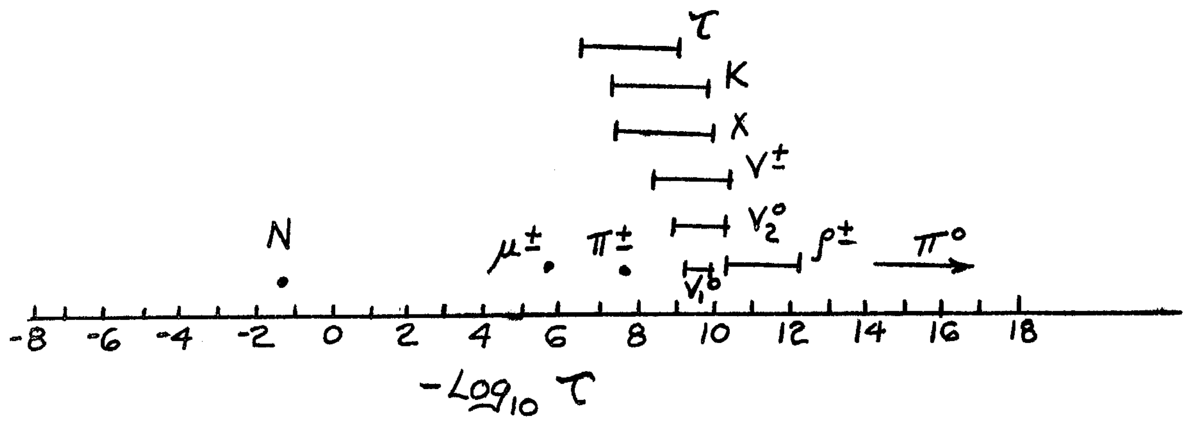
Data on Heavy Mesons

	Particle 1 P	Particle 1 D	Particle 2	Particle 3 P	Particle 3 D	Particle 4 P	Particle 4 D
$\bar{\alpha}$ deg/(100 $)^{1/2}$	0.105		0.105	0.132	0.260	0.122	0.22
$\gamma = 1/(1-\beta^2)^{1/2}$	1.220	1.13	1.225	1.45	1.45	1.58	1.58
$\beta^2 \gamma$	0.400		0.408	0.760	0.760	0.947	0.947
μ_0 (Mev)	649	141 107	634	268	134	234	129
M_0 (m_e)	1270	276 210	1240	525	263	458	254
	±140		±215	±100	±30	±100	±35
	0.573	0.466	0.578	0.716	0.716	0.774	0.774
p (Mev/c)	454	74 56	449	278	146	286	158
T (Mev)	143	18 14	143	121	60	136	75
$10^{11} \tau_0$ (Sec)	6.4			1.1		2.0	

$$\mu_0 = \frac{K}{\bar{\alpha} \beta^2 \gamma} \quad (Z = 1)$$

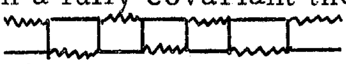
Oppenheimer then closed the session by showing a logarithmic plot of the lifetimes of unstable particles as had been suggested earlier by Fermi. The neutron, π, and μ seem to have well established lifetimes. For some of the other particles it is not known whether the letter corresponds to a single particle or whether a particle exists at all. V^0's are most readily detectable by cloud chamber yet short enough to decay in a cloud chamber. The τ, K, χ are characterized by having been seen to decay in a plate and cannot have very short lives. The τ, K, χ have probably been seen to decay in flight in a cloud chamber and consequently cannot have very long lives. The $\zeta^{\pm}$ may or may not exist but if it does it is observed to decay in photographic plates in rather short distances. In any event it doesn't live very long. For the ζ^0, the Nobel Prize for undiscovering a particle has been won. There is some piling up in the region around 9. There are some rather large gaps so far unfilled between

-3 and 6 and between 9 and 14. Fermi: "Which are all gaps accessible to experimental observation." Oppenheimer: "One is so dependent on identification of the decay process and these processes keep coming in; it would be a lot to say that these regions are really empty. I think it is more likely that they have been missed." Fermi: "It is a striking plot and probably has a meaning too I would say." Oppenheimer: "I hope our great grandchildren when they attend the 2038 conference in Rochester will take it for granted that they know these things."



THEORETICAL CALCULATIONS OF PION-NUCLEON SCATTERING
Saturday afternoon, Professor H. A. Bethe presiding.

Bethe opened the session by remarking that the logical order of the proceedings would have been, first, Dyson's presentation of his equations for the pion-nucleon scattering, followed by Bethe's remarks on their solution, then Wentzel's calculations on this subject and on nuclear forces, Brueckner and Watson's calculations on the potential approach and its relation to the Lévy potential and finally Low's covariant calculation along the lines of Chew. However, since Brueckner, Watson and Low were leaving very shortly, Bethe thought it best to have them speak first.

Low reported that Schwinger's variational method can be applied very immediately to the Bethe-Salpeter equation. He had proposed to use plane waves as trial functions, but Dyson suggests that this is a very bad approximation, (cf below). Low is attempting to solve the pion-nucleon scattering problem [illegible]ng the five boxes of Dyson, but in a fully covariant theory. That is, he is iterating a series of diagrams such as  . The interaction [illegible]n is the relativistic generalization of the non-relativistic $\vec{\sigma}\cdot\vec{\nabla} + \phi^2$ theory. The wave functions for the scattering problem are

$$\Psi^{+} = \Phi + K_1 K_2 G \Psi^{+}$$
$$\bar{\Psi}^{-} = \bar{\Phi} + \bar{\Psi}^{-} G K_1 K_2$$

. In terms of these wave functions the scattering matrix is given by $S = (\bar{\Phi}_f, G\Psi_i^{+})$ or equivalently by $S = (\bar{\Psi}_f^{-}, G\Phi_i)$. In form this is exactly the same as the normal scattering equation with a potential; therefore, Schwinger's derivation of the variational method goes through identically. So far no way has been found of checking the approximations used. The only result to date is the trivially soluble four dimensional problem of a product potential. The variational principle gives an exact solution for this case, but it is trivial since it is just the generalization of the delta function interaction and almost any method gives an exact solution. Dyson commented that this approach includes a lot that he has left out and that if the Bethe-Salpeter equation could be solved it would be a much better approximation than his and much

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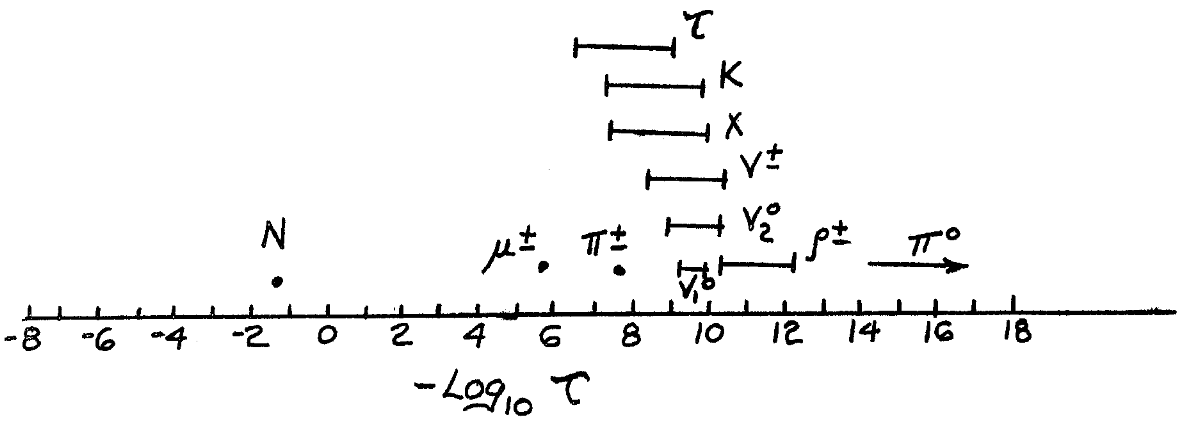

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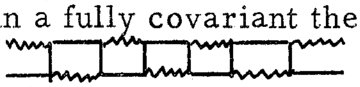
Low reported that Schwinger's variational method can be applied very immediately to the Bethe-Salpeter equation. He had proposed to use plane waves as trial functions, but Dyson suggests that this is a very bad approximation, (cf below). Low is attempting to solve the pion-nucleon scattering problem [illegible]ng the five boxes of Dyson, but in a fully covariant theory. That is, he is iterating a series of diagrams such as  . The interaction [illegible]en is the relativistic generalization of the non-relativistic $\vec{\sigma}\cdot\vec{\nabla} + \phi^2$ theory. The wave functions for the scattering problem are

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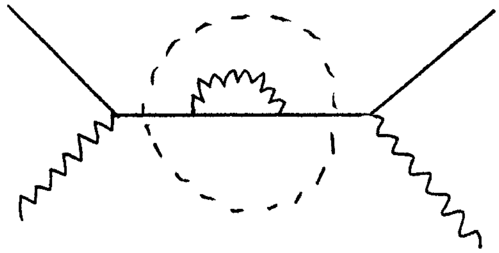


Their approach is to insert the renormalized S_f' in the circled region. Low's approach is not the same since he takes essentially the zero order approximation to the Goldberger, Gell-Mann equation but does include modifications of the vertices, etc.

Watson reported on the work which he and Brueckner are doing in constructing integral equations to study meson-nucleon scattering, nucleon-nucleon scattering, etc. The method is closely related to that of Tamm and Dancoff but uses Goldberger's formal algebraic approach and the Lippman-Schwinger integral equation. The method is capable of generalization to non-linear theories with or without nucleon pair production. For simplicity, only the linear case of a Hamiltonian ($H_0 + H'$) with no pair production and linear coupling of the meson field was discussed at this session. The Lippman-Schwinger equation can be written as $\Psi = \Phi_a + \frac{1}{E+i\eta-H_0}H'\Psi = \Phi_a + \frac{1}{a}H'\Psi$ where $a = E + i\eta - H_0$. In terms of the Møller scattering matrix defined by $\Psi = \Omega\,\Phi_a$ this becomes the algebraic equation $\Omega = 1 + \frac{1}{a}H'\Omega$. Chew and Goldberger give the formally exact solution to this equation $\Omega = 1 + \frac{1}{a-H'}H'$. The procedure is to reduce this solution algebraicly to a form in which one can actually do calculations. For this purpose we use a set of algebraic relations in this equation which sequentially separate off those parts of the potential which are non-diagonal in occupation numbers. When this has been done an infinite number of times we are left with $\Omega_{sc} = 1 + \frac{1}{a}\underline{V}\Omega$. Here $\underline{V}$ is diagonal in occupation numbers so that this is a standard form of the Schroedinger equation. The first step is $\Omega = 1 + \frac{1}{a-\Delta_0}H' + \frac{1}{a-\Delta_0}\Delta_0$ where $\Delta_0 = H'\frac{1}{a}H'$. Note that the second term is bilinear in the meson field variable ϕ whereas the first term cannot contribute in the asymptotic region because it contains an odd number of field variables. Therefore, we can write $\Omega_{sc} = 1 + \frac{1}{a-\Delta_0}\Delta_0$. We note that this equation is of the same form as the original solution with Δ_0 replacing the potential. The first Tamm-Dancoff approximation consists in neglecting the off-diagonal elements of Δ_0; this is the potential that Chew used and would be diagonal. creating or absorbing two mesons. We define $U_0 = NDP\Delta_0$ and $V_0 = DP\Delta_0$ where NDP and DP stand for non-diagonal part and diagonal part respectively, in the sense of occupation numbers. Therefore, U_0 creates or absorbs two mesons while V_0 is a scattering intensity in the sense that it is diagonal in occupation numbers. We can then show by induction that:

$$\Delta_n = U_{n-1}\frac{1}{a-\underline{V}_{n-1}}U_{n-1}$$

$$U_n = NDP\,\Delta_n$$

$$V_n = DP\,\Delta_n$$

$$\underline{V}_n = \sum_{l=0} V_l$$

If this theory converges we can show that in the limit as n approaches infinity $\underline{V} = \lim_{n\to\infty} \underline{V}_n$. This can be evaluated as follows

$$\Delta_n = U_{n-1}\, w_{n-1}\, \frac{1}{a}\, U_{n-1}$$

where

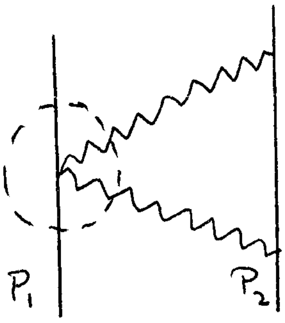
$$w_{n-1} = 1 + \frac{1}{a}\, \underline{V}_{n-1}\, w_{n-1}$$

$\underline{V}$ is diagonal in occupation numbers so that a solution of this last equation is simply a solution of a scattering problem. It is in fact of the form of the Lippman-Schwinger equation except that it corresponds to scattering off the energy shell. Note that the U_n are homogeneous functions of the field variables of order ϕ^{2n}. Every field variable contained in the U_{n-1} on the right must emit a meson and every field variable contained in the U_{n-1} on the left must absorb a meson, except in the lowest order in which the order of these processes may be inverted. In other words, there is no mixing of emission and absorption. Every field variable has to either emit or absorb a meson, and these virtual mesons are scattered by a solution to the Schroedinger equation before being reabsorbed. The problem of constructing this series of potentials therefore involves the solution of a scattering problem diagonal in occupation numbers. The first approximation gives what one gets by using the Tamm-Dancoff method. These results are easily generalized to include nucleon pair production or any non-linear meson interaction.

Note that there are 2^n mesons present in the intermediate state, so that the energy denominator becomes quite large. Thus even for the fourth order potential there will be sixteen mesons present in the intermediate state, and each higher state doubles the number. This suggests that as a rough approximation we may neglect $\underline{V}_{n-1}$ in $\frac{1}{a-\underline{V}_{n-1}}$. It is then possible to agrue that, at least in a cut-off theory, the series can converge for some value of the coupling constant. Since these U's are applied inductively, actually one has $a' \frac{1}{a} a' \frac{1}{a}$ for each one that occurs, so one obtains a factor $(2^n !)^2$ in the denominator times a numerical factor that depends on the cut-off. the numerator contains $(2^n !)^2$ as a factor also from the permutations in the ordering of the operators; therefore, at the very worst, the factorial dependences will cancel. Actually the rearrangement of the spin and isotopic spin matrices may well cut down the size of the numerator considerably below this upper estimate. Convergence is, therefore, likely for some value of the coupling constant. Note that the series is G^2, G^4, G^8, G^{16}, G^{32}, ... ; This is a genuine power series if the $\underline{V}$'s are neglected. It corresponds to 2, 4, 8, 16. . . . . virtual mesons present in the intermediate state.

Brueckner commented that the n! dependence of the numerator, which had caused Dyson to think that the ordinary perturbation theory expansion might not converge, has bee [illegible] which makes convergence much more likely. Chew was assured that it was correct to say that since each potential has a larger number of mesons associated with it, that the range of each potential becomes smaller than the last at a great rate. In principle this approach leaves out nothing, although the simplified version presented here has left out nucleon pair production for reasons of simplicity. The chief limitation of this approach is that it may be impossible to recognize singularities unambigiously and hence impossible to carry out a renormalization program. The proof of convergence is still shaky, but not as shaky as in the customary perturbation expansion. The trivial renormalization of the incoming waves can be done easily. Watson thinks that possibly it may prove feasible to look at the formal form of the series and by grouping terms pick out self-energy effects, etc. Oppenheimer commented that it has always turned out in the past that when you use occupation numbers and distinguish diagonal and off-diagonal things, it is not easy to follow singularities in the higher orders.

Watson concluded with a comment on the effect that his and Brueckner's method is likely to introduce by modifing the fourth-order nuclear force potential. Consider the



following diagram. If we consider first proton-proton scattering, the initial isotopic spin is 1; the emission of the meson from P_1 leaves that nucleon in an isotopic spin 1/2 state, but the meson can interact in both the 1/2 and 3/2 states with P_2, which brings in the large 3/2 scattering state. However, in the neutron-proton system, the initial isotopic spin is 0 for the triplet scattering, and hence in a diagram of this sort the 3/2 scattering state will not enter because isotopic spin is not conserved. Brueckner added that the essential difference compared to the usual fourth-order theory is that the emitted mesons do not go into plane wave states but into eigenstates of the scattering problem for a single meson. This is a very essential difference because the effect of the scattering potential on the meson is to modify the wave function and, therefore, to make the calculation with the usual plane wave intermediate states somewhat misleading, and probably one should take into account the effect of the lowest order potential on the meson. This effect has been left out in the Lévy potential and the resulting modification of V_4 might give a coefficient much less than the one used by Lévy; at least Brueckner suspects that this will be the effect.

Jastrow has calculated the resulting changes in the low energy p rameters if the V_4 of Levy is reduced to 1/4 its original value and the coupling constant increased from 10 to 15. The results are summarized in the table given below.

$V = V_2 + \alpha V_4$

	$\alpha = 1$	$\alpha = 0.25$	observed
Singlet range	2.5	2.5	2.6 ± .2
Triplet range	1.7	1.9	1.7
Quadrupole moment	2.1	2.7	2.7
Core radius singlet	0.5	0.4	-
triplet	0.5	0.6	-
Coupling constant	10	16	(distances in 10^{-13} cm)

The net effect is a considerable improvement in the agreement of the quadrupole moment with experiment while the effective ranges do not agree as well. This calculation differs from Lévy in that the singlet and triplet core radii do not fall at the same point for this particular choice of coupling constant; however, a slightly larger coupling constant would bring them back to the same value without altering the low energy properties very greatly. The essential conclusion is that the low energy properties are extremely insensitive to the coefficient of V_4 provided one makes compensating adjustments in the coupling constant. This comes about because the $1/(r)^3$ singularity in the tensor force and V_4 contribute about equal amounts to the well volume of the deutron. Blatt is coding the problem for the Illinois computer and when the code is ready (in about a month) will be able to carry out such calculations in about five minutes. The first application that Brueckner and Watson are planning to make with their method is to find out precisely what changes it does introduce into the Lévy potential.

Dyson now described in more detail the equation he had discussed

qualitatively on Thursday afternoon. The original wave equation is set up as in Goldberger or in Lippman and Schwinger. We are interested right at the start in standing wave solutions, so we want real integral equations in which the small negative imaginary parts are absent from the energy denominators and in which all the resonance denominators are taken with principle values. Our original Schroedinger equation is $(H_0 - E)\Psi = -H_1\Psi$. We substitute into this equation the assumed solution $\Psi = \Phi + P\left[\frac{1}{H_0 - E}\right]G$ d since $(H_0 - E)\Phi = 0$, we get $-H_1\Psi = G$ so that $G = -H_1\left(\Phi + P\left[\frac{1}{H_0 - E}\right]G\right)$.
This solution is the Schroedinger equation involving only standing waves because all the amplitudes can be made simultaneously real. G is a many component affair from which we wish to abstract a wave equation for a one meson wave function only. If our one meson wave function for a meson of momentum k is g(k) and the corresponding two meson wave function for mesons of momentum k and k' is g(k, k') we have the coupled integral equations

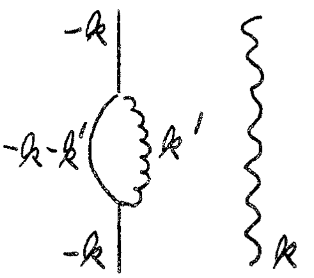
$$g(k) = i\,G\int dk'\,L(k, k')\,P\,\frac{1}{E_{k+k'} + w_k + w_{k'} - E}\,g(k, k')$$

$$g(k, k') = i\,G\,L'(k, k')\left[\phi(k') + P\,\frac{1}{E_{k'} + \;\,_{k'} - E}\,g(k')\right].$$

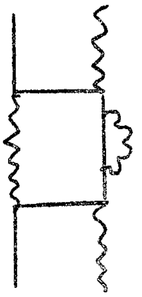
Here L is the matrix element of H_1 for the emission or absorption of a meson and G is the coupling constant. There will be two more terms for cases where nucleon pairs are involved. Even here something is left out because, due to Bose statistics, the equation for g(k, k') should be a symmetric function of k and k'; therefore, we must add to the whole expression the term with k and k' erchanged, and similarly elsewhere. Substituting back in to the equation for the single meson wave function we obtained an equation of the form

$$g(k) = G^2\int L(k, k')\,P\,\frac{1}{E_{k+k'} + w_k + w_{k'} - E}\left\{L'(k, k')\left[\phi(k') + \frac{1}{E_{k'} + w_{k'} - E}\,g(k')\right] + L'(k', k)\left[\phi(k) + \frac{1}{E_k + w_k - E}\,g(k)\right]\right\} dk'$$

Altogether this equation contains nine terms of which four involve g (k') on the right while five involve g (k) on the right. The five terms are the result of self energy processes such as that indicated in the diagram below. We note that the meson with



momentum k takes no part in this process. Such processes are perfectly allowable and ought to be included; one has no excuse for throwing them out except that it is simpler if one does. Since the terms occur only as a multiplicative factor of g (k) the correct treatment would be to transfer them to the left hand side obtaining an equation of the form $[1 + S(k)]\,g(k) = \int K(k, k')\,g(k')\,dk'$.
This factor $[1 + S(k)]$ in a covariant treatment would correspond to using S_f' rather than S_f for the propagation of a proton, that is, the inclusion of diagrams such as given below. That is, presence of mesons in intermediate states allows the proton to



have a self energy. However, in our formalism we are prevented from evaluating such terms properly because they diverge and there is no method of subtracting the self energy parts unambiguously. Hence, it is absolutely necessary to carry out the renormalization in a covariant scheme before applying the Tamm-Dancoff approximation. This problem remains unsolved. Since we have no way of treating these terms correctly we just throw them away. This will make quite a difference;

how many we cannot say.

The equation we then have is still rather complicated since it involves the charge coordinates of the meson and nucleon and the four components of the Dirac spinor for the nucleon, or 24 components in all. That is, we have a system of 24 coupled integral equations whose variable is three-dimensional. This can be simplified in a perfectly standard way when we use the fact that we know a number of the constants of the motion. The charge coordinates are eliminated completely by using pure isotopic spin states, reducing the 24 components to 4. The small Dirac components are eliminated by using the fact that the wave function is by definition a superposition of positive energy states for the proton only. Hence, the small components can be expressed in terms of the large components and can be eliminated. This brings the equation down to a two component equation involving Pauli spin matrices. Angular momentum and parity are constants of the motion. The angular variation can be eliminated by assuming that g(k) is expanded in a series of Legendre polynomials, that is, $g(k)=\sum Y_{J,\ell}(\hat{k}, \text{spin})\, g(|k|)$. Similarly, the incident plane wave can be written $\phi(k)=\sum Y_{J,\ell}(\hat{k}, \text{spin})\, \phi(|k|)$ where the same spherical harmonics occur. Substituting these expressions into the equation, it is then possible to carry out the integration over angles. The unpleasantness only comes about from the presence of the recoil energy denominator $E_{k+k'}$. This introduces interactions in all possible states of J and ℓ. However, the energy denominator can be expanded in shperical harmonics of the angle between k and k', that is, $\frac{1}{E_{k+k'}+C}=\sum_n P_n(\theta)\, Y_n(|k|,|k'|)$. After this expansion, the angular integrations are trivial and after carrying them out one obtains the same spherical harmonic of k that one had originally of k' times a kernel that is a function of the magnitudes of k and k' alone. This is an expansion in powers of v/c that is convergent for all values of v, so it is not an approximation. In fact, all except two terms vanish identically, so that there is no problem of convergence. The resulting equation for the $p_{3/2}$ state is approximately

$$g(k)= -\frac{G^2}{24\pi^2}\int_0^\infty \frac{k'^3\, k\, dk'}{(E_k\ w_k\ E_{k'}\ w_{k'})^{1/2}}\ \frac{Ek+M}{\bar{E}^3}(E_k+E_{k'}+w_k+w_{k'}-M-E)\frac{2\bar{E}+C}{(\bar{E}+C)^2} f(k')$$

where $f(k')=\delta(E_{k'}+w_{k'}-E)+P\left[\frac{1}{E-E_{k'}-w_{k'}}\right] g(k')$

$$\bar{E}=\frac{1}{2}\left[M^2+(k+k')^2\right]^{1/2}+\frac{1}{2}\left[M^2+(k-k')^2\right]^{1/2};\quad C=w_k+w_{k'}-E$$

(The exact equations for all six states obtained by this method are given in Appendix IV.) Two energy denominators occur; $E_{k'}+w_{k'}-E$ gives the singularity on the energy shell while $\bar{E}+C$ gives a singularity only above the threshold for two meson production. Note that the energy of the state appears in the kernel so that if we want to obtain g for a given energy we have to solve these equations for each energy in which we are interested.

It remains to answer the question of the meaning of the δ function occurring in the equation, the meaning of g, and how it is related to the phase shift for the state that we are considering. These questions can be answered by looking at the equation in configuration space. In momentum space we have the three-dimensional wave function $\psi(k)=\left[\phi(k)-\frac{1}{E_k+w_k-E}g(k)\right]Y_{J\ell}$.

In configuration space this becomes $\psi(r) = Y_{J,l}(\vec{\nabla})\,[\phi(r) - f(r)]$. The question is, therefore, what function of r these should be. The incoming wave is an eigenfunction of H_0 and hence is a δ function in momentum space. For purposes of normalization it is convenient to take this as $\delta(E_k + \;\; - E)$; then $\phi(r) = \frac{\sin k_0 r}{r}$. The operator $Y(\vec{\nabla})$ simply converts the sine into the appropriate Bessel function for the angular momentum involved. The Fourier transform of f(r) is in general complicated, but we can tell what it will be where the singularity occurs and find in fact that the symptotic behavior depends only on $g(k_0)$. The energy denominator in fact gives us $\frac{\cos k_0 r}{r} + o(\frac{1}{r})$ as this symptotic form. Hence we write

$$\psi(r) = Y_{J,l}(\vec{\nabla})\left[\frac{\sin k_0 r}{r} - \pi\frac{\cos k_0 r}{r}\, g(k_0) + 0(\tfrac{1}{r})\right]$$

$$= Y_{J,l}(\vec{\nabla})\left[\frac{\sin(k_0 r\ \delta)}{r} \quad 0(\tfrac{1}{r})\right]$$

and we can see immediately what the phase shift is; working inside the differential operator we have $\frac{\sin(k_0 r + \delta)}{r}$; this is correct since the Y operator merely shifts the phase of this function by $l\frac{\pi}{2}$ as is required. Therefore, $\tan\delta = -\pi g(k_0)$. The above avoids any discussion of the transition from standing waves to outgoing waves.

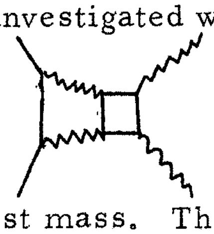
At this point Blatt made the following comment for Chew, who had already left. Chew has done the same calculation nonrelativistically and checked whether the omission of certain terms such as S(k) was admissible. That is, he expanded in a power series and tried to find out what the first order corrections to these equations would be. He found that in fact certain charge renormalization terms are not properly included by the simple dropping of the renormalization terms. In particular, the energy E in the principle value $\frac{1}{(E - H_0)}$ is modified by charge renormalization, so that charge renormalization has not been done properly. Dyson said that this is certainly true; S(k) would appear in every energy denominator and we must certainly try to do this.

Dyson then discussed the attempt to solve his equation by means of a variational principle. This principle can be derived quite generally. Suppose we have the equation $\psi(x) = \phi(x) + \int K(x,y)\, V(y,z)\psi(z)$ and in analogy to the approach above we define $g(x) = \int V(x,y)\psi(y)$; $f(x) = \int V(x,y)\phi(y)$ then $g(x) = f(x) + \int V(x,y)\, K(y,z)\, g(z)$. We assume V and K are symmetric since this can always be accomplished by an appropriate definition of g. Consider the quantity $Y = \int g(x)\phi(x)dx = \int f(x)\psi(x)dx$. The general Schwinger variational principle may then be written $"Y" = [\int g\phi]^2 / \int\psi g - \iint g(x)K(x,y)g(y)$ since one can show readily that this variational principle gives only a second order error in Y when any trial function is inserted for g; here $\phi(x) = \delta(x - z)$; $f(x) = V(z)$ Hence our variational principle for g(z), which is in fact the tangent of the phase shift which we wish to compute, is simply $"Y" = [g(z)]^2 / \int\psi g - \iint g(x)K(x,y)g(y)$. This variational principle has been tested against an equation whose solutions are known exactly and which is very similar to the equation with which we are dealing. This equation is $g(x) = \frac{b}{x} + \gamma\int^{\infty}\frac{1}{\max(x,y)}\, g(y)dy$. The kernel of this equation is quite similar to that of the equation which we are actually interested in solving and in fact is less singular. The solution for this equation is $g(1) = \frac{2b}{1+(1-4}$ In the physical situation the δ-function gives the Born approximation to g, that is, matrix element of the operator on the energy shell or in this simple case b/x. Just as in the real case, the second term is a linear operator operating on the wave function. Using the Born approximation b/x in the variational principle gives $"g(1)" = \frac{b}{1-\gamma}$. The equation has a singularity for $\gamma = 1/4$ but the solutions are not badly behaved up to this singularity, which is not very pathological. In fact the wave function g(x) would become infinite at a scattering resonance, whereas in this example it remains finite even at the singularity. For $\gamma = 1/4$ the true solution $g(1) = 2b$ while the Born approximation trial function in the variational principle gives "g(1)" $= [4/3]b$. This is not surprising since the Born approximation is not a good trial function. However, if as is usually done, the equation is iterated once one obtains the first iterate $g_1(x) = \frac{b}{x}(1+\gamma+b\gamma\frac{\ln x}{x})$. One usually assumes that by using the first iterated solution in the variational principle one is pretty safe and gets a very close approximation to the true answer. However, in this case, we obtain "g(1)" $= \frac{b}{1-\gamma-\gamma^2(1+4\gamma)/(1+\gamma)^2} = \frac{3}{2}b$ for $\gamma = \frac{1}{4}$. Thus the improvement is slight even though this represents the square root correctly to terms in γ^4. This is really the point that distinguishes our equation and makes it rather different from what one would get from a cut-off. We have here not a consequence of relativity but of the structure of the equation. Therefore, you must know g quite well if only to get a very rough idea of the tangent to the phase shift. One has to come down quite a ways below the singularity in order to get results which are even good to 25% with the variational principle. The kernels in the physical equation vary more abruptly than in the simple case which we have been discussing here so that our example is by no means extreme.

In order to test the variational principle in the physical case, calculations have been carried out at 110 Mev for coupling constants of 10 to 15 using Born trial functions in the variational principle and then by a numerical method. The numerical method consisted of replacing the integration by seven points and solving the resulting linear equations, and gives not too bad an approximation. Thus for $G^2/4\pi = 10$ the first Born approximation for the phase shift is 1/5 of the experimental shift. The variational principle with Born approximation wave function gives the experimental shift correctly. However, the exact solution gives only 1/3 of the experimental value.

For a coupling constant of 15 the ratio of the exact solution to the Born solution is -6.4; that is, one has gone over the resonance and has a phase shift of 140°. By interpolation and a little more calculation it is found that the dependence is a very sensitive function of the coupling constant and that $G^2/4\pi = 14$ gives phase shifts which are close to the experimental values.

Since experimentally the s phase shifts are apparently rapidly varying, we tried to find out whether this feature came out of the equations. For the isotopic spin 3/2 state we found, just as Chew did, that the tangent of the phase shift is of the order of 2k/M which is the right order of magnitude but has a completely wrong energy dependence. It was therefore investigated whether the meson-meson scattering which has so far been omitted, that is, diagrams of the form indicated could change the result. For orientation, the meson-meson scattering was calculated simply in Born approximation at low energies compared to the proton rest mass. There is an s state of isotopic spin 0 and 2 and a p state of isotopic spin 1. The Born interaction is extremely large numerically since it depends on G^4 and there are only a few factors of to bring this down. In fact, it is so strong that



one does not have any faith in the results at all. In terms of a potential, the potential would be greater than the rest mass of both mesons over most of its range. The renormalization has not been done, but Dyson thinks that it is not really necessary. The interaction is so singular even with renormalization that we really do not know what to do with it. The forces are attractive in the states of isotopic spin 0 and 1; in the state of isotopic spin 1 there is no renormalization problem and the forces there are still so strongly attractive that there will be a catastrophe, that is, the integral equation has a bound state at low negative energies so that the scattering phase shifts calculated from the equation are completely meaningless. It appears, therefore, that the meson-meson interaction will have to be included phenomenologically. It is true that the catastrophe occurs only for large G and it is conceivable that the coupling constant might be brought down. But this does not make very much sense since so many processes which have not been taken into account will make drastic modifications [illegible] the forces. It seems that there is no way to separate a good first approximation from other effects. Presumably, the Born approximation is not too bad in this case since the intermediate states all have very high energy.

Serber raised the question whether the fact that g(k) contains quite high meson momenta would not render the experimental observation of meson-meson scattering quite difficult. He had in mind the experiment proposed by Piccioni to scatter mesons of about 1.5 Bev from hydrogen and to observe pairs of mesons coming off at such momenta and energies as would correspond to free meson-meson scattering. Dyson, however, did not think that this experiment would be very seriously messed up. Although the high momenta are certainly present in the wave function, they are not really that important. This is true because the low momentum part of the wave function is not very strongly coupled to the high momentum part, and the kernel of the integral equation decreases rather rapidly even before you get to the relativistic cut-off.

The g[illegible]al conclusion reached by Dyson is that the main qualitative features are correct or, as Oppenheimer put it, that all the classic arguments that the conclusions of the pseudoscalar meson theory were in disagreement with experiment are wrong.

In response to a question from Breit as to what extent Dyson's work clarifies the relation between the large meson-nucleon scattering and the nuclear forces, Oppenheimer made the following remark. "The situation now, I am afraid, is that in Lévy's account of the collision of a neutron with a proton, some of the terms which are relevant for the collision of a meson with a proton have been included, and in Dyson's account of a collision of a meson with a proton some of the terms that Lévy thinks are important have been included, but that there is no complete correspondence and it is not perfectly clear that the relatively important terms are the same."

(The session was adjourned for tea.)

THEORETICAL CALCULATIONS

(second part)

Saturday afternoon, Professor H. A. Bethe presiding.

Professor Bethe opened the second half of the theoretical session by discussing certain very approximate methods he had used to obtain solutions of Dyson's equations. These consist of approximating the integral equation by a differential equation, which is possible only under certain assumptions. We note that the kernel of the integral equation is equal to a function of k alone times a function of k' alone times a function of $\bar{E}$ which depends essentially on the larger of k or k'. That is,

$$\bar{E} = \frac{1}{2}\left[(M^2+(k+k')^2\right]^{\frac{1}{2}} + \frac{1}{2}\left[(M^2+(k-k')^2\right]^{\frac{1}{2}} \begin{array}{ll} \rightarrow E_k & k \gg k' \\ \rightarrow E_{k'} & k' \gg k \end{array}$$

The approximations are good to second order; even when k and k' are comparable, they are not too bad. In the relativistic region the above is still an exact statement; therefore, the approximation is only serious in some intermediate energy region and then only for comparable k and k'. In order to simplify the work the same approximation has been made in the multiplicative factor $(E_k + E_{k'} + w_k + w_{k'} - M - E)$. Similarly, in the term C (see page 79) for w_k much greater than $w_{k'}$, $w_{k'}$ is replaced by μ, and conversely; with these two approximations the multiplicative factor becomes equal to $\bar{E} + C$. The integral equation then becomes $g(k) = \int A(k)B(k')\,\mathcal{K}\,(\max k,\ k')g(k') +$ inhomogeneous term. It is convenient to introduce a new function which we will call a which is g divided by g as given by the Born approximation; note that the kernel factors only piecewise, giving different factors according as k or k' is larger.

Now note if we are honest there are really six different regions which have to be considered because one must also compare k and k' with k_o, the momentum of the incident meson, which enters into the Born approximation. All the same, if one factors the kernel in the various regions, $k' > k > k_o$, $k > k_o > k'$ etc., one obtains a second order differential equation which has the great advantage that it can be solved. It has the further advantage that it is also easier to use it to determine the energy dependence of the solutions. Further approximations have been made which are not altogether necessary and lead to the differential equation

$$y'' = -\gamma \frac{k^3}{(w-w_o)} \frac{1}{(w-w_o-\mu)^2} \frac{(E+M)}{(E+w)} \frac{(1+2w^2)}{(1+\frac{2}{3}w^2)}\, y + 2\,\frac{4w^2-1}{(1+2w^2)}\; 2\, y'$$

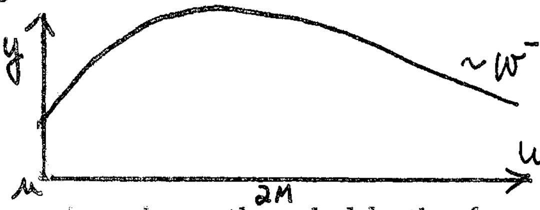
$$y = \frac{a}{(1+2w^2)}\ 1/2; \quad a = g/g\ \text{Born}; \qquad \frac{G^2}{24\pi^2} = \gamma$$

In contrast to the previous notation, $E = (M^2+k^2)^{1/2}$ and w_o is the energy of the incident meson. The first two denominators correspond to the singularity on the energy shell and the singularity for double meson production respectively.

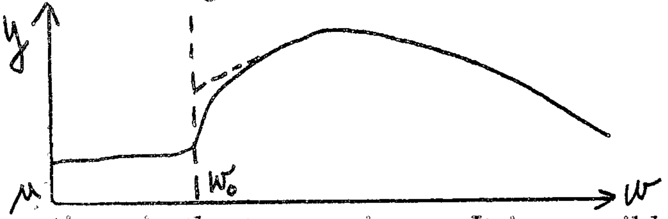
If we look at the equation in the interesting region of large W, that is, for high momenta or small distances, we get approximately $y'' = (-\frac{9}{4}\gamma + 2)\frac{y}{w}2$

As is well known, the solution of an equation of this form is simply a power of the meson energy and in fact we find $a \sim w^{3/2(1 \pm \sqrt{1-\gamma})}$. If γ is small there are two asymptotic behaviors, one decreasing and one increasing; the positive sign gives an symptotic behavior which is never normalizable, while the negative sign is always normalizable. $W^{3/2}$ is the limit of normalizable wave functi ns, so only the second solution is acceptable. If $\gamma=1$, both solutions are not normalizab le nd in fact oscill ate wildly for high momenta of the meson; therefore the theory makes sense only for $\gamma < 1$ or $G^2/4\pi < 6\pi$. If the integral equation is not mutilated, one obtains precisely the same result with the exception that 6π is replaced by $5\pi/(32-10\pi) = 29.6$; this gives an impression of the accuracy with which the differential equation represents the integral equation. Similar limitations as to the size of the coupling constant come in at various points in the theory, for example, in the large meson-meson interaction.

For $\gamma < 1$, it is possible to integrate the ifferential equation and this has been done in fact for $\gamma = 3/4$ with a coupling constant of $G^2/4\pi$ approximately 14. When $k_0 = 0$, that is, at threshold, the resultant wave function is sketched below.

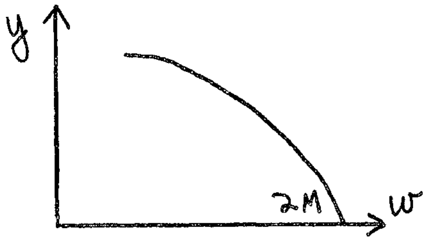


Note that there is a large effect from high momenta comparable to the nucleon rest mass and that the asymptotic behavior falls off very slowly, namely as $W^{-1/4}$. For incident energies above threshold, the form of the wave function similar when plotted against $(W - W_0)$ (rather then w itself), except that the wave function's deriv ive is logarithmically infinite at W_0 and there is a change in slope when one passes into the region below W_0. This is because the wave function satisfies two different differential equations in the two regions.



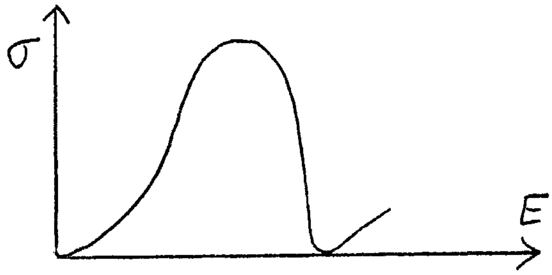
It is possible to show by going back to the integral equation that the value of a at $W=W_0$ is very important. In fact if $a=0$ at this point, then one has a resonance. For $W_0 = 2\mu$, this is nearly r ached for a coupling constant of 14, and by increasing the coupling constant, one obtains a resonance at zero kinetic energy.

It is important to treat the high energy part of the kernel correctly and not in terms of a cut-off, since the high-energy dependence is the feature that gives to y a flat maximum near $w = 2M$ and thus permits the wave function to pass through zero at $w=\mu$, with only a quarter-wave rather than a half-wave in the low energy region. Thus, for instance, if momenta greater than the nucleon rest mass did not contribute one would have the situation indicated below, and one would never be able to get a



resonance at zero energy with a coupling constant as low as 14. If the wave function went to zero at a point $w > w_0$, for $w_0 = \mu$, this would indicate a bound state of proton and π meson at a lower energy than the sum of the rest masses of the two particles.

In order to investigate the behavior of the resonance, the solutions have been expanded in terms of $x = w_0 - \mu$; so far only the first order terms in x have been calculated but this will be improved upon. The result is $\tan\delta = \frac{.22-x}{.15-x} \tan \delta_B$. Here the unit is taken as the nucleon rest mass so that 0.15 corresponds to μ and gives a resonance at 140 Mev. Therefore, G is too large to agree with experiment. Note that soon after the phase shift goes through 90° it also goes to 180°, is, we have an anti-resonance and the cross section for meson scattering, or presumably for photomeson production, would ave the general appearance sketched below. In response to a question from Oppenheimer, Bethe admitted that this expansion x has neglected terms in $\left(\frac{x}{0.15}\right)$, that is, it is an expansion in terms of the meson mass. However, in expanding the resonance term, the de ominator was not expanded but rather the numerator, so that this is not as bad as it would seem at first sight. Further what has been seen so far of the higher terms indicates that a more exact approximation will still exhibit this phenomenon. Bethe believes that it is possible that the resonance could be postponed but not abolished and it is interesting to see that one can get such a peculiar looking resonance which is in fact very similar to the curve shown us by the Cal. Tech. data.



Wentzel went on to talk abo the same kind of theory, namely, the pseudoscalar meson theory with seudoscalar coupling; $\bar{\Psi} G \gamma_5 \tau_\alpha \phi_\alpha \Psi$. He thinks we have had plenty of evidence in these sessions that there is so far no good mathematical treatment of this kind of theory. Hence he will compare two approaches and see how far certain omissions in one approach or the other are important. The approach Wentzel would like to propose is very lowbrow compared to what we ve been hearing. It starts with the Dyson-Foldy transformation, which yiel other more complicated interactions in return. In this transformed representation it is not easy to carry out renormalizations, and in fact by using this approach one prohibits the derivation of any quantative numerical values. Hence we will restrict ourselves to qualitative aspects and will not attach any significance at all to numerical values that may emerge from such a theory. We will treat nucleon recoils as something small and cut off divergent integrals at approximately the nucleon rest mass (or twice the nucleon rest mass, or half the nucleon rest mass.) Wentzel would agrue that this kind of theory is not so bad as it would seem at first sight. One characteristic term that occurs is $M \rightarrow M^* = M(1 + G^2\phi^2)^{1/2}$ where $\phi^2 = \sum_\alpha \phi_\alpha^2$, and appears in the Hamiltonian density as $M^*\bar{\Psi}\Psi$. In spite of the large G we expand the square root obtaining $M + \frac{G^2\phi^2}{2M} - \frac{1}{8}\frac{G^4\phi^4}{M^3}$. I ne retains the term quadratic in ϕ_α it is possible to solve the resulting theory in a rigorous cla sical way by expanding in normal vibrations. Is there any reason to believe that the next terms of this expansion are negligible? One can argue as follows: (parenthetically, one should realize that when one has made this transformation and talks about mesons these are no longer the same kind of mesons as were present in the original theory.) In this new language we retain terms containing at most two mesons in the field at one time and interpret $\phi^4 \sim \phi^2$ operator $\langle \phi^2 \rangle$vacuum where these occur with the nucleon density occurring still as a factor. Since the location of the nucleon is fixed the above becomes $\phi_0^4 \sim \phi_0^2 \langle \phi_0^2 \rangle$vac. but since the ϕ^2 term represents a repulsive potential, the meson will be kept away from the nucleon and the stronger the coupling the greater is the reduction of the value of the meson wave function at the position of the nucleon i. e. ϕ_0. Let us compare the term in which the expansion is made in the weak coupling and the strong coupling cases. This is $\frac{G^2 \langle \phi_0^2 \rangle_{vac}}{M^2} \sim \begin{cases} G^2/4\pi \sim 10 \text{ for weak coupling} \\ 4\pi/G^2 \sim 1/10 \text{ for strong c[illegible]} \end{cases}$ Hence for $G^2/4\pi$ of the order of 10 such as Lévy uses, the strong coupling expansion parameter is 1/10 or smaller, justifying this approach.

At this point Schiff asked how one got around the difficulties with regard to renormalization that Lévy had run into in such a calculation. Oppenheimer commented that Lévy's trouble came from taking ϕ seriously as the quantized field whereas here one has restricted oneself to a one quantum subspace, and these difficulties do not arise.

Wentzel now studies the effect of this term $G^2 \phi^2/2M$. In the original representation transitions involving nucleon pairs were important. These no longer occur since they have all been swallowed up in this term just as in the case of the A^2 in ordinary electrodynamics. Wentzel claims that when he has solved this theory rigorously to all orders of G he takes account automatically of successive nucleon pair processes; of course, there are other terms such as the p terms which have so far been neglected. Wentzel now asks what this approximation does for the deuteron problem. He solves the problem of two fixed nucleons at a distance R surrounded by a meson field, and calculates the change of self energy as a function of R. In the weak coupling approach G^2 is small and one obtains the fourth order Bethe forces, the K_1 found by Lévy, and given by $V_4 = -\frac{6}{\pi}\left(\frac{G^2}{8\pi M}\right)^2 \frac{\mu}{R^2} K_1(2\mu R)$. Lévy in his paper mentions in small print that there are higher order diagrams which induce one to change G to some constant G_1 which he does not know. But Wentzel can say that this is taken into account by the multiple nucleon pair-effects to all orders automatically in his theory and he obtains

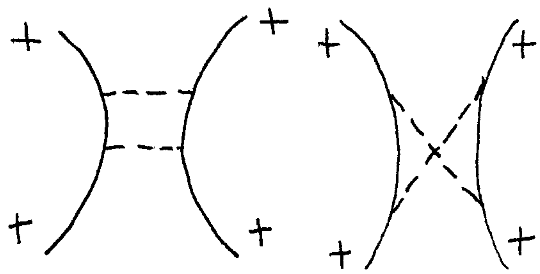
$$G_1^2 = \frac{1}{\frac{1}{G^2} + \frac{\bar{k}}{2\pi^2 M}}$$

$\bar{k}$ = [illegible] of momentum using a linear cut-off. With Lévy's value of G this gives a reduction by a factor of the order of 10, which may be exaggerated, but certainly a factor of 4 is possible.

Oppenheimer commented that there is a great difference between this formal view and Lévy's problem. He thinks Lévy would argue that if you go to some large internuclear distance, then the strength of the fields induced at one nucleon by the other is not very great, and that the approximation of V_4 with the old constant is not too bad. As the nucleons come closer and closer then more and more complicated things happen and you get into a region in which not only V_4 is not there, but everything is different. Now this is an extreme view; that it should give the same 1/4 as the strong coupling theory would be most amazing. Kroll objected that he could not see why a similar reduction would not occur also in V_2. Thus there is a term in Wentzel's theory given by $\vec{\sigma}\cdot\vec{\nabla}\phi \frac{1}{1 + \frac{G^2\phi^2}{M^2}}$ Wentzel does not think so because if one expands the denominator in this term, then [illegible]

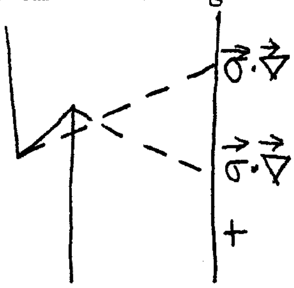
$$\vec{\sigma}\cdot\vec{\nabla}\phi\left[1 - G^2\langle\phi_0^2\rangle_{VAC}/M^2 - - -\right]$$

This second term in the expansion, by a previous argument, is about 1/10 so that one can argue that the effect is small. Feynman objected that the real correction is the alteration of ϕ due to the fact that it is in a p state for one nucleon but not for the other. Wentzel agreed that this could be so. At this point the insensitivity of the results to the exact alue of the coefficient of V_4 was again discussed, and the reader can refer to the discussion by Jastrow for the pertinent details. Oppenheimer stressed that the fit never was very quantitative and that it was pointless to try to argue the numbers in detail. Kroll insisted that a lot of pure charge renormalization effects were included by Wentzel's procedure and that it would be unfair to omit them in V_2. Oppenheimer and Kroll argued that the cut-off certainly did not take care of renormalization effects and that just what would happen when they were included was completely uncertain; in fact, Oppenheimer asserted that the only possible statement now is that the ratio of V_2 and V_4 is really unknown. Feynman went even further and stated that not only the ratio but also the shape, the spin, and the isotopic spin dependence were unknown, drawing his point from the work of Taketani who has shown that the σ and τ commutators give numerical factors up to 96 in the coefficients, which give 40% corrections at $x = 1$ and a correction bigger than V_2 itself at $x = 0.7$. Feynman elucidated by drawing the following



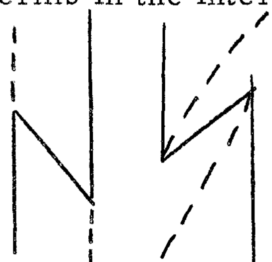
These two terms are proportional to $(\mu G/2M)^2$ and look like they are going to be small; Lévy in fact asserts that they cancel, but this is not true. Oppenheimer agreed that this is a real mistake in Lévy's calculation.

While he was at the blackboard Feynman also drew the following sketch and pointed out that such a diagram will produce a strong spin-orbit coupling due to the fact that in order to preserve Galilean invariance, the gradient must be replaced by $\vec{\nabla} - \frac{G}{M}\vec{p}$; but these combine to give $(\vec{\sigma}\cdot\vec{\nabla})(\vec{\sigma}\cdot\vec{p}) + (\vec{\sigma}\cdot\vec{p})(\vec{\sigma}\cdot\vec{\nabla}) = \vec{\sigma}\cdot\vec{\nabla}\times\vec{p}$. Hence, for instance in O^{16}, these effects will add together for all nucleons and one will obtain a very large spin-orbit force on a single nucleon outside the closed shell.

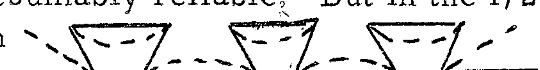


Wentzel added that the $\vec{\sigma}\cdot\vec{\nabla}$ term is characterized by the coupling constant $f \simeq G/M$ and for strong coupling this is much greater than $1/M$, so that the theory is presumably applicable to these terms as well. Oppenheimer objected that the theory has not been renormalized. Wentzel went on to the π meson-nucleon scattering problem and considered first the s states which again are given by the term $(G\phi)^2/2M$. Again, a rigorous solution is obtainable and the cross section is given by $\sigma_{tot} = G_1^4/4\pi M^2$ where G_1 is the same G_1 as was obtained before. The order of magnitude is quite reasonable, but this scattering is independent of isotopic spin, since it depends on $\sum_\alpha \phi_\alpha^2$. For a long time Wentzel thought this was a serious objection, but now thinks that there is a possibility of getting around it. One notes that one has (see Drell and Henley) $\frac{G^2}{2M}(\phi^2 + 1/2M\ \vec{\tau}\cdot\vec{\phi}\times\vec{\pi})$. Here $\vec{\pi}$ is proportional to the meson energy times $\vec{\Phi}$, so that on the energy shell the second term is a small correction; but if one includes reaction this is large for meson energies comparable to the nucleon rest mass. One, therefore, investigates whether this correction is in the right direction. Since τ is involved one cannot solve by normal vibrations, so one considers single meson states and calculates stationary solutions and phase shifts and compares with the rigorous solution to see the order of magnitude of the correction. One obtains $\tan\alpha = \frac{G^2 k/4\pi M}{A + B(\lambda, w)}$ Here λ is for I = 3/2, -2 for I = 1/2, and B depends on the energy $w = (\mu^2 + k^2)^{1/2}$ though not strongly. For low energy values B is given by $B = \frac{(1 + \lambda C_1)^2 + \cdots}{(1 - \lambda^2 C_2)^2 + \cdots}$. The constants A, C_1, C_2 all depend on the cutoff. The weak coupling result is obtained by $A \to 0$, $B \to 1$, while if one takes the $G^2 \Phi^2/2M$ term alone, one still has $B = 1 (C_1 = C_2 = 0)$ but $A \neq 0$. In the latter case, one may compare A with the rigorous value. The result is that the denominator $w_k^2 - w^2$ occurring in the k integral for the rigorous solution is now replaced by $2 w_k(w_k - w)$ which means an error at most by a factor of two in A. If one includes C_2 and adopts a cut-off $\bar{k} \sim M$, then one can make the I = 1/2, s phase shift α_1 much smaller than $\alpha_3 (I = 3/2)$, or even zero. For instance, at 140 Mev, one can obtain $\alpha_1 = 0$ and $\alpha_3 = 15°$. So one has a possibility in this theory for explaining the difference between the two isotopic spin states. If experimentally the dependence of α_3/k on energy proves to be strong, however, other things must be included.

Here Dyson gave another possible explantion for the difference between the phase shifts for the two s states in the frame-work of his theory. There are two important terms in the interaction for s states which are indicated by the following diagrams

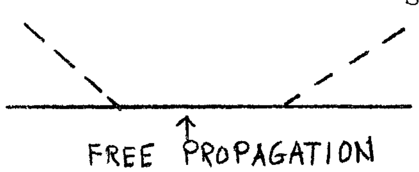


Only the first occurs for the isotopic spin 3/2 state but both occur for the isotopic spin 1/2 state. Apparently accidentally, the matrix elements are of the same order of magnitude, (although the accident becomes explicable in terms of the Foldy transformation) and the contribution to the 3/2 state turns out to be the same as the sum of the contributions from both diagrams to the 1/2 state. The equation for the 3/2 state can be solved without renormalization so it is presumably reliable. But in the 1/2 state one has the sequence indicated by the diagram



. Clearly such a sequence will be drastically affected by mass renormalization and the resulting scattering in the 1/2 state could be vastly different from that of the 3/2 state. Yang questioned whether this could be an $s_{1/2}$ state because of parity, but Dyson explained that this is possible because it involves an interaction with an anti-proton, which has opposite parity to the proton. Oppenheimer commented that in general we simply do not know what to do about the higher order terms and everything could be most misleading. There is one exception and that is the one mistake in Lévy. The numerical ratio of the change in V_2 to V_4 is certainly unknown.

Wentzel concluded with a remark on the work of Goldberger and Gell-Mann who have considered the following diagram. One asks how the free nucleon propagation



function $1/\gamma p + M$ is altered by the presence of self energy bubbles, which are here off the energy shell. The answer is that this goes over to $\frac{1}{\gamma p\, f(p^2) + M\, g(p^2)}$ where the use of Lévy's coupling constant gives a reduction by a factor of 1/10, which reduces the 1/2 state relative to the 3/2 state. This difference occurs because in the 3/2 state one has the diagram  and here they say the effect is much less drastic because the convergence is much better for the energy denominator. Dyson said that this is true since this diagram belongs to the anti-proton. He added that the Gell-Mann, Goldberger problem deals with the $p_{1/2}$ state, but that the same thing would happen in an s state. At this point Bethe closed the session.

EXPERIMENTAL PHYSICS SESSION

Saturday afternoon, Professor B. Rossi presiding.

Rossi remarked that the subjects for discussion would be (1) mesic X-rays, (2) new particles, (3) the related problems of π^0 lifetime and electron pairs, (4) μ meson production in stars and (5) the general problem of nuclear interactions.

Rainwater reported on the measurement of X-rays from μ mesic atoms by Fitch and Rainwater at Columbia. When a μ meson stops in solid matter the meson is captured into Bohr orbits about the nucleus. For a bare-point nucleus the radii of the Bohr orbits are smaller, and the binding energies larger, by a factor $M_\mu / m_e \approx 210$ than for electron orbits. Fermi and Teller have shown that the meson cascades towards the nucleus in transitions such that ℓ tends to equal $\ell_{max} = (N-1)$ so the most probable states occupied are the 3d, 2p, 1s, etc. For Pb the 2 P $\rightarrow$ 1S radiative transition takes $\sim 10^{-18}$ sec., while nuclear capture from the K shell takes $\sim 7 \times 10^{-8}$ sec. (see report by Reynolds.) For π mesons the nuclear absorption is so strong that the 2 P$\rightarrow$1S transition is seen only in light elements (see following report by Platt).

For a μ meson of 210 m_e and a point nucleus, the predicted Dirac energies in Mev are given below, assuming no anomalous mesic moment and only coulomb interaction.

Z	$2P_{3/2}$	$2P_{1/2}$		$(2P_{3/2} \rightarrow 1S)$	$(2P_{1/2} \rightarrow 1S)$	Observed
13	0.121	0.121	0.485	0.364	0.364	0.352
22	0.347	0.349	1.393	1.046	1.044	0.95
29	0.603	0.609	2.432	1.829	1.823	1.55
82	4.916	5.474	21.34	16.42	15.187	5.3 or 6.0
82*	4.63	4.81	10.08	5.45	5.27	5.3 or 6.0

The observed energies are shown in the last column. The last line gives the corresponding result calculated, assuming a constant charge density for r=0 to $r_0 A^{1/3} = R$ (where $r_0 = 1.3 \times 10^{-13}$ cm was used), with zero density for $r > R$. The results for the finite nuclear size for Pb give about a factor of 3 reduction in the transition energies. The $2P_{3/2}$, $2P_{1/2}$ fine structure splitting shows the great sensitivity of the X-ray energy to nuclear size.

The Dirac equation for this problem can be written approximately as the sum of a Klein-Gordon term and terms involving $\frac{dV}{dr} \times \frac{d\psi}{dr}$ and $\frac{1}{r} \times \frac{dV}{dr} \times (\underline{\ell} . \underline{S})\psi$ which are characteristically Dirac-type terms. The last term gives the fine structure splitting. The extra Dirac terms were computed as perturbation corrections giving the following results (in Mev).

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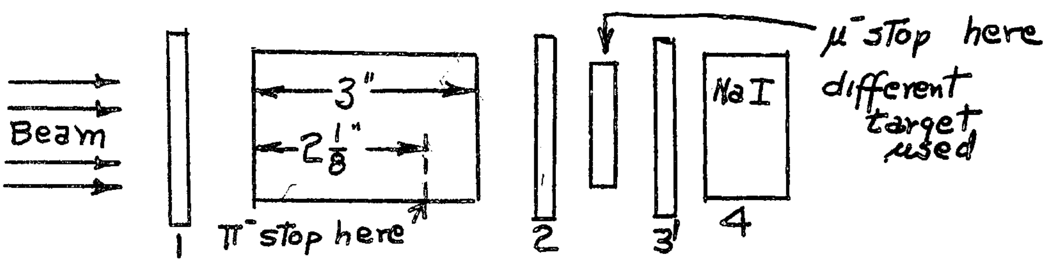
	K. G.	$\frac{dV}{dr}\ \frac{d\psi}{dr}$	$\frac{1}{r}\frac{dV}{dr}(\underline{\ell}\cdot\underline{S})\psi$ term	Sum
$2P_{3/2}$	-4.72	0.032	0.062	-4.63
$2P_{1/2}$	-4.72	0.032	-0.125	-4.81
1S	-10.24	0.16	0	-10.08

Perturbation calculations show that a 1% change of nuclear radius (R) gives 0.05 Mev change in X-ray energy for Pb, which is easily within the precision of the experiments. For any other assumed shape of nuclear charge density vs r, similar accuracy can be obtained for the nuclear size but the shape cannot be determined from the X-ray energies alone. Thus, better than 1% accuracy is possible in specifying nuclear radius (after the choice of nuclear model is made).

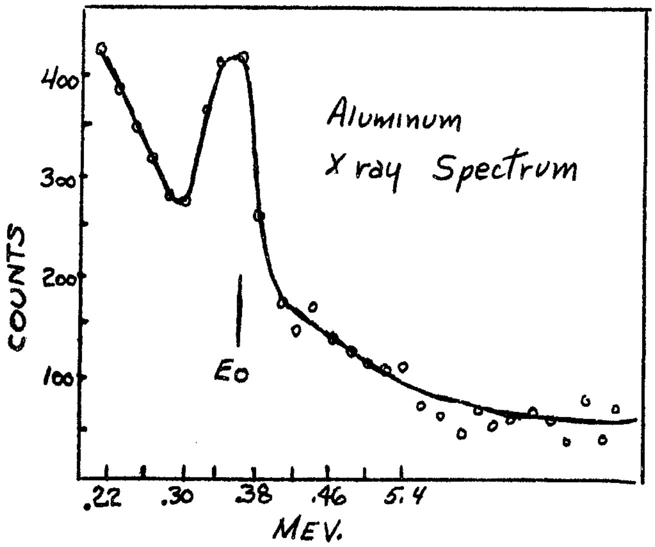
The Nevis negative meson beam contains mesons of well-defined momenta, which are ~95% π^-, and ~5% μ^- of equal momenta. The μ^- are probably formed from π^- decay near the Be target where the π density is high. The background rate outside the main 6 foot shield was too high, so that an additional 8 foot concrete shield was used at some distance from the outside wall. The background was further reduced by bringing the beam to the floor with double-focusing wedge magnets. The resulting [illegible] flux over ~2" diameter circle was ~500/sec.

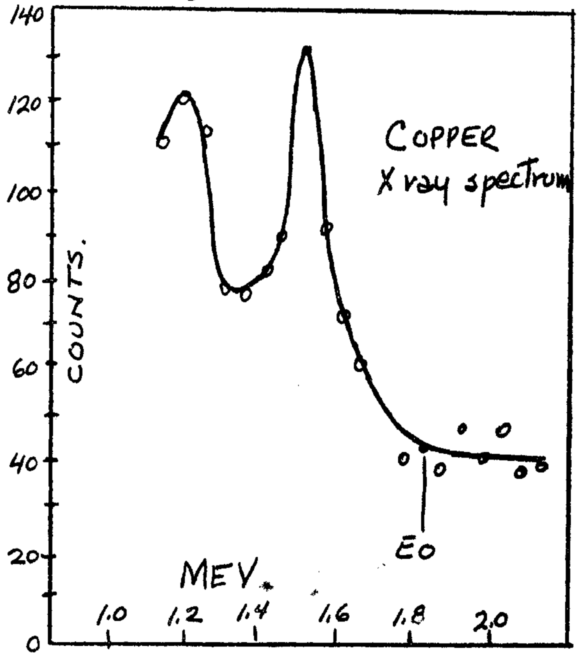
The detection system is shown below. There are four detectors denoted by 1, 2, 3' and 4. Detectors 1, 2 and 3' are thin stilbene crystals and 4 is a 2" diameter NaI crystal. The latter is used to determine the X-ray energies. An event is determined by a coincidence between 1, 2 and 4 and an anticoincidence in 3'. The beam is incident upon crystal 1 which is followed by approximately 3" of copper. The range of the π mesons is $2\ ^1/8" \pm ^1/8"$ and they do not leave the copper. The μ mesons do get out and stop in an absorber between crystals 2 and 3'. A 1, 2, 3', 4 fast coincidence (~10^{-7} sec) triggers a slower (~1 to 5 μsec) pulse height analyzer which views the NaI pulses.



The NaI crystal was calibrated with the Na^{24} 1.38 Mev and 2.76 Mev photons. Since both photons are emitted with the same intensity, this gives a measure of the sensitivity of the NaI crystal at the two energies. The higher energy region was calibrated with the 4.4 Mev photons from a Po-Be neutron source. In this case an extra 2% Doppler broadening of the line is observed.

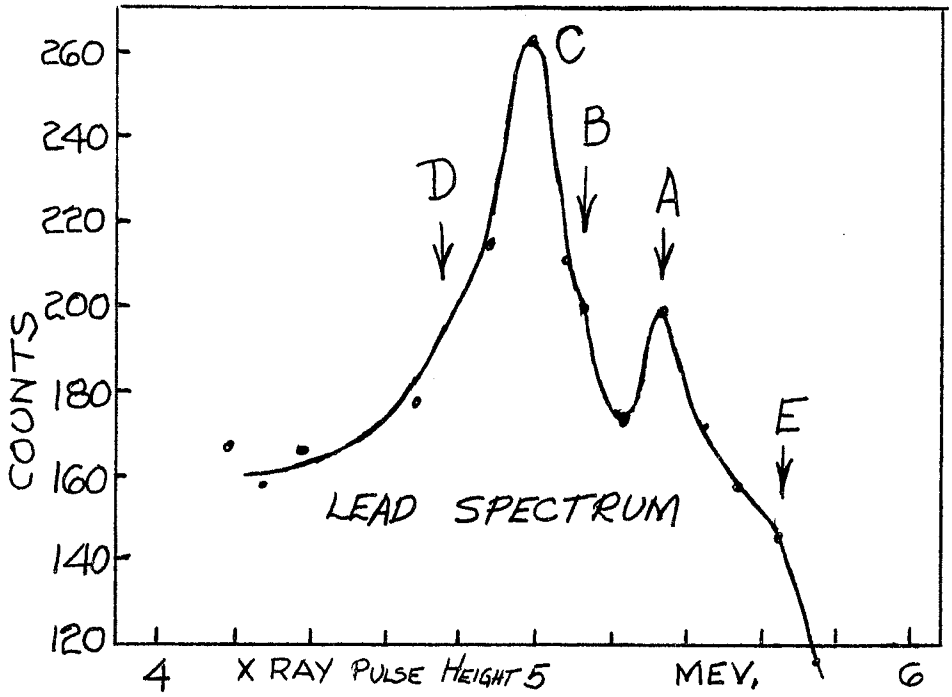
Good γ-ray curves have been obtained for Z = 13, 14, 22, 29, 30, 80, 82, 83. Poorer data were obtained for Z = 50, 52 and 81. The X-ray peaks are observed at the expected positions for Z $\leq$ 30. For Z = 80, the fine structure splitting is seen but we are not yet certain of the interpretation of the peaks in terms of pair, pair + 0.511 Mev, Compton, and full energy. This difficulty should be resolved soon by decreasing the analyzer interval width, and by using $\sim 10^{-8}$ sec fast coincidences to eliminate most of the nuclear capture γ rays. The results for $Z = 13$ and 29 together with the predicted (Dirac) transition energy E_0 for $R = 0$ are shown below.





The shift of the peak with Z and the rapidly increasing difference from the $R = 0$ predicted position is evident. For these elements, $R = 1.4A^{1/3} \times 10^{-13}$cm gives a match between the predicted and observed energies. For a reasonable choice of R, the meson mass $= 210\ m_e$ gives a much better fit than $205\ m_e$ or $215\ m_e$.

The result for Pb is shown below over the region from 4 to 6 Mev.



The dip at 5.2 Mev repeated in several runs and is probably real. Hence, at least two close lines are present. The peak at the "hesitations" at B D, and E and the peak at A tend to repeat in many runs. Depending on the interpretation of the structure, the proper splitting ~ 0.2 Mev is probably seen, and the full energy of the $2P_{3/2} \rightarrow 1S$ is 5.3 or 6.0 Mev if A is the full energy or C is the pair energy. The peak corresponding to C came at 4.8 Mev for

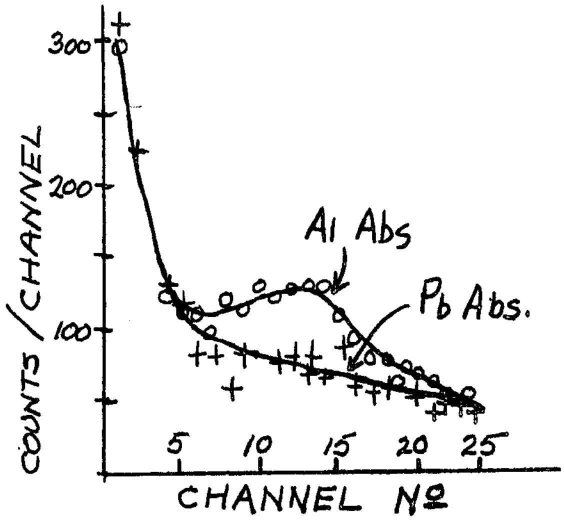
Z =80 and at 5.0 Mev for Z =82 and 83. With improved techniques the results for Z≠80 should be improved and the interpretation clarified.

Alvarez asked about the sensitivity of this experiment to the distribution of nuclear charge. Rainwater replied that this experiment has a strong sensitivity only to nuclear size. For the model of constant density up to a certain radius and zero density outside, the nuclear radius is $\sim 1.3 \times 10^{-13}$ cm. Assuming constant density to a certain radius and then a triangular drop off, you get essentially the same optical transition energy as if you continued on straight with constant density and then cut off sharply. The tr angle has to be 1.4×10^{-13} cm wide or narrower. Other nuclear shapes have not been tried. More detailed calculations of the optical energies and the effects of nuclear size are being made.

Platt reported on the measurement of X-rays from π mesic atoms by Schulte, McGuire, Camac and Platt at Rochester. This experiment is similar to the one just described by Rainwater except that π mesons were used. X-rays from the 2p→1s transition have been observed in Be, C and O. There is a characteristic difference between this experiment and the one reported by Rainwater, namely, nuclear absorption is a competing factor in the π but not in the μ case. Consequently for elements of even very small Z, probably for Z=2 or 3, nuclear absorption from the 2p state predominates over the 2p→1s transition. In our experiment an attempt was made to measure the competition between X-ray emission and nuclear absorption. In particular, we measured the fraction of the stopped π mesons that gave rise to the 2p→1s transition. The π X-ray energies for Be, C and 0 are 44, 100 and 178 Kev, respectively. The competition was measured for C and 0 but not for Be because of the high background at the lower energy.

The experimental setup is similar to that at Columbia. A 40±2 Mev π^- meson beam is resolved in energy by the cyclotron fringing field and also by an auxiliary magnet. The beam consisted of 80% π mesons, 13% electrons about 113 Mev energy and 7% μ mesons of about 45 to 58 Mev energy. The detector is shielded from the general cyclotron background by about 50 tons of copper. The X-ray counting rate was of the order of a few per minute while the background rate was 1/4 million per second during the beam time. The detector consisted of four scintillation counters, the first three being organic liquid counters and defining the entering π^- meson beam. The fourth counter was a NaI crystal and measured the X-ray pulses. The π mesons were degraded in energy by an aluminum wedge between crystals 2 and 3 and stopped in a target just in back of crystal 3. Between the meson stopper and the Na I X-ray detector were X-ray absorbers. In most of the measurements lead and aluminum were used of which the aluminum had slightly greater stopping power for charged particles, but much more transparency for X-rays. A fourfold fast coincidences ($\sim 3 \times 10^{-8}$ sec.) triggered a slower pulse ($\sim 0.2 \times 10^{-6}$ sec.) from the NaI crystal to a 24 channel pulse height analyzer.

The graph on the following page shows a typical pulse height spectrum from carbon. The upper and lower curves are for aluminum and lead X-ray absorbers, respectively. The difference between the curves is attributed to the 100 Kev X-rays from carbon. Note that the resolution is poorer than that of the Columbia work. This is probably due to both the narrower gate and the lower energy quanta in our



experiment. The pulse amplitude calibration was made with the 73 Kev lead fluorescent radiation. An arti light pulser built into the photomultiplier shield was calibrated against the lead line and used as a secondary standard. The light pulser was operated during the beam time to check the 100 Kev pulse resolution under operating conitions. A further check for carbon X-rays was made with the critical absorption technique. The Pb K critical absorption edge is at 88 Kev and that of Th is at 110 Kev. The 100 Kev carbon line lies between these two. Thus the Th - Pb absorption should be different. This difference was observed. The results of this experiment are: let Y b the yield of X-rays per π meson stopped in the absorber.

Carbon (graphite stopper)	Y	11 ± 2%
Oxygen (water stopper)	Y	20 ± 7%

This is a rather surprising result because the 2p→1s transition probability goes as Z^4, while the probability for the nuclear absorption from the 2p state varies as Z^6. Thus, the yield should be less for oxygen compared to carbon. Using the Z^2 dependence and the carbon results the yield for oxygen should be about 6%. The low nuclear absorption rate in oxygen may be due to the fact that O^{16} is a doubly magic nucleus with closed shells. The carbon yield can be checked with the ~50 Mev π^- star production cross section measurements at Columbia. Marshak and Messiah have shown that if only the p part of the meson interaction is responsible for star formation and if the nuclear matrix element does not vary rapidly with energy, the nuclear absorption rate from the 2p state in C should agree with experiment.

Another run was made with various carbon hydrogen compounds: C, CH_2 and C_6H_6. From the Panofsky experiment we know that the mesons are not captured in hydrogen, but when caught in atomic orbits of hydrogen are transferred to orbits of the [illegible] Z atoms. The yields from these stoppers were 11% for C, ~6% for C_6H_6 and ~6% for CH_2 with poor statistics for the compounds. The results on the carbon compounds suggest that the yield for oxygen is only a lower limit and a higher value should be obtained with a pure oxygen target instead of H_2O. In answer to a question, Platt said that the yield computations required knowing (1) the crystal geometry, (2) the detector efficiency and (3) the number and position of the stopped π mesons. For the evaluation of the oxygen and carbon data, only the crystal efficiency was different and this was a factor of about 2 lower for oxygen.

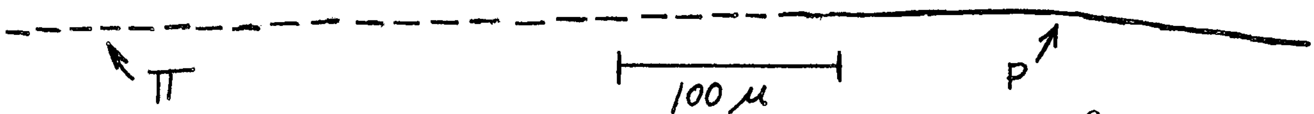
Roberts then made some theoretical remarks on the mesic molecule. A comparison of the X-ray yields from the graphite and carbon compounds stoppers indicates that there are about half as many carbon X-rays produced by the compounds. On the other hand, Panofsky's experiment shows that less than 1/2% of the mesons are captured in hydrogen, thus, essentially all the mesons are absorbed by the carbon nucleus. A possible mechanism for this difference is the following: some of the π^- mesons entering the CH_2 are originally captured in a high orbit around a proton and form an entity which looks like a neutron, i.e., a small neutral particle which can diffuse from atom to atom with thermal velocities. There is time for many collisions since the times for atomic orbit transitions are long. If this entity gets near a carbon atom, there will be a net attraction between the two, even at large distances of the order of the K shell which in carbon is approximately 10^{-9} cm. Something like a hydrogen molecular ion is formed, that is, two positive charge centers, carbon ($Z = 6$) and proton ($Z = 1$), are bonded by the meson. Within the carbon electronic K shell there can be many states of this mesic molecule. The electrons are outside of this region but, as Fermi suggested, may act as potential absorbers of energy and produce Auger transitions. The mesic molecule has peculiar properties. The usual Born-Oppenheimer separation of energies into the electronic, rotation and vibrational energies is not very valid. An approximate calculation of the levels in the ground state give for the rotational energy constant B_0 about 1/2 Kev and for the first vibrational level v_0 approximately 15 Kev.

Let us consider the excited states of the molecule in more detail: remembering that the nuclear absorption probability is proportional to the square of the wave function at the nucleus, then the main contribution comes from the proportion of the σ states, i.e., states in which the orbital angular momentum about the internuclear axis is 0. In carbon, for s states up to about $N = 5$ or 6, the lifetime for nuclear absorption is less than 10^{-15} sec. When a neutral mesic proton approaches a carbon nucleus the meson follows one of the many potential energy curves corresponding to the various mesic states of the molecule. Even for the unstable states the meson might stay in the region of the minima for the order 10^{-15} sec. This time is long enough for nuclear absorption because of the large σ component of the states. All the states become a mixture of the eigenfunctions of the angular momentum about the internucleus axis. This also occurs for the highly excited states of the hydrogen molecular ion. For the mesic molecule the mixing of these states is even greater. Thus it seems plausible that in the mesic molecular system the nuclear absorption can occur without optical transitions to the ground state. Even if we assume that the molecule is formed in a stable configuration, the meson does not have to jump to a single state as in the case of an atom. With the addition of rotation and vibration states to the molecular system the meson will not make as many high energy optical transitions.

Camac emphasized in reference to Platt's report that the π^- star production cross section for mesons possessing 50 Mev energy or less can be estimated from the mesic X-ray yields. The extrapolation of the Columbia data for carbon at about 50 Mev agreed quite well with the X-ray yield data. Using the oxygen X-ray yield, the π^- star production cross section should be less than 1/3 that of carbon.

Reynolds reported on the μ meson absorption experiments by Keuffel, Harrison and Reynolds at Princeton. There has been published a report on the measurements of the μ meson nuclear absorption probabilities in which the data were compared to Wheeler's predictions. These measurements were made with mercury, lead and bismuth absorbers. Since then a group of five elements in the vicinity of $Z = 50$ have been measured to about 8% precision. There seems to be no fine structure in this region but the cross section is uniformly lower than the Z^4 law would predict. The theoretical work has been considerably refined by Demster in which he takes a single particle model with an adjustable oscillator potential and adjusts the energy levels empirically. He finds good agreement with our experimental points. He also predicts that there should be no neutron absorption in calcium which is consistent with Sard's work. It is interesting to note that the nuclear absorption time from the 1s state is quite long even for bismuth $Z=83$; the time is 0.07μsec.

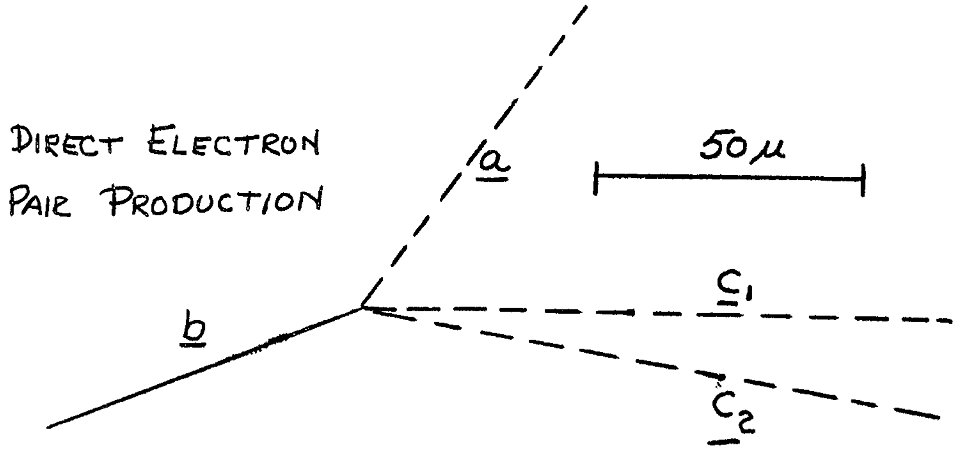
Schein spoke on the possibility of detecting V^o production by pions from the Chicago cyclotron. V^o- particle production from the Chicago cyclotron has been undertaken by J. Fainberg, K. Brown, R. Glasser, and M. Schein. The experimental arrangement was as follows: A 227 Mev π^- meson beam enters a 5" long carbon target. Two sets of photographic plates are placed at the side of the target and shielded in the direction of the π beam and in the backwards direction with 8" thick lead bricks. One set of plates were close to the target the other was 4 cm away. In the early work reported last year by Lord, the plates were directly in the π beam. Since the last report we have observed a few more events. We have attempted to produce the reaction $\pi^- + p \rightarrow V_1^o + (?)$, where the V_1^o has very small kinetic energy. The V_1^o decay $V_1^o \rightarrow p + \pi^-$ was looked for in the photographic plates. For low energy V_1^o the proton and meson come off in practically opposite directions, and the Q value is the sum of their kinetic energies. Accepting only events in which the meson comes in the backward direction with respect to the pion beam, we found three events in the close plates and no events in the back plates. Two of the three events have been analyzed so far. The best event is shown below



A proton and a meson come off in almost opposite directions, 170^o apart; the proton stopped in the emulsion giving a definite range so that its initial momentum can be determined very accurately. The meson's energy was obtained by multiple scattering measurements. For this event, the proton had 110 Mev/C momentum, and the π meson had 104 Mev/C momentum, the accuracy of the latter being 10%. Note that the momenta are approximately equal, suggesting a two body disintegration. Assuming the reaction $V_1^o \rightarrow p + \pi^-$ and taking the 10^o angle into account, the Q value for the reaction is 38 Mev, in very good agreement with cosmic ray work. The proton and the pion energies are 6.4 and 31 Mev, respectively. It is difficult to explain this event as a star. If it were, since the momentum is balanced and the energy is not balanced, the only type of star possible is one with neutral particles going at right angles to the charged particles and in opposite directions to each other. This would be a very peculiar star. Another similar event gives a Q value of 32 Mev. In the second case, the dip angle was larger, so that the measurement was not as accurate. The third event has not been analyzed completely but it has a Q value of approximately 44 Mev. For this event, the π meson does not have a long path in the emulsion. All this work is consistent with the cosmic ray data; however, it is preliminary and should be considered as suggestive until more work is done. In particular, the numbers and types of one-prong stars should be investigated in more detail. The cross section for the "strange" events compared to the meson interaction cross section in carbon is estimated to be of

Schein also reported on some strange nuclear stars produced by pions found by J. Fainberg, K. Brown, and D. Williams. Six similar cases of a peculiar type of star have been observed. An incident π^- meson with 227 Mev energy produced a star on a photographic plate. A meson comes off in the backward direction; for example, in one of these cases the meson leaves at 160^o and with 136 Mev energy, and a proton goes in the forward direction with approximately 110 Mev energy. These two particles together possess the kinetic energy of the incident meson, and, thus, the energy is used up. Since the proton carries off most of the momentum there appears to be no conservation of momentum. No nuclear recoils have been observed. So far, no explanation for this type of event has been given.

Finally, Schein discussed the emission of high energy electron pairs in high energy meson collisions. This work was carried out in collaboration with J. J. Lord, J. Fainberg, D. Haskin, and R. Glasser. The incident meson energies were 122, 141, 227 Mev. An example of such an event is shown below. a is a 141 Mev incident meson, b is a proton and C_1 and C_2 are electrons. The pair tracks are always at minimum ionizations and since their momenta are sometimes small, their masses have been estimated to be less than 10 electron masses. For the event shown in the figure, the momenta of the electrons are over 200 Mev/c. Fermi asked whether there were displacements between the center of the star and the origin of the pairs. Schein replied that they looked very carefully and always found that the pair originated within a micron of the star.



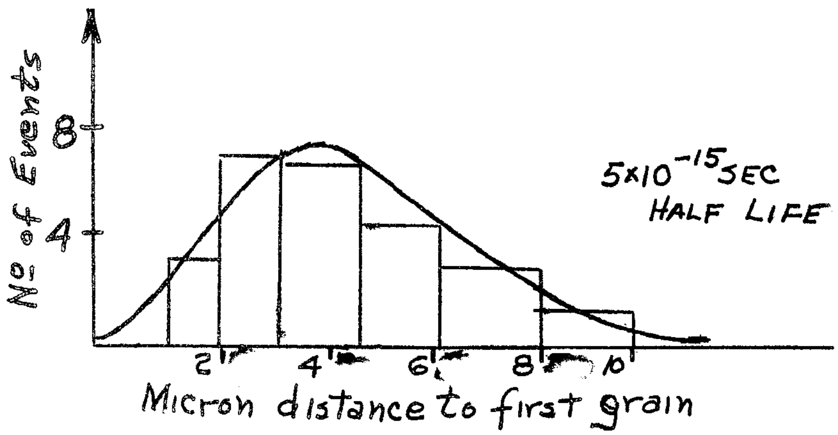
The mechanism for this pair production was considered. Conversion of single high energy nuclear γ rays was ruled out because of the Berkeley results which showed that the number of high energy γ rays was very small. It was estimated that this effect could not produce more than one pair in ten thousand events. That is, 1°/o radiation times 1°/o pair conversion. However, we observed a rate of one direct pair per 500 stars. We thus assumed that the pairs came directly from π^0 decay. The pairs could not come from a conversion of one of the π^0 photons since it could not materialize so quickly. Using a reasonable value for the π^- charge exchange cross section and the factor of 1/80 for the direct pair formation in π^0 decay, the calculated electron pair yield agrees with the result of one in 500. However, the angular correlation of the pair electrons is in poor agreement with the theoretical angular correlation. The observed angular correlation is too close. For the 200 Mev pair shown in the figure the angle is 0.3^o, whereas the theory gives an average angle of 10^o. Fermi said that the distribution strongly favors small angles. The distribution goes as $1/\theta$ and levels off at very small angles, but it is true that the mean angle is about 10^o. Schein continued: Out of a total of 8 pairs measured, 4 pairs have $\theta < 2^o$ where θ is the correlation angle between the pairs. The largest angle observed was 25° for a v ery low energy pair. There is one case where a 200 Mev pair has $\theta = 6°$. The latter event seems to agree with the direct pair production decay scheme.

The lifetime was next estimated. The π^0 kinetic energy can be estimated from measurements of the electron pair energies and angles. Knowing the π^0 energy and the distance from the origin of the pair to the star, the π^0 lifetime can be estimated. Unfortunately, for the cyclotron the π^0 energies are too small to give large displacements. For the 200 Mev π^0 pairs a gap of 4.8 microns should be observed for a mean lifetime of 10^{-14} seconds, a lifetime reported by the Rochester group. In all 8 events observed, the gap was less than two microns. The distance was determined for the pairs with large angles by extrapolating the tracks to a point. For the small angles there was always a grain within the first two microns. It can be estimated that the lifetime is either equal to or less than 5×10^{-15} seconds. Another peculiar feature is that all the 8 pairs are emitted in a backward direction.

Ritson then made some remarks concerning the measurement of the π^0 lifetime by Kaplon, Ritson and Walker at Rochester. The remarks were purely negative since they are now unsure of the published value of 10^{-14}sec. In fact, because of the Bristol work, they are not even sure that the measurements referred to the lifetime of the π^0 meson. Hence, a shorter lifetime than 10^{-14} sec. is quite possible. The reason for our doubt is due to the fact that in the initial observations using a stack of alternating lead and photographic plates, there was a very high energy event. Lower in the stack separated from the star by several radiation lengths of material there was the origin of a double core electronic shower. There were not many particles around the shower origin. There are three explanations for the event: (1) a π^0 decay at this origin which formed a double-core shower, (2) one of the secondary particles caused another interaction which produced γ rays, and (3) there is another type of particle which decays. The secondary interaction explanation had been ruled out because the energy of the electronic shower was comparable to the initial energy and there were no fragments or particles at the shower origin. The point of the so called π^0 decay was obtained by extrapolating the axes of the two cores back to a point.

However, recently another similar event has been observed with a secondary electron shower originating a large distance from the primary event. From the angle between the cores, the π^0 energy was estimated to be about 10^{11} ev. In the first event, the energy was 5×10^{12} electron volts. Using a lifetime of 10^{-14} seconds for the π^0 in the second event, it is extremely unlikely for the π^0 to decay so far from the original interaction. The lifetime must be at least 2 or 3×10^{-14}sec. Again, with the second event, the shower contains a large fraction of the original energy and there is no visible interaction at the origin of the photon pair. Thus, the second event also has the characteristics of a π^0 decay. Such a long lifetime is inconsistent with the results of the other groups and now we doubt our interpretation of the first event as a π^0 decay. Marshak remarked that the secondary interaction at the shower origin seemed rather improbable and an alternative explanation could be that a heavier neutral meson decay was being observed, since the energies in the electronic cores were uncertain.

Perkins reported on the Bristol measurement of the π^0 lifetime using the electron pairs that appear to come out of stars made by Anand, Daniel, Davies, Mulvey, and Perkins. The work is similar to that reported by Schein. The π^0 lifetime is determined by measuring the distance between the star and the origin of the direct decay pair. If the frequency of pairs is plotted as a function of the distance of the pair origin to the star one finds a peak within the first 5 microns and a constant smaller frequency for larger distances. The peak is believed to be due to direct pair formation the π^0 decay while the smaller frequency at larger distances is due to the conversion of one of the π^0 photons. Sixty-two direct pair electrons have been observed. The following graph shows the distance in microns to the first resolved grain of the electron pair. A similar distribution was measured for protons having twice minimum ionization and coming directly from stars. The distribution of the distances from the first proton produced grain to the star should be exponential, but due to the large number of grains at the star it is impossible to resolve grains at the center. Thus, there is an initial gap. Assuming that (1) the first grain distribution is that observed for the protons, and (2) the π^0 energy spectrum is the same as the charged pion spectrum, then the lifetime fitting the data best is about 5×10^{-15} sec. with a spread from 3 to 12×10^{-15} sec It is clear from the data that the pairs originate away from the stars and that the π^0 has a finite lifetime greater than 10^{-15} sec. H. Anderson said that the measurements of the grain density for close electron pairs was measured to be only 1.5 times minimum. Perkins replied that they find twice minimum. The mean distance to the first grain is about 4.0 ± 0.2 microns for the twice minimum proton track and 4.9 ± 0.2 microns for the electron pair tracks. The conclusion that the π^0 lifetime is finite is supported by the fact that the high energy pairs originate farther from the star. For opening angles less than 0.025 radians, the average distance to the first grain is $4.4 \pm 0.3\,\mu$; for opening angles greater than 0.025 radians, the average distance to the first grain is $5.4 \pm 0.4\,\mu$. The ratio of π^0 decay electron pairs to the two γ decay is $1.3 \pm 0.4\%$, in agreement with the work of Steinberger and Dalitz.

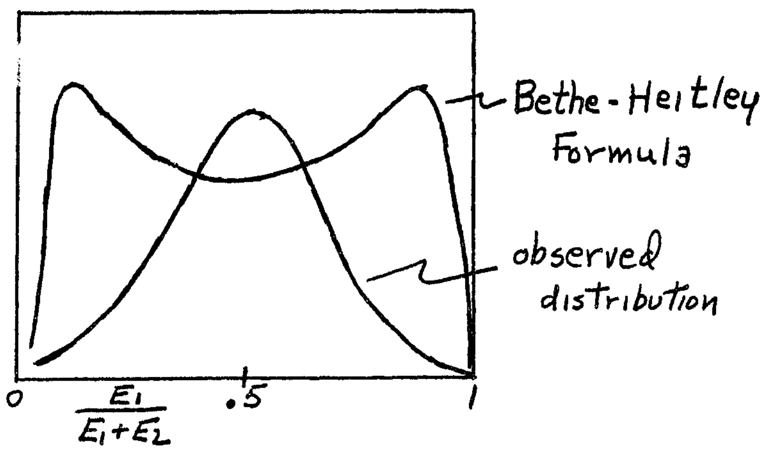


Perkins next described the Bristol evidence for direct μ meson production in stars. There are only two examples of direct μ meson production in stars. In one of these events there was a large star from which a μ meson appears to leave. It travels 735 microns and decays into an electron with an energy of 20 Mev or more with a 25% uncertainty in the energy. Alvarez asked what would be seen if the μ came from a forward decay in flight of a π. Perkins said that in general there would be a large angular deflection and not a forward decay in flight, but even if there was a decay in the forward direction, there would be a factor of 2 or more change in the grain density. There is always the chance that a π meson decays in the first grain, but this is unlikely. Alvarez then pointed out that the chance for a π meson with a velocity of $\beta \simeq 0.1$ to decay within the first 10 microns is 1 in 10^5 π's leaving stars. Because so many stars have been seen at Bristol, there is reasonable chance to see such an event. However, a total of only 275 slow π^- and 30 slow π^+ particles have been observed ejected from stars and coming to rest in the same emulsion. Fermi said that since the range of the μ is just a little over that for a π decay at rest, the π meson had a good chance to decay near the star. In the second event, Perkins continued the μ meson had a range of 350 microns and the decay electron had an energy of 40 ± 10 Mev. The frequency of these events is less than 3% of the π mesons produced in stars. Goldschmidt said that a similar event was observed a couple of years ago at Bristol.

Segre mentioned the work on the possible photo production of V particles by Bernardini, et. al., at Illinois. The bremsstrahlung beam of the Illinois betatron was sent into a cylinder of aluminum with photographic plates placed in the center. The plates were shielded from the direct γ ray beam by a lead block. About 8 cc of emulsion has been scanned. Many π mesons and protons were found plus 2 events which looked like V particles. Assuming a lifetime of 10^{-9} sec. for the V particle, the yield is 10^{-4} of the π meson yield.

Fermi then spoke of the negative results on V^o production by 450 Mev protons obtained by Garwin at Chicago. If V^o particles are produced there should be two equally likely modes of decay, namely, (1) $V^o \rightarrow p + \pi^-$ and (2) $V^o \rightarrow N + \pi^0$ followed by $\pi^0 \rightarrow 2\gamma$. Thus, for the V^o particles traveling a short distance before decay, one would expect a source of γ radiation originating a few centimeters from the target. A 450 Mev proton beam irradiated a target; in the vicinity of the target a search was made for high energy γ rays in the vicinity of the target with a very well collimated γ ray detector. No events were found which put an upper limit on the cross section of 10^{-32} cm^2 per nucleon. Alvarez said that a similar experiment, attempting to measure the π^0 lifetime, was done by York at Berkeley. He found no radiation starting 0.001" from the target.

Perkins made a remark concerning the relative energies of the two electrons in the raction $\pi^0 \rightarrow 2\ e + \gamma$. The following graph shows the relative energy distribution of the electrons in the decay process $\pi^0 \rightarrow 2\ e + \gamma$. The abscissa is the ratio of the energy of one of the electrons to the sum of the energy of both of the electrons. Also shown on the graph is the Bethe-Heitler pair production distribution. The ordinate shows the relative number of events. Note that the two distributions are quite different. The average energy of the direct electron pairs was compared to the average energy of the pairs produced by one of the photons. The ratio of the related to the direct pair energies was 0.90 ± 0.24. The uncertainty is too large to draw any definite conclusions.



Perkins also remarked on the lack of direct evidence for ρ^0, as indicated by the work of Danysz, Harris, Juritz, and Lock. Bristol no longer believes direct their observation of the ρ^0. The independent existence in emulsions for a ρ^0 can only be shown by an event in which a pair of π mesons have an origin a finite distance from the star. This is what one observes for the π^0. There may be indirect evidence for the ρ^0 since there is a very strong angular correlation between pairs of π^0 mesons coming out of stars. Of the correlated pairs which have been analyzed, 15 have an apparent $Q \leq 15$ Mev; 10 of these are consistent with $Q = 2.5 \pm 1$ Mev, assuming two-particle decay. Alvarez points out that if the ρ^0 exists for a very short time then it is like the Be^8 nucleus which dec into two α particles.

A. Sachs reported on the results at Columbia on the reaction $\pi^0 \rightarrow 2e + \gamma$. A counter experiment was performed by looking at the two electrons coming directly from a liquid H_2 target. The results were 8 ± 1.7 pairs per 1000 events compared to the theoretical prediction of 6 per 1000 The reaction of $\pi^0 \rightarrow 2e$ without any γ rays has a cross section less than 1 per 2000 events. An independent experiment by Sargent and Rinehart with a hydrogen filled diffusion cloud chamber where the π^- mesons stop in the gas is also in progress. The results so far are that no direct pairs have been observed in 200 π^- stoppings. However, in the same set of pictures there are 20 μ stopping showing decay electrons.

Walker spoke on his search for pair correlations in cosmic ray showers. An extensive search was made for pairs of penetrating particles originating in penetrating showers in carbon. In 100 showers no angular correlation was found between pairs of particles which could not be explained by chance coincidences. The correction for chance coincidences is rather difficult and one must deal with many events. It was concluded that less than 1/4 % of the shower particles in carbon are emitted in pairs.

Fermi stated that for high energy stars there may be an angular correlation between pairs of particles which are due to a process that has an intermediate state. This intermediate state may be a purely quantum mechanical state which is virtual or it may exist for a very short time. Two particles with momenta $\underline{P}_1$ and $\underline{P}_2$ that, in general, are quite different have a very small angle between them. The idea to be used here is essentially the same which is used in the production of electron pairs by γ rays. For example, consider the reaction $\pi^0 \rightarrow 2e + \gamma$. There is a high probability fo the two electrons to have a small angle between them, and the momenta $\underline{P}_1$ and $\underline{P}_2$, in general, do not have the same length. This can be explained by the following quantum-mechanical picture. Suppose there is an intermediate state A that normally emits a photon plus other things. Occasionally, A will go to a virtual photon with slightly different energy and materialize into a pair. The pair has momenta $\underline{P}_1 + \underline{P}_2$. Thus the intermediate photon must have the momenta $\underline{P}_1 + \underline{P}_2$. The energy of the photon is $c|\underline{P}_1 + \underline{P}_2|$ where c is the velocity of light. The energy of the final state with the two electrons is $c|\underline{P}_1| + c|\underline{P}_2|$ If the rest mass energy of the electrons can be neglected then the geometrical requirement for $c|\underline{P}_1| + c|\underline{P}_2|$ to be approximately equal to $c|\underline{P}_1 + \underline{P}_2|$ is that there is a very small angle between the two particles. However, the vector $\underline{P}_1$ and $\underline{P}_2$ do not have to be the same length. Thus, for relativistic particles there should be an angular correlation, if there is an intermediate state.

Perkins said that the π pairs observed at Bristol come from low energy stars and the π mesons had non-relativistic energies. This is required for scattering and grain counting measurements. Fermi replied that his explanation would not work for low energies.

Shutt reported on the measurement of the interaction of π mesons with helium with Thorndike, Fowler, Whittemore and Fowler at Columbia. This work was carried out for 60 Mev $\pi^{\pm}$ mesons and 105 Mev π^- mesons. Events were observed in a diffusion cloud chamber containing helium with no magnetic field. The results are shown in the table. The 60 Mev π^+ and π^- data are lumped together. The numbers are the cross sections in millibarns per helium nucleus. The numbers in the parentheses indicate the number of events observed.

σ elastic

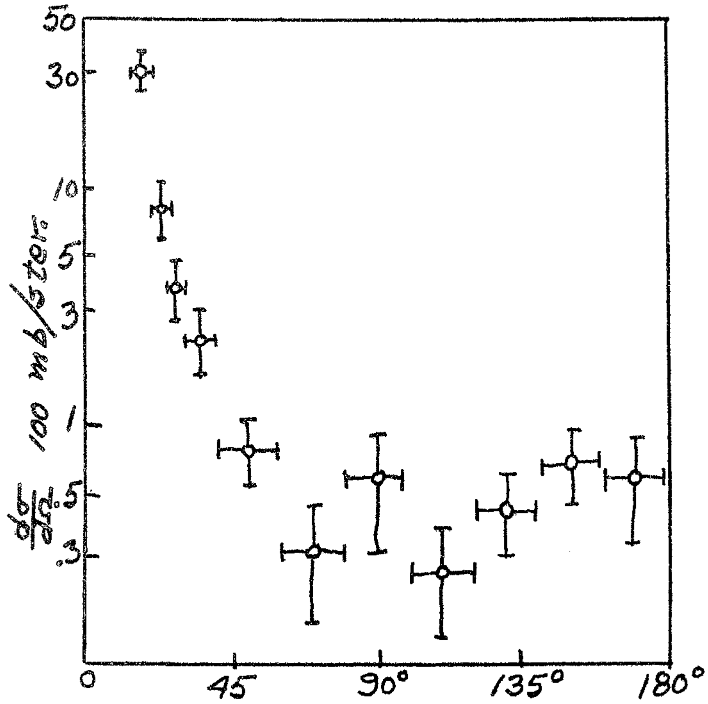
meson energy	Total	forward	backward
60	37 (10)	7	30
105	71 (25)	37	34

σ interaction

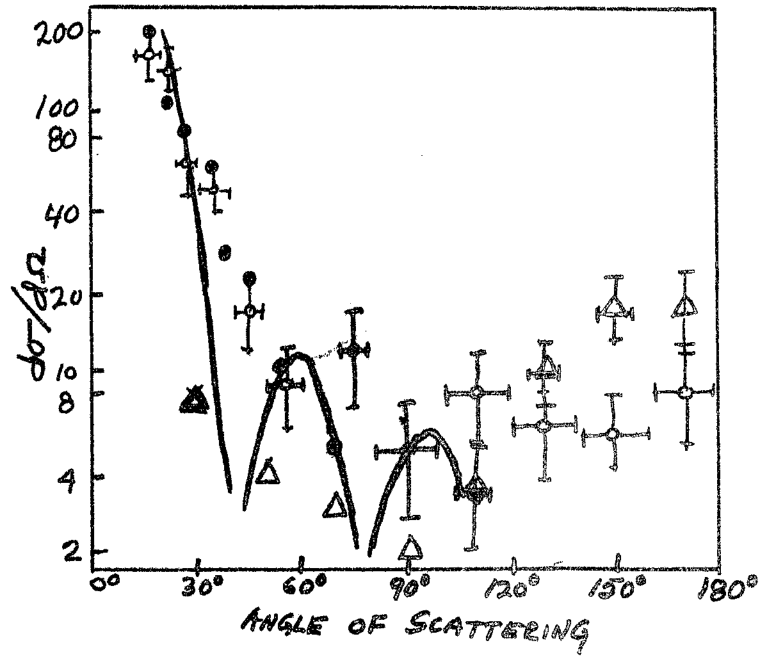
meson energy	total	inelastic scattering	absorption and charge exchange	absorption	charge exchange	prongs frwd.	prongs bkwd.
60	52(14)	15	37	15	22	9	0
105	133(47)	51	82	20	62	23	6

The main splitting of the data is into elastic scattering and other interactions. For the elastic scattering the total cross section is further divided into forward and backward scattering. For the 60 Mev π^+ elastic scattering if one assumes that the π^++P scattering is mostly P wave while the π^++N scattering is mostly S wave, then since the interference produces a backward scattering, the S and P phases would be opposite. The high forward elastic scattering for the 105 Mev π^- scattering is probably due to the increase in diffraction scattering due to the large interaction cross section. The interaction cross section is split into two groups: (1) inelastic scattering and (2) absorption and charge exchange. The latter two were difficult to separate. However, because of the prong distribution, variation of the cross section with energy, and other factors, the two types of processes are estimated individually. The number of prongs for the absorption and charge exchange events are also shown in the table. No that the prongs are predominantly in the forward direction.

Lederman reported on the 130 Mev π^- interaction cross sections in carbon and lead performed with Kessler and Rogers. A cloud chamber was used with two 1/8" lead plates and a graphite plate between them. A 130 Mev π^- beam traversed the chamber, and 50,000gm/cm^2 of C and Pb were traversed. No V particles were observed, giving a cross section of less than 0.2% geometric. Events were looked for in which a π^- meson stopped in the graphite and a related electron pair was produced in the lead. Both electrons of the pair must have more than 10 Mev energy. From an enormous number of meson traversals there were only 8 cases of π stopping and a related pair. The background was essentially zero, especially for pairs produced in the backward direction, i. e., at an angle of 110° or greater with respect to the incoming meson direction. Assuming π^- charge exchange and the π^0 decay into two γ-rays, the cross section is 5% of geometric or 10% of the total number of stars in lead and carbon. Using Steinberger's value of 1/80 for the π^0 decay rate into direct electron pair, they observe one direct pair per 500 stars in agreement with the work of .chein. Actually, it is not shown that there is charge exchange, since only one high energy photon associated with the meson interaction was observed. In the same experiment they looked for nuclear interactions in lead and carbon. This supplements similar work done with 60 Mev mesons. The results for lead and carbon are shown below.



125 Mev π^- on lead
-ϕ- elastic scattering
Δ inelastic scattering ΔE > 60 Mev
σ_d( ) ≃ 620 mb
$\sigma_{in} \geq 620$ mb
$\sigma_a = 2200 \pm 290$ mb



130 Mev π^- on carbon
-ϕ- elastic scattering
Δ inelastic scattering ΔE ≥ 60 Mev
X elastic at 62 Mev on carbon
$\sigma_{el}(> 15^{\circ}) = 185 \pm 20$ mb
$\sigma_{inel}(\Delta E \geq 60) \geq 80$ mb
—— black body diffraction

Comparing the de ction scattering of the 60 and 130 Mev data, it is surprising that the angular distributions are the same. Theory predicts that for the higher energy the curve should be pulled in to smaller angles. The inelastic scattering cross section in carbon is 80 millibarns for energy loss greater than 40 Mev. The backward inelastic scattering is four times larger than the forward scattering and is strongly suggestive of the pion-nucleon distribution.

Leprince-Ringuet asked what was the highest energy proton observed from a nucleus capture of a π^- at rest. Several people said that protons of energy greater than 100 Mev have occasionally been observed.

Appendix I: THE RELATIVISTIC RISE IN IONIZATION IN NUCLEAR EMULSION, B. Stiller and M. Shapiro, N. R. L.

Further work has been done to determine the amount and rate of increase of ionization loss I at high energies.[1] In this work scattering measurements have been made on long tracks of many particles with velocities in the interesting velocity range. A comparison of ionization loss has been made between electrons and heavy particles. All of the data used has been taken from a single plate. Blob counting is used instead of grain counting. No variations in $G_{min.}$ are found in the plate used within the limits of errors of the experiment.

Results showing blob density as a function of γ may be found at the end of session 5. For $\gamma > 100$ only electrons were used for $10 < \gamma < 100$ mesons and electrons were used and for lower velocities, protons and mesons.

To arrive at a value of $G_{p\ell} / G_{min.}$, a least squares determination of $G_{p\ell}$ was made using 19 electron tracks with 100. The least squares slope is $.15^{o} \pm 5^{o}$. The final result is $G_{p\ell} / G_{min.} = 1.14 \pm .03$, which is higher than Voyvodic's[2] results; also the rise to the plateau is more gradual.

Calculations were made of I using Halpern-Hall[3] theory for AgBr using a restricted energy loss. The loss was restricted to less than 2 Kev and 5 Kev. Either of these fit the data reasonably well. In particular, the data do not conflict with theory as to the energies ($\gamma > 100$) at which saturation of ionization loss sets in.

1. M. Shapiro and B. Stiller, Phys. Rev., 87, 682 (1952).
2. Pickup and Voyvodic, Phys. Rev., 80, 89 (1950). Also L. Voyvodic at the Bristol Conference, December, 1951.
3. O. Halpern and H. Hall, Phys. Rev., 73, 477 (1948).

Appendix II: V PARTICLE PRODUCTION, W. D. Walker, University of Rochester and N. M. Duller, Rice Institute.

In the course of an experiment on high energy nuclear interactions in carbon, pictures of 500 penetrating showers originating in carbon plates inside a cloud chamber were obtained.

Only two cases of V^{o} decay and one case of $V^{\pm}$ decay were found. Assuming a search efficiency of 50% as compared to that of Fretter[1], the apparent rate of production of V^{o}'s seems to be a factor 4-6 lower than he has found.

The difference between this experimental arrangement and Fretter's seems to be:

1. His showers were generated in Pb and ours in C.
2. The triggering requirements in this experiment were more stringent so that our median energy could be higher by a factor of 1 1/2 or 2 than in Fretter's experiment. ($\bar{E}_{estim.} \approx 20-30$ Bev). The difference in energy was estimated by roughly comparing average counting rates.

It seems possible that V_1^0's are formed more often in interactions of lower energy than those in this experiment. If this were the case, energetic showers in a heavy nucleus, in which secondary multiplication occurs, would give rise to more V_1^0's than showers in a light nucleus.

About one charged V decay was expected according to the Manchester [2] data if the lifetime is greater than 10^{-9} sec.

1. W. B. Fretter, P. R., 83, 1053 (1951)
2. Barker, Butler, Sowerby and York, Phil. Mag., 43, 1201. (1952).

Appendix III: POSSIBLE NEUTRAL PREDECESSOR OF THE τ MESON, M. Annis, Washington University, St. Louis and M. Goldhaber, Brookhaven National Laboratory, Upton, New York.

It has recently been suggested[1] that the τ meson may often arise from the decay of a neutral predecessor, tentatively considered as the neutral counterpart of the $\chi^{\pm}$ meson, decaying in the following way: $\chi^0 \to \tau^{\pm} + \pi^{\mp}$ (Q ~60 Mev)[2].

We have, therefore, considered more closely the recently published picture of a τ meson decay[3] to find out whether it is compatible with the above decay scheme. One finds indeed a particle in the picture which can be interpreted as being the π meson. This particle and the meson meet at a point above the cloud chamber within the uncertainty due to multiple coulomb scattering. The particle traverses one 1/4" Pb plate and shows a possible nuclear scattering in the next Pb plate. The π meson, the particle and the apparent origin of the nuclear event lie in a plane, again within the expected uncertainty due to multiple coulomb scattering. The Q value, assuming the above decay scheme, is 85^{+385}_{-75} Mev. The life-span of the "χ^0" in this event is of the order of 2×10^{-10} sec in its own restframe.

1. M. Goldhaber, Bull. of the Am. Phys. Soc., Cambridge Meeting, Jan. 1953, Vol. 28, Abs. R .
2. Leighton, Wanlass and Anderson, Phys. Rev. 89, 148 (1953).
3. M. Annis and N. F. Harmon, Phys. Rev. 88, 202 (1952).

Appendix IV: DYSON'S NON-COVARIANT INTEGRAL EQUATION FOR PION-NUCLEON SCATTERING.

$$g(k) = \frac{G^2}{8\pi^2} \int_0^\infty \frac{k'^2\, dk'}{(E_k w_k E_{k'} w_{k'})^{1/2}} \times$$

$$\Big\{ Q \frac{E_k + M}{E_k + w_k + E_{k'} + w_{k'} + M - E} \qquad \text{(For S states only)}$$

$$+ Q \frac{kk'}{(E-M)(E_{k'}+M)} \qquad \text{(For } P_{1/2} \text{ states only)} \qquad (1)$$

$$+ \frac{1}{2} Q' (E_k + M) \left[H_\ell + (E_k + E_{k'} - M) \quad K_\ell \right]$$

$$+ \frac{1}{2} Q' \frac{kk'}{E_{k'} + M} \left[H_{\ell \pm 1} + (E_k + E_{k'} + M) \quad K_{\ell \pm 1} \right] \Big\} f(k')$$

where

E = energy of the system (meson + nucleon) in the center of mass system.

$= E_{k_o} + w_{k_o}$

k, k' = wave numbers; $E_k = (M^2 + k^2)^{1/2}$; $w_k = (\mu^2 + k^2)^{1/2}$

Q = 3 for I = 1/2   Q = -1 for I = 1/2
  = 0 for I 3/2   = 2 for I = 3/2

In the last line take 1 for $j = \ell + 1/2$
                            -1 for $j = \ell - 1/2$

$$f(k) = \delta(E_k + w_k - E) + P \frac{1}{E - E_k - w_k} g(k) \tag{2}$$

$$\tan\delta = -\pi g(k_o) \tag{3}$$

$$\text{and } H_\ell = \frac{1}{2\bar{E}r} \int_{1-r}^{1+r} \left(\frac{c}{c+Z} - \frac{b}{b+Z} \right) P_\ell \left(\frac{Z^2 - 1 - r^2}{2r} \right) dZ \tag{4}$$

$$K_\ell = \frac{1}{2\bar{E}^2 r} \int_{1-r}^{1+r} \left(\frac{1}{c+Z} + \frac{1}{b+Z} \right) P_\ell \left(\frac{Z^2 - 1 - r^2}{2r} \right) dZ \tag{5}$$

where $\bar{E} = \frac{1}{2}(E_s + E_d)$ $E_s = \left[M^2 + (k+k')^2 \right]^{1/2}$ $E_d = \left[M^2 + (k-k')^2 \right]^{1/2}$

$$r = \frac{E_s - E_d}{E_s + E_d} \qquad b = \frac{E_k + E_{k'} - E}{\bar{E}} \qquad c = \frac{w_k + w_{k'} - E}{\bar{E}}$$

Special cases:

$$H_o = \frac{1}{2\bar{E}r} \left[c \log \frac{1+c+r}{1+c-r} - b \log \frac{1+b+r}{1+b-r} \right] \tag{6}$$

$$K_o = \frac{1}{2\bar{E}^2 r} \left[\log \frac{1+c+r}{1+c-r} + \log \frac{1+b+r}{1+b-r} \right] \tag{7}$$

$$H_1 = \frac{1}{2\bar{E}r} \left[\frac{c}{2r} (c^2 - r^2 - 1) \log \frac{1+c+r}{1+c-r} \quad \frac{b}{2r} (b^2 - r^2 - 1) \log \frac{1+b+r}{1+b-r} + (c - c^2 - b \quad b^2) \right] \tag{8}$$

$$K_1 = \frac{1}{2\bar{E}^2 r} \left[\frac{c^2 - r^2 - 1}{2r} \log \frac{1+c+r}{1+c-r} + \frac{b^2 - r^2 - 1}{2r} \log \frac{1+b+r}{1+b-r} + (2 + b - c) \right] \tag{9}$$

Since r is never much greater than 0.05, these expressions can be expanded in power series giving as the leading term:

$$H_0 \doteq \frac{1}{\bar{E}}\left(\frac{1}{1+b} - \frac{1}{1+c}\right) \tag{10}$$

$$K_0 \doteq \frac{1}{\bar{E}^2}\left(\frac{1}{1+b} + \frac{1}{1+c}\right) \tag{11}$$

$$H_1 \doteq \frac{kk'}{3\bar{E}^3}\left[\frac{1}{(1+c)^2} - \frac{1}{(1+b)^2}\right] \tag{12}$$

$$K_1 \doteq -\frac{kk'}{3\bar{E}^4}\left[\frac{2+c}{(1+c)^2} + \frac{2+b}{(1+b)^2}\right] \tag{13}$$

Further, in the $P_{3/2}$ state H_2 and K_2 have been neglected giving for $I = 3/2$ the approximate equation

$$g(k) \approx -\frac{G^2}{24\pi^2}\int_0^\infty \frac{kk'^3\,dk'}{(E_k w_k E_{k'} w_{k'})^{1/2}}\;\frac{E_k + M}{\bar{E}^3}\;\times$$
$$\left[(E_k + E_{k'} + w_k + w_{k'} - M - E)\,\frac{2\bar{E}+c}{(\bar{E}+c)^2} + (E-M)\,\frac{2\bar{E}+B}{(\bar{E}+B)^2}\right] f(k')$$

with $B = \bar{E}b = E_k + E_{k'} - E$ and $C = \bar{E}c = w_k + w_{k'} - E$

The second term inside [ ] corresponds to negative energy intermediate states and is only about 5% of the first term. If it is dropped one obtains the equation given in the body of the text. Bethe's first approximation is to use $(E_k + E_{k'} + w_k + w_{k'} - M - E) \approx \bar{E} + C$.

Appendix V: ELASTIC PION-DEUTERON SCATTERING, E. Arase, Gerson Goldhaber and [illegible]. Goldhaber, Columbia University.

The elastic scattering of 140 Mev negative pions by deuterium has been studied in D_2O loaded Ilford G5 photographic emulsions. (Deuterium content 0.11 gm/cm^3 of loaded emulsion.)

By "area scanning", 876 nuclear interactions were found in an effective π meson pathlength of 290 ± 40 meters in emulsion. Since elastic $\pi + D$ scattering events can be identified from the energy momentum conservation, all 1-prong scattering events were examined according to the following three criteria:

1) The coplanarity of the three prongs.
2) The angular correlation between the scattered meson and the recoil prong.
3) The correlation between the range and angle of the recoil prong, when ending in the emulsion.

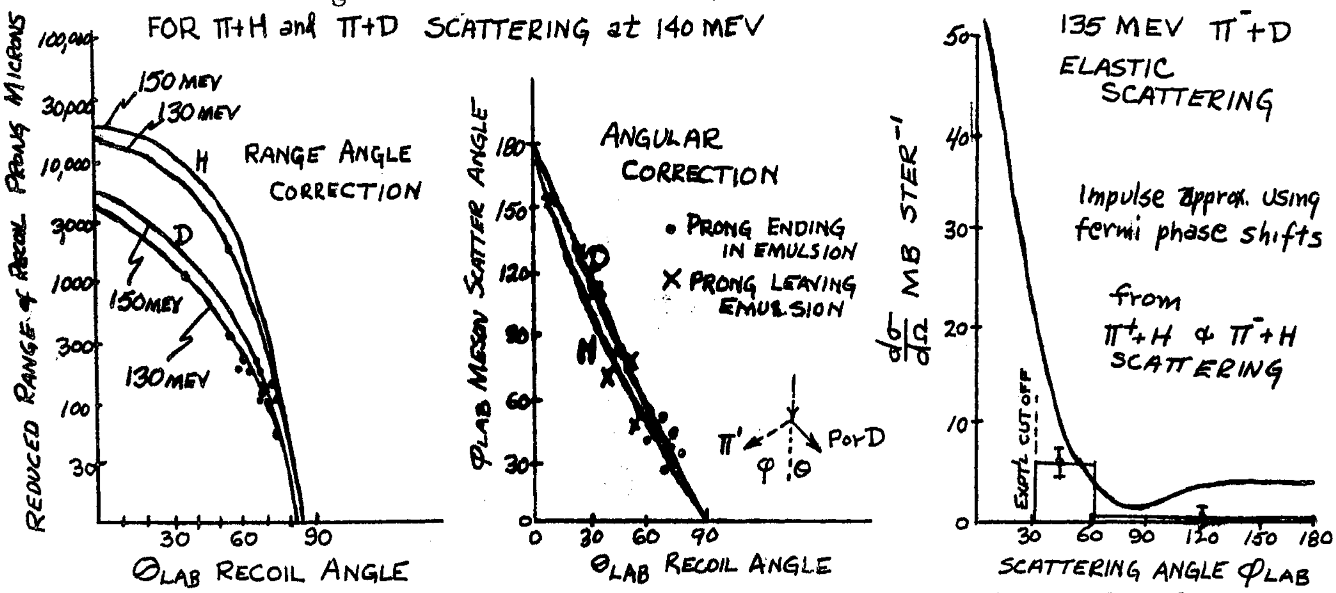
By this analysis 20 1-prong scattering events were identified as elastic $\pi^- + D$ scatterings and 3 as $\pi + H$ scatterings. As the scanning efficiency decreases for events with a recoil prongs shorter than 50 microns, a safe cut-off at 100 microns prong length was taken. This corresponds to a cut-off angle of 30° for the scattered meson.

The differential cross section is found to be strongly peaked in the forward direction (see table), with 13 events in the angular interval 30^{o} - 60^{o} and 5 events in the interval 60^{o} - 180^{o}.

Concurrently with this work Thomas A. Green has calculated the differential cross section of elastic π+D scattering. He evaluated an impulse approximation by use of the Fermi phase shifts based on the positive and negative π+H scattering at 135 Mev. (see table)

$\Delta\varphi$lab	$\Delta\sigma$expt'l	$\Delta\sigma$impulse
30 - 180	19 - 4.5 mb	50 mb
30 - 60	13.6 - 3.5 "	25 "
60 - 180	5 - 2.5 "	25 "

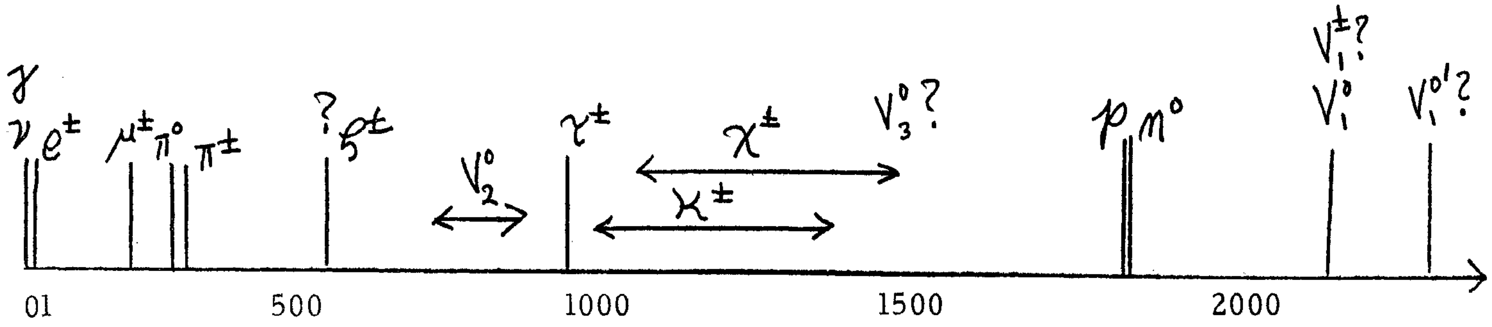
Although our statistics are not very good as yet, there appears to be definite disagreement with the impulse approximation calculation. Namely, the experimental cross section obtained so far is considerably smaller than the calculated value particularly in the backward direction. However as was pointed out by Brueckner the neglect of multiple scattering in the impulse approximation results in too large a total cross section.



An experiment is being carried out at Columbia by Goldhaber and Lederman on scattering of 180 Mev π^{+} mesons of hydrogen. The high energy positive meson flux was obtained by internal cyclotron exposures using loaded and normal nuclear emulsions. Events in which mesons collide with hydrogen of the emulsion are identified by the kinematics of the collisions. To date 23 scatterings have been observed yielding a preliminary cross section of 140 mb. The data is still too preliminary to give information on the resonance behavior of the π^{+}+ P energy curve.

Appendix VI: THE UNSTABLE "ELEMENTARY" PARTICLES OR MEGALOMORPHS

Particle Products	Observed by	Lifetime (sec.)	Q	Mass	Statistics	Spin	Parity
? $V_1^{0\prime} \to p + \pi^-$	c. c.	$> V_1^0$	~75 Mev	$2270m_e$?	F. D.	n/2?	-
$V_1^0 \to p + \pi^-$	c. c.	3.5×10^{-10}	37 Mev	$2190m_e$	F. D.	n/2?	-
? $V_1^\pm \to p + (?)^0$	c. c.	?	?	?	?	?	?
$n^0 \to p + e^- + \nu$	Spectrograph & counters	740	783 Kev	$1837m_e$	F. D.	1/2	-
$V_3^0 \to K^- + \pi^+$	c. c.	?	?	$M_p > m_{V_3^0} > m_\pi$	?	?	?
K $\left\{ S^\pm, \chi^\pm, V^\pm \right\} \to \pi^\pm + (?)^0$, $M_0 \sim 800 m_e$	c. c. c. c. & emul.	2×10^{-8} - 2×10^{-9}	115 Mev	$1400m_e$	B. E.	0?	S?
K $\left\{ \kappa^\pm, V^\pm \right\} \to \mu^\pm + ?2\nu$	emul. c. c.	?	?	$1100m_e$	F. D.?	1/2?	-
$\tau^\pm \to \pi^\pm + \pi^+ + \pi^-$	emul. & c. c.	10^{-8} - 10^{-9}	75 Mev	$975m_e$	B. E.	0?	PS?
$V_2^0 \to \pi^+ + \pi^-$	c. c.	$\sim 10^{-10}$	210 Mev	$850m_e$	B. E.	0?	S?
? $\zeta^\pm \to \pi^\pm + (?\pi^0)$	emul.	$?10^{-11}$	40 Kev $<Q<$ 6 Mev	$552m_e$	B. E.	0?	S?
$\pi^\pm \to \mu^\pm + \nu$	counters	2.3×10^{-8}	5.9 Mev	$276m_e$	B. E.	0	PS
$\pi^0 \to 2\gamma$, $\to e^+ + e^- + \gamma$	counters emul. & counters	$\lesssim 5\times10^{-15}$	135 Mev	$266m_e$	B. E.	0	PS
$\mu^\pm \to e^\pm + 2\nu$	counters	2.15×10^{-6}	105 Mev	$212m_e$	F. D.	1/2	-



MASS IN m_e

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Stevenson, M. L. (Berkeley)
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Sun, K. H. (Westinghouse)
Thaler, R. M. (Yale)
Thompson, R. W. (Indiana)
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