Session III

NUCLEON-NUCLEON INTERACTION

Chairman: H. A. Bethe

MARSHAK: Introductory survey

I would like to point out that this session on the nucleon-nucleon interaction was a last minute decision and in trying to prepare these introductory remarks I perhaps will have left out the work of quite a few people. I apologize beforehand.

I should like to make it clear at the outset that I shall be speaking only about the two nucleon interaction. It is well known that the work of Breit and of Landau and Smorodinsky has shown that the extremely short range and great strength of the nuclear force imply that the features of the two nucleon interaction are rather insensitive to the detailed shape of the force at low energies. Of course, the spin exchange properties of the central force and the existence of a tensor interaction are determined by the low energy experiments. As more and more high energy scattering experiments have been performed - first single scattering, then double scattering, then triple scattering - the nuclear force problem has become more sharply defined and the question may be fairly asked whether there now exists sufficient information to establish the essential properties of this nuclear force. I shall try in these introductory remarks to indicate why I believe that this question probably has an affirmative answer.

There have been two general approaches to the problem of nuclear forces, the meson-theoretic approach and the phenomenological approach. In actual fact, the meson theory of nuclear forces has a history almost as long as the phenomenological approach, which, one might say, started with the discovery of the neutron. Yukawa's first attempt in 1935 to explain the strength, short range, and saturation character of nuclear forces on the basis of an exchange of charged and neutral field quanta of intermediate mass opened up a subject which has since had numerous and exasperating disappointments. A major obstacle to progress was that for a long time the meson assumed to be responsible for nuclear forces was wrongly identified with the cosmic ray mu meson and lack of knowledge of the specific properties of the mu meson left room for a multiplicity of meson theories of nuclear forces - based on different assignments of spin and parity to the meson and corresponding to different types of coupling to the nucleon (direct versus gradient coupling, single versus meson pair coupling, single coupling versus a mixture of couplings and many other combinations graced by the names of quite a few members of the present audience). It was only after the discovery of the pi meson that it became clear that if nuclear forces in reality have a field-theoretic origin, the pion field must be the field chiefly responsible for these forces. The rapid development of what we now call classical pion physics led to results which all gave strong support to the qualitative features of Yukawa's original hypothesis.

The working out of a quantitatively correct pion theory of nuclear forces still remained however. An intimation of the form of the coupling of the pseudoscalar pion with a nucleon came from the new developments in quantum electrodynamics. The renormalization techniques which were so successful in dealing with the infinities of quantum electrodynamics, when applied to the pseudoscalar field showed that only the ps (ps) theory was renormalizable (with the proviso that one must include a term of the form ϕ^4 in the Hamiltonian to remove the infinities in pion -pion scattering). The crucial problem of the pion theory of nuclear forces was therefore to work out the predictions of the renormalizable ps (ps) theory, to compare the quantitative predictions with experiment and thereby to decide whether the ps (ps) theory is correct. The fulfillment of this program turned out, however, to be an enormously difficult task.

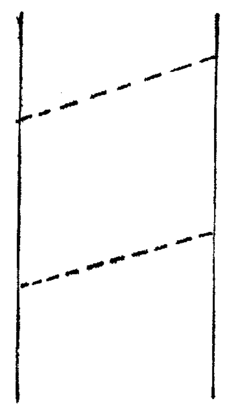
Lévy, and then Klein, made the most sustained effort to find out precisely what the ps (ps) theory of nuclear forces actually predicts. It was clear immediately that the odd character of the Dirac γ_5 operator in the ps (ps) theory required as a minimum a calculation of the second and fourth order contributions to the nuclear force within the framework of some method involving an expansion in powers of the coupling constant. Levy adopted the non-adiabatic Tamm-Dancoff method and was able to show that the second order nuclear interaction becomes a static potential (containing both central and tensor parts) at sufficiently large distances or at sufficient low kinetic energies of the nucleon. He showed that at very small distances, $\lesssim 1/M$, the second order two-nucleon interaction becomes repulsive insofar as the S-states of the system are concerned. At intermediate distances and at high energies, corrections to the second order static interaction certainly have to be taken into account in order to obtain quantitatively correct answers. Lévy's work on the fourth order interaction was much less definite in its outcome, apart from the numerical errors pointed out by Klein. The fourth order radiative corrections, which can be shown to be more important than the second order radiative corrections, were not really calculated and the pion-pion scattering types of diagrams were not fully considered. The short range Wigner force which is usually taken as the fourth order contribution to the so-called Lévy potential is only a small part of the total fourth order interaction which results if one attempts to make a consistent expansion in powers of the coupling constant and in the ratio of the pion to the nucleon rest masses. Not unexpectedly Klein's work and the more recent work of Greene and Feldman on the sixth and higher order interactions in the ps (ps) theory have thrown further doubt on the sum of the second and fourth order interactions in the Lévy potential as a suitable static potential.

Despite these many reservations, the Lévy potential in the form $V_2 + \lambda V_4$ (where λ is a parameter which permits one to simulate the radiative corrections to the fourth order interaction, and a repulsive core is included) permits a reasonable fit of the low energy data on the two-nucleon interaction. However, when the Lévy potential is used to calculate the two nucleon scattering at a moderately high energy like 150 Mev, the results are terrible. The Yale group and Gelernter here have made these calculations and found that the pp cross section was strongly peaked in the forward direction at 150 Mev, contrary to experiment. It was soon realized that the strong forward peak in the pp cross section is a consequence of the short ranged fourth order central potential. However, when the polarization is calculated for pp scattering, without the fourth order potential, one finds that the polarization is not only much too small but of the wrong sign as well.

Enough work was done by the Yale group and by Gelernter to show that varying the parameters in the Lévy potential would not yield any reasonable agreement with the experimental data for pp and np scattering at high energies. An independent phenomenological investigation of the two nucleon interaction by Gammel, Christian and Thaler showed that no charge-independent combination of central and tensor potentials with a Yukawa shape and with a hard core would fit all of the experimental data up to 300 Mev. They arbitrarily varied the depths, ranges and core radii of this mixture as well as the spin and isotopic spin dependence. A very severe limitation on these potentials was provided by the polarization effects in pp scattering.

As we have pointed out, the term which is very damaging to the Levy potential at high energies is V_4. This term arises from the dominant S-wave term in the pion-nucleon interaction in the Foldy-Dyson transformed Hamiltonian, namely ϕ^2 . It is well known that this term predicts the wrong sign for the isotopic spin 1/2 S-phase shift in pion-nucleon scattering and, in a manner which I believe is not yet completely understood, is somehow suppressed by higher order terms. Indeed, Klein has shown a connection between the experimental fact that the S-wave in pion-nucleon scattering is small and the need to suppress the fourth order term in the Lévy potential. It would therefore seem reasonable to restrict oneself to the P-wave interaction between the pion and the nucleon in order to ascertain the dominant features of various phenomena involving pions and nucleons at moderately high energies. This has been the approach of Chew and Low with the well-known successes for pion-nucleon scattering and photopion production up to several hundred Mev.

Two years ago, Gartenhaus examined the consequences of Chew's procedure for the two-nucleon interaction. Assuming P-wave coupling between pions and nucleons and an extended source for the nucleons, Gartenhaus calculated the two-nucleon potential up to fourth order in the coupling constant. He found that the resulting potential at large distances is similar to the static potential previously derived on the basis of the Brueckner-Watson version of the ps (pv) theory first calculated by Taketani and his co-workers in Japan.



I won't go into the subtleties of the diagrams which are considered by the two groups. The essential point was that the diagram which the Taketani group took into account, doesn't give a bound state for the deuteron. Brueckner and Watson gave some arguments why this diagram should be eliminated, and obtained binding for the deuteron. Gartenhaus essentially omits this diagram, and his potential from the extended source model goes over into the Brueckner-Watson potential at large distances. At small distances, however, it turns out that the tensor potential approaches zero and the central potential in even states is strongly repulsive. The Gartenhaus potential turns out to give quite a good fit to all the low energy (below 10 Mev) two-nucleon data with the same values for the renormalized coupling constant (.089) and cut-off energy (6 μ) which are required to fit the pion-nucleon scattering and photopion production. However, just as in the case of the Lévy potential, the Gartenhaus potential fails conspicuously when an attempt is made to match the experimental data on pp and np scattering at energies like 100 and 150 Mev. The dashed curves in Figures 1 to 10 show the poor fit of the Gartenhaus potential. I believe that other meson-theoretic two-nucleon potentials which have given reasonable fits of the low energy data, but which contain combinations of central and tensor forces, will not match the existing experimental data up to 150 Mev.

One may argue, as many of us did a decade ago, that the meson theory of nuclear forces leads one to expect a breakdown of the potential description of the two-nucleon interaction when one attains energies in excess of 100 Mev, and that a strong velocity dependence of the nuclear force is actually implied by meson theory itself. Since we know so little about the nature of this velocity dependence, it is perhaps reasonable to conjecture that the bulk of this velocity dependence would be embodied in a spin-orbit type of two-nucleon interaction, at least up to an energy of something like 150 Mev. This hope is certainly nurtured by the success of the shell model for complex nuclei. Indeed, Elliott and Lane have shown that an intrinsically attractive spin-orbit interaction between two nucleons will explain the apparent increase in the strength of the single particle splittings, from 3 Mev to 6 Mev, as one proceeds up the P shell. Moreover, Fermi and others have shown that a spin-orbit interaction between one nucleon and the nucleus will explain many of the features of nucleon polarization by complex nuclei at high energies. It is to be noted that the spin-orbit force required by Fermi and others is much stronger (by a factor of about 15) than the Thomas spin-orbit force which is normally expected.

The most detailed examination of the role of the spin-orbit interaction in explaining the high energy scattering data (in particular, single pp scattering) was carried out by Case and Pais several years ago. They showed on the basis of Born approximation that a strongly singular short-tailed spin orbit potential would give a triplet contribution to the differential pp cross section which is peaked at 90° (and thereby balances the contrary tendency of the singlet contribution of the central potential). At the same time, the spin-orbit contribution would be rather energy insensitive at the higher energies (above 100 Mev). These features would help to explain the rather constant isotropic pp cross section from 100 to 300 Mev. Unfortunately, more accurate calculations by Goldfarb and Feldman demonstrated that a mixture of central plus spin-orbit interaction was incapable of explaining all of the high energy scattering data.

Before I turn to the latest attempt to derive a satisfactory two-nucleon potential, I should like to mention two other types of attack which have been helpful, albeit inconclusive. Wolfenstein has analysed the single, double and triple scattering data at high energies, and has derived upper and lower bounds on the amount of spin-orbit and tensor contributions to the scattering matrix. He finds that the spin-orbit term accounts for 35% to 70% of pp scattering at 300 Mev and at 90^{o}, whereas the tensor term accounts for only 2% to 20%. These limits could be translated into corresponding limits on the spin-orbit and tensor interactions if the Born approximation were valid. The very existence of polarization effects implies the breakdown of the Born approximation and one may therefore not necessarily conclude that the spin-orbit part is more important than the tensor part of the two-nucleon force. The second method of attack to which reference has been made is the attempt to derive a satisfactory set of phase shifts which will match all of the scattering data at a given energy and lead to sensible variations of the phase shifts as a function of energy. This program has been pushed by many groups, by the Italian group under Clementel, by Ohnuma and Feldman and by Stapp, Ypsilantis and Metropolis. This approach is very laborious and has thus far not yielded unique results. If one can draw one general conclusion from the many acceptable phase shift solutions which have been derived, it is that neither the spin-orbit force nor the tensor force alone suffices in triplet states.

It was clear therefore that the next step was to investigate the possibility of explaining all of the two-nucleon scattering data on the basis of a mixture of central, tensor and spin orbit forces. I know that many groups (among them the Yale group) have thought of this next step and I believe that the Los Alamos group has obtained some very interesting results about which Dr. Gammel will report at this session. I should like to take the remaining time to tell you of some results which Mr. Signell and I have obtained. To be completely honest, I suppose that we should label our attempt as phenomenological. However, I believe that the fact that we achieved rather striking agreement with experiment with our first choice of parameters probably means that our potential possesses a somewhat deeper significance. At least, I shall try to make it appear that this is the case.

It occurred to us that if one must work with a mixture of central, tensor and spin-orbit forces, then a reasonable charge-independent potential to take is:

$$V = V_G(r) + \underline{L}\cdot\underline{S}\; V_0\, x^{-1}\frac{d}{dx}\left(e^{-x}x^{-1}\right) \quad , \quad r > r_c ,$$

$$= V_G(r) + \underline{L}\cdot\underline{S}\; V_0\, x_c^{-1}\left[\frac{d}{dx}\left(e^{-x}x^{-1}\right)\right]_{x=x_c} \quad , \quad r \leq r_c ,$$

$$x = r/r_0 \quad , \quad x_c = r_c/r_0 \quad , \quad V_0 > 0 \quad ,$$

where V_G is the Gartenhaus potential and V_0, r_c and r_0 are parameters characterizing the spin orbit potential. We chose the Gartenhaus potential as the central plus tensor part of the two-nucleon force because it appears to have the most plausible meson-theoretic basis and because it fits the low energy data so well. Since the spin orbit term vanishes in S-states and is repulsive in the $3D_1$ state, the combined Gartenhaus and spin orbit potential should also give a good fit of the low energy data. The spin orbit term is chosen as intrinsically negative to accord with the shell model (because Elliott and Lane have shown that this is to be preferred insofar as the single particle P shell splittings in complex nuclei are concerned). As regards the radial dependence of the singular spin orbit force, we chose the Thomas - Yukawa form (first suggested by Case and Pais) with the parameters taken from the work of Goldfarb and Feldman; these parameters are $V_0 =$ 30 Mev, $r_c \simeq 0.21 \times 10^{-13}$ cm $= 1/M$, $r_0 = 1.07 \times 10^{-13}$ cm. One important difference is that we replaced the zero cut-off of Goldfarb and Feldman by a straight cut-off, which we think is more reasonable. Moreover, we have chosen a non-isotopic spin dependence of the spin orbit force. In general, you could still multiply V_0 by $A + B\, \tau_1 \cdot \tau_2$.

With these arguments to bolster our spirits, and Gammel's tabulation of the Gartenhaus potential to feed into our IBM 650 program, calculations were first made of the pp and np scattering cross sections at 150 Mev with the results shown in Figures 1 - 4.

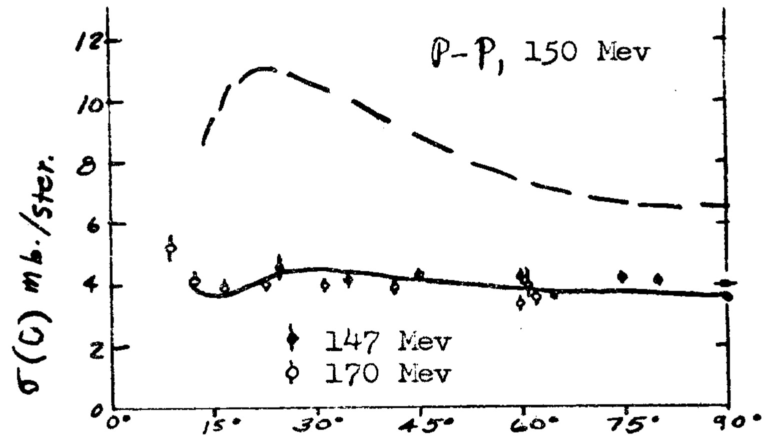


Fig. 1

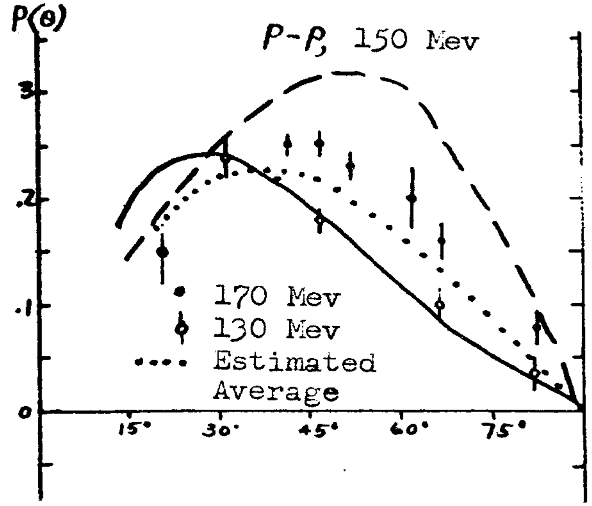


Fig. 2

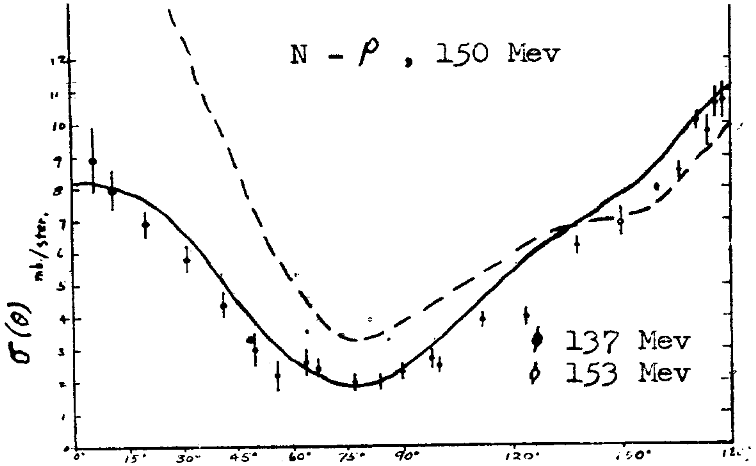


Fig. 3

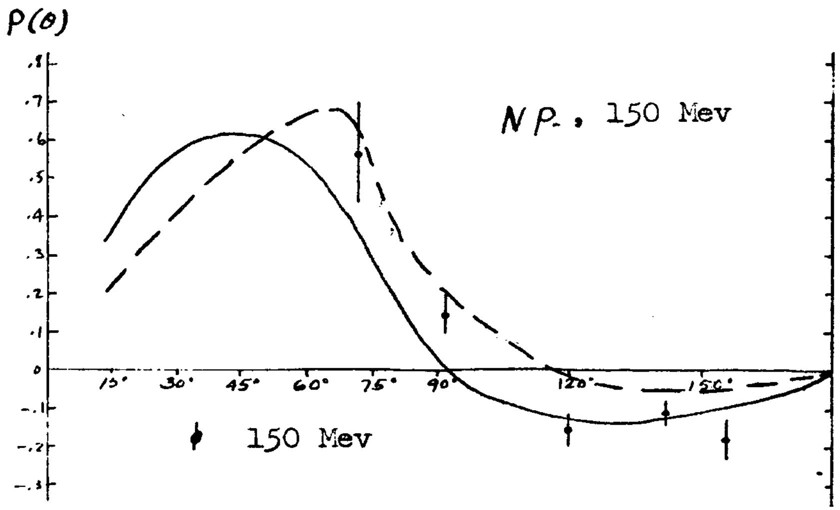


Fig. 4

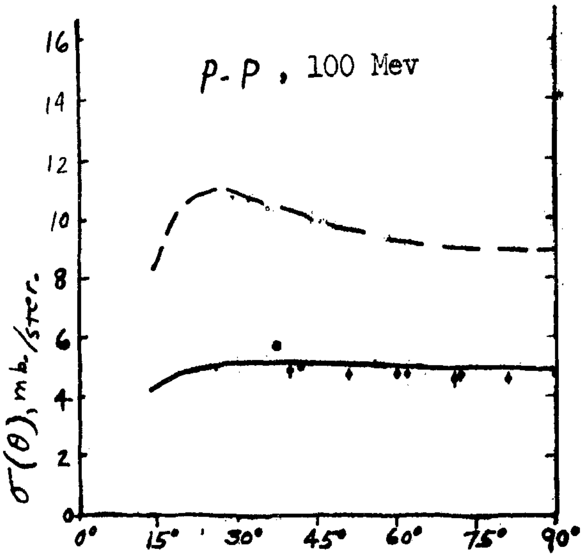


Fig. 5

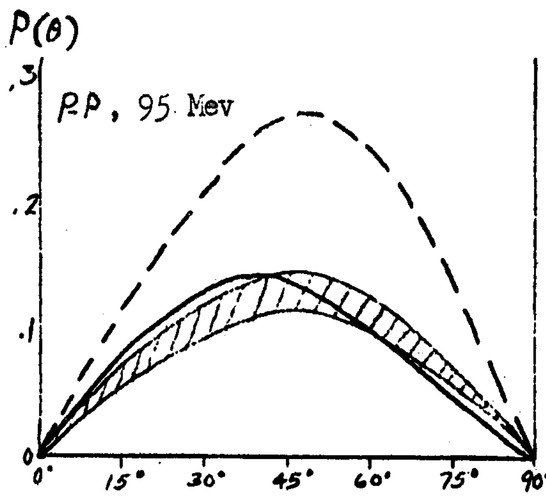


Fig. 6

Fig. 1 shows the pp scattering at 150 Mev. The solid line is always our prediction, the dashed line that of the Gartenhaus potential; the points are experimental. Fig. 2 shows the pp polarization. If the sign of Vo is changed, the polarization comes out with the wrong sign. Fig. 3 shows np scattering, and Fig. 4, the np polarization at 150 Mev.

These results looked promising, and so the cross sections were calculated at lower energies (including the Coulomb amplitude in the pp calculations). At 95 Mev we were fortunate to have some new unpublished pp and np polarization measurements of the Harwell group and at 40 Mev the new and very accurate pp scattering measurements of the Minnesota group (unpublished). The results are shown in Figures 5 - 11.

Fig. 5 shows pp scattering at 100 Mev, Fig. 6, pp polarization at 95 Mev, Fig. 7, np scattering at 90 Mev, and Fig. 8, np polarization at 95 Mev. Ours are still the solid curves, the Gartenhaus potential gives the dashed lines, and the points are experimental.

Fig. 9 shows the np scattering cross section at 40 Mev. Fig. 10, showing the pp scattering at 40 Mev, includes the recent data of the Minnesota group. They have a 1% accuracy, much better, of course, than we are trying to achieve at this point. Fig. 11 shows the np scattering at 18 Mev.

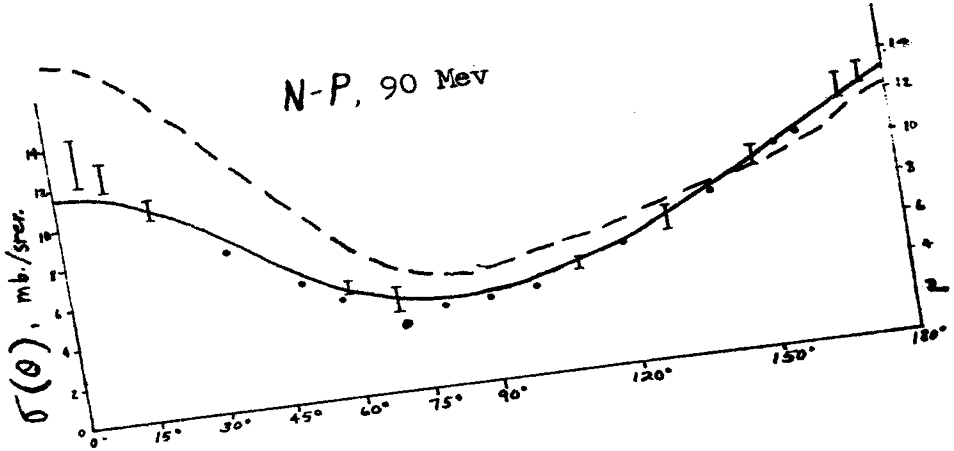


Fig. 7

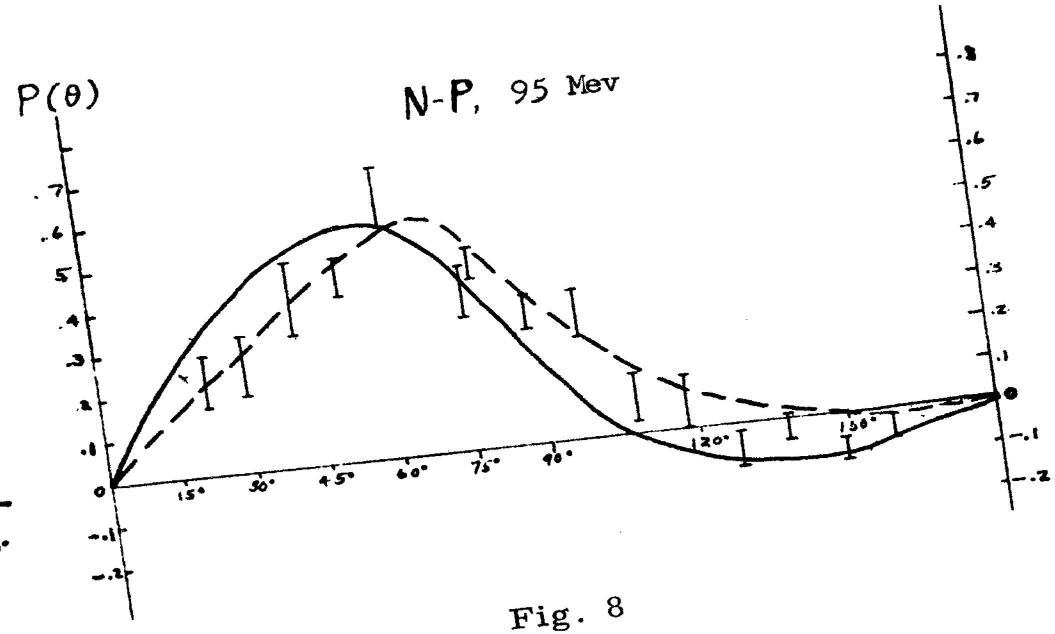


Fig. 8

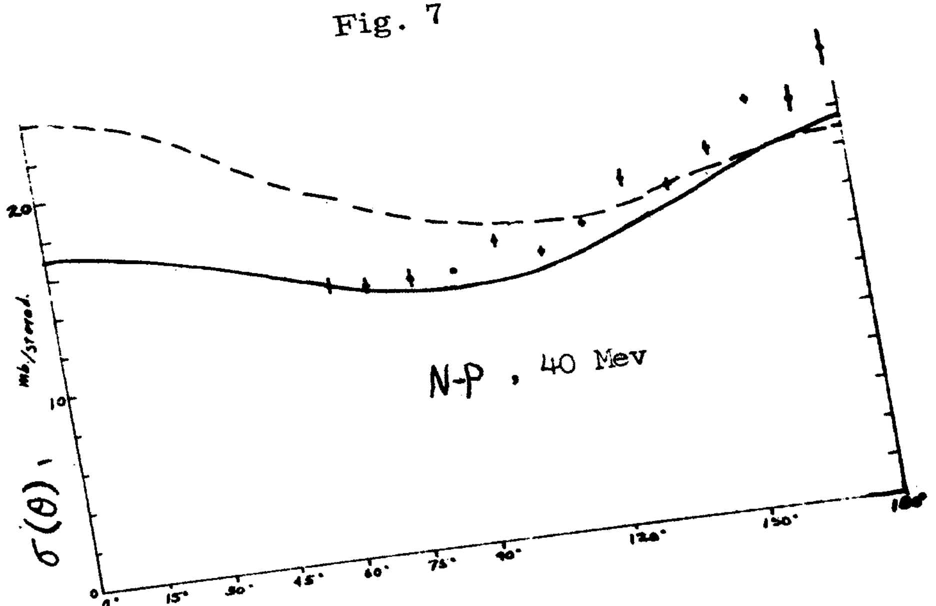


Fig. 9

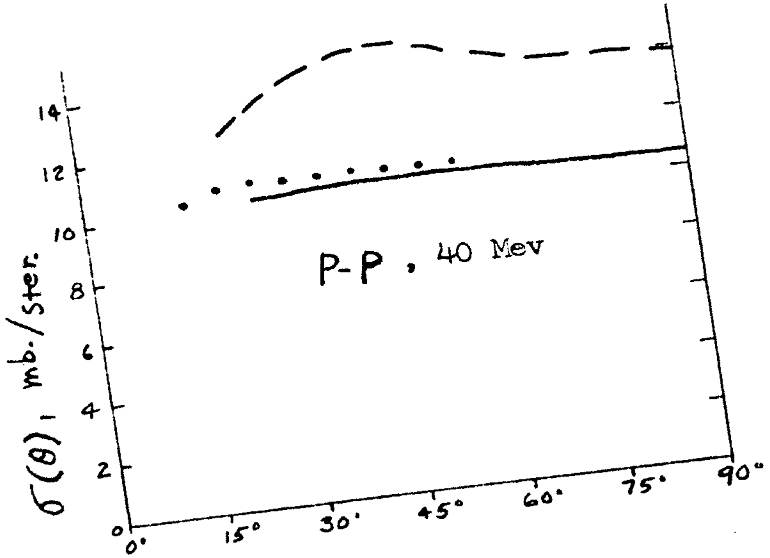


Fig. 10

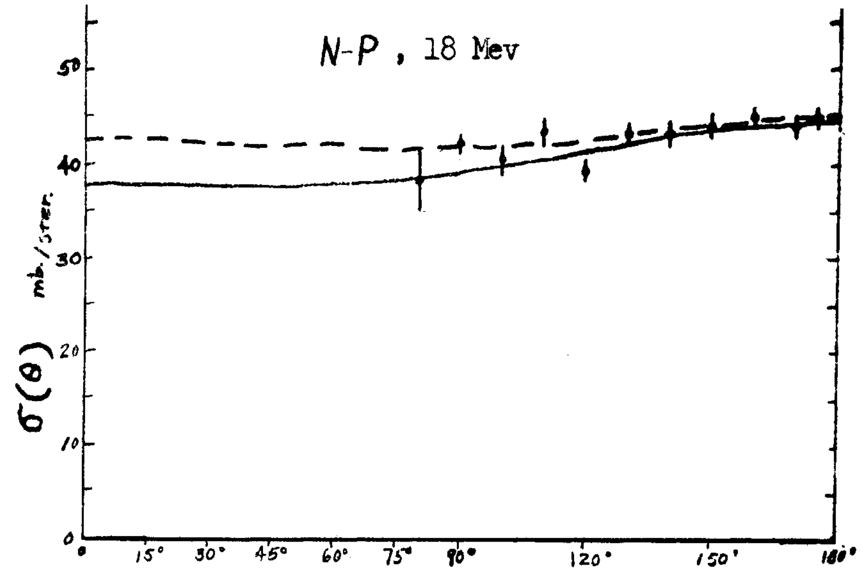


Fig. 11

The agreement between our potential and experiment is not perfect by any means but it seems clear that the Gartenhaus potential plus spin orbit force with the sign needed for the shell model gives a better fit of the pp and np scattering data up to 150 Mev than any potential previously considered.

We have tried to analyze some of the reasons for the great improvement of our potential over the Gartenhaus potential. Perhaps a comparison of the phase shifts predicted by our potential and the Gartenhaus potential will be helpful. Tables 1 - 2 show the energy dependence of the phase shifts for our potential.

Table 1 shows our singlet phase shifts. You notice the decrease in the singlet S-phase shift because the repulsive core is taking over more and more. Concerning the singlet P-phase shift I want to remark that the Gartenhaus potential in the singlet odd parity states is repulsive at large distances and then comes down to a rather deep well. Gammel and Thaler have shown that as a result the singlet P system has a bound state. However, if you just take the repulsive part and set the potential equal to zero where it becomes negative, then you get the same phase shifts to within 1^o or 2^o. So the experimental agreement remains without a $'P_1$ bound state.

Table 1

Singlet phase shifts in degrees

	$^1S_0$	$^1P_1$	$^1D_2$	$^1F_3$	$^1G_4$
18 Mev:	51.4	-4.8	0.2		
40 Mev:	42.9	-10.5	1.0	-0.3	
100 Mev:	28.1	-18.5	3.8	-2.4	
150 Mev:	19.4	-22.1	5.9	-4.0	0.5

Table 2

Triplet phase shifts in degrees

	$^3S_1$	$^3P_0$	$^3P_1$	$^3P_2$	$^3D_1$	$^3D_2$
18 Mev:	74.4	3.1	-2.5	1.5	-1.4	1.0
40 Mev:	57.5	8.5	-6.3	4.6	-5.5	5.3
100 Mev:	34.5	15.8	-14.0	9.8	-16.8	19.3
150 Mev:	22.9	15.6	-17.2	11.5	-23.3	25.9

	$^3D_3$	$^3F_2$	$^3F_3$	$^3F_4$	$^3G_3$	$^3H_4$
18 Mev:	0.1					
40 Mev:	0.9	-0.2	-0.0	0.1	-0.3	
100 Mev:	6.1	-1.5	-0.7	1.1	-3.2	
150 Mev:	10.0	-2.8	-1.6	2.6	-5.6	-0.2

Table 3

Triplet phase shifts in degrees (150 Mev)

	$^3S_1$	$^3P_0$	$^3P_1$	$^3P_2$	$^3D_1$	$^3D_2$
G.S.	22.9	15.6	-17.2	11.5	-23.3	25.9
G.	29.7	57.3	-5.6	21.0	-20.7	31.3
	$^3D_3$	$^3F_2$	$^3F_3$	$^3F_4$	$^3G_4$	$^3H_4$
G.S.	10.0	-2.8	-1.6	2.6	-5.6	-0.2
G.	6.9	-0.2	-1.0	0.7	-6.7	-0.0

G.S. = Gartenhaus and spin-orbit

G. = Gartenhaus

Table 2 shows our triplet phase shifts. One trouble with some phase shifts, such as those of Lomon and Feshbach for example, is that the $^3P_0$ phase shifts are too large and can give a bound state. Our spin orbit potential is intrinsically attractive, and hence attractive in the $^3P_2$ state and repulsive in the $^3P_0$ state. This leads to a decrease of the $^3P_0$ phase shifts compared to the Gartenhaus potential. Previous analyses had also shown that opposite signs for the $^3P_2$ and the $^3F_2$ phase shifts are extremely helpful for an understanding of the pp polarization curves.

Table 3 shows a comparison of the triplet phase shifts at 150 Mev, between ours and the Gartenhaus potential. Notice how large the $^3P_0$ is for the Gartenhaus potential, and the repulsive spin orbit force helps.

It is possible, of course, that the overall agreement between the predictions based on our potential and the wide range of available scattering data up to 150 Mev is a sheer accident. I do not believe that this is the case. I believe that the evidence is very strong that a mixture of central, tensor and spin-orbit force is needed to explain the experimental data on the two-nucleon interaction up to 150 Mev. Indeed, I believe that the agreement between experiment and the predictions of our potential is sufficiently encouraging to justify the hope that our potential contains many of the essential features of such a potential model.

In conclusion, I should like to make some very brief remarks concerning the meson-theoretic status of the spin-orbit interaction. It was already shown by Lévy and Klein that the ps (ps) theory predicts a very singular spin-orbit force in fourth order; this spin-orbit force is actually nothing more than

$$V_{s.o.} = \frac{\underline{L}\cdot\underline{S}}{2M^2} \frac{1}{r} \frac{d}{dr} V_4 ,$$

where V_4 is the fourth order short range central force of the Lévy potential. This spin-orbit force is intrinsically repulsive (and therefore of the wrong sing for the shell model) and moreover has a very dubious status because it is a consequence of the S-wave pion-nucleon interactions. More recently, Okubo at Rochester has looked into the meson-theoretic basis for a spin-orbit interaction between two nucleons. He has computed the fourth order spin-orbit potential which is predicted by an extension of the Gartenhaus method taking into account the nucleon recoil, through the condition of Galilean invariance. The spin-orbit potential turns out to be attractive, provided he takes into account the fourth order diagram omitted by Brueckner and Watson (and by Gartenhaus) in computing the central and tensor forces; otherwise, the fourth order meson-theoretic spin-orbit potential is still repulsive. Okubo has also looked into the possibility of deriving a spin-orbit interaction by taking account of K mesons in the intermediate state. Unfortunately, if one restricts oneself to fourth order, the K mesons do not contribute to the spin-orbit potential. It is evident therefore that meson field theory gives only the vaguest possible hint of the existence of a strong short range attractive spin-orbit two-nucleon interaction which seems necessary to explain the experimental results.

<u>THALER</u>: Phenomenological potentials

Everything I have to say represents joint work with John Gammel.

I think that I would like to go directly to the very simple argument that can be made to show that a spin-orbit force must exist in the two nucleon interactions. This argument can be made most rigid, with least room for wiggling out, if one considers the

310 Mev pp phase shift analyses which were done by the Berkeley group, Stapp and others (Phys. Rev. 105, 302, (1957)). This shows, I think, unequivocally, that there must be a spin orbit force, at least at high energies, or if not a spin oribt force then some other interaction which splits the P-wave phase shifts in a different way from the tensor force.

The 310 Mev phase shift analyses led to 5 independent acceptable phase shift solutions. These are the solutions labeled 1 through 4 and 6 by Stapp. None of these solutions for the triplet P-waves can be achieved in Born approximation with a pure tensor force or a pure spin orbit force. The evidence is extraordinarily clear. I would like to point out which of these solutions we think is believable. Once we have that, the job of getting all the data with an interaction becomes rather straightforward. Then we'll know the effective range expansion at low energies, we'll know all the phases at high energies and we can't do too much wrong in between. This is essentially the point of view that I would like to discuss.

The solution numbered 6 is unacceptable for reasons that will be discussedby Professor Bethe. Then we can talk about the first four solutions of Stapp. Solutions 2 and 4 are, I think completely ununderstandable from the point of view of an interaction in potential form because the D-wave phase shifts which these solutions predict are much too small to go with the S-wave which is required. This argument, I think, can be made very precise. We have some considerations in press which point this out. This leaves us with essentially two of the Stapp solutions, namely solutions 1 and 3. We believe that their solutions 3 cannot be fitted with a potential for a somewhat technical reason. Namely, if one tries to find a potential interaction which gives reasonable fits to the P-waves required by solution 3, one finds that one can never reproduce the required coupling between the $^3P_2$ and $^3F_2$ phase shifts. We have therefore ruled out solution 3 on the grounds that it is not conceivable from a potential point of view. This leaves us with solution 1 of Stapp which, anyway, is the best overall fit to the data. One cannot, however, rely on the root mean square fit to the data at present.

We might also mention that here is a very good place where experiments can settle the question, because if one looks at the solutions that are given by these sets of phase shifts one sees that simple scattering experiments with large angles of scattering can distinguish between the various solutions.

If we look at solution 1, the $^1S_0$ and $^1D_2$ phase shifts predicted correspond closely to one of the singlet even parity potentials discussed in a paper by us which appeared in Phys. Rev. 105, 311 (1957). That is a potential with a core of .4 fermis, perfectly rigid, with an attractive Yukawa tail of range .69 fermis and depth 425 Mev, adjusted to fit the effective range expansion. This, we assume, tells us what we need to know about the singlet even parity interaction to begin our analysis.

Now that we have a unique set of phase shifts, we can make unique remarks based on the order of the triplet P-phase shifts at 310 Mev. We immediately see that there is a necessity for both a spin orbit and a tensor interaction. The $3P_2$ is largest, the $3P_0$ is in the middle, and the $3P_1$ phase shift is smallest in Born approximation. This can only be achieved by a combination of tensor and spin orbit forces. This Born approximation argument is a qualitatively good one if one makes exact calculations.

On the other hand, one can now talk about the F-wave from solution 1 of Stapp, and there we see that the order of the F-waves is such that no spin orbit interaction is required. Central force alone can account for the order of the F-wave phase shift at 310 Mev. This makes it very clear that if one puts in a spin orbit force, it must have sufficiently short range so that it has an appreciable effect on D-waves and has no effect on the F-waves, which correspond to much larger impact parameters. So arguing just from the high energy data, we know at least the ball park in which we want to find ourselves; namely, we want to use an extremely short range spin orbit interaction tacked on to a tensor force.

In order for a potential to mean anything, the same analysis must hold at low energy. Using the phase shifts which fit everything from zero to 10 Mev., one then makes the phase shift analysis at 18 Mev for the P-waves alone. There are only two acceptable solutions, and these solutions for the phase shifts can be achieved with a pure tensor force. One solution requires a tensor force which is attractive in the $3P_0$ state; the other would be given by tensor forces repulsive in the $3P_0$ state. The attraction in the $3P_0$ state for the tensor force is required by the high energy 310 Mev data, so we can eliminate the extraneous solution as not being in accord with the kind of energy dependence we are looking for in our phase shifts. Thus we find that the sign of the spin orbit force is fixed and it must have an extremely short range. Incidentally, the sign agrees with Marshak's guess, which is in qualitative agreement with the spin orbit force in heavy nuclei. One comes to the conclusion that the tensor force is long ranged and attractive in the $3P_0$ state.

If one specializes the potentials to have the shape of a rigid hard core and a Yukawa shape outside, one is very quickly and very definitely led to a unique best fit potential. This unique best fit potential has, in the triplet odd parity state, likewise a core of about .4 fermi. The tensor force in the $3P_0$ state has a depth of about 22 Mev for the Yukawa shape with a range of about 5/4 fermi. The spin orbit force has a repulsive depth of more than 7, 000 inverse fermis, but that 7, 000 is not a terribly meaningful number, because it goes with a range of .27 fermis and core size of .4 fermi.

In a moment I'll show you how good this potential is, but I would like to say at this point that what we have done is about as far as one can get with the assumption of a given shape of interaction. One is left then with the problem of giving meaning to the term "static potential". Let us admit the possibility that a spin orbit force multiplied by a potential can be talked about as "static". The potential usually identified as velocity dependence can easily, from a phenomenological point of view, be thought about in terms of the shape of the interaction. Someone tells me that a potential is very strong at low energies and very weak at high energies; then I can say that this really means that it has a very long tail and it has a piece cut out of the middle. This is exactly equivalent to saying that it has a certain velocity dependence. We have adopted the second point of view, namely, that we are not looking for velocity dependences and will blame all such effects on shape. We also want to make the point that this qualitative argument is independent of the precise shape that we used for the interaction.

Fig. 12 is the pp angular distribution. At 70 Mev we first considered the effect of the shape. The solid curve represents the Yukawa shape and now we experimented with a tensor force with a hole in the middle, because the results seemed to indicate that we need a somewhat weaker tensor force at high energies. So the dashed curve represents a tensor force which is Yukawa at long distances and goes to zero at the core. Both the data and the potential model give indistinguishable results between 170 and 310 Mev. The solid curve is peaked forward. When one changes the tensor force to fix things up, one gets the dashed curve. If, at 310 Mev , we put in the Stapp singlet phase shift, instead of our phase shift, we obtain the dash-dot curve.

Fig. 13 represents the polarization in pp scattering at 310 and 170 Mev. The solid curve is again what one gets from a pure Yukawa shape, the dashed curves what one gets from the tensor force with a hole in the middle, and the dash-dotted curve is the result of using the Stapp singlet phase shifts, instead of our own. It would be a little more comforting if the curve at 170 Mev had a little more tilt to it. The new data at 140 Mev indicate that this tilt is possibly quite meaningful. We fail to reproduce it in detail, but we say again that this is due to our singlet force not being quite right, and also due to the fact that we haven't got the shape quite adjusted to give us the best over-all fit.

In Fig. 14 we plot the polarization divided by $\sin\theta \cos\theta$ at 45 degrees as a function of energy, as predicted by our interaction. It fits absolutely at 310 Mev because it was so concocted, and

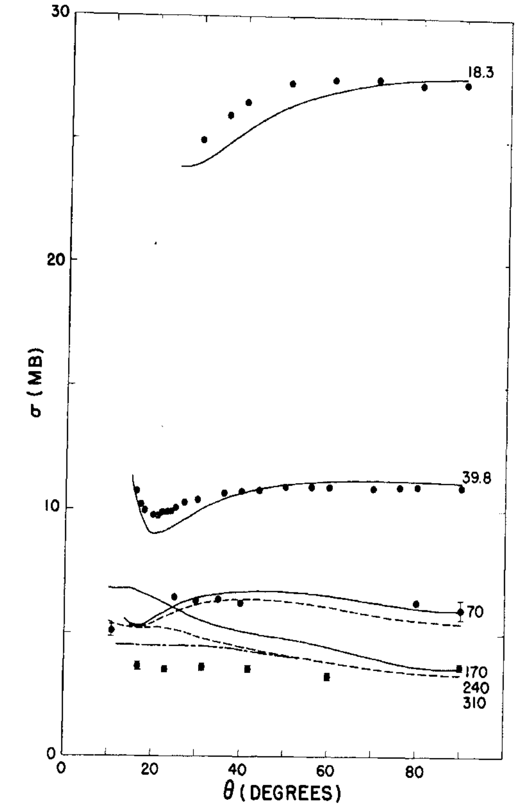


Fig. 12

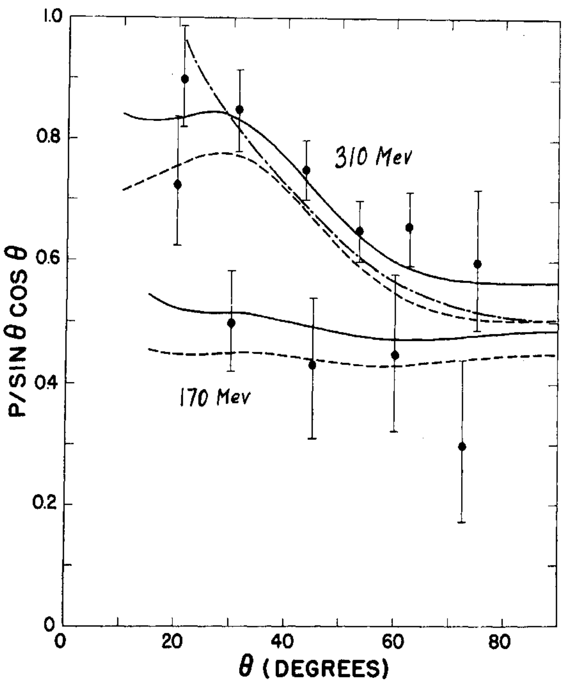


Fig. 13

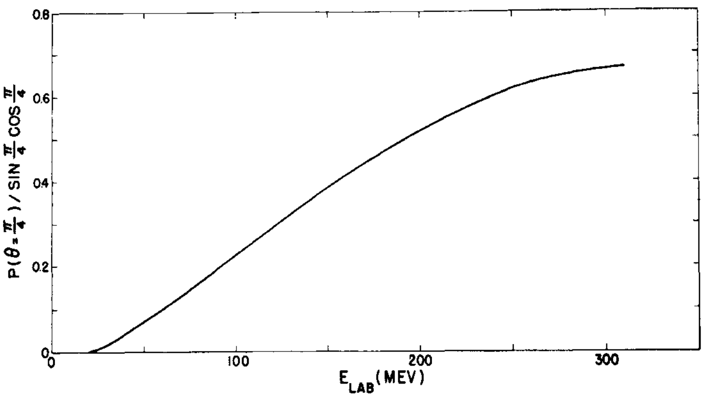


Fig. 14

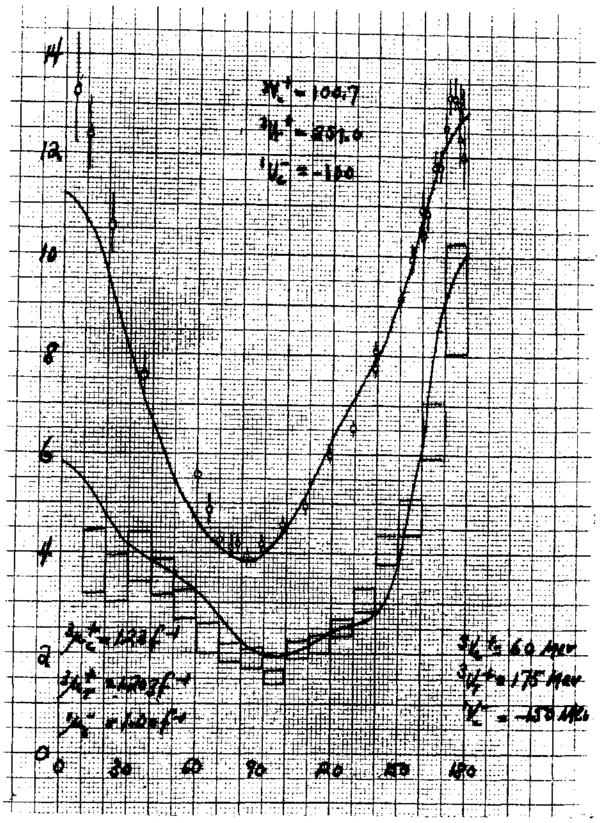


Fig. 15

agrees with Stafford's results at 100 Mev. His 140 Mev data would fall also on that curve if there were no $\cos^2\theta$ terms. We do not predict a large enough $\cos^2\theta$ term in the polarization at 140 Mev, but again agree around 170 Mev. The slight discrepancy in this region, we repeat, has to do with shapes of potentials. We have also looked at pp scattering and the question is, can one do anything about charge independence? On Figures 15 and 16 you see from our first indications that things are

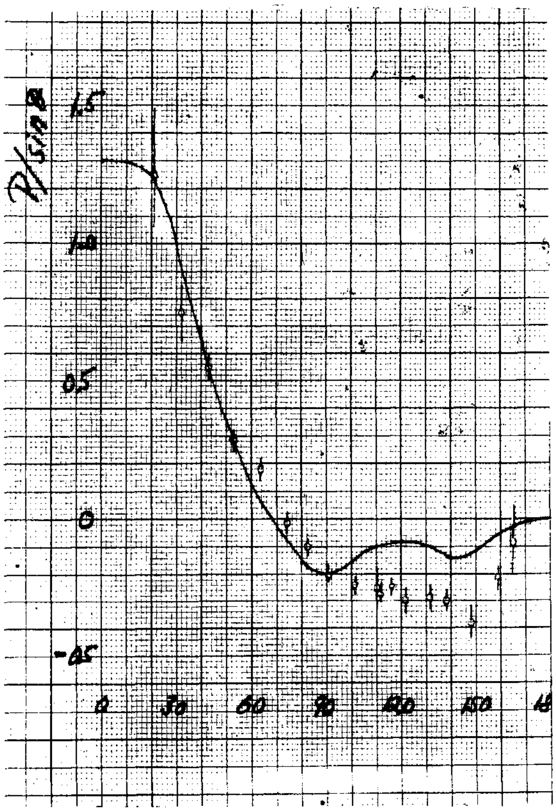


Fig. 16

reasonably good. The lower curve of Fig. 15 represents the 310 Mev np data and our preliminary fit.

Fig. 15 also shows our fit at 90 Mev. This again requires a very short range spin orbit force, of about the same range that we discussed for the pp scattering in the triplet state. In Fig. 16 we see the polarization at 310 Mev. This comes out all right too. Again we find that we need a somewhat stronger tensor force at the lower energy than at the higher energy, which will come out all right when we put into the theory a tensor force with a hole in it, which we have not yet done.

DISCUSSION

BRUECKNER: Has the effect of spin orbit coupling in the even states been determined in these calculations?

THALER: That's the reason I say these things are extremely tentative. It's clear that the spin orbit forces make contributions only to the D state and not to the S state. We haven't looked closely at this. These curves were only a few hours old when we left, but we think that we will have something to say about this very shortly. We believe that we won't get into any serious trouble, but we really don't know.

BRUECKNER: The meson theory as calculated by Taketani, by us, and by Gartenhaus gives different answers which unfortunately depend somewhat on the approximations made in computing the potentials. The diagram Marshak indicated, we, and also Gartenhaus, have thrown away. The disregarding of these terms is not agreed to by Taketani and his co-workers. Because of this, one can't make any unambiguous statement. However, if one does make our approximation, and there are reasonably believable grounds for arguing that this is correct, then one finds that the meson theory makes very characteristic predictions about which forces have a long range, which forces are attractive, and which are repulsive. The characteristic feature of the meson potentials is that the longest range force is a tensor force, because this is the dominant contribution to the second order interaction and therefore is the only part of the force that has the range of the meson Compton wave length. The short range forces from the fourth order are dominantly central forces and the theory predicts a certain assignment of signs to these forces. In other words, it predicts a certain assignment of spin and isotopic spin dependence. I understand that the Gammel-Thaler potential and, of course, the Gartenhaus potential, both agree in that the assignment of signs and ranges which fits the data is also that given by the meson theory. In a sense, this is an extremely nice confirmation of the qualitative features of the meson predictions. It is certainly true, however, that the meson theory does not make a quantitative prediction. This is particularly true since, as Dr. Klein has recently shown, the second and fourth order potentials are strongly modified in magnitude by higher order effects.

MARSHAK: Let me answer one of Dr. Brueckner's questions, with reference to the deuteron. The deuteron is badly needed for the pp scattering in the triplet odd state, where the isotopic spin is one. So you could put in an isotopic spin dependence, for example, just to make the spin orbit force zero in the $T = 0$ state. Actually, some work which has been done on the isotopic spin dependence shows that you don't alter the np data very much by just throwing away the spin orbit force in the triplet even states. This is one source of hope. That is why we haven't been very much upset about the deuteron.

KLEIN: I would like to point out for the record that there is a new fundamental approach to the problem of deriving meson theoretical potentials that has been developed by McCormick and myself and by Miyazawa. The basic idea is to derive a formula which makes maximal use of all the information which has been obtained from the pion-nucleon scattering. It would be premature to give a report here because we have not yet reached the point of confronting that potential with reality. However, we have gotten far enough to actually compute what we believe to be the qualitative character of the second and fourth order potential. The basic result was stated by Professor Brueckner, namely, that the effect of the resonance in pion-nucleon scattering gives large corrections to the central part of the Taketani, Machida, and Onuma, and Brueckner and Watson, and Gartenhaus potentials.

MARSHAK: In what region are the corrections?

KLEIN: They are corrections to the major part of the fourth order central potential. The second order tensor force is not affected. The central force is seriously modified.

BETHE: Do you know anything about the spin orbit force?

KLEIN: The program so far does not include examination of the spin orbit force.

BETHE: Well, I hope that you will tell more about this in the theoretical session. (See session IV).

BREIT: I would like to make a few observations regarding these two papers. In the first place, the investigation of phase shifts is far from unique. Until this meeting, it was generally agreed that Stapp's solutions might not be the only solutions. At this conference last year, that was the general opinion and the solutions were about the same as they are now. At present, the opinion of the Los Alamos group is different, in the sense that they consider that Stapp has exhausted this very many-dimensional space. I think this point is an open one, judging from the experience of a smaller space involved in pion-nucleon scattering. The other thing I find appropriate to remark is that the spin orbit potential is not a theoretical potential as it is put in. It is phenomenological, in the nature of a parameter. Secondly, all of the potentials are separate; that is, both the tensor and the spin orbit potential may have velocity dependencies in them that are different from the velocity dependence of the central potential. In fact, if one takes a sixteen component equation that would approximate the ps theory taken statically without retardation, then boils it down to a four component equation; one gets a spin orbit and a tensor force which are velocity dependent. Therefore, if one uses a potential to fit the data involving the spin orbit dependence and gets agreement, it is not clear that that agreement is more than a parameter fit. That being so, just what the results mean is not too clear.

R.G. SACHS: I would just like to call attention again to the fact that if one believes in such velocity dependent phenomenological potentials, then the magnetic properties of the system are affected. In particular, I wonder whether anybody has looked at the magnetic moment of the deuteron.

MARSHAK: As I pointed out, in the deuteron there may be no spin orbit force. I would also like to point out that there is a limit to the energy at which one can reasonably expect a static potential to be meaningful. 300 Mev is perhaps already a little high. We went to 150 Mev because we felt that that would be sufficient for interesting applications to complex nuclei.

GOLDBERGER: You will probably not be surprised to learn that there is also a dispersion relation approach to the problem of the two nucleon potential. Work has been carried out by Nambu, Oehme, and myself. This work is in a preliminary stage. There will be a few remarks on this by Dr. Oehme in Session IV.

CHAMBERLAIN: Could I respond a little bit to one of Professor Breit's remarks? The Stapp, Ypsilantis, and Metropolis method of seeking out solutions and trying to decide when they had found what they hoped would be all the usable solutions, was to require that the machine be asked to use random phase shifts and then work from those to better and better fits. It stopped when it reached the condition that each phase shift set had been found at least three times, and there were no solutions which had been found less than three times. The hope was that, to the extent to which all of these solutions could be thought of as wells of more or less equal depth, which is a very questionable assumption, one would have a very good probability of not overlooking solutions. As far as I know it is very difficult to be exhaustive on this point. One simply tries more and hopes to find more solutions.

BREIT: When you speak of wells do you mean a well in the surface of the function that represents the sum of the squares of the deviations?

CHAMBERLAIN: Yes, I do.

BETHE: Selection of phase shifts

One of the questions which has been raised is the phase shift solution. While Chamberlain has given good arguments why Stapp probably has found most of the important solutions, there is still the question of how to select between these many solutions.

There are five acceptable solutions, as already mentioned by Thaler. These fall into two classes - one of which contains four solutions and the other contains one. This last solution is called solution 6 by Stapp and collaborators. It is characterized by the fact that it has a very large $^3P_0$ phase shift and all of the other phase shifts are relatively small. This characteristic is also contained in the attempt to explain the nuclear interaction by Lomon and Feshbach. If you will remember, they talked of boundary conditions for the wave function of two nucleons, at a certain moderate distance between the two nucleons, and their two solutions have the characteristic that the $^3P_0$ phase shift is large. If you look at the numbers you will find that the $^3P_0$ phase shift is pretty much the same as it is in the Stapp solution 6. Generally one looks on Stapp solution 6 as very similar to the Feshbach-Lomon boundary condition results.

The Stapp solutions were based on extremely extensive experiments from Berkeley. The Segre group obtained triple scattering results at all sorts of angles, in addition to polarization and differential cross sections. Only on the basis of such extensive experimental results was it possible to determine these phase shift solutions. Nevertheless, further experiments and results would be needed to discriminate between the five solutions. I tried to see what theory would say on the discrimination between the solutions.

The particular way which I chose was to compare the scattering from the nucleon with the scattering from a complex nucleus, for instance from carbon, the latter having been investigated experimentally by Segre's group at Berkeley. It is an old idea that the scattering from a nucleus should be obtainable by an optical model from the nucleon-nucleon scattering. In the case of polarization this has been particularly suggested by Watson with his collaborators and has been worked out also by Tamor, who first showed that relatively small polarization in nucleon-nucleon scattering can give rise to large polarization in the scattering of a nucleon by a nucleus. I should also mention the work of Fernbach and collaborators on this subject.

Now what I have tried to do was to make this connection quantitative. You have to do two things - you have to analyze the scattering by the nucleus, and then you have to compare it with the scattering by an individual nucleon. On the first point, namely the analysis of the scattering by the nucleus, one is helped greatly by an observation which was made independently by Kohler, by Levintov, by Gerry Brown and by myself, namely that the polarization of the scattering from the nucleus comes out exactly the same when you use Born approximation and when you use more elaborate methods. I'll describe what these more elaborate methods are. They are to take the potential between nucleus and nucleon, to calculate the phase shift using a WKB approximation, to assume that the potential is small compared to the energy, which is certainly very well justified, and then to calculate the scattering by summing over partial waves. This is exactly what Sternheimer and other people have done to analyze the scattering of the nucleon by the nucleus and it is, I think, well justified. You have to make only two further assumptions to make the results agree with Born approximation: namely A) you have to use small angles, smaller than the diffraction minimum in the elastic scattering (this is in order that the sum over partial waves can be replaced by an integral without important destructive interference) and B) you have to assume that the spin orbit force gives you contributions to the phase shift which are small enough so that you can neglect the third power of that contribution. Assumption (B) is very well justified. You can then directly analyze the experiments and determine both the central potential and the spin orbit potential. Born approximation is correct only for the polarization, not for the absolute cross section. The correction to the Born approximation can be made analytically once you have assumed some distribution of the nucleons in the nucleus.

My attempt in this theory was to do everything without even an IBM 650.

You have to do two things. One is - you have to get the form factor of the nucleus. In the analysis by Fernbach and collaborators one form factor was assumed for the nuclear scattering, and another for the Coulomb scattering. This is unnecessary. If you assume that the nucleons act more or less as point scatterers in both cases, then it is well known that the Coulomb scattering can be described by the same form factor as the nuclear scattering. Secondly, each part of the scattering amplitude, Coulomb and nuclear, has a complex phase and one has to compare these complex phases in order to determine the interference. It can be shown that these complex phases are pretty much the same at the angles of interest for the experiments, angles of about 3 to 7°.

When all this is done, I take the experiment, I calculate the form factor (in order to calculate that, I take the Stanford data from electron scattering for the distribution of nucleons in the nucleus, which in the case of carbon I'm sure is about the same for the protons and for the neutrons), and dividing by the calculated form factor I then get an absolute number for the cross section without the form factor. From this I can subtract the Coulomb scattering. What remains contains two parts - the nuclear scattering and the interference. The interference gives me essentially the real part of the potential, and the nuclear scattering gives me the sum of the squares of real and imaginary parts of the central potential. Then I correct for the Born approximation, and when all this is done, I get numbers. These numbers are as follows: the real part of the potential is <u>equivalent</u> to a scattering amplitude - not the actual scattering amplitude but that you would have if the Born approximation were valid - of about 3 fermi. The imaginary part is equivalent to a scattering amplitude of 12 fermi, and the spin orbit coupling gives a Born amplitude of 24 fermi times the angle in the laboratory system. That is:

$$u \simeq 3$$
$$w \simeq 12$$
$$s \simeq 24\,\theta$$

I am most interested in the last number. I can compare this last number with what I would obtain from the nucleon-nucleon scattering by translating it to the scattering on the nucleus. In making this translation one has to pay attention to the center-of-mass factor which was discussed particularly by Watson and Riesenfeld. This center-of-mass factor is essentially just the ratio of the wave numbers in the laboratory system and in the center-of-mass system. Then one can use the various phase shift solutions of Stapp et al. It should be noted that I do not use any model of nuclear forces here but merely phase shifts.

The result is that all the 5 acceptable solutions of Stapp give $s \simeq 30\ \theta$ within about 20%. This shows on the one hand that the scattering by a nucleus can be well represented as the sum of the scatterings by individual nucleons. On the other hand, the scattering and polarization by a complex nucleus cannot be used to select between the 5 otherwise acceptable solutions of Stapp et al.; this can only be done by further experiments, especially correlation of the polarization of the two nucleons emerging from a nucleon-nucleon collision.

Let me make two further remarks about the connection of this result with other results. First, the imaginary part of the potential can be used to calculate the total inelastic cross section of the carbon nucleus at this energy. It comes out to be 200 millibarns, which is the observed value. Second, the spin-orbit interaction is relatively small. It is the same as was found by Fernbach and collaborators, and about one-third of that proposed by Levintov analyzing the same data. There are additional arguments in favor of the smaller interaction.

(See Appendix for further comment by Bethe.)

STAFFORD: Nucleon-nucleon scattering and polarization experiments

I am going to give you a progress report on the nucleon-nucleon scattering results that were obtained at Harwell during the last year. Let me remind you that the Harwell machine has a maximum energy of 175 Mev and an external proton energy of 147 Mev. Some of our measurements have already been mentioned this morning, but it is useful to get all the final numbers down together in one place.

Last year at this Conference Taylor reported some measurements at 142 Mev of the p-p differential cross section and p-p polarization These results have now been finalized. (Taylor and Wood) If we write the differential cross section in the form

$$\frac{d\sigma}{d\Omega} = \left(\frac{d\sigma}{d\Omega}\right)_{90^\circ} \left(1 + a\cos^2\theta\right),$$

then we get

E	$\left(\frac{d\sigma}{d\Omega}\right)_{90^\circ}$	a
142 Mev	3.82 0.06 mb/ster	- 0.07 .03
98 Mev	4.41 0.05 mb/ster	0.02 .03

The polarized cross section, again by Taylor and Wood at 142 Mev is

$$P\left(\frac{d\sigma}{d\Omega}\right)/\sin 2\theta = \left[(0.72 \pm .04) + (0.36 \pm .06)\cos^2\theta\right] \text{mb/ster},$$

and at (98 $\pm$ 1) Mev:

$$P\left(\frac{d\sigma}{d\Omega}\right)/\sin 2\theta = \left[(0.50 \pm .04) + (0.08 \pm 0.07)\cos^2\theta\right] \text{mb/ster}.$$

These results are contained in the following figures.

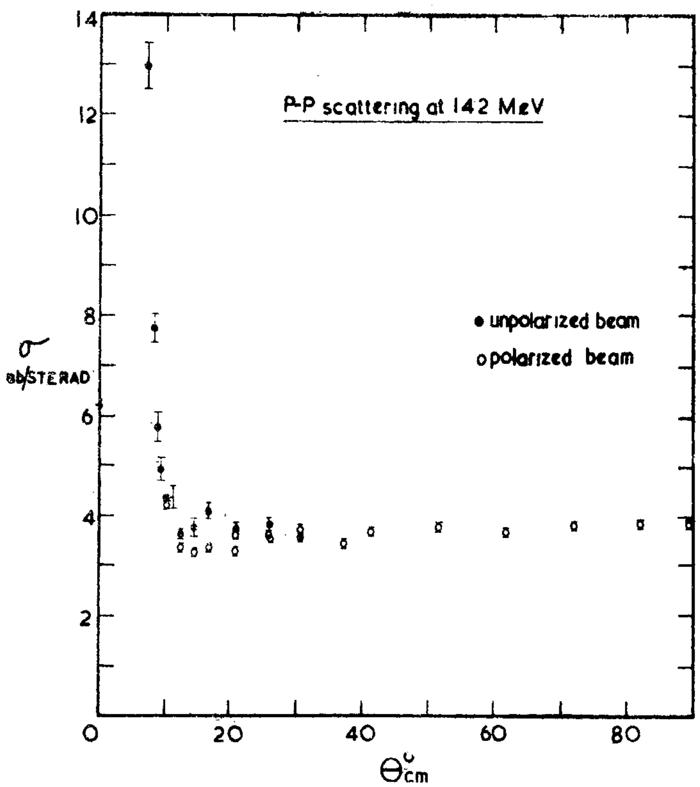


Fig. 17

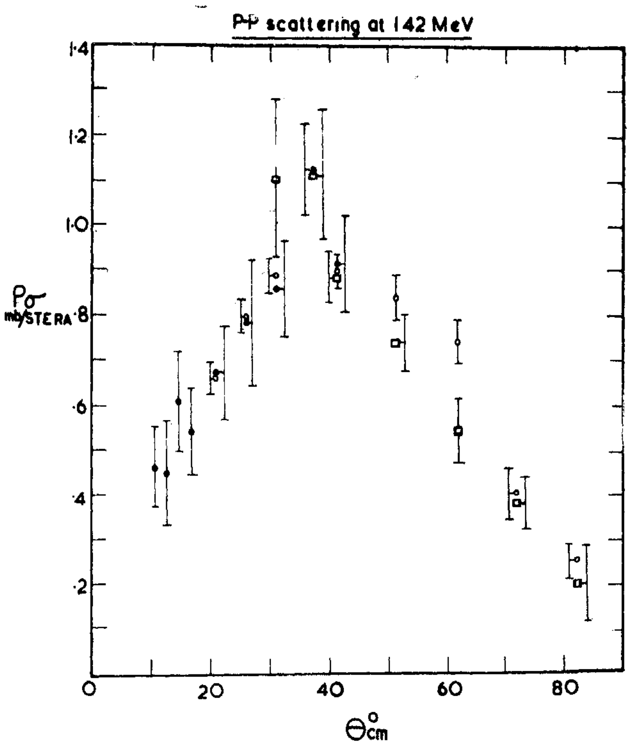


Fig. 18

Fig. 17 shows pp scattering at 142 Mev. There are two sets of measurements: one with an unpolarized beam and the other, in circles, with the polarized beam in which the left and right measurements have just been added. Note how isotropic the distribution looks. There is some uncertainty in the region where nuclear Coulomb interference exists.

Fig. 18 shows the polarization results. There are three sets of measurements, the early ones by Dixon and Salter which were reported last year, and two sets of measurements by Taylor and Wood.

The large $\cos^2\theta$ term indicates that in the phase shift analysis one needs to go to at least a partial F-wave. As there are no triple scattering results at this energy and no comprehensive np polarization results, we decided to drop in energy and try to get a complete set of measurements at something like 98 Mev. These results are also shown. I might point out that at 90° the differentail cross section is very close to the measurements made by Kruse, Teem, and Ramsey some years ago. They obtained a somewhat larger value for the coefficient a. I believe, though, that recent measurements suggest that we are now in agreement. The pp scattering results at 98 Mev are shown in Figures 19 and 20.

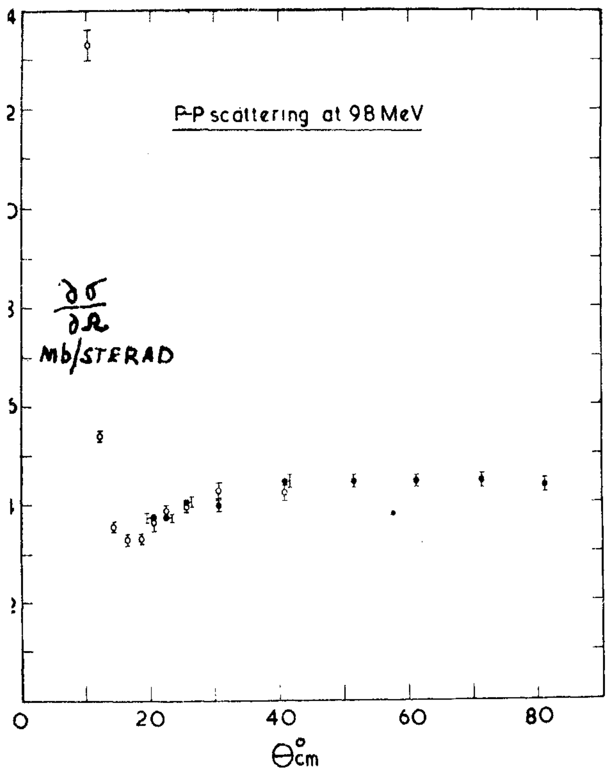


Fig. 19

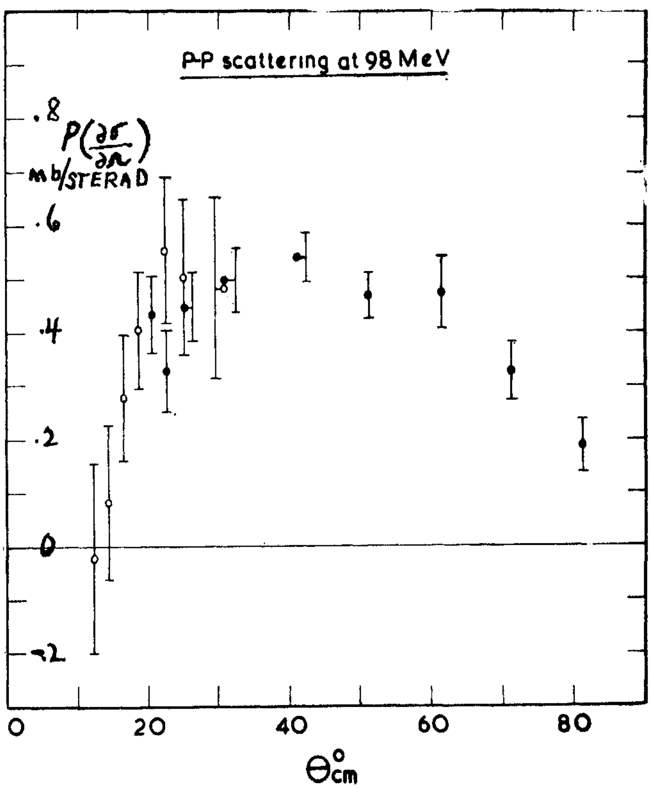


Fig. 20

Preliminary measurements of the neutron proton cross sections at 95 ± 2 were given at last year's conference. The final result obtained by Hillman, Whitehead and myself is given by

$$P\left(\frac{d\sigma}{d\Omega}\right) = \sin\theta\left[(1.17 \pm 0.11)\, P_0(\cos\theta) + (2.34 \pm .23)\, P_1(\cos\theta) + (0.21 \pm 0.26)\, P_2(\cos\theta)\right].$$

In view of the large size of the P_1 term, one can show that it is necessary to do a phase shift analysis with S, P, and D partial waves at least. The np polarization results at 95 Mev are shown in Fig. 21.

The effective energy is quoted at 95 ± 2 Mev. The "effective energy" was obtained by measuring a total cross section and deducing an equivalent neutron energy from it. This energy was kept constant as the angle was changed. The neutron spectrum is in fact triangular in shape and extends from 75 Mev to 120 Mev and this should be borne in mind when theoretical curves are fitted to the experimental results.

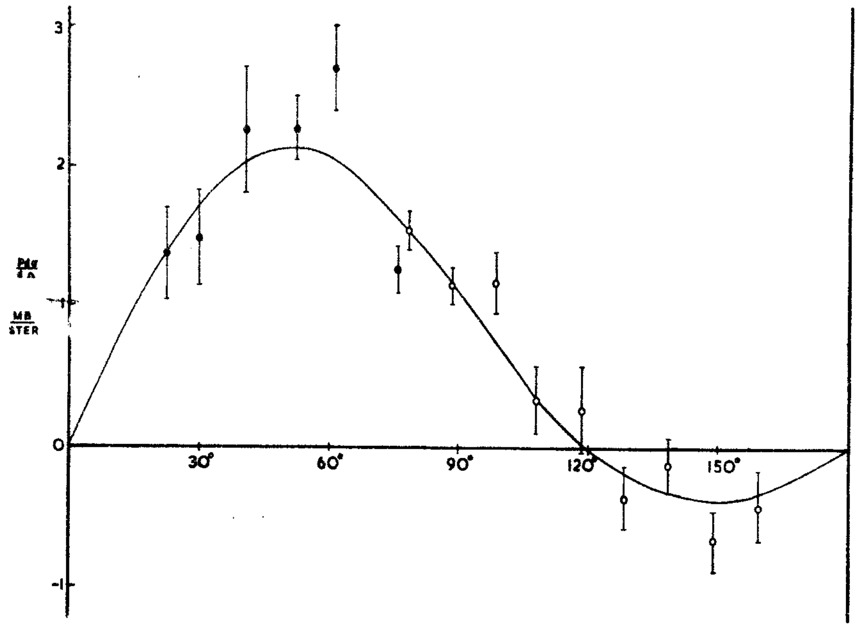


Fig. 21

With these results, R.J.N. Phillips, who was at Rochester last year, has attempted to do a phase shift analysis using S, P, and D partial waves. He has done this with a desk computer and so really all that he can claim to do is to get regions of fit. I don't think he will claim that the actual values quoted are necessarily the exact ones. Some of his results are shown in Fig. 22.

A PHASE-SHIFT ANALYSIS OF NUCLEON-NUCLEON SCATTERING AT 95 Mev

TABLE I

T = 1, phase shifts in degrees

Fit No.	δ_0	δ_2	δ_1^0	δ_1^1	δ_1^2
1a	35	1	-23.5	5	7
1b	32	0.5	-26	7.5	6
2a	26	-2	-22	15	6
2b	25	-4	-17	15	8
3a	30.5	-1	30	-5	6
3b	31	-1	25	-7	8

TABLE II

T = 0, phase shifts in degrees

Fit No.	δ_1	δ_0^1	ε	δ_2^1	δ_2^2	δ_2^3
1a,b	-37	33	20	-17	9	0
2a	-32	43	20	-9	-5	0
2b	-24	49	25	-5	-10	0
3a	-34	37	0	-15	10	0
3b	-29	40	-5	-15	10	0

TABLE III

T = 1, phase shifts in degrees

Examples using a single F-wave

Fit No.	δ_0	δ_2	δ_1^0	δ_1^1	δ_1^2	δ_3^2	δ_3^4
A	35	0.5	-24	6	6	—	0.2
B	37	0	-22	5	6	-1.2	—

Fig. 22

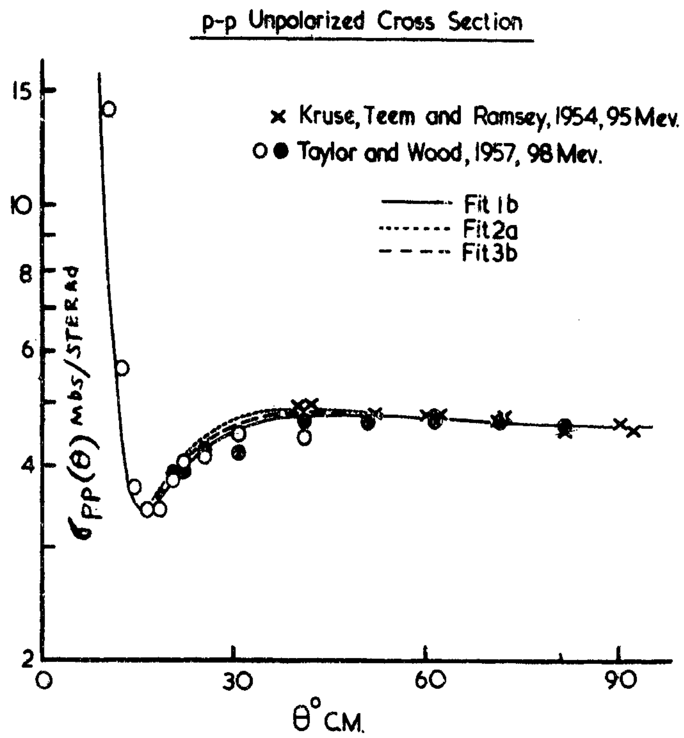


Fig. 23

Essentially he has found three regions of phase space which fit the results fairly well. Fig. 23 shows the fit to the experimental p-p scattering results. Some adjustment of the absolute scale has been made by Phillips to bring the measurements of Kruse, Teem and Ramsey and those of Taylor and Wood into agreement. This is justifiable because there is an uncertainty of ± 3% in the absolute calibration of the proton flux and hence in the absolute values of the cross sections used. At the smallest angle there is also a differences of some 5 mb per steradian between the experimental and theoretical values. The cross section is varying rapidly in this region and this discrepancy may be due to a change in the effective energy of the proton beam used. The introduction of some F-waves would reduce the discrepancy.

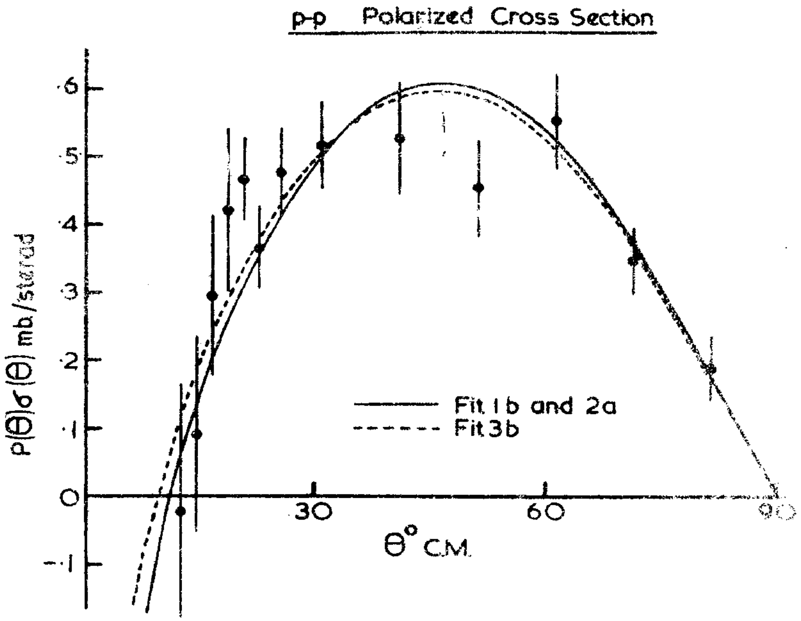


Fig. 24

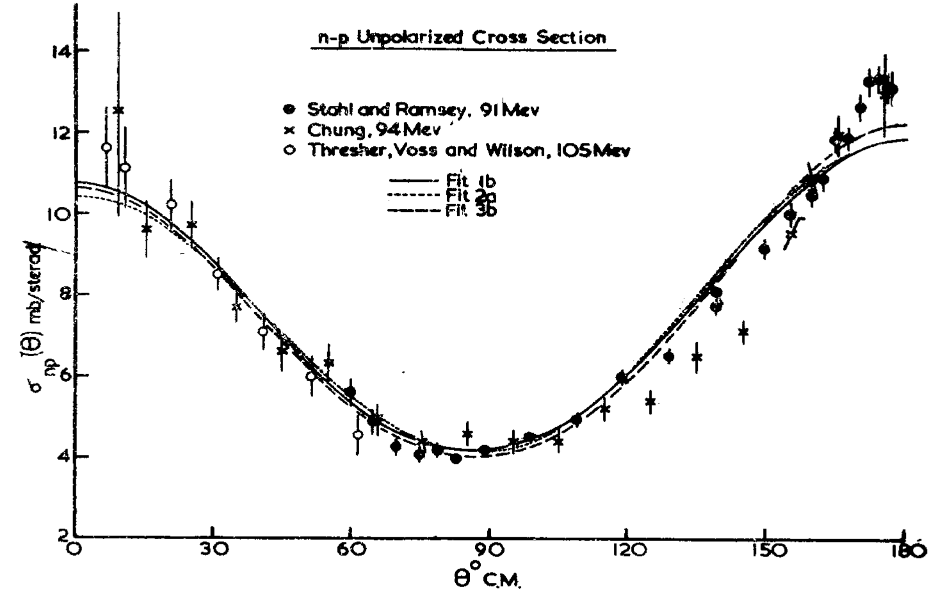


Fig. 25

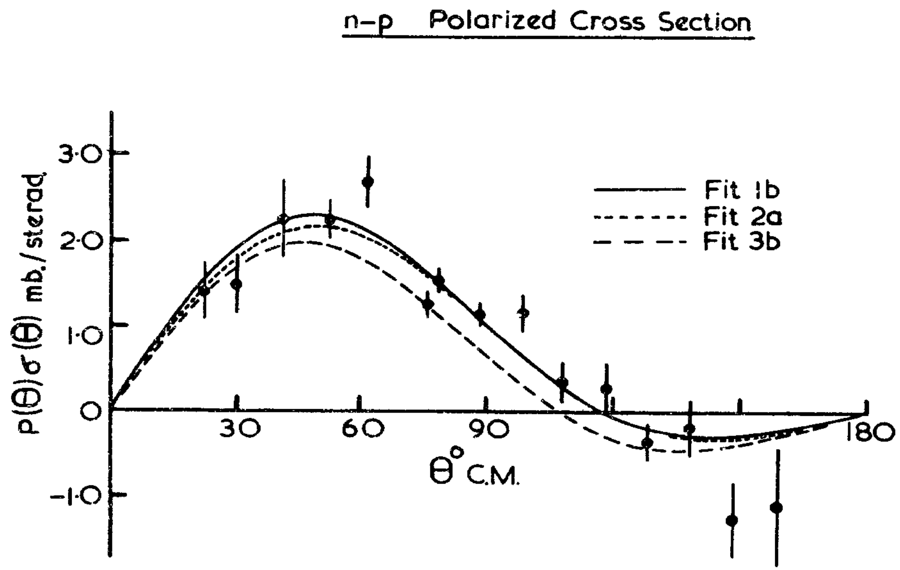


Fig. 26

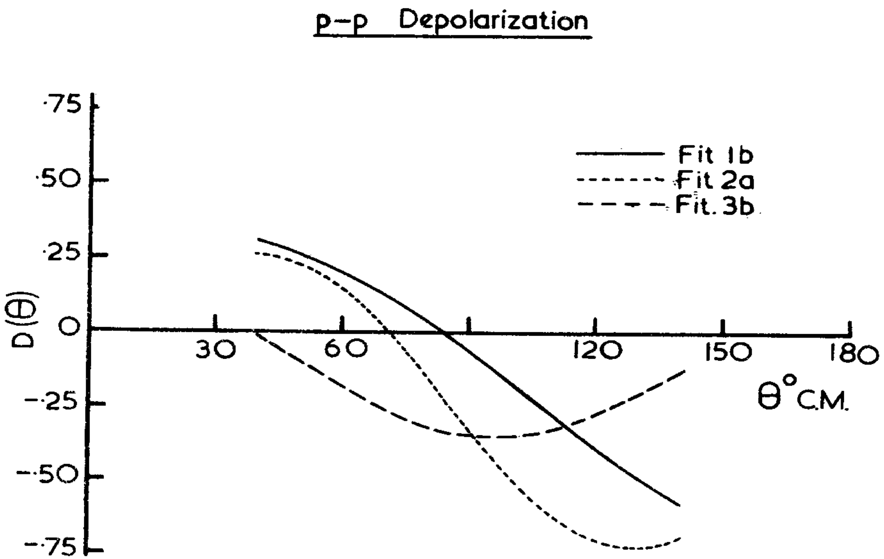


Fig. 27

Fig. 24 shows the polarized pp cross section with Phillips' fit. Fig. 25 gives the np unpolarized cross section. You will see again that there are some worrying discrepancies. At small and large angles, for instance, I think the points are significantly higher than the curve.

Fig. 26 shows the np polarized cross section. Again I think the Ib solution is the most satisfactory.

All in all, among Phillips' phase shift results, the Ib is the one which is favored by the experimental group and it is also the fit which seems to be more satisfactorily in agreement with the Stapp and Ypsilantis and Metropolis phase shifts, (I think solution #1 of the Stapp phase shifts for higher energy) . Preliminary measurements of the depolarization at 140 Mev by Taylor and Wood would favor fit 3.

I am now going to give a short preview of what one can expect in the future. Taylor and Wood have begun a measurement of the depolarization parameter at 142 Mev. This experiment using a counter technique will cover the small angle region and a team from University College London using helium filled cloud chamber will make a measurement at least at one large angle.

Whitehead, Tornabene and myself have started a measurement of the np polarization at 75 Mev and we hope perhaps to get down to 40 Mev. The 75 Mev results show a polarization about half the value at 95 Mev.

Scanlon, Thresher and I have developed a neutron time of flight technique which will enable us to measure the n-p differential cross section at all energies from 40 Mev to 140 Mev and over a range of angles. The principle of the technique is to deflect the circulating proton beam on to a thick target and to time the neutrons over a 26 metre flight path. Times of flight are converted into a voltage pulse so that the results can be presented on a kicksorter. A typical result is shown in Fig. 28.

This result was obtained by placing a plastic scintillator detector in the neutron beam. On the left is a γ ray peak which is very useful for giving an absolute timemeasurement and also for determining, this is quite important, the time resolution of the apparatus. Following the γ ray peak there is a time interval in which no events are detected and this is followed by the neutrons. A thick target was used and so neutrons of a wide range of energies were detected. The flattening at 50 Mev is produced by the lead which was placed in the beam to reduce the γ ray intensity.

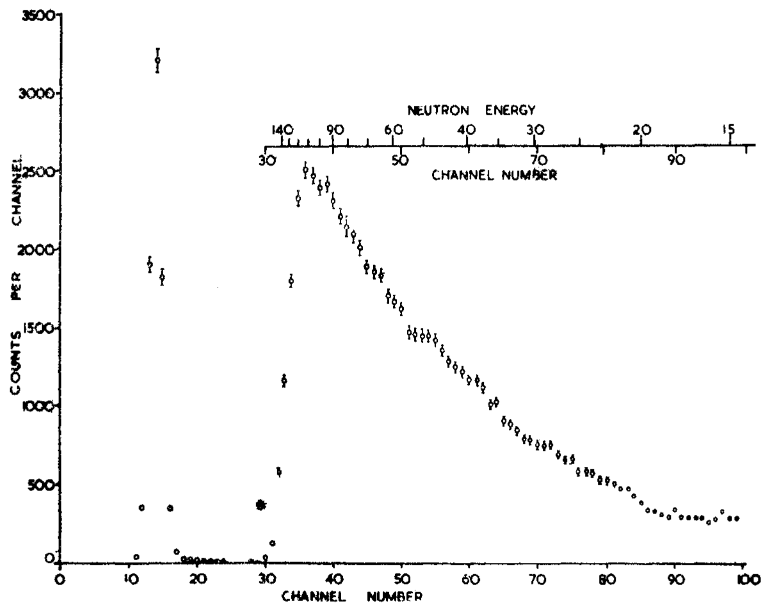


Fig. 28

RICHARD WILSON: Proton-proton polarization experiments

Just recently at Harvard we have started again on pp scattering. This is a group consisting of myself, Professor Ramsey, Professor Cormack, and Palmieri. We have been doing pp polarization. Our data has slightly higher precision than the Harwell data, and I am glad to say that in the pp system the two experiments essentially agree. In Figures 29, 30, 31 and 32 we have plotted the asymmetry ϵ as a function of center of mass angle. We have only obtained these in the last month or so and we're not quite sure of the beam polarizations to the same accuracy with which we can do the asymmetry points. The same 146 Mev data are plotted in Figures 29 and 30. In Fig. 30 there is undoubtedly a forward peaking. There might be some slight semblance of $\cos^4\theta$ dependence though one can't be sure at this stage.

The next thing we did was to go down to 118 Mev as shown in Figures 31 and 32. We have the same sort of accuracy. Now, as you see from Figure 31, $\epsilon(\theta)/\sin 2\theta$ has essentially no $\cos^2\theta$ term. We have had no time to prepare the corresponding figures for the 95 Mev experiment which we have also done. We haven't yet been able to write out all the corrections necessary to get the unpolarized cross section. We have just taken one or two points, and

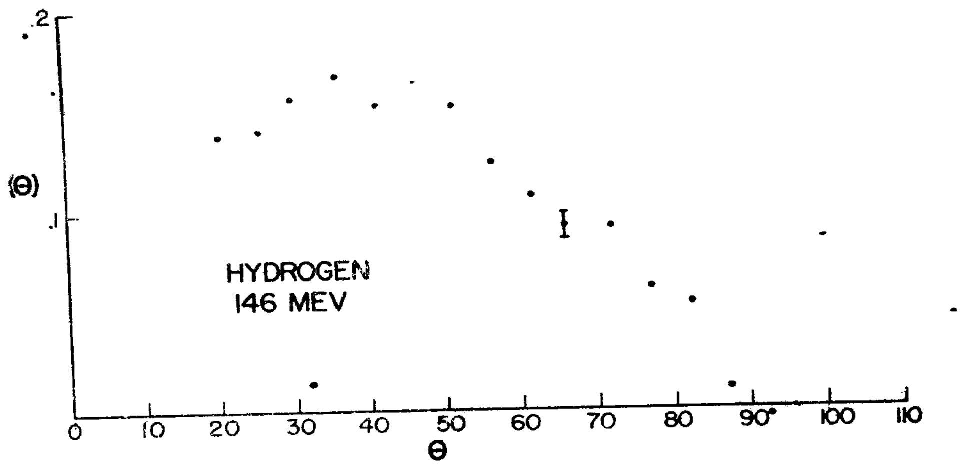


Fig. 29

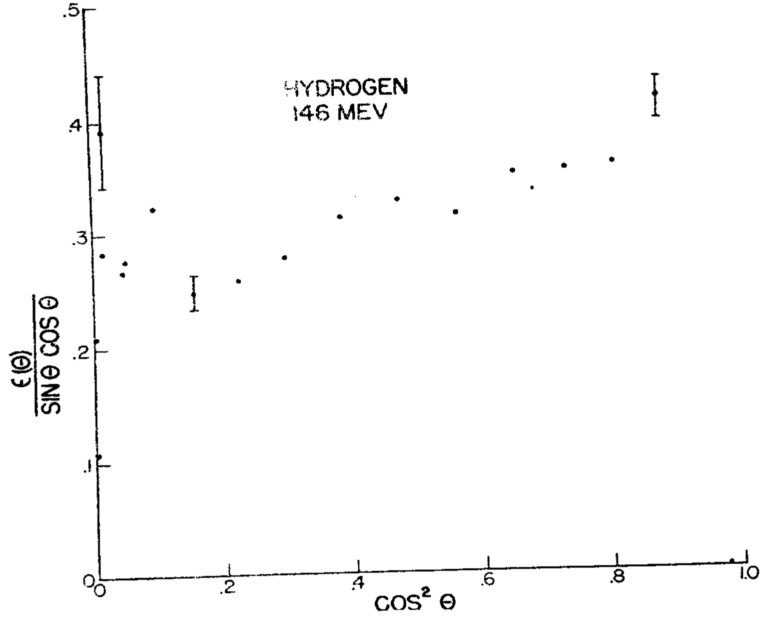


Fig. 30

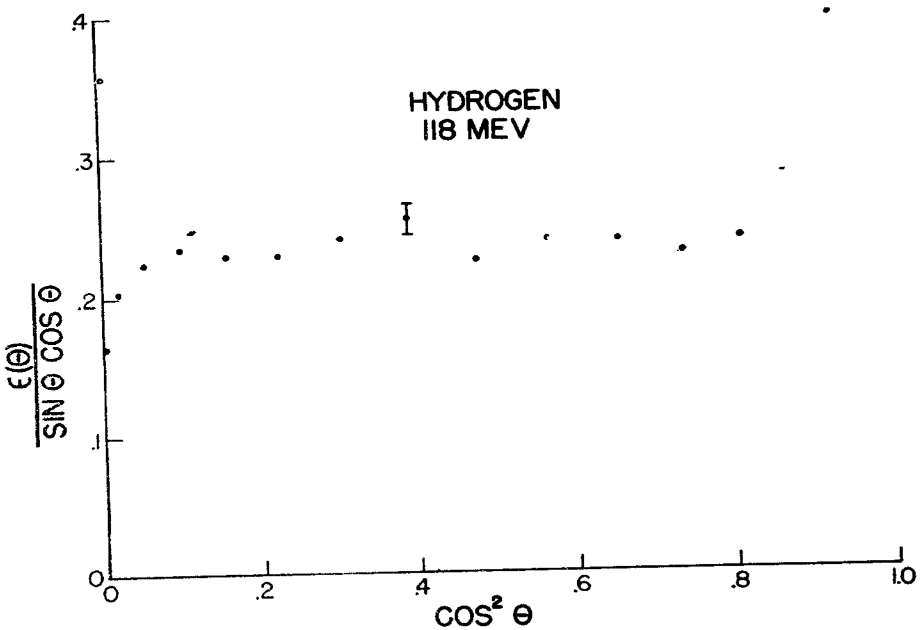


Fig. 31

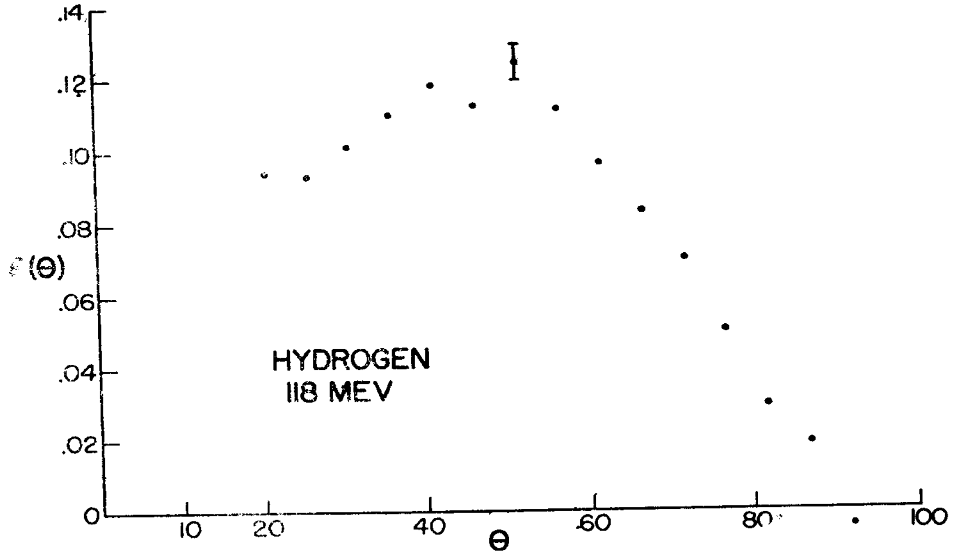


Fig. 32

and I am glad to say that down to 20° in the center of mass, i.e. the 20°, 40°, and 90° points at 95 Mev, we are in complete agreement to about 3% with the results of Taylor and Wood. We seem to be definitely slightly lower than the old results of Kruse and Ramsey. So this is a rather happy situation.

I was originally going to give the results differently from Stafford, but in order to compare quite readily I will multiply by what we believe is the beam polarization, 75% and the differential cross section he has.

146 Mev $\quad P \frac{d\sigma}{d\Omega} = (.82 \pm .05) + .41 \cos^2\theta$

118 $\quad = .64 + (0.15 \pm .1) \cos^2\theta$

95 $\quad = .55 + (0.06 \pm .1) \cos^2\theta$.

We chose 95 Mev to get near the old Kruse and Ramsey figures. The term at 95 Mev changes sign depending on whether one includes the 20° center of mass point or not. As one is beginning to enter the Coulomb interference region, we don't see any significance in this.

The basic error at themoment is about $\pm .05$, but it will eventually come down to .03 when we make sure of the beam polarization. These errors and values are not obtained by least square analysis. At the moment the points are rather accurate compared to any systematic errors, and numbers are obtained merely by a ruler. It is interesting perhaps to point out that at the higher energies we essentially need the $\cos^2\theta$ term which Thaler does not want, while Marshak would like it.

DISCUSSION

BETHE: I think I would like to urge once more triple scattering experiments. I heard that Harwell is planning depolarization experiments. Is there any possibility of doing also the rotations?

STAFFORD: Yes, we have that in mind. I can't guarantee when it will be done, but we plan to use a solenoid magnetic field to rotate the plane of polarization of the protons. We believe this can be done.

BETHE: One of the rotation experiments, of course, can be done without an auxiliary magnetic field, as you know.

STAFFORD: Which one?

BETHE: The quantity R of Wolfenstein.

WILSON: The present plans at Harvard are to go down to 40 Mev with the same sort of accuracy to join on to the Minnesota data. We also plan to start triple scattering experiments.

GAMMEL: (Referring to Fig. 22) The four negative singlet D_2 phase shifts are not understandable in terms of a potential. The singlet D_2 phase shift must be attractive on the basis of a standard model for a potential which has a hard core surrounded by an attractive region. Thaler and I show in a paper which is in press that you cannot get these repulsive phase shifts. The result you actually get with a potential for the singlet D_2 phase shift is about 5^o. I think on Marshak's slide this morning I saw 3.9^o at this energy. According to what Marshak and Thaler said this morning, none of these $^3P_0$ phase shifts would be in agreement with their results. The tensor force is attractive in the $J = 0$ state, and so is the central force, and that tends to give you a positive phase shift. The spin orbit force is short ranged and at this energy of about 95 Mev it does not play an important role. So both Marshak and Thaler would have shown positive $^3P_0$ space shifts. As a matter of fact, none of the sets of space shifts shown here are in agreement with the first two talks presented this morning.

BETHE: This is unfortunate.

STAFFORD: I don't think one should be worried too much about this. As I said, we should prefer the lb set of phase shifts. One should also keep in mind that only a desk computer was used.

GAMMEL: These phase shifts are a take-off on the Feshbach-Lomon phase shifts. That negative $^3P_0$ phase is going to come in inherently. Also, the D_2 phase shift is too small to be understood in terms of potentials. It's too bad that you analyze experiments only at one energy. I think otherwise things would have been changed considerably.

BETHE: Anyway, the experiments are very fine. In this case, however, I think one would really prefer a high speed computer for the phase shifts.

THALER: I want to remark that it is quite clear that in the data which have just been presented, the $\cos^2\theta$ term enters with extraordinary rapidity. At 118 Mev it is still negligible, but at 142 Mev it is certainly rather large. I am not suggesting that this is incorrect. I merely am suggesting that it is very easy for a model to miss exactly the point at which F-waves begin to make themselves really felt.

BREIT: Relativistic corrections to Coulomb effects

The subject of relativistic corrections enters into the analysis of data, especially in pp data at small angles, where you have the Coulomb interference region. Also in np data, there are related so-called magnetic effects. The manner of making relativistic corrections has been treated by Garren in two papers and by myself and by Ebel and Hull at Yale. The object is usually to make corrections fo electromagnetic effects only. The viewpoint is that one can transform to the center of mass system and deal with the phase shift there so far as the nucleon-nucleon situation goes. In the electromagnetic effects, the off-hand idea is that the Coulomb effects will be important only for small angle scattering, which usually means distant collisions. Therefore some of the literature contains that kind of treatment relying entirely on the Coulomb scattering involving distant collisions. On the other hand, and this is the main thing that I want to point out, this is not the correct procedure because the distortion of the wave function by the nucleus can be estimated to be appreciable. It is not necessary to make a covariant treatment of this matter. It can be understood rather simply, because the energies involved are not extremely relativistic.

There is a prototype of the situation in the ordinary atomic theory. Some of the effects are just concerned with plain ordinary spin orbit interaction, which one has known for many years. There are in the interaction energy terms of the kind:

$$H' = -\frac{e\hbar}{2mc}\frac{e}{c}\Bigg\{\underbrace{\frac{1}{2}\left(\vec{E}_1\times\frac{\vec{p}_1}{m}\right)\cdot\vec{\sigma}_1}_{\text{"Thomas term"}} - \underbrace{e\left(\frac{\vec{r}_{12}}{r^3}\times\frac{\vec{p}_1}{m}\right)\cdot\vec{\sigma}_1}_{\text{"magnetic term"}}\Bigg\}.$$

The subscripts refer to the two particles. $\vec{E}$ is the electric field.

One knows from atomic spectra that it would be entirely wrong to calculate this without considering the actual wave function.

By the way, in the nuclear case one has to bring in, in addition to that interaction, the anomalous moment. For the anomalous moment you get the factor four-thirds, that is

$$3\frac{e\hbar}{2mc} \longrightarrow (3+4\mu_a)\frac{e\hbar}{2mc} ,$$

where μ_a is the anomalous Pauli moment for the proton. One can understand the three as being essentially a two plus one, and the four is, essentially, two plus two. The one is a one rather than a two in this case because you have a magnetic term which is cut down to one-half by the Thomas term. For the anomalous moment part, you have the full magentic term operating.

I think one can understand the relativistic effects in this way. The remaining question is reliability of estimates which neglect wave function distortion. It should have been rather clear from the old considerations of Niels Bohr regarding the correspondence principle applied to Coulomb scattering, that in the pp case only very seldom may one use an approximation in terms of classical orbits. This is the case where the important parameter is $\eta = e^2/\hbar v$. This is often exceedingly small, of the order of one hundredth, in the high energy cases, while it is essential according to Bohr that η be much greater than one. For this reason, you find that the scattering at small angles has the following peculiarities. The term which enters on account of the Coulomb scattering is of this structure:

$$\exp(-i\eta \ln \mathcal{S}^2)/\mathcal{S}^2 \quad , \quad \mathcal{S} = \frac{\sin \theta/2}{\lambda}$$

where θ is in the center of mass system. This terms can be expanded in partial waves and then you obtain a sum of terms of the form:

$$\sum (2n+1)\left(1+\frac{1}{2}+ \qquad +\frac{1}{n}\right)P_n(\cos\theta) .$$

This series converges slowly. You will, however, find that the first couple of terms, let's say S, P, and D terms, before the series reverse sign, contribute an amount which is almost the same as the whole answer. These are the terms in which the relativistic treatment of η is important. However, you could say it should not be very much trouble, because you have the same difficulty in the non-relativistic situation. That is, you could argue that you should add the nuclear phase shift to the Coulomb phase shift. Although that is all right in the nonrelativistic case, it is not all right in the relativistic one, as one sees by looking at the components that have been eliminated.

If you look at the spin orbit interaction you find the situation is much accentuated. A typical term is of the kind $(1/2\delta) - 1$.

I mean that the sum of all of the terms, say for the ^3P interaction, can be approximated in this fashion. You find in this case that the first couple of partial waves are of the order of magnitude of the whole. Therefore, in the treatment of np scattering at small angles, the magnetic effects become important; the same holds in the treatment of pp scattering in the region of the Coulomb interference.

I think one simply does not know the magnitude of the relativistic corrections. They have to be ascertained by a more detailed calculation with a particular model.

MOON: Small angle proton scattering at 970 Mev

I want to report on some work on small angle scattering of protons at about 970 Mev on carbon, done by Goldsack, Lock, and Batty at Birmingham with nuclear plate track counting as the observational technique.

By way of background it's worth saying that if one has a beam of protons which may be polarized and puts in a target (in this case a target of carbon), and behind, at a reasonable distance, places a nuclear emulsion plate and looks for tracks at a certain region in the plate, then one can identify those tracks which come, or appear to have come, from the scattering.

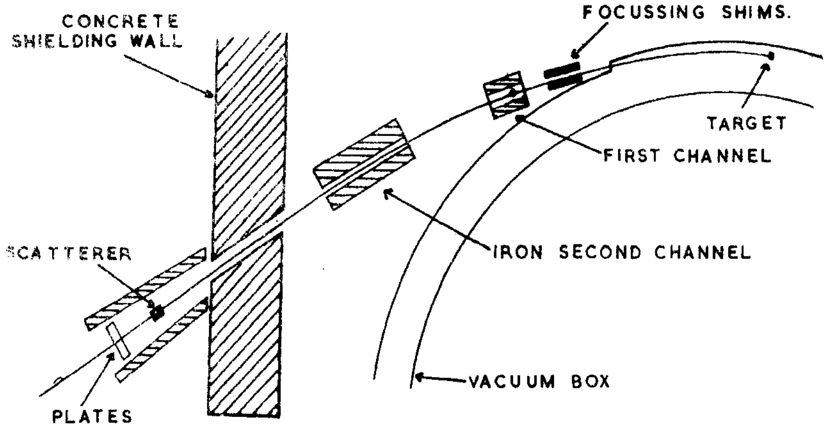


Fig. 33

In Fig. 33 you see the experimental set-up. In the fringing field of the machine you have some pieces of iron in a particular configuration suggested to us by

Richard Wilson (from Harvard). This helps to counteract the rather odd shape of the fringing field and enables you to get a well focussed parallel and homogeneous beam through the shielding. As it gets to the target the beam is something like 4 centimeters high and 2 centimeters wide. The plates are put in about a meter behind. One scans tracks, picking out those tracks that occur at a certain place in the plate and seem to have come from the scatterer.

Fig. 34 shows the results, obtained at quite small angles. The angular resolution is about $1/2^{\circ}$. The points that one really wants to look at are those with the circles on them. They are compared with the sum of two intensities, namely the diffraction intensity as calculated by the black disc method (with the experimental value for the total diffraction cross section) and superimposed on that, the Coulomb scattering. The latter is calculated with the relativistic formula, remembering that the protons go through the nucleus so that one has to make a correction for the charge distribution. This is not very large.

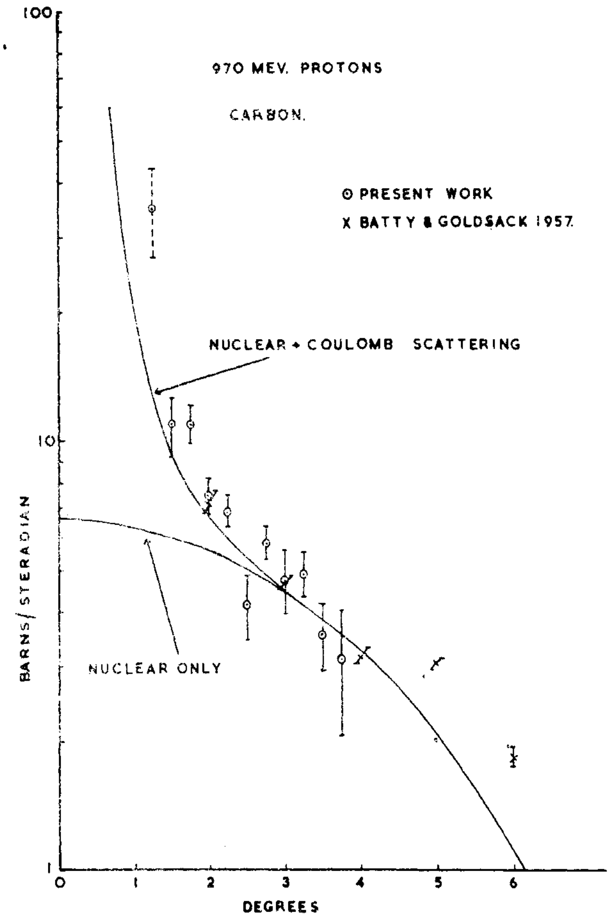


Fig. 34

The point that is worth making, I think, is that the experimental results agree tolerably well with the sum of the intensities of the nuclear and the Coulomb scattering. They would certainly not agree with destructive interference and probably not with constructive interference. So, as far as the evidence goes, the two things look to be practically 90° out of phase, which is what G.E. Brown told us they should be. I think Dr. Bethe would be inclined to agree with this.

DISCUSSION

PEIERLS: I would just like to comment that the theoretical curves shown with these preliminary results, both as regards cross section and polarization, were obtained by Brown following arguments extremely like those that Bethe has reported earlier. The way they are fitted is essentially using a central potential which is purely imaginary, purely absorptive. This of course goes exactly in the same direction as the data that Bethe gave before, only more so. We could not, at this stage, exclude a small real part. The accuracy of the data does not allow a very precise fit. The spin orbit potential determined from considerations consistent with Bethe's has the obvious radial dependence and is purely real. Now this is less obvious, that it should be purely real; but the data seem to indicate that it must at least be very largely real.

BETHE: It seems to go on pretty much the same way at 1000 Mev as at 300 Mev.

COOL: Pion interactions with nuclei at 1 Bev

These remarks are actually for about the same energy but for different particles, because what I'm reporting is some preliminary survey work on the interaction of pions in the range around 1 Bev in interaction with complex nuclei. This is work by Cronin, Abashian and myself.

The survey we made included the following absorption cross sections: (1) in various nuclei from Be to Pb at 0.97 Bev, (2) in C at 0.6, 0.8 and 1.4 Bev, and (3) in Al, Ca, and Pb at 1.5 Bev/c momentum for both π and protons. Diffraction cross sections were also obtained (1) in Be, C, Al, and Ca at 0.97 Bev and (2) in C at 0.6, 0.8, and 1.4 Bev.

In the case of pions, for which both the real and imaginary parts of the forward scattering amplitude are known, the data can be directly compared to predictions of the optical model. Such comparisons have been made both for tapered Fermi type nucleon distribution of the form required by electron scattering data and for a uniform (square-well) distribution of radius R $= r_0 A^{1/3}$. For the Fermi distribution, the assumption has beem made that the fall-off distance is the same for the nuclear potential as for the proton distribution.

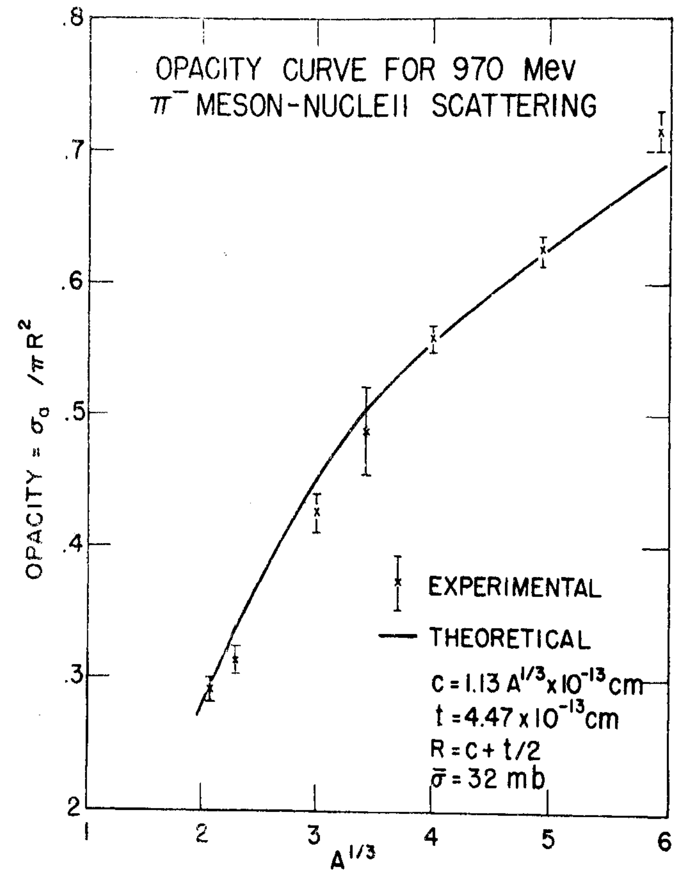


Fig. 35

All of the absorption data are in excellent agreement for a Fermi distribution with a single radial parameter $C = (1.14 \pm 0.04) A^{1/3} \times 10^{-13}$ cm. The result is shown in Fig. 35. This value is to be contrasted with the value of $C = 1.07 A^{1/3} \times 10^{-13}$ cm. obtained from the high energy electron scattering data of the Stanford group, which is 6% smaller than that from the pion data. The data are, however, consistent with the same value as that obtained from the electromagnetic data if the range of the pion-nucleon force is taken to be 1×10^{-13}cm. This range is about equal to that found from pion-nucleon scattering measurements. A uniform distribution with $R = r_0 A^{1/3}$ is not consistent with the data.

The absorption cross sections in the light elements are not very sensitive to the value of the radius. As a result, they lead to an experimental value of $\bar{\sigma} = 34 \pm 3$ mb. in nuclear matter at 0.97 Bev, to be compared to that derived from the elementary pion-nucleon cross sections of 33 mb.

Small corrections for the Pauli exclusion principle and the direct absorption of pions by nuclei do not alter the conclusions above. They are, however, of the proper direction and magnitude to remove a small discrepancy between the predicted and measured pion and proton cross sections for Al and Ca.

The measured diffraction cross sections appear to be 20%-30% larger than those predicted by the optical model and the dispersion relations. The situation is shown in Fig. 36. The value of k_1/K obtained from the elementary pion-nucleon cross sections and the dispersion theory is $k_1/K = 0.05$, while that required by the data is $k_1/K = 0.4 \pm 0.15$. Thus, although the errors are large, there is an indication that the real potential is several times larger than that predicted. Further and more accurate measurements will be required to verify this conclusion.

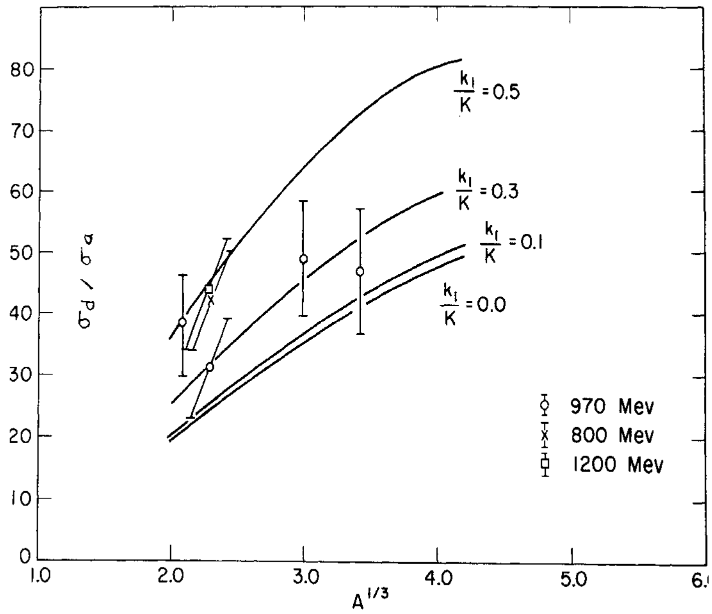


Fig. 36

LOFGREN: Proton-proton scattering

A pp scattering experiment has been carried out at the bevatron by Cork, Wenzel and Causey. It was done with an internal beam and an internal target and a pair of coincidence counters set at the appropriate angle to define the elastic event. They measured the differential cross sections of pp reactions at 8 different angles and 3 different energies. The total elastic cross sections obtained from integrating these data are as follows:

	Energy	σ
pp elastic	2.24 Bev	17 millibarns
(C.W. and C.)	4.4 "	10 "
	6.15 "	8 "
Mixed pp and np inelastic (Williams)	50 "	21 ± 4 "

R. W. Williams has asked me to point out that he has a cosmic ray point at an energy of about 50 Bev that is a mixed np and pp cross section and is inelastic. It was determined by examining the meson production by 50 Bev cosmic ray nucleons on Fe .

The results are reviewed in Fig. 37.

Fig. 38 shows the new bevatron points in comparison with others.

The errors indicated on the new points are the statistical and the other random errors. In addition, there is an approximately 15% error in the absolute value, the chief contribution to which is some uncertainty in the calibration of the total beam current.

CROSS SECTIONS FOR PENETRATING-PARTICLE PRODUCTION
BY HIGH-ENERGY NEUTRONS AND PROTONS ON IRON NUCLEI.

Energy Range (Bev)	Median Energy (Bev)	Neutron Cross Section (barns)	Proton Cross Section (barns)
28 - 58	37	0.60 ± .04	0.59 ± .05
58 - 121	77	0.62 ± .05	0.61 ± .06
121 - 387	178	0.67 ± .13	0.79 ± .25
28 -387	50	0.61 ± .03	0.61 ± .04

Fig. 37

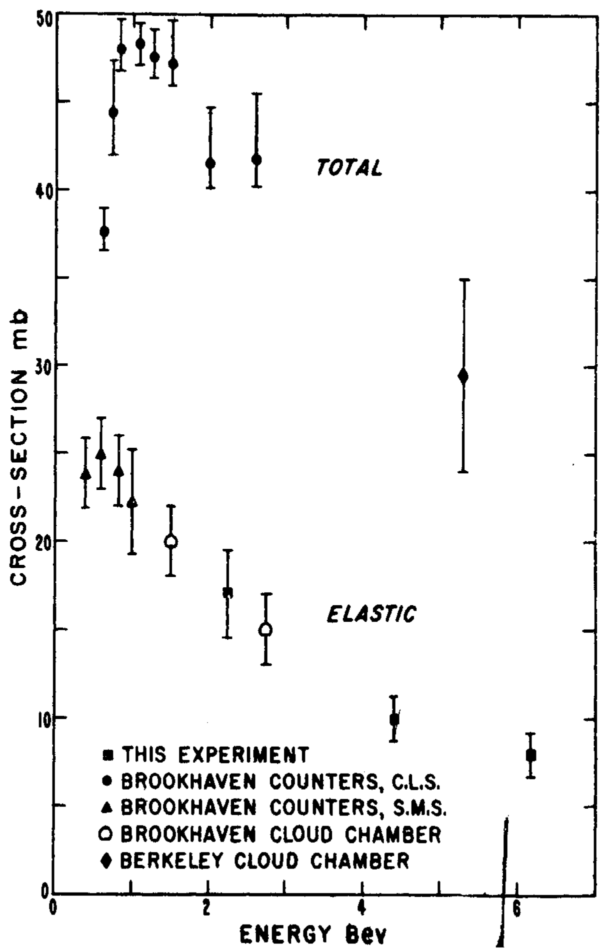


Fig. 38

APPENDIX

BETHE: Note on selection of phase shifts.

At the time of the Rochester Conference, I believed that Stapp's solution 6 gave a spin-orbit scattering of less than 10 θ, one-third of the observed value. This was due to the assumption that the $T = 0$ phase shifts which were then not available would behave the same way as the $T = 1$ phase shifts of Stapp. In May 1957, Gammel and Thaler analyzed the neutron-proton scattering in phase shifts and showed that the $T = 0$ phase shifts tend to compensate any differences which may exist in the polarization predicted from the $T = 1$ phase shifts. Moreover, there was a numerical error in my older calculations for solution 6, $T = 1$. My conclusion in April that solution 6 could be ruled out, and the corresponding conclusion for the Feshbach-Lomon phase shifts, was therefore erroneous.