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S E S S I O N D

PION AND NUCLEON SCATTERING

Chairman, P. Signell

Nucleon-Nucleon Scattering *

by

G. Breit

Yale University

Nucleon-Nucleon (n - n) scattering cannot be covered in half an hour. Many aspects of it will therefore be left out and valuable contributions will remain unmentioned. Apologies to the authors of the papers containing these are therefore in order.

The discussion will be confined to energies at which meson production has a negligible effect. Scattering can then be described by means of real phase shifts and coupling parameters, which are referred to collectively as <u>phase-parameters</u> or <u>phases</u> for short. The adequacy of such a description rests on general invariance considerations involving such firmly believed in matters as the isotropy of space. While some of the assumptions may eventually turn out not completely correct, they will not be seriously questioned in this review.

Phase-parameter analyses of nucleon-nucleon scattering could in principle be performed by determining the elements of the scattering matrix at fixed energy and angle. This has not been done so far. There are however analyses making use of data at many energies and scattering angles at once, which will be referred to as <u>multiple energy</u> analyses and a number of <u>single energy</u> analyses each at one energy and many angles or of groups of data clustered around one energy. Since about 1959-60 the probable general types of phase-energy dependence have settled down to essentially one. All analyses make use of Wolfenstein's [1] and Wolfenstein and Ashkin's [2] classic work on the form of the n - n scattering matrix and its relationship to observables: the cross sections, polarization, triple scattering parameters, polarization correlations. An important ingredient is the employment of the one-pion exchange (OPE) values of the phases for the higher orbital angular momenta $L\hbar$, first advocated by Taketani and coworkers [3] then demonstrated to be of great value in the single energy analysis of p-p data at 310 MeV by Moravcsik [4] and by Cziffra et al. [5] and in multiple energy analysis by the Yale group [6,7]. A few samples of the kind of agreement there is between various analyses will now be illustrated. A key to abbreviations used in referring to single energy analyses is shown in Figure 1. Figure 2 shows the phase K_o for state $^1S_0$ as a function of incident laboratory energy E_ℓ in the Yale multiple energy data searches YLAM and YRB1(K_o) compared with single searches. The YLAM fit is that [8] of 1960. The YRB1(K_o) is in the June 1965 edition, an earlier version of which was shown [9] at the Dubna Conference. In Figure 3 phase $^3\delta^P_o$ for ^3P_o is similarly compared. In neither case is the agreement perfect. In Figure 4 the multiple and single energy searches are compared for $^3\theta^F_4$. Here the disagreements are larger both between the earlier and later Yale multiple energy fits and with single energy results. States of higher L and total angular momenta $J\hbar$ usually show larger discrepancies. The Livermore group has multiple energy fits in several editions, similar to those from Yale. In Figure 5 the results for the 1961 version of Yale fit YLAN3M and the June 1965 edition of YLAN4M in the case $^3\theta^S_1$, the phase of $^3S_1$ are compared with those of single energy searches. In Figure 6 the comparisons are made for $^1P_1$. These samples are taken from a chapter by R. D. Haracz and the speaker in a forthcoming book on High Energy Physics of the Academic Press. The later Yale searches include many more data than the earlier, use a better data treatment and include effects of nuclear magnetic moments. They are being improved along directions to be mentioned presently.

n - n scattering is studied partly in order to reach a better understanding of nuclear structure, partly to determine the nature of the n - n

interaction, partly because of its bearing on the general theory of elementary particles. This talk is concerned more with the latter two topics than with the former. One might ask in this connection. (a) What is the cause of n - n interactions? (b) What simplifying principles apply? (c) What is learned about other phenomena? The questions are of course interrelated.

The approach can be made by attempting to form a completely quantitative theory. Or else one can try to isolate features of the phenomena appearing to have the strongest bearing on the mechanism involved. This kind of distinction can be illustrated by the development of the quantum mechanical theory of atomic structure. There was little doubt about the general soundness of the theory in terms of the Coulomb law of force combined with non-relativistic quantum mechanics much before the theory was applied to many body problems. Even now the two electron problem has been treated in detail only in special cases and yet most physicists accepted, many years ago, Dirac's famous statement concerning quantum mechanics explaining all of chemistry and most of physics. It was not necessary to explain all the details of many electron spectra in order to ascertain the assumptions and basic equations of the theory.

Similarly the complete reproduction by theory of phenomenological phase-parameters is hardly needed for the establishment of basic laws of the n - n interaction. A reliable calculation of the phases is difficult, the many body problem being complicated by divergence troubles of field theories. A dispersion theoretical treatment could avoid these troubles. According to an early paper of Goldberger, Grisaru and MacDowell [10], it is necessary however to make use in such treatments of unavailable values of the nucleon-antinucleon scattering matrix in the unphysical region. There is thus as yet no open road to the quantitative discussion of $\mathcal{N}-\mathcal{N}$ interactions comparable in completeness to non-relativistic quantum mechanics to which Dirac's statement applied. Existing calculations involve therefore conventions regarding approximations, since no truly logical way is available. It may be possible however to ascertain the processes causally connected with the $\mathcal{N}-\mathcal{N}$ interaction through evidence mainly concerned with $\mathcal{N}-\mathcal{N}$ interactions at not too small internucleon distances and to clarify a few topics concerning them such as the accuracy of long range charge independence from a comparison of the pion-nucleon coupling constant g derived from p-p, n-p and n-n interactions; the question of whether the pion-nucleon coupling is pseudoscalar or a linear combination of pseudoscalar and pseudovector couplings; the accuracy of conservation of parity, time reversal and other kinematical symmetries in $\mathcal{N}-\mathcal{N}$ interactions; the experimental evidence for the mathematical form of the OPE; the degree of adequacy with which the exchange of vector mesons together with two-pion exchange (TPE) is able to account for the intermediate distance interaction; the agreement between values of g^2 from nucleon-nucleon as compared with those from $\pi-\mathcal{N}$ scattering.

The last two topics are related. When g_0^2 is obtained from $\mathcal{N}-\mathcal{N}$ scattering by adjustment of the OPE contribution to give best agreement with experiment, precision of adjustment is impaired if the minimum L, L_{min}, included in the OPE set of phases is high, the whole of the OPE being decreased thereby. One of the probably important uses of TPE and vector meson exchange estimates is the determination of corrections to these non-OPE effects for L somewhat below the pure OPE limit. Since the needed corrections to OPE are small this is easier than accounting for the whole interaction but is not completely separated from the latter, the space localization of effects having no rigorous justification.

In Figure 7 are shown some values of the pion-nucleon coupling constant $g_o^2 = g^2 / \hbar c$ in the 1962 period. The n-p value of Ashmore et al. has been obtained from their 350 MeV experiments by Chew's pole consideration procedure. The other values are from phase shift analyses. The differences between p-p and n-p values are within the uncertainties of the determinations. An effect of magnetic moment corrections is seen in the last two entries. The effect of different assignments of the $^1G_4$ phase is seen by comparison of the second and sixth entries. There exist many more determinations of the coupling constant than in this and the next slide but it would not be practical to show all of them. In Figure 8 some 1965 values are shown. Reasonable consistency of values from different analyses is apparent. In some determinations there are larger variations probably caused by effects other than OPE. On the whole the n-p values have shown a tendency to be lower than those from p-p analyses.

Some additional effects have been recently estimated by Seamon, Friedman and the speaker [11]. The I = 1 phases from p-p analyses have to be corrected for electrostatic effects before they are used in n-p analyses. For purposes of orientation this has been done using the Yale potential. The change in the I = 1 phase-parameters affects the I = 0 searched phases. Both changes affect the adjustment of g_o^2 as in Figure 9. Minima of D, the mean weighted sum of squares of deviations of calculated and measured observables obtained by varying the phases of the searched set used in the third row are called D_{min}^S, those in the fourth obtained in succeeding adjustments of g are called D_{min}^g. The legend also explains the symbol $(g_o^2)'$ against the preset g_o^2.

The last two rows are believed the more accurate. The last two columns show an effect in the direction of better agreement with the p-p value. The readjustment of I = 0 phases is usually negligible but for $^1P_1$ the shift is - 0.048 and - 0.042 radians at 260 and 350 MeV respectively falling outside the parallel shift uncertainty ± 0.030. For $^3D_1$ it is - 0.013 for the 105 - 172 MeV interval. In Figure 10 are shown values of $(g_o^2)_{best}$ in p-p scattering and the negligible effects on them of varying the computational procedure. The value often used in π -p scattering as $f^2 = 0.08$ corresponds to $g_o^2 = 15.5$ if m_{π^o} is used in the conversion corresponding to p-p scattering and 14.5 if the mass of π^+ and π^- is used instead According to recent work of Samaranayake and Woolcock $f^2 = 0.0822 \pm 0.0018$. Figure 11 shows the effect of the apparent violation of charge independence in the ^1S_o state. This partly offsets the effect of the Coulomb corrections but leaves 85% of it. Since the error matrix uncertainties in g_o^2 are ± 0.42 for p-p and ± 0.92 for n-p the exact validity of long range charge independence has not been proved but previous indications of its violation loose weight as a result of the estimates. The numbers obtained are of course less significant than the existence of the effects which should be caluclated using a more reliable model than the hard core potential. Single energy searches with mock data show effect of the same order of magnitude as those mentioned.

An analysis of low energy n-p data made by H. P. Noyes [12] gave a singlet effective range $(^1r_o)_{n-p}$ 10 to 20% smaller than $(^1r_o)_{p-p}$, in contradiction with charge independence. Exact agreement is not expected but a 10% effect would be surprising. A consideration of the evidence by Friedman, Seamon and the writer [13] confirms Noyes' result for data used by him but emphasizes possibilities of systematic errors such as dynamic effects of molecular electrons above epithermal energies, effects of molecular binding and intermolecular interactions in measurements of the coherent n-p scattering cross section and of possible deviations from the effective range approximation. Figure 12 shows graphs of $\delta\sigma^{o}$, the systematic error in the total, zero energy n-p scattering cross section, against δf_H, the systematic error in the coherent scattering length constrained by $({}^{1}r_{o})_{n-p} = 2.7\underline{F}$ assuming singlet and triplet shape parameters (0.040, -0.040), (0.025, -0.025) and (0,0). The heavy lines are $E < 5$ MeV, the light for $E < 10$ MeV. Light dashed horizontal and vertical lines are for standard deviations of σ^{o} and f_H. The systematic errors of the latter are conceivably much larger. A systematic error correction of -0.15% to σ^{o} at 0.493, 3.204 and 5.874 MeV was speculatively assumed for these plots. Even if the possibility of systematic errors in σ^{o} and f_H is discounted there is the possibility of satisfying all conditions on the parts of the full lines within the rectangle formed by the dashed ones. In Figure 13 the effect of changing the assumed systematic errors at the three energies to -0.30% is is illustrated. The probability of partial reconciliation with charge independence is even higher than before. A more definite comparison of the ${}^{1}S_{o}$ effective ranges may call for improved measurements of the total cross section between 0.4 and 5 or 10 MeV, of f_H preferably by a method other than liquid mirror reflection and for improved estimates of effects of molecular electrons.

The difficulties in obtaining a precise $({}^{1}r_{o})_{n-p}$ are partly caused by the confinement to a small energy interval needed in order to isolate df/dE from higher derivatives. The essential quantity is however the difference $(dK_{o}/dE)_{p-p} - (dK_{o}/dE)_{p-n}$ at small but not necessarily vanishing energies after correction for Coulombian effects. Since 20 MeV is small compared with the pion mass an average of this difference over such an energy interval should be as informative as the effective range. Such a substitution of a chord for a tangent to the k cot K_{o} versus energy curve appears capable of answering the physical question provided auxiliary experiments on polarization, correlation coefficients and triple scattering parameters can be performed well enough, to furnish corrections for a few low L waves. Collaboration with R. E. Seamon [14] has shown that such an experimental program is promising. In the calculations mock experiments with realistic errors of a set of observables overdetermining the phases were used in an error matrix calculation to obtain uncertainties of the phase shifts. In a mean i.e. chord equivalent of $({}^1r_o)_{p\text{-}p}$ an accuracy of better than 1% perhaps even better than 0.4% seems possible. In the n-p case it appeared hard but possible to otain a better accuracy than 3.6% over a 20 MeV energy range. The inclusion of very low energies does not interfere with the plan but is not vital. It should thus be possible to obtain evidence concerning charge independence in the ${}^1S_o$ state additional to that contained in the "scattering length" information through extrapolation to E = 0. The controversial question of the n-n scattering length is apparently being resolved in the direction of agreement with charge independence-symmetry, the new value of Baumgartner, Conzett, Shield and Slobodrian from $T(d,He^3)$ 2n giving 16.1 ± 1.0 F in good agreement with 16.4 ± 1.3 F of Haddock et al. from $\pi^- + d \rightarrow 2n + \gamma$ and in agreement with 16.9 F estimated on the assumption of charge symmetry by Heller, Signell and Yoder.

On the basis of his measurements concerned with R, $\mathcal{A}_x$ and P_s and other evidence concerning p-p scattering Thorndike [15] finds that the parity conserving, time reversal-noninvariant coupling of ${}^3P_2$ to ${}^3F_2$ states is $\lesssim$ 7% of its maximum possible value between 140 and 210 MeV and that the parity nonconserving, time reversal-invariant coupling of ${}^1S_o$ and ${}^3P_o$ states is $\leq$ 70% of its maximum possible value at 140 MeV. The parity nonconserving, time reversal-noninvariant coupling of ${}^1S_o$ and ${}^3P_o$ states is found by him to be $\leq$ 60% of its extreme negative value. The writer is unaware of any other reliable test to have indicated a breakdown of the usual kinematical symmetries in nucleon-nucleon interactions.

Footnotes

* This research was supported by the U. S. Atomic Energy Commission and the U. S. Army Research Office - Durham.

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[16] E. H. Thorndike, Phys. Rev. 138, B586 (1965).

Figure 1.

Abbreviations used for Single Energy Fits in Phase-Parameter Figures.

Tables mentioned in last column are those in the references.

Abbreviation	Reference	Remarks
M	MacGregor et al. (1961)	p-p; 68.3, 95 MeV; Table III
MAD	MacGregor et al. (1954)	p-p, n-p; 142 MeV; Table IX, Column 6
MA	MacGregor and Arndt (1965)	p-p, n-p; 95, 142, 210, 310 MeV; Table VII in following reference.
N	Noyes et al. (1965)	p-p, n-p; 25, 50 MeV; Table VII
S'	Gotow et al. (1962)	p-p; 213 MeV; Table VII
SIGA SIGO	Signell et al. (1964a)	p-p; 51.8 MeV; Table II, 6 parameter searches
S_{13} S_{16}	Signell et al. (1964b)	p-p; 213 MeV; Table VIII, 13 and 16 parameter searches
SM	Signell and Marker (1964)	p-p; 142 MeV; Table III, OPE (11)
S	Signell (1964a)	p-p; 50 MeV; Table IV, 5 parameter search
	(1964c)	p-p; 96.5, 310 MeV; Figs. 1-11, modified phase analysis
	(1965)	p-p; 27.6 MeV; Table IV
K	Kazarinov et al. (1962)	p-p, n-p; 40, 95, 147, 210, 310 MeV; Tables IV, V, VI, Set 1
H	Hoshzaki et al. (1963)	p-p; 52 MeV; Table 1
P	Perring (1962)	p-p; 68.3, 98 MeV; Tables 5 and 7, Solution 1
	(1963)	p-p, n-p; 142 MeV; Tables 1 and 2
BP	Batty and Perring (1964)	p-p, n-p; 50 MeV; Tables 1 and 2

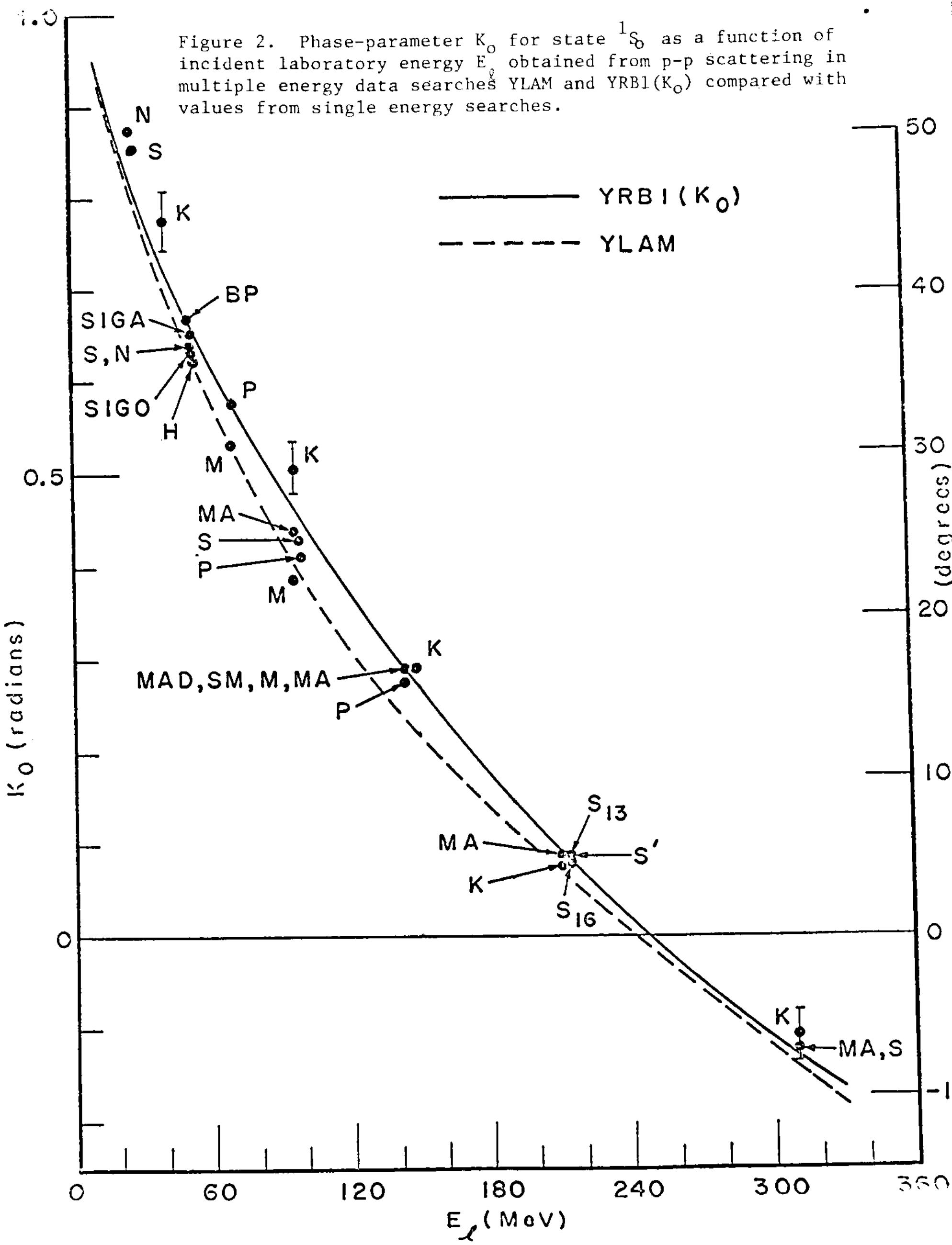


Figure 2. Phase-parameter K_0 for state $^1S_0$ as a function of incident laboratory energy E_ℓ obtained from p-p scattering in multiple energy data searches YLAM and YRB1(K_0) compared with values from single energy searches.

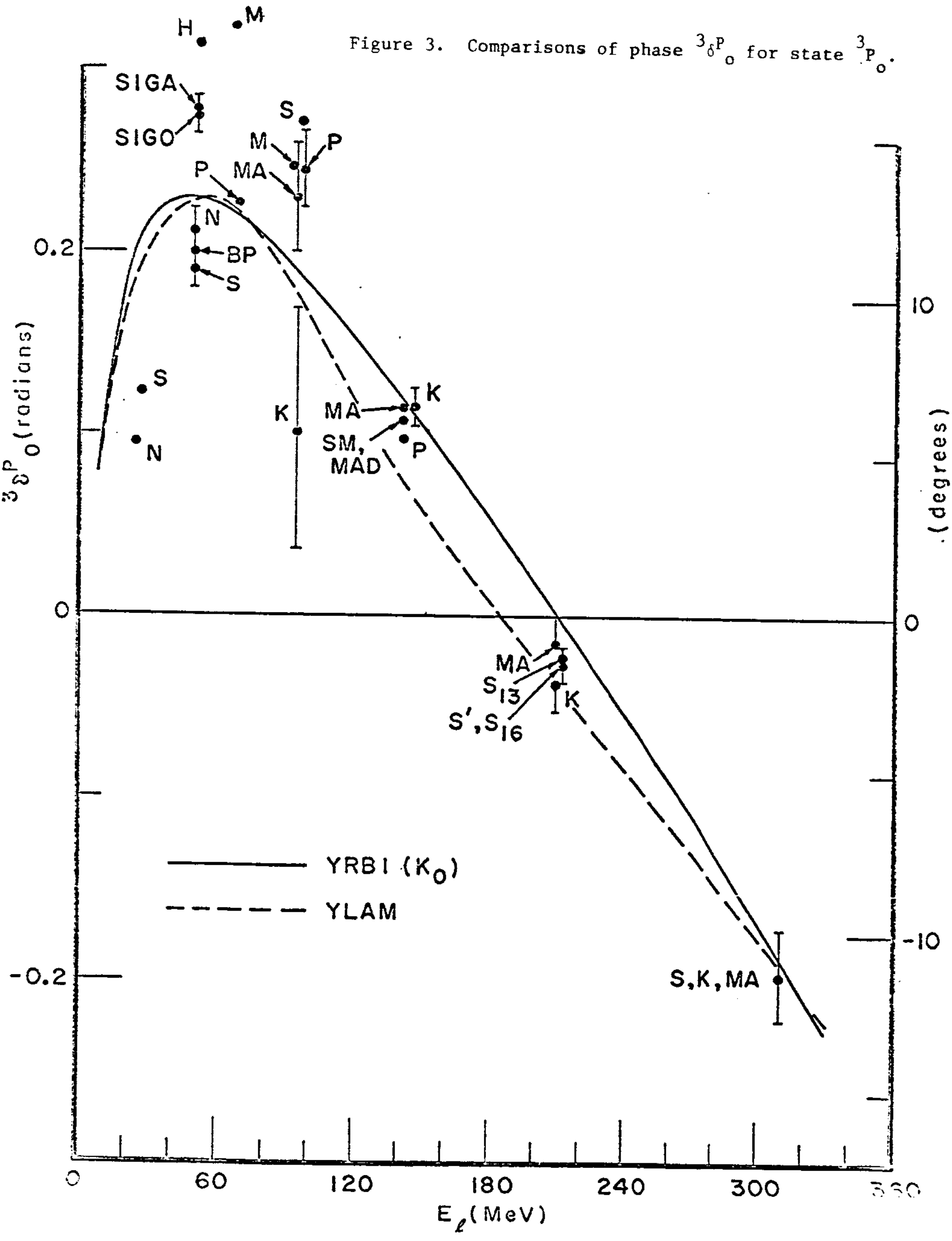


Figure 3. Comparisons of phase $^3\delta P_o$ for state ^3P_o.

Figure 4. Comparisons of phase $^{3}\theta F_{4}$ for state $^{3}F_{4}$.

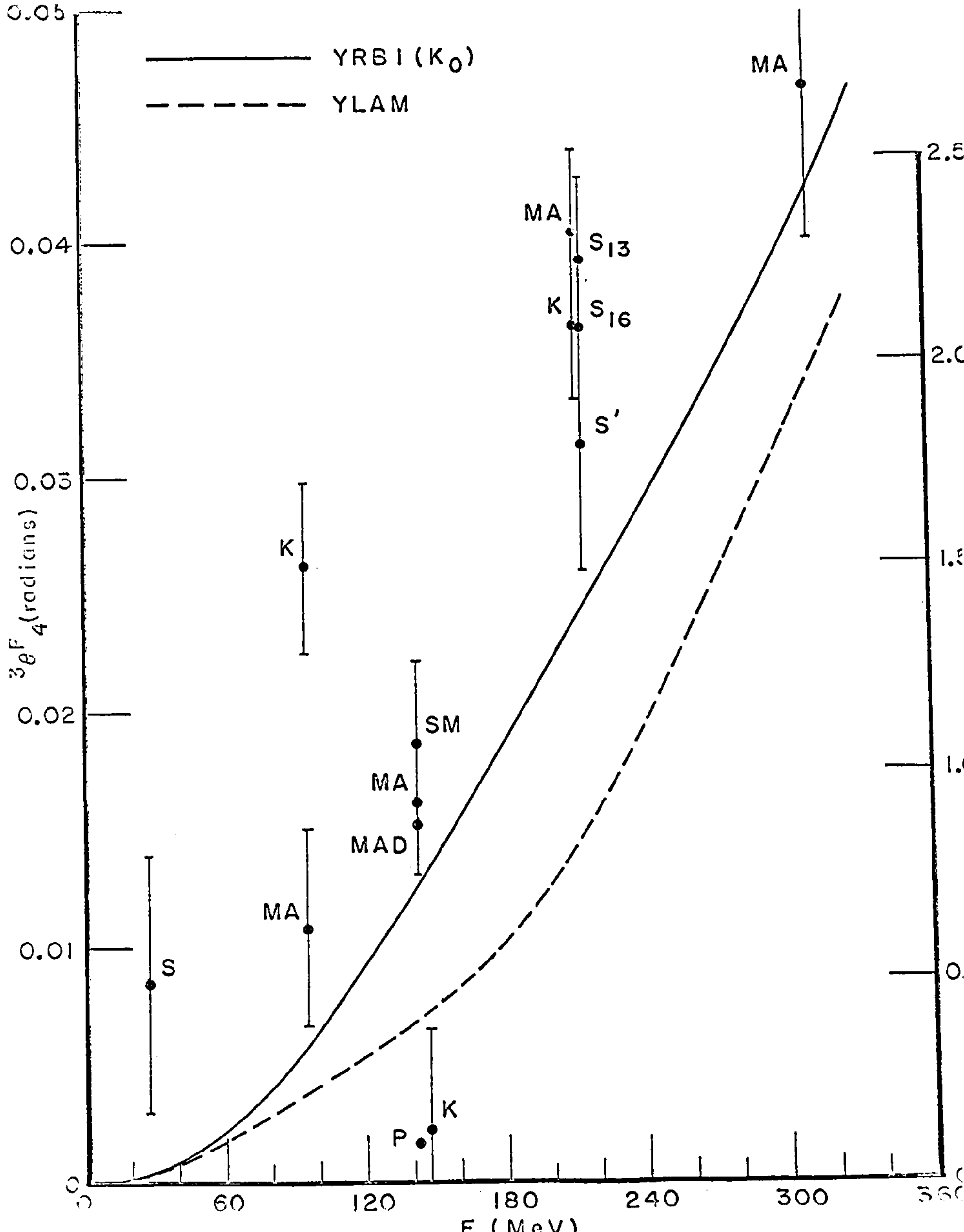


Figure 5. Phase-parameter ${}^{3}\theta^{S}_{1}$ for state ${}^{3}S_{1}$ obtained in multiple energy data searches YLAN3M and YLAN4M compared with values from single energy searches.

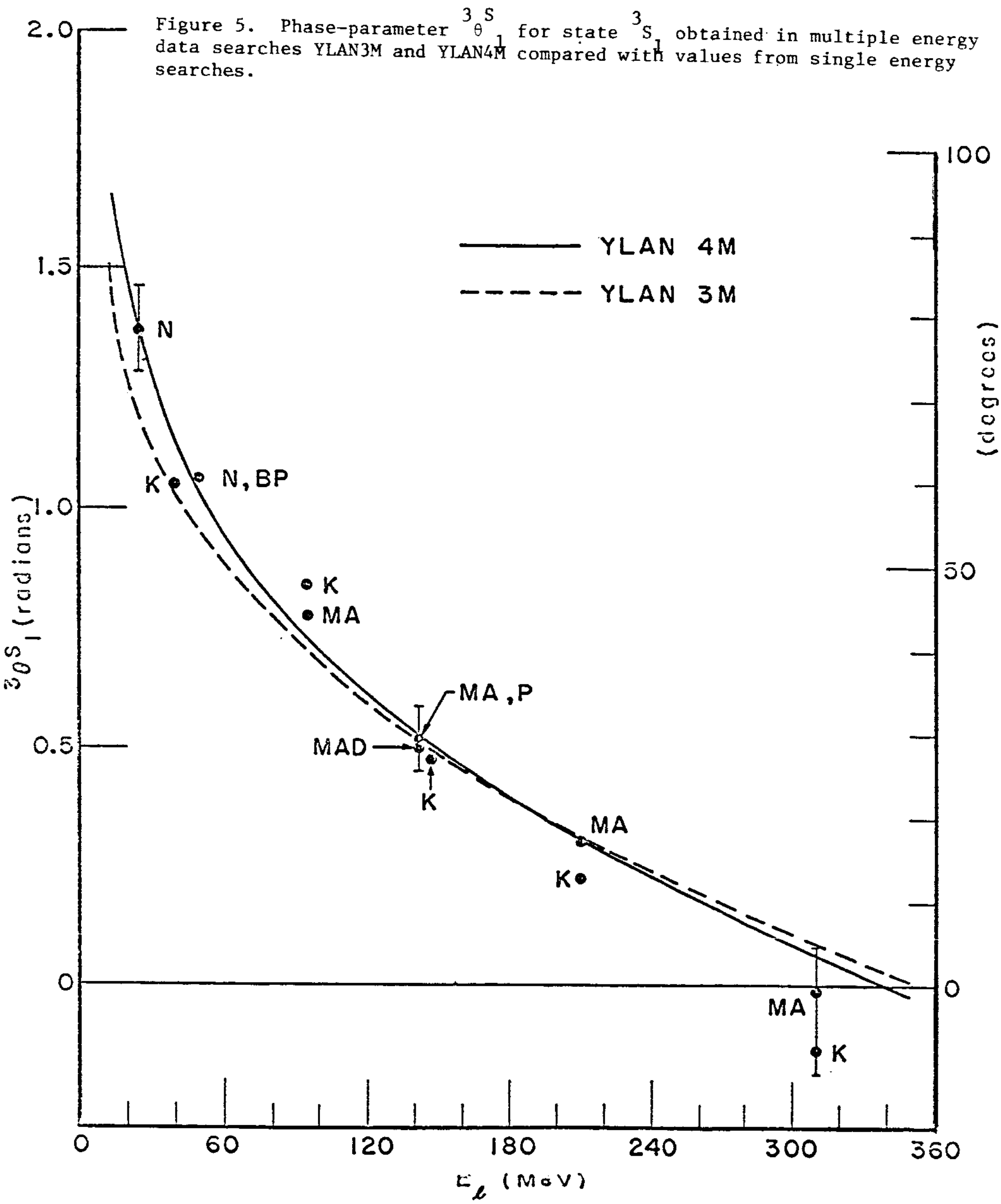
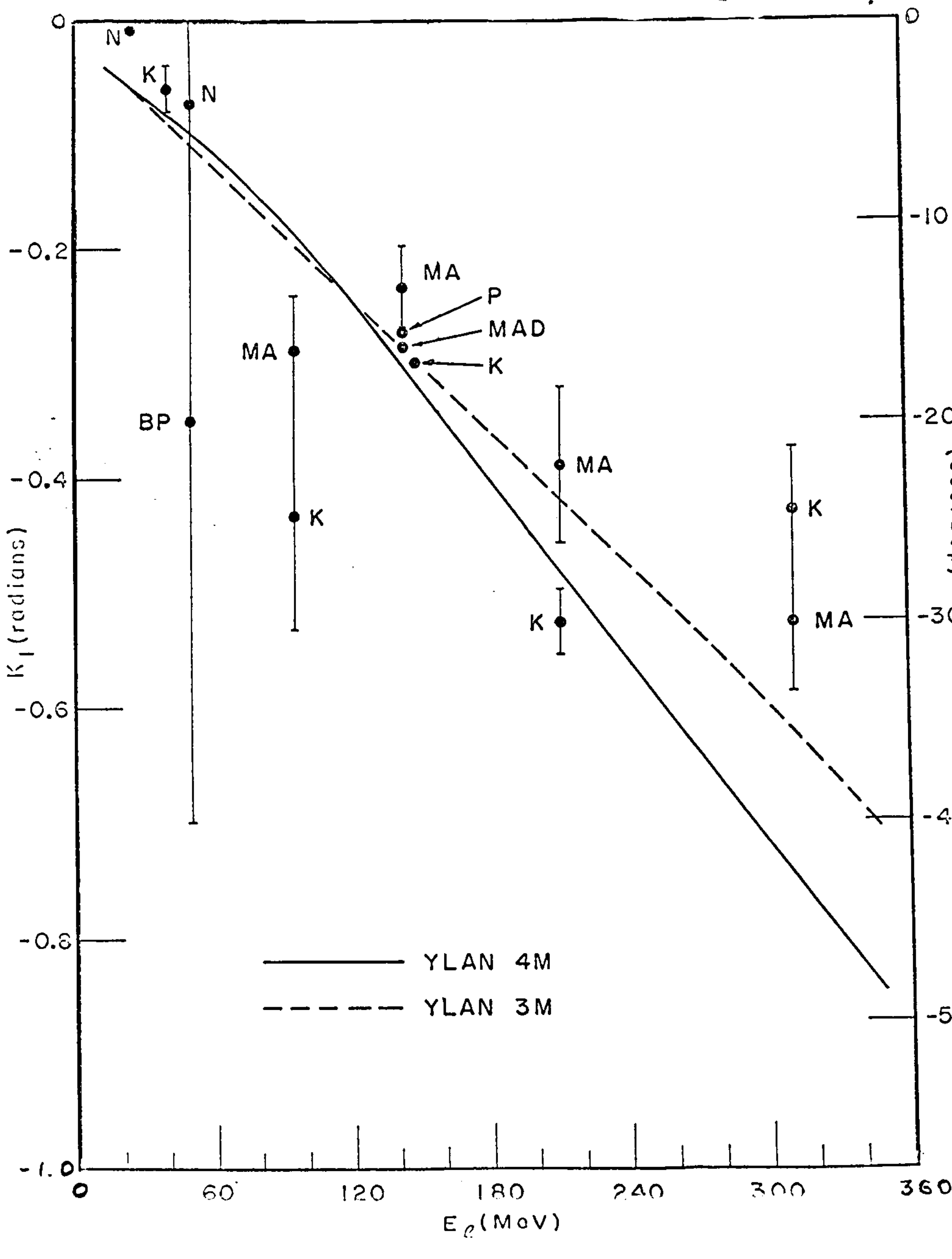


Figure 6. Comparisons of phase K_1 for state $^1P_1$.



n-p	14.3 ± 1.0	Ashmore et al. (1962)	350 MeV; σ_{Ω} only; cf. Eq. (VI.22) of this chapter.[b]
p-p	13.7 ± 0.8	Breit et al. (1962b)	YLAM, L>3 in OPE, magnetic moment effects included.[c]
n-p	14.1 ± 0.8		"YLAN3M-350", I=0,1; e_{π}^2 = 0,1; factor $D^{\frac{1}{2}}$ included.
	13.7 ± 1.6		"YLAN3M-350", 0 ; = 0,1; " "
	14.9 ± 3.4		" L>4" 1 0,1; " "
p-p	(14.7 ± 0.9		K_4 non-OPE; otherwise L>3 in OPE; magnetic moment effects included.
	15.5 ± 1.0)		Same as above but magnetic moment effects omitted.

A. Ashmore, W.H. Range, R.T. Taylor, B. M. Townes, L. Castillejo and R.F. Peierls, Nucl. Phys. 36, 258 (1962).

G. Breit, M. H. Hull, Jr., F. A. McDonald and H.M. Ruppel, Proc. 1962 International Conference on High-Energy Physics at CERN (J. Prentki, ed.), p.134. CERN, Scientific Information Service, Geneva, Switzerland).

Figure 7. Values of g_o^2, 1962 period.

	Arndt and MacGregor (1965)		Searches at many energies with 363 p-p and 341 n-p data and with numbers of adjustable parameters as follows[e]
p-p	13.8 ± 1.9		35
p-p	13.9 ± 1.0		24
(p-p)+(n-p)	13.0 ± 0.7		58
(p-p)+(n-p)	13.4 ± 0.7		66
p-p	15.1 ± 0.4 ($\pm$ 0.6)	Yale (1965) unpublished	Searches at many energies; ~780 data. Effects of magnetic moments included.
n-p	13.9 ± 0.9 $\pm$(1.1)		Searches at many energies with ~860 data. Effects of magnetic moments included. Detailed mass treatment. Numbers in ( ) are standard errors with $D^{1/2}$ included.

R. A. Arndt, and M. H. MacGregor, University of California Lawrence Radiation Laboratory Report UCRL-14252. (unpublished).

Figure 8. Values of g_o^2, 1965 period.

Values of $(g_o^2)_{best}$ in n-p scattering.

(g_o^2 preset at 10.5, 12.5, 14.0 and 17.5)

	Coulomb Corrected		Coulomb Corrected- - Coulomb Uncorrected	
Method	Cubics	Parabolas	Cubics	Parabolas
Straight Line	15.10	15.54	1.04	1.16
D_{min}^S	14.608	14.89	0.76	0.73
D_{min}^g	14.625	14.88	0.81	0.70

Values in rows marked D_{min}^S and D_{min}^g are obtained respectively from minima of D_{min}^S of D for variations of "searched set" of phases and from D_{min}^g, the minima obtained in further variations of g_o^2 yielding values $(g_o^2)'$ of g_o^2 corresponding to D_{min}^g.

Figure 9.

Values of $(g_0^2)_{best}$ in p-p scattering.

(g_0^2 preset at 10.5, 14,0, 17.5)

Method	Cubics	Parabolas
Straight Line	15.04	15.02
D_{min}^S	----	15.06
D_{min}^g	15.09	15.10

Figure 10. Values of $(g_0^2)_{best}$ in p-p scattering.

Values of g_0^2 for Violation of Charge Independence in $^1S_0$, for Coulomb Corrections alone, and without Corrections.

(g_0^2 preset at 12.5, 14.0, 17.5)

	(D_{min}^S, g_0^2)	$(D_{min}^g, (g_0^2)')$	Str. Line
(A) With Ch. Ind. Vi.	14.69	14.69	15.04
(B) Coul. Corr. alone	14.83	14.82	15.20
(C) No Corr.	13.98	13.98	14.00
(B-A) / (B-C)	15.6%	15.2%	13.3%

Figure 11. Values of g_0^2 for Violation of Charge Independence in $^1S_0$ for Coulomb Corrections alone, and without Corrections. The last row gives the decrease in the shift produced by the Coulomb effect that is caused by the apparent violation of charge independence in the $^1S_0$ state.

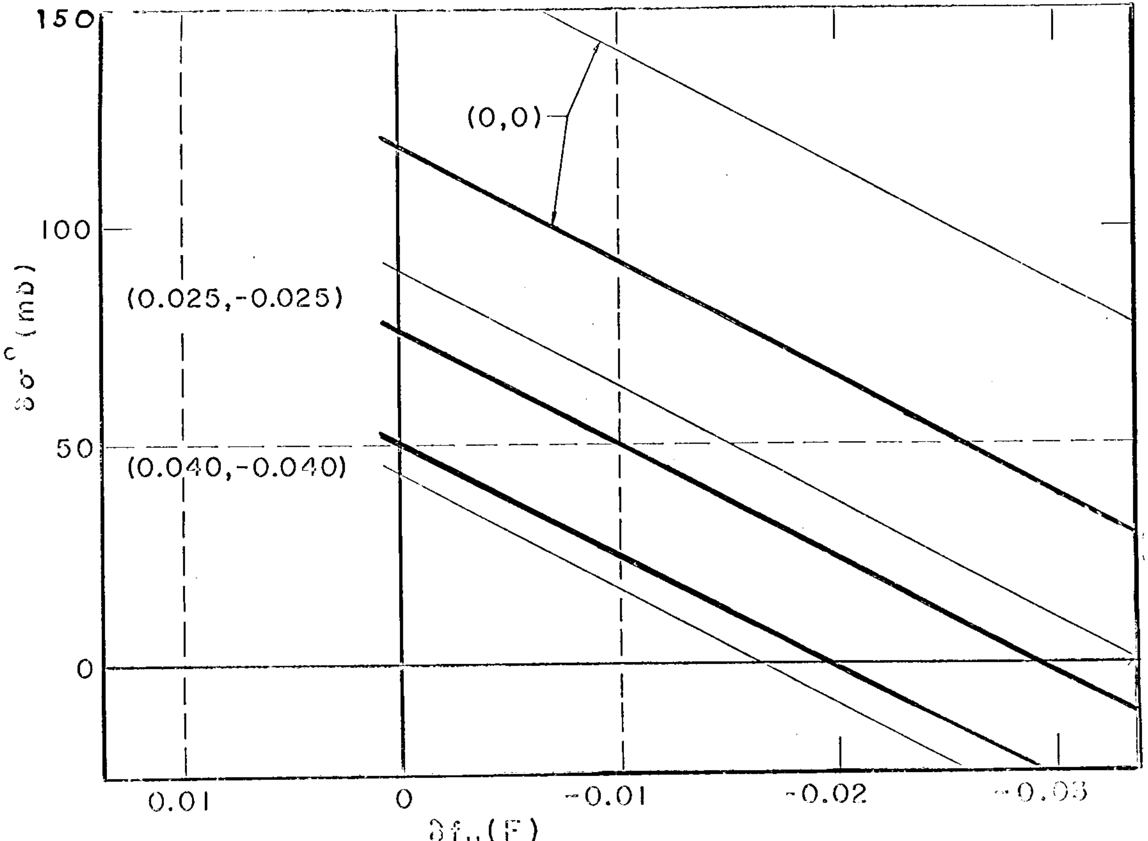


Figure 12. Relationship of systematic errors $\delta\sigma^o = \delta\sigma^{tot}_{n-p}$ (E = 0) and $\delta f_H = \delta f_{n-p}$ (E = 0) for $({}^1r_o)_{n-p}$ = 2.7F assuming singlet and triplet shape parameters (0.040, -0.040), (0.025, -0.025),(0,0). Heavy lines for E < 5 MeV, light lines for E < 10 MeV. Light dashed horizontal and vertical lines for standard deviations of σ^o and f_H. Systematic error correction of-0.15% to σ^o assumed at 0.493, 3.204, and 5.874 MeV.

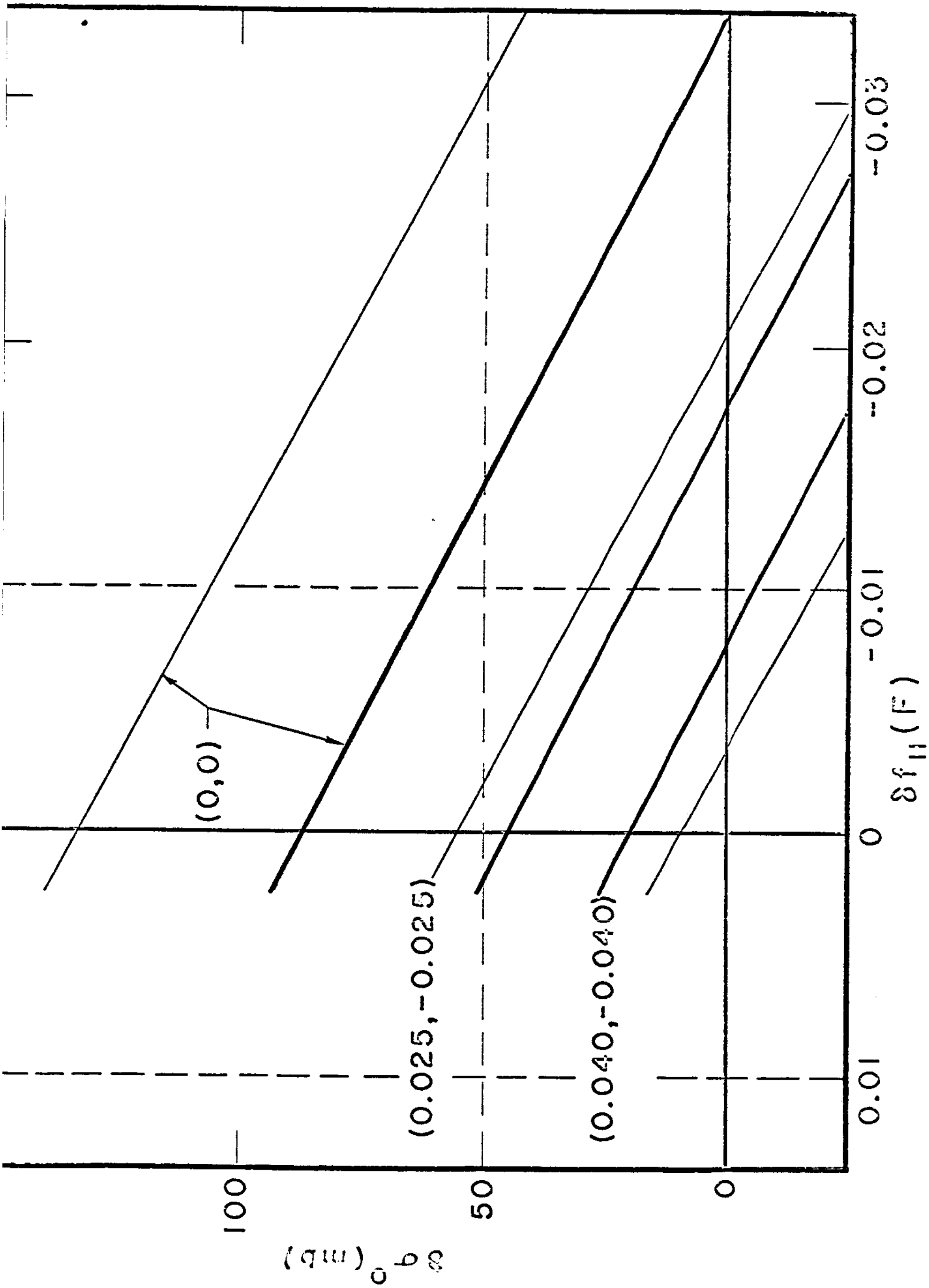


Figure 13. Conventions as in Figure 12. Correction for systematic errors changed to -0.30%.

SIGNELL: Is it true that the difference between YLAM and YRB 1 is mainly wheth singlet G_4 is one-pion-exchange or not?

BREIT: In 1959-1960 period, there were two fits that were competitors. YRB 1 started from the Gartenhaus potential, the R comes from Rochester. There was line in the many dimensional phase shift space which connected YRB 1 and YLAM. YLAM was searched down better than YRB 1 and had a lower X^2, but the energy dependence of k_0 was more reasonable in the case of YRB 1 than in the case of YLAM I think a good part of the reason might have been that in the search YLAM a gro of phase shifts was reassigned to the OPE at the 150 MeV and the fact that that change was made reflected itself in a region of high curvature for the YLAM k_0 phase energy dependence. However, it didn't appear in shape of the Amati-Leade Vitale curves. Now, I know that they don't completely fit the experiment, but might think that their shape had some meaning. So, the YRB 1 k_0 was used in or to get started on the new YRB 1 k_0 search and the other phases were taken from YLAM and the compromise between the two resulted in the YRB 1 k_0. The k_4 is so times used one way, sometimes another.

ROSE: In your comments about eliminating the discrepancy between the n-p and p scattering lengths, you mentioned that you had looked at certain ideal experime Could you describe what these ideal experiments were and what sort of accuracie you would want?

BREIT: Oh, you mean what I talked about as mock data?

ROSE: Yes.

BREIT: You are referring to the single energy searches with mock data, I belie

In connection with the difference in the two coupling constants?

ROSE: No, in respect to the discrepancy between the n-p and p-p scattering lengths.

BREIT: Oh, the effective range...I think for that plan what was done was...one of the last items I was talking about. We took the current fits of YRB 1 k_0 and YLAM 4m. Numbers were calculated from the existing correction functions. From those phases, observables were calculated - values of σ, $\sigma(\theta)$, D, R, C_{nn}, etc. Then standard errors were assigned either through looking through the literature, or seeing what one might think would be possible to obtain in a measurement or by consulting some people; in the case of p-p data, L. C. Northcliff; in the case of n-p data, Drake, and afterwards, Perkins (Los Alamos). Then the error matrix was calculated. Now one might think that such a procedure is not realistic because it does not include the scatter in the experimental points that always exists around any fit - the scatter of the mean values that an experiment obtains. On the other hand if you look at the equations, you see that in the error matrix calculation the scatter really does not enter. It enters only in the last factor that we apply when we multiply by square root of our D; χ^2/no. of observations. But just to be entirely on the safe side we took the paper by Caiffra, et. al., on the 310 MeV p-p data and did it both ways - repeated their work, got essential agreement with their numbers and also put in values calculated from the phase shifts that were obtained from that fit and saw that we got the same answers. I might add that the number of experiments that was used was usually quite large - usually larger than what is available at present for any one energy range.

PION-NUCLEON PHASE SHIFT ANALYSES

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INTRODUCTION

Our knowledge of the pion-nucleon interaction has increased tremendously in the last five years. And yet we seem to be just as confused concerning the fundamental principles underlying this strong interaction as we were five years ago. Many scattering experiments have been performed, many phase-shift analyses have been made, and several attempts at semi-theoretical calculations of the phase shifts have been published. We shall consider only those phase shift calculations and analyses that attempted to determine the phase shifts over wide energy ranges. The available phase shift analyses at one energy are in agreement with one or more of the analyses done over wide energy ranges.

The dominant characteristic of the pion-nucleon interaction is the resonance behavior, which apparently occurs in about one-half of the states below 1 Bev. The known number of resonances has more than doubled in the last few years. Table I lists the resonances that are presently considered as definite or strong possibilities up to 1000 MeV.

TABLE I. POSSIBLE PION-NUCLEON RESONANCES (0-1000 MeV)

State ($\ell_{2T,2j}$)	Pion Laboratory Kinetic Energy (MeV)	Π-N C.M. Energy (MeV)
P_{33}	∿ 200	∿ 1240
P_{11}	↓ 600	↓ 1500
S_{11}	600	1500
D_{13}	630	1530
S_{31}	850	1660
D_{15}	840	1650
F_{15}	900	1690
S_{11}	900	1690

In discussing the various resonances we shall use the following terminology:

Define $x = \left.\frac{\Gamma_{el}}{\Gamma}\right]_{E = E_{res}}$, where Γ = total width of resonance and Γ_{el} = elastic width of resonance. ($\Gamma = \Gamma_{el} + \Gamma_{in}$, where Γ_{in} = inelastic width of resonance.) Then a resonance is an

"elastic resonance" when $x = 1$
"inelastic resonance when $1 > x > \frac{1}{2}$
} Phase shift passes through 90° at resonance

"highly-inelastic resonance" when $\frac{1}{2} > x > 0$
} Phase shift passes through 0° at resonance

The phase shift behavior indicated is strictly true only if the resonance has no background. (See Table II.)

Since resonance behavior is the dominant characteristic, after briefly considering the theoretical calculations, we shall discuss the various phase shift analyses in terms of the resonances they contain. Then we shall consider the non-resonant phase shifts.

THEORETICAL CALCULATIONS

The recent theoretical calculations by Carruthers[1)] at Cornell University; by Donnachie, Hamilton and Lea[2)] (DHL) at University of London; and by Kikugawa, Hiroshige, and Ino[3)] at Hiroshima University are the most comprehensive. All three do essentially the same thing, i.e., they use experimental information about Π-N and Π-Π resonances to calculate the Π-N phase shifts.

By means of partial-wave dispersion relations with nucleon exchange and pion-nucleon resonance exchange in the u channel, Carruthers[1)] showed how the various exchanges mutually induce resonances. He ignored possible t channel meson resonance exchanges, and thus did not claim quantitativeness. The main result is a

TABLE II

Resonance Equation:

$$A_{res} = \frac{-\frac{1}{2}\,\Gamma el}{(E-E_{res})+\frac{1}{2}i\Gamma} = \frac{\chi}{\varepsilon - 1} = \frac{1}{2i}\,(\eta e^{2i\delta} - 1), \quad = \frac{2}{\Gamma}\,(E_r - E)$$

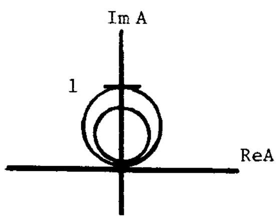
Unitary combination of resonance and background:

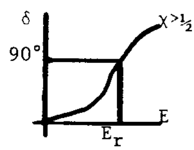
$$A = A_{back} + \underbrace{\eta\ back\ e^{2i\delta\ back}}_{\delta\ back} A_{res} = A_{back} + A_{res} + 2iA_{back}A_{res} = \frac{1}{2i}(\eta e^{2i\delta}-1)$$

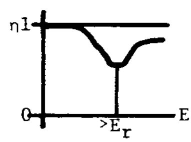
∴ Resonance behavior:

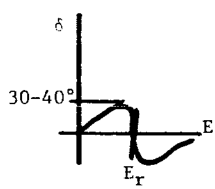
In terms of partial-wave amplitude | In terms of phase shifts and absorption parameters

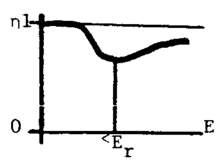
No background:



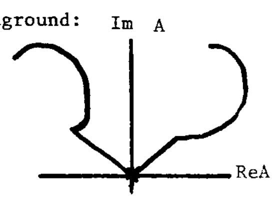


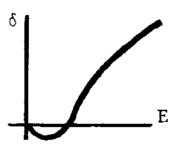


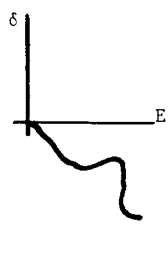


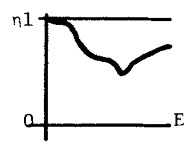


Background:









"super-bootstrap" principle which states that a resonance in a $T = \frac{1}{2}$, $j = \ell - \frac{1}{2}$ state is induced by exchanges of $T = 3/2$, $j' = \ell' + \frac{1}{2}$ states, where $\ell' \leq \ell$; and that a resonance in a $T = 3/2$, $j = \ell + \frac{1}{2}$ state is induced by exchanges of $T = \frac{1}{2}$, $j' = \ell' - \frac{1}{2}$ states, where $\ell' \leq \ell$.

Donnachie and Hamilton[2c] disagree with some of the details of the "super-bootstrap" principle, and lay the blame on the neglect of the short range interaction. Instead of evaluating the dispersion relation for the partial wave amplitude $f_\ell(s)$, they use $F_\ell(s) = f_\ell(s)/q^{2\ell}$ where q is the c.m. momentum. The factor $q^{2\ell}$ suppresses the short range part of the interaction. The predictions as to which states should resonate agree with Carruther's calculation, but the details differ. Using a peripheral approximation[2a] they were able to calculate the nonresonant P, D, and F wave phases up to 400 MeV, with results in decent agreement with experimental values. Later, by means of unitarity requirements[2b], they were able to estimate the short range parts of the pion-nucleon interaction, and thus extend their calculations to ~650 MeV.

The work of Kikugawa et al[3] in Japan is simpler; it utilizes what is variously called the "K matrix" or "damping theory" method. That is, the S matrices for a given partial wave are calculated for the appropriate one-particle-exchange processes in the s, u, and t channels using the experimental masses; then the K matrix, or tan δ, is set equal to the sum of the S matrices. One has by this recipe

$$K = \tan \delta = \sum_i S_i \,.$$

The partial-wave S matrix is then calculated by the standard formula

$$S = \frac{1 + iK}{1 - iK} \,,$$

thus achieving unitarity. The coupling constants are varied (and, also, the masses are varied within limits) to fit the experimental phases. The agreement with experiment up to ~300 is good, and the coupling constants and masses obtained are reasonable.

RESONANCES IN PHASE SHIFT ANALYSES

There are two basic kinds of methods that have been used to obtain phase shifts: (1) Single-energy analyses and (2) energy-dependent analyses. In the former, phase shifts are obtained at each individual energy and in the latter an energy parameterization of the phase shifts is used. At Livermore[4)] we chose to do an energy-dependent analysis because of the success that had been achieved in similar nucleon-nucleon phase shift analyses. (A pion-nucleon energy dependent analysis was done by Anderson[10)] in 1956 in which he used a parameterization similar to the Livermore parameterization. He did not obtain good results because of lack of data.)

At about the same time we began our work, weveral other individuals or teams began earnestly attempting to do extensive phase shift analyses over wide energy ranges. Table III lists the various extensive phase shift analyses that are currently available. (See Table III.)

Preliminary Livermore results were reported at the Sienna Conference in 1963 and in the author's MIT Thesis in 1963.[11)] This analysis used a parameterization in which relativistic Breit-Wigner resonance[12)] forms with variable parameters could be used for any state, with the background and resonant phase shifts and absorption parameters expressed as power series in the c.m. momentum with variable coefficients. Unexpectedly, besides the P_{33} resonance at ~200 MeV and a D_{13} resonance at ~630 MeV, the P_{11} state exhibited a resonance behavior at ~600 MeV.

Table III. Extensive Phase Shift Analyses

Investigators (Ref.)	Location	Abbreviation	Type of Analysis	Energy Range	Resonances
Roper, Wright, and Feld (4)	MIT, Livermore Laboratory	Livermore	Energy dependent	0-350 0-700 300-700	P_{33}, P_{11}, D_{13}
Bransden, O'Donnell, and Moorhouse (5)	Univ. Durham, Rugherford Laboratory	Rutherford	Energy dependent	100-350 300-700 700-1100	P_{33}, S_{11}, D_{13} D_{15}, F_{15}
Auvil, Donnachie, Lea, & Lovelace (6)	Univ. of London	(ADLL) London	Single energy	300-700 700-1000	D_{13}, S_{31}, D_{15} F_{15}, S_{11}
Bareyre, Bricman, Stirling, & Villet (7)	Saclay	Saclay	Single energy	0-1000	P_{33}, D_{13}, P_{11}, S_{11} S_{31}, D_{15}, F_{15}
Hull and Lin (8)	Yale Univ.	Yale	Energy dependent	0-350	P_{33}
Cence (9)	Hawaii Univ.	Hawaii	Single energy	300-700	none

Bransden, et. al.[5] at Rugherford Laboratory used an energy dependent parameterization that was designed specifically to satisfy a partial-wave dispersion relation. The left cut is approximated by a series of poles with variable parameters, and the right cut by a ratio of polynomials with variable coefficients. Thus, resonances can occur in any state as the data please. They get the P_{33} resonance at ∿205 MeV in the 100-350 MeV analysis. In the 300-700 MeV analysis they get two solutions, both with D_{13} and S_{11} resonances and a possible P_{11} resonance at ∿600 MeV:

Solution #1: D_{13} inelastic resonance at ∿625 MeV.
S_{11} highly-inelastic resonance at ∿690 MeV.

Solution #2: D_{13} inelastic resonance at ∿630 MeV.
S_{11} inelastic resonance at ∿612 MeV.

(The S_{11} resonance has been shown by Hendry and Moorhouse[6c] to most probably by due to a resonance in the η-N inelastic channel.) This group has recently completed a 700-1000 MeV analysis[6b] which contains an inelastic D_{15} resonance at 840 MeV, an inelastic F_{15} resonance at 890 MeV, and a second inelastic S_{11} resonance at ∿910 MeV. It does not have the S_{31} resonance that the two analyses discussed below have.

The London single-energy analysis of Auvil, et. al.[6] has a definite D_{13} resonance at ∿620 MeV, and a possible P_{11} resonance with background at ∿600 MeV. It also contains an S_{11} behavior that is consistent with a highly-inelastic resonance in that state at ∿700 MeV. This was a series of single energy analyses in which the DHL values of the small phase shifts were used as "data" and the large phase shifts were varied for best fit. Then the complete set of phase shifts were used to reevaluate the partial-wave dispersion relations in the small phase shift calculations. The cycle was repeated until agreement between input and output was achieved for the small phases. A later extension[6b)] of this analysis to ∿1 BeV contains resonances in the S_{31} state at ∿850 MeV with a large background, in the D_{15} state at ∿840 MeV, in the S_{11} state at ∿900 MeV, and in the F_{15} state at ∿900 MeV. The S_{31} and D_{15} are highly-inelastic and the others are inelastic. By parameterizing the imaginary part of the partial-wave amplitude on the right-hand cut by a series of functions of energy with parameters determined by fitting the experimental phases, they were able to do dispersion relation calculations for the resonant phases except the P_{11}, as well as for the non-resonant phases. The results compare well with the experimental phases.

The Saclay single-energy analysis of Bareyre, et. al.[7)] has the same resonances as the London analysis plus a definite P_{11} resonance at ∿600 MeV. They were the first to report the highly-inelastic D_{15} resonance at ∿850 MeV. The double S_{11} resonance behavior is present, as is the S_{31} highly-inelastic resonance at ∿850 MeV. They had previously found a unique solution at 410 and 492 MeV. They then required that a solution at higher energies must be consistent with the unique 492 MeV. solution. Thus, a unique solution was obtained at higher energies. Around 700 MeV there was a possibility for two solutions, but by requiring continuity with higher energies they rejected one of them - one in which the P_{11} phase shift decreased after reaching 100°.

The 0-350 MeV analysis done by Hull and Lin[8)] at Yale University is an energy-dependent one similar to the Yale nucleon-nucleon analysis.[13)] They did extensive phenomenological and semiphenomenological fits. In the latter they calculated the F waves by means of dispersion relations, rather than determined them from data. Their results and the Livermore 0-350 MeV results[5c)] are in fairly good agreement.

The single-energy analysis by Cence[9)] at University of Hawaii contains no resonances in the 300-700 MeV range; all of the phases are with ±45°. This type of solution was discarded in the analyses at Livermore and Saclay because of poor fit to the data. Also, Draxler and Hüper[14)] at Karlsruhe claim it is inconsistent with forward dispersion relations. The analysis is a single-energy one with some degree of smoothness used as a criterion in selecting the solution.

Nonresonant Phases

Considering only states up to $\ell = 3$, the definitely nonresonant phases below 1 BeV are P_{31}, P_{13}, D_{33}, D_{35}, F_{17}, and F_{37}.

The analyses that have been extended to 1 BeV have the F_{37} phase steadily increasing such that one can say that it probably is the main cause of the 1350 MeV bump in the $\pi^+ - p$ total cross section. Of course, this bump may be as complicated as are the 600 and 900 MeV bumps, in which case there may also be resonances in any of the other $T = 3/2$ states.

The F_{17} state is a particularly innocuous one - the phase is practically zero everywhere and there is no appreciable absorption up to 1 BeV.

The D_{33} phase hovers about 0^o. Some analyses have it slightly positive at some energies and slightly negative at others. Some analyses have it slightly negative everywhere. DHL[2] predict it to be negative. The absorption is not very large - an $\eta \geq 0.8$ in the analyses.

The other three states, D_{35}, P_{31}, and P_{13}, all have negative phases as predicted by DHL[2] and do not exhibit much absorption. The largest phase in magnitude is the P_{31} which reaches as much as -30^o at 1 BeV. Again, DHL predict that it should be largest.

CONCLUSION

The fact that there is agreement among the many recent analyses about the gross features of the pion-nucleon phase shifts gives one confidence that these gross features are correct. The one exception is the analysis by Cence.[9)] His solution is apparently one that is easily rejected by using energy-dependent analyses or dispersion relations.

This agreement in coarse detail among the Livermore, Rutherford, London, Saclay, and Yale solutions is a good basis for hope that the intermediate energy pion-nucleon phase shifts will soon be uniquely known in detail. Also, justified hope exists that the phase shift analyses can be extended to much higher energies as data becomes available.

As representative of the recent results, we show in Figures 1 through 5 the S_{31}, S_{11}, P_{11}, D_{15}, and F_{15} results of Bareyre, et. al.[7] at Saclay. They used more data than did Donnachie, et. al.[6] but did not require satisfaction of partial-wave dispersion relations. The biggest disagreement among the different solutions is the energy dependence of the P_{11} phase shift. The importance of this disagreement is enhanced by the fact that a P_{11} resonance does not fit into the SU_6 scheme of particle classification, whereas the S_{11}, S_{31}, and D_{15} resonances do fit. A careful determination of which experiments best determine the P_{11} state needs to be made.

It seems that we are at the point where much reflection needs to be made as to what experiments should be performed to distinguish among these various solutions. At Livermore we developed a technique for plotting observables versus angle at any energy or versus energy at any angle. We were able to do this for any set of phase shifts when we could fit an energy parameterization to them. Thus we could readily find energy and angle ranges where different solutions had greatest disagreement. Also, one or more phases could be varied and the effect on the observables determined. This kind of thing needs to be done for the solutions that are available now. One can be very certain that the most important experiments will be the measurement of the spin-rotation parameters, which have never been measured at any energy. Technology is advanced such that these measurements are now possible. The cost in time and resources is so great that careful determination of what to measure is necessary, and several experimental groups are currently involved in such deliberations.

It is always advisable to check different methods of obtaining physical information against each other. The differing phase shift analysis methods should continually be interchecked. The single-energy analyses oftertimes do not yield unique phase shifts in situations where energy-dependent analyses can give essential uniqueness. But there are inherent weaknesses[4,9] in a stage of given complexity in energy-dependent analyses. Single-energy analyses are helpful in finding these weaknesses. The weaknesses as they are found can be reduced by increasing the complexity, as long as computers are available that can handle the complexity required. So far the computer capabilities have been more than adequate for the task, but the availability of them for these kinds of calculations is another thing. Thus a strong case exists for simultaneously performing both types of analyses with close communication between the performers.

Apparently partial-wave dispersion relations are consistent with the experimental data to a high degree of accuracy. These dispersion relation calculations require a good deal of input information, and can hardly be regarded as calculations from minimum first principles. Such a program of calculation from first principles seems a long way off.

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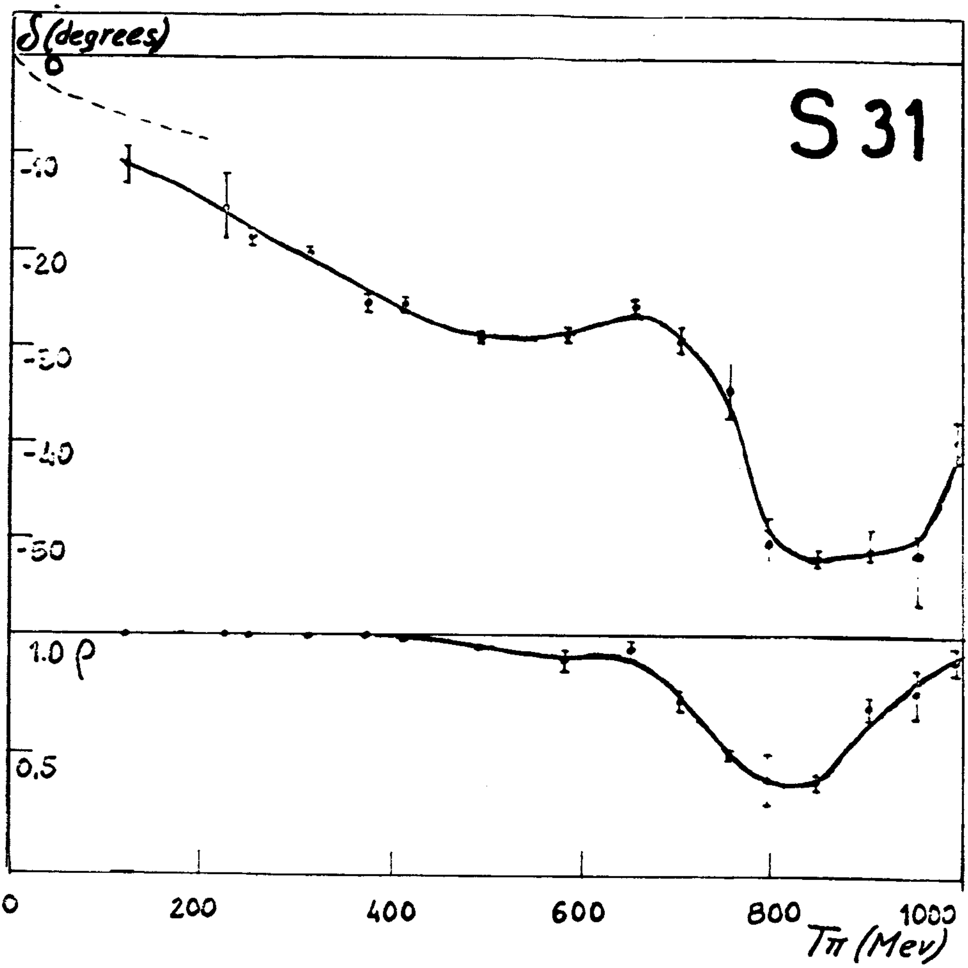
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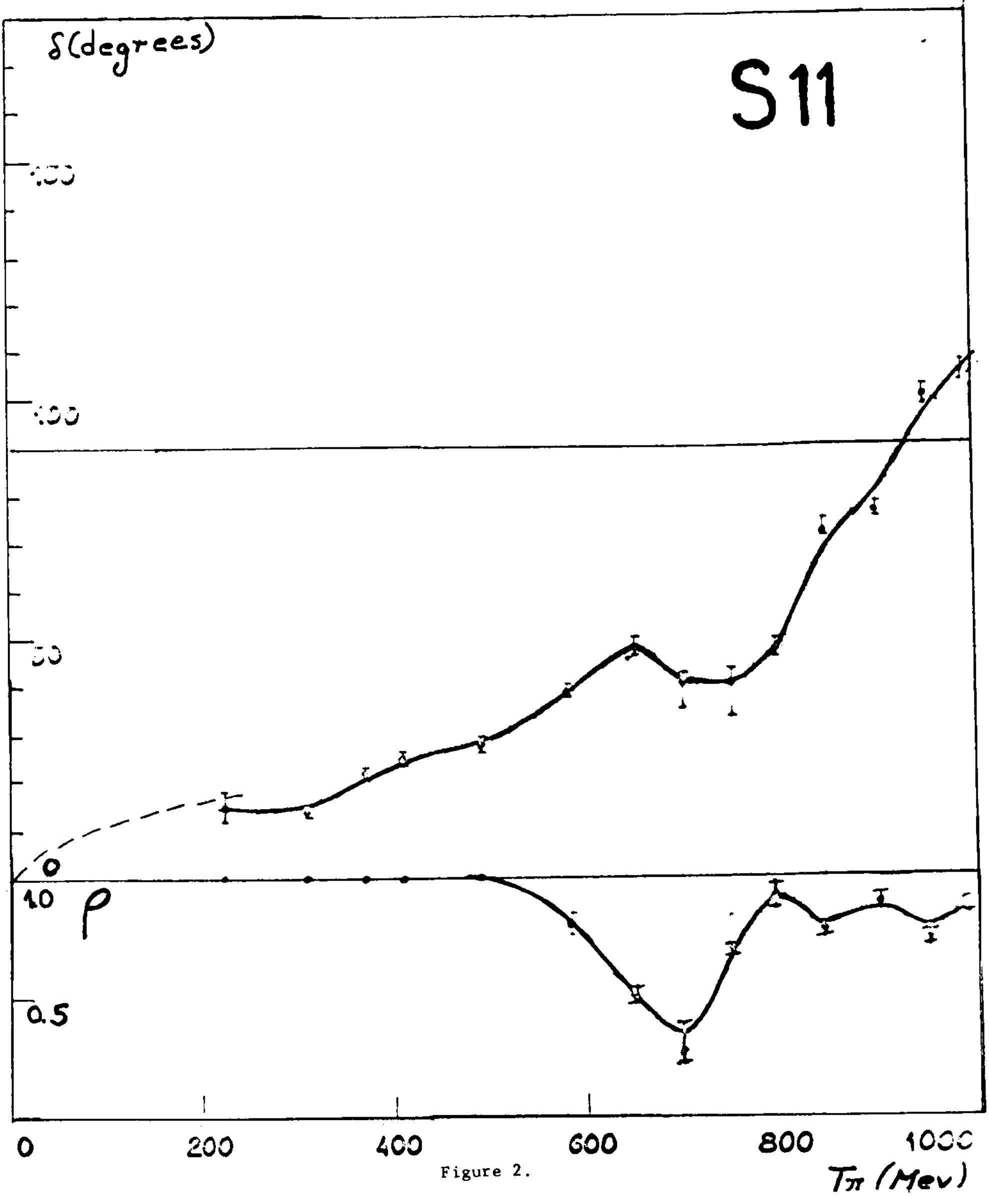

Figure 2.

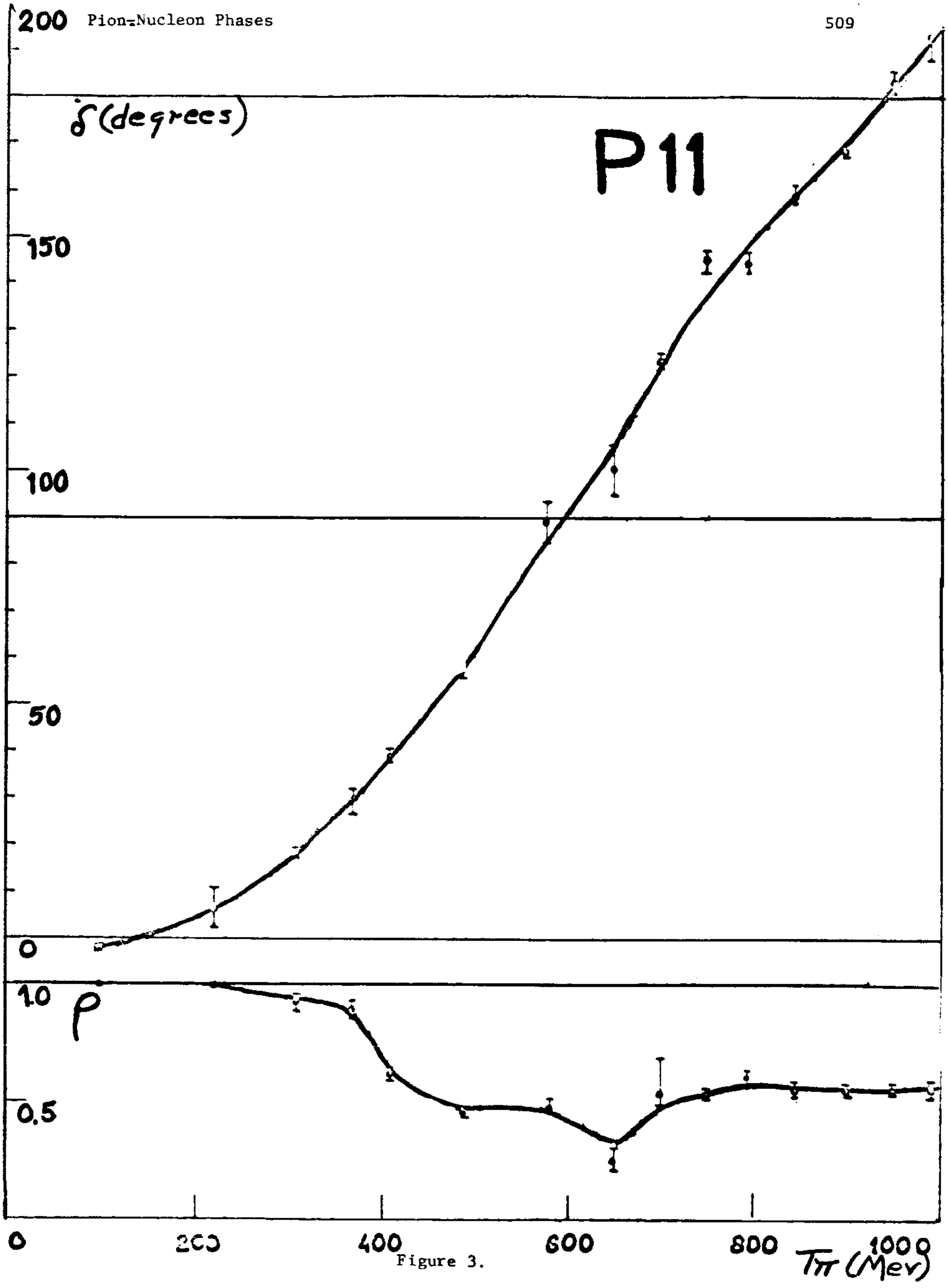
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Figure 3.

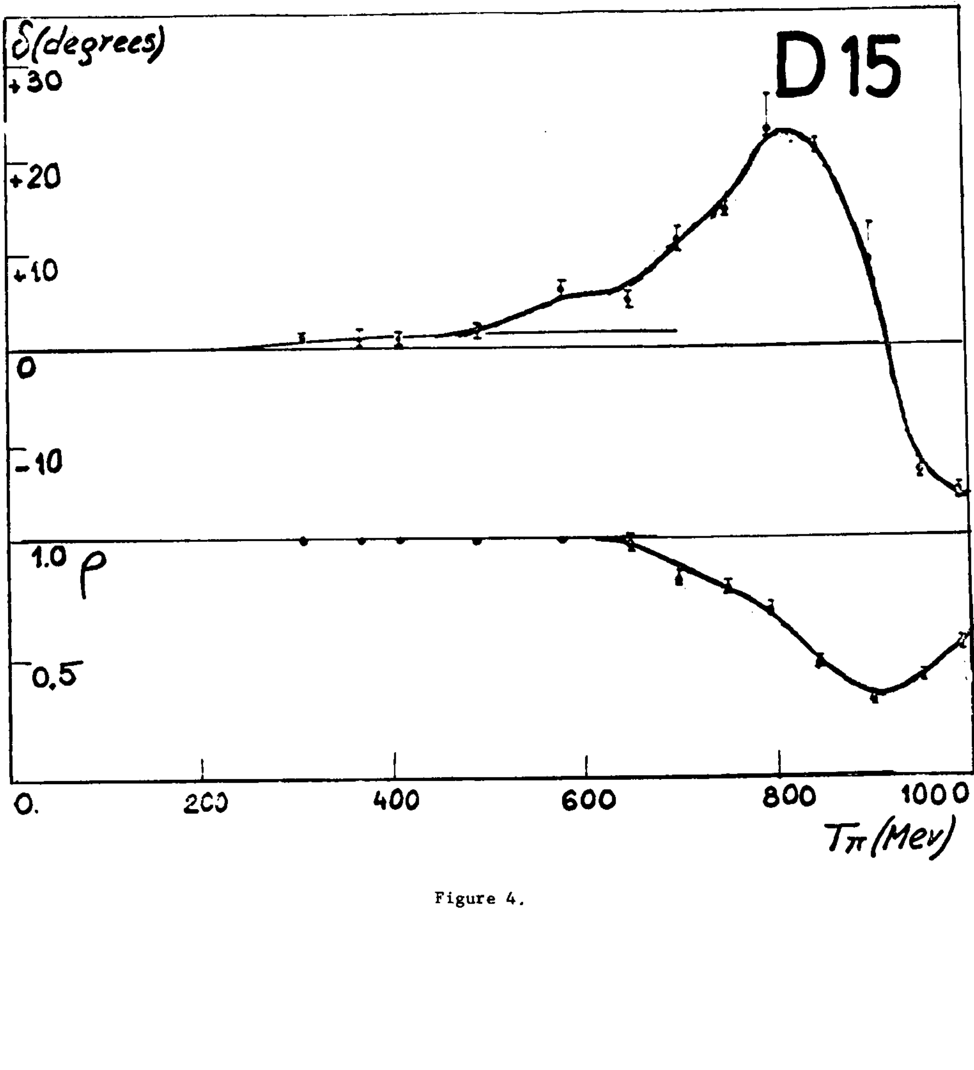

Figure 4.

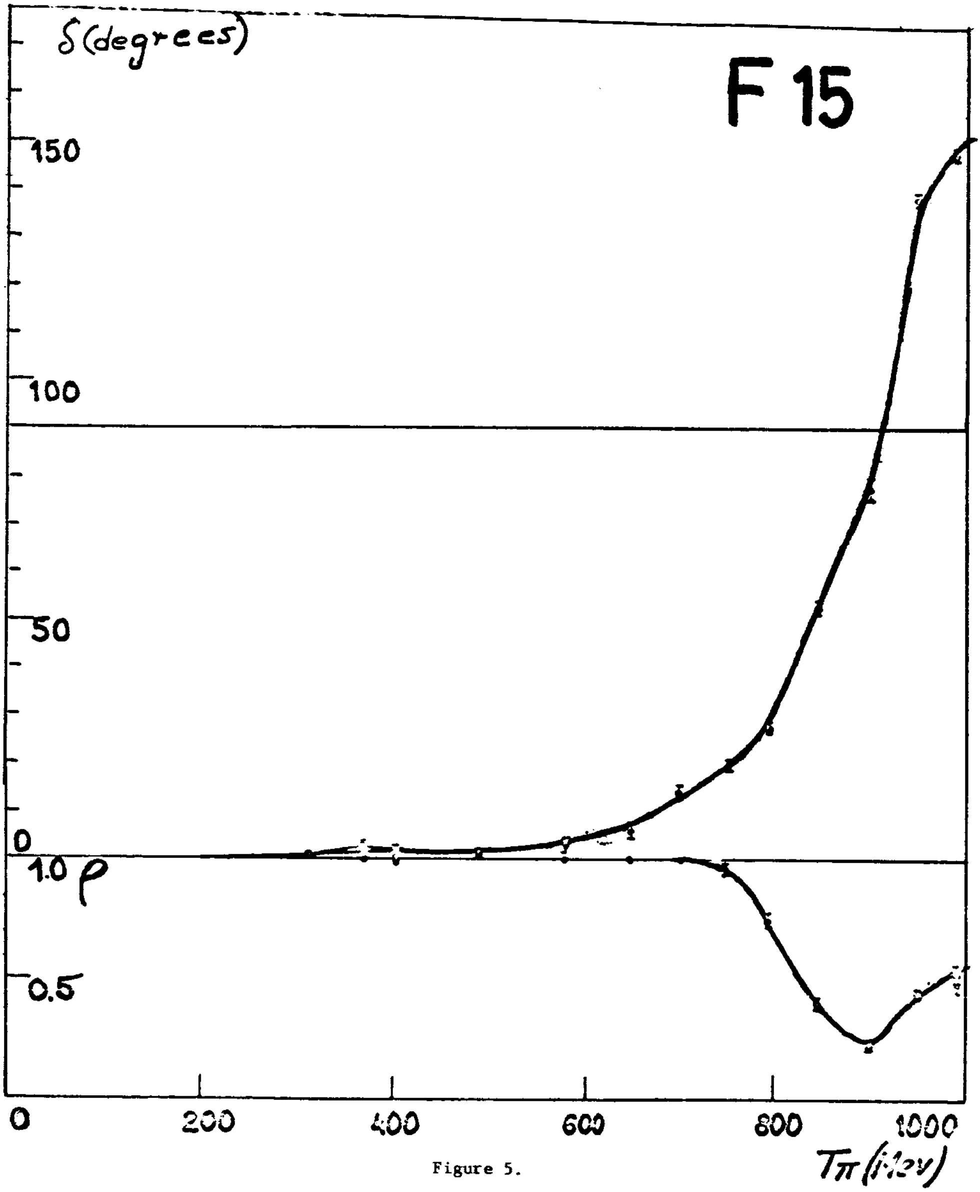

Figure 5.

ERICSON: I would like to ask a question (for my information) that perhaps falls a little bit outside of what you have said. There seems at present to be a controversy on the point of the scattering lengths in the singlet and the triplet for the pion-nucleon phase shifts. There is also a disagreement, it seems, between scattering data and the data that you would get out of the Panofsky ratio for the scattering lengths. Can you comment on what is the actual situation on this?

ROPER: Well, of course, that's all below 100 Mev. This is a 100 to 1 Mev conference, but only a few of these went down that low in analyses. I did at the beginning but our analyses were inter-dependent analyses, and I would not claim quantitativeness as far as the scattering lengths are concerned down in that region. I do not know the situation on those low energy. I've heard of it, but I do not know what the situation is. I think someone told me that Hamilton and Woolkock re-calculated the scattering links and now it has better agreement with the experimental values - I believe at Liverpool.

ERICSON: The thing is not that these quantities are not known with a pretty good precision, but that in certain combinations the uncertainties are largely overclaimed. There was a cancellation by a certain combination of them. It was the one part in 50 and, according to a new analysis, is only to about one part in 7. There seem to be several bids on it so I'm completely confused. This happens to be of great importance to us in our analysis of a pi-mesic atom and things of this kind, so I would be very happy to have the opinion of a specialist on this.

ROPER: I'm no specialist at this energy, but Professor Huler at Karlsru has written me that he is doing these calculations now too, and he feels like the errors that have been put on them are much too small with people who have done these calculations before. I really can't answer that, but maybe someone here can.

BREIT: In connection with symmetries, symmetry considerations for particle physics, there is of course a great temptation for people to use resonances like the ones here have been talking about. Now it seems to me that it is therefore very desirable to be sure that those resonances are really resonances. Of course, the uncomfortable part of the whole matter is that the definition is rather mathematical and in a way abstract and does not immediately connect up with physical things that we can feel with our hands and see with our eyes in a simple way. Therefore, I am very curious to know to what extent one can claim that it is necessary to have such terms, in the analysis, of single resonance with background. Now, of course, I also have certain personal interest in it because I did write wome papers around 1930 and 1940 in which there was formula used with a background plus a resonance term which was used as an approximate formula for ordinary nuclear resonances for low energy nuclear physics. That was used for a case of a many-channel reaction. It just appeared to be something one could formulate rather simply, mathematically. Of course, for low energy nuclear physics, one does not believe such a formula as being more than an approximation, and certainly Wigner's R matrix, where its assumptions are justified, is much superior. And if one looks at Wigner's R matrix, or uses common sense, the background is, itself, not a constant. In fact, one would have difficulty in deciding whether it is the background term or the velocity x, the background term, that should be a constant. Once one admits such variations, one doesn't have a firm mathematical prescription for data analysis. Would it be possible, therefore, in such work, to give some kind of limits of error regarding whether the 1 pole + energy independent background is more or less uniquely determined? Could one, perhaps, put limits on the variability of the background that would be admitted?

ROPER: I don't know of any way to do it. I would have liked to have known a way to do it.

BREIT: Well, for example, in data analysis one could make an analysis in which the background is strictly constant, another one in which it varies linearly, and vary that parameter...

ROPER: I've tried these kind of things. I found that I could get better fits when I allowed energy dependence in the background.

BREIT: But then from an error matrix, one can see what the limits of error are on the coefficient of the energy.

ROPER: Well, I would not want to restrict the results here to my analysis. It was an energy dependent analysis, but several of these analyses I'm talking about here are single energy analyses. I think then they went in and fit the phase shifts with parameters like this, too. When they tried to fit a resonance to them, for instance the ones with large background (there are only a few of these that have large background - namely the S_{31} and the S_{11}, and probably the P_{11}, but maybe not so much), only those two s waves would have tremendously large backgrounds. Most of the others resonate before the phase shifts get very large. So it's true that they really, in effect, do the same.

BREIT: In those cases, you are fully justified in calling them states.

ROPER: I think you are right. The S_{11} and the S_{31} do have this uncertainty about them. Of course, the SU(6) people like them because they fit.

BREIT: That's just where the danger seems to come in. Weiskoph had an article in the Physics Today a year or two ago. He was very skeptical of what people call resonances in high energy physics for such reasons as this.

NON-DYNAMICAL STRUCTURE OF PARTICLE REACTIONS

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The interplay of the theoretical and experimental aspects of a science has been subject to many discussions. It turns out that sometimes theory develops faster and produces predictions to be confirmed by future experiments, whereas at other times a theoretical "break-through" is lacking, and much experimental information accumulates waiting for eventual theoretical interpretation. In view of this often unmatched development of theory vs. experiment, it is of interest for a theorist to find ways of presenting his predictions so as to be in a most convenient and nevertheless not too specialized form for the experimentalist of the future. Similarly, an experimentalist likes to be able to summarize his results in such a way that it is amenable to interpretation by any theory-to-be-developed. It is for the purpose of finding such a meeting place, such a common ground of experimentalists and theorists that phenomenology has developed. With its help it is also more possible for experimentalists to make progress in the absence of a suitable theory, because phenomenological schemes often help in deciding which experiments are likely to be interesting or at least useful regardless of what the shape of future theories might be.

Elementary particle physics during the past two decades has witnessed such an unmatched development between theory and experiment. A staggering amount of experimental information has been accumulated, and although we have some sporadic understanding of their meaning, a general theory of the processes studied in these experiments is still missing. Some progress in the theoretical understanding has been made at the lowest energies, and some think that we are beginning to get a grasp on very high energy processes also. The most difficult energy region to understand seems to be the intermediate energy range, and it is therefore very timely indeed to have a conference on this subject.

Since a basic theoretical understanding of particle processes in the intermediate energy range is lacking, it is even more important to have an efficient phenomenological scheme in this region. It seems, however, that this is a difficult problem. At low energies, the two most successful phenomenological tools have been the phase shift analysis and the empirical potential. Neither of these two is appropriate, however, in the intermediate energy range. Phase shifts at higher energies become not only complex but also too numerous to be a convenient tool, and the validity of potentials in the relativistic region is at best dubious. At very high energies, other, semi-classical phenomenological considerations have proved to be of use, but also these lose their applicability when the energy is lowered.

In this talk I would like to discuss a phenomenological framework which is quite well applicable to all spins and to all energies and thus also to intermediate energies. The framework itself is by no means new, and has been used in connection with some of the particle reactions studied so far. For nucleon-nucleon scattering, for instance, it is related to the formalism of Wolfenstein parameters, and more generally it is referred to as the method of invariant amplitudes or form factors. During the past year, however, some additional understanding[1-11] has been gained concerning the properties of these form factors and the relationship between the physical observables which are determined by the form factors, and this will be the subject of my talk. The judicious use of these amplitudes permits one to separate the purely dynamical part of any reaction (i.e. the part of the interaction which depends on the specific form of the forces acting among the particles), and the part (which we will call non-dynamical) which depends only on general conservation laws, and hence is on a firmer footing.

Our discussion will be carried out in terms of the M-matrix of a reaction. This M-matrix depends on the momenta and spins occurring in the reaction. It is a rank-zero tensor (i.e. scalar or pseudoscalar) in ordinary three- (or four-) dimensional space, and is a matrix in the combined spin spaces of the particles participating in the reaction.

Let us give an example. Let us consider the reaction

$$0 + s \to 0 + s' \tag{1}$$

where 0 denotes a particle of zero spin, s a particle of spin s, and s' a particle of spin s'. The M-matrix for this reaction can be written as[3]

$$M(s',s) = \sum_{J=|s'-s|}^{s'+s} \sum_{r} a_J^r \, T_{[J]}((p)^r) ; S_{[J]}(s',s) \tag{2}$$

where a_J^r denotes a scalar form factor (invariant amplitude) which depends only on rotation invariants formed from the momenta; $T_{[J]}((p)^r)$ is a tensor of rank J depending on a set of momenta $(p)^r$, where r is the distinguishing label of the particular set of J momenta; $S_{[J]}(s',s)$ a rectangular spin matrix tensor of rank J, which is a $(2s'+1) \times (2s+1)$ matrix in spin space, the symbol ":" denotes contraction over all tensorial indices. The sum over r goes over all momentum sets that can be formed from the independent momenta in the reaction, consistent with conservation laws.

We should emphasize that all of the dynamical information is contained in the form-factors a_J^r, and that the $T_{[J]}:S_{[J]}$'s can be written down purely from the knowledge of the general conservation laws. In this talk, therefore, we will have nothing further to say about the structure of the form factors themselves, and all our results will follow from the structure of the $T_{[J]}:S_{[J]}$'s which we can determine uniquely.

It turns out to be very convenient in practice to span all the momenta that occur in the reaction by three orthonormal unit vectors. These can be defined as follows:

$$\hat{\ell} \equiv \frac{\vec{q}-\vec{q}\,'}{|\vec{q}-\vec{q}\,'|} \qquad \hat{m} \equiv \frac{\vec{q}\times\vec{q}\,'}{|\vec{q}\times\vec{q}\,'|} \qquad \hat{n} \equiv \hat{\ell}\times\hat{m} \tag{3}$$

where $\vec{q}$ and $\vec{q}\,'$ are two non-coplanar momenta occurring in the reaction. We see from the definitions that $\hat{\ell}$ and $\hat{n}$ are true vectors, while $\hat{m}$ is a pseudovector.

Equation (2) gives the M-matrix for reaction (1) when only rotation invariance is assumed. If parity is also conserved, further restrictions can be imposed, so that the overall intrinsic parity of the M-matrix is +1 or −1. We will call such M-matrices M^+ and M^-, respectively. Clearly, these restrictions demand that in M^+ the number of ℓ's plus n's appearing in each of the $(p)^r$'s must be even, while in M^- that number must be odd.

We can now turn to the relationship of the M-matrix to the physical observables L. This relationship, when only rotation invariance is assumed, is

$$L(S_I,S_F) \equiv \mathrm{Tr}\left\{MT_{[J_I]}(\{p\}^{r_I}):S_{[J_I]}(s,s)M^{+}T_{[J_F]}(\{p\}^{r_F}):S_{[J_F]}(s',s')\right\} \qquad (4)$$

where $T_{[J_I]}:S_{[J_I]}$ and $T_{[J_F]}:S_{[J_F]}$ describe the initial and final states, respectively, of the particles participating in the reaction. The notation for these is similar to that used in the M-matrix itself, except that the spin operators that enter these initial and final state descriptions are always square matrices in spin space, while those in the M-matrix are, in general, rectangular.

When parity conservation also holds, the observables can be written as

$$L^{++}(S_I,S_F) \equiv \mathrm{Tr}\left\{M^{+}T_{[J_I]}(\{p\}^{r_I}):S_{[J_I]}(s,s)M^{+\dagger}T_{[J_F]}(\{p\}^{r_F}):S_{[J_F]}(s',s')\right\} \qquad (5)$$

and

$$L^{--}(S_I,S_F) \equiv \mathrm{Tr}\left\{M^{-}T_{[J_I]}(\{p\}^{r_I}):S_{[J_I]}(s,s)M^{-\dagger}T_{[J_F]}(\{p\}^{r_F}):S_{[J_F]}(s',s')\right\} \qquad (6)$$

where L^{++} is an observable for a reaction when the product of the intrinsic parities of the participating particles is +1, and L^{--} is the observable when this product is -1.

In addition to L^{++} and L^{--}, we can also form the quantities

$$R^{+-}(S_I,S_F) \equiv \mathrm{Tr}\left\{M^{+}T_{[J_I]}(\{p\}^{r}):S_{[J_I]}(s,s)M^{-\dagger}T_{[J_F]}(\{p\}^{r_F}):S_{[J_F]}(s',s')\right\} \qquad (7)$$

and

$$R^{-+}(S_I,S_F) \equiv \mathrm{Tr}\left\{M^{-}T_{[J_I]}(\{p\}^{r}):S_{[J_I]}(s,s)M^{+\dagger}T_{[J_F]}(\{p\}^{r_F}):S_{[J_F]}(s',s')\right\} \quad (8)$$

These have no direct physical meaning in themselves if parity conservation holds, but will play an important role in later discussion. They will be referred to as pseudo-observables, since they look like observables but are pseudoscalars and not scalars as all observables are.

So far we have discussed only the relatively simple reaction given by Eq. (1). We will now show that more complicated reactions (involving more particles of non-zero spin) can be analyzed in terms of such simpler reactions.

For this purpose we will introduce the notion of a basic reaction or irreducible constituent reaction. By this we will mean a reaction containing only one boson with non-zero spin, or only two fermions. For instance, Eq. (1) with s and s' denoting fermions is an irreducible constituent reaction. For such reactions the observables can be calculated by

$$L_{J_I J_F}(r_I,r_F) = \sum_{J_1,J_2} \sum_{r_1,r_2} a_{J_1}^{r_1} a_{J_2}^{r_2 *} X_{J_1 J_I J_2 J_F}^{r_1 r_I r_2 r_F} \quad (9)$$

where

$$X_{J_1 J_I J_2 J_F}^{r_1 r_I r_2 r_F} \equiv \mathrm{Tr}\Big\{S_{[J_1]}(s,s'):T_{[J_1]}(\{p\}^{r_1})S_{[J_I]}(s,s):T_{[J_I]}(\{p\}^{r_I})$$

$$\left[S_{[J_2]}(s',s):T_{[J_2]}(\{p\}^{r_2})\right]^{\dagger}S_{[J_F]}(s',s'):T_{[J_F]}(\{p\}^{r_F})\Big\} \quad (10)$$

Explicit formulae for the so-called four-traces given by Eq. (10) have been derived. Nothing more complicated than such a four-trace ever arises in an irreducible constituent. Similarly, only such four-traces arise in the pseudo-observables.

Now let us consider a more complicated reaction, for example

$$A + B \rightarrow 0 + C \tag{11}$$

where A is a boson of arbitrary spin, and B and C are fermions of arbitrary spins, whose M-matrix is M_1. For the purposes of our non-dynamical investigations, this reaction can be thought of as a composite of the two reactions

$$A + 0 \rightarrow 0 + 0 \tag{12}$$

and

$$0 + B \rightarrow 0 + C \tag{13}$$

with M-matrices M_2 and M_3, respectively. By this we mean, that we can write[3,8,10]

$$M_1^+ \; (=) \; M_2^+ \otimes M_3^+ + M_2^- \otimes M_3^- \; \text{[illegible]} \; M_1^- \; (=) \; M_2^+ \otimes M_3^- + M_3^- + M_2^- \otimes M_3^+ \tag{14}$$

Here (=) means "non-dynamically equal", that is, equal as far as the structure of the $T_{[J]}:S_{[J]}$'s in Eq. (2) is concerned, but not as far as the values of the form factors are concerned.

Using this result we can then write for the observables (using an abbreviated notation)

$$L_1^{++} = L_2^{++}L_3^{++} + L_2^{--}L_3^{--} + R_2^{+-}R_3^{+-} + R_2^{-+}R_3^{-+} \tag{15}$$

and

$$L_1^{--} = L_2^{++}L_3^{--} + L_2^{--}L_3^{++} + R_2^{+-}R_3^{-+} + R_2^{-+}R_3^{+-} \tag{16}$$

where the subscripts 1, 2 and 3 again refer to the reactions (11), (12) and (13), respectively.

Further inspection shows that in Eq. (15) and (16), for a given observable for reaction (11), either only the first two terms, or only the last two terms are non-zero. According to this, we call an observable the Class I type or the Class II type, respectively.

A similar structure is evident for reactions which are composites of more than two irreducible constituents. Thus, for instance, the observables in a composite of three irreducible constituents have four classes, the observables in a composite of four have eight classes, etc. The pseudo-observables have an identical class structure.

The class structure is of some interest, for instance, for the complete experimental determination of the form factors. It can be shown, for example, that the form factors can never be completely determined by carrying out experiments in one class only. Since the observables in different classes often differ from each other in experimentally very tangible ways, the above statement is of practical significance. It can thus be shown, for instance, that in pion photoproduction it is impossible to determine completely all the form factors using only unpolarized photons or photons polarized in or perpendicular to the reaction plane.

The observables within a class can, however, be further subdivided.[10] It will be recalled (see Eq. (9)), that any observable can be written in terms of a sum of bilinear products of form factors. It turns out, however, that not all possible bilinear products appear in all of the observables. Instead, these products can be subdivided into sets (we will call them productsets), which are mutually exclusive and together include all products. It can be shown, that the observables in turn can be subdivided into what we will call subclasses in such a way that all the observables in a given subclass depend on the products in one productset only, and no two subclasses depend on the same productset.

Let us investigate this situation in greater detail first for irreducible constituents. There we get four subclasses for the observables, which can be characterized by whether the number of ℓ's, m's and n's is even or odd. Accordingly, the four subclasses are (ξ,ξ,ξ), (ξ,υ,ξ), (υ,ξ,υ) and $(\upsilon,\upsilon,\upsilon)$, where ξ means "even" and υ means "odd". One can also predict which bilinear products of form factors will appear in the observables of which subclass. For example, the subclass (ξ,ξ,ξ) will contain the real parts of products in that productset which is characterized by (ξ,ξ,ξ) (i.e. the product of the $T_{[J_i]}:S_{[J_i]}$'s $(i = 1,2)$ belonging to the two form factors in any product in that productset has an even number of ℓ's, m's and n's), and the imaginary part of the products in the productset characterized by $(\upsilon,\upsilon,\upsilon)$. Similar prescriptions can be given for the other three subclasses of an irreducible constituent reaction.

A similar subclass structure exists also for the pseudo-observables of an irreducible constituent: there are again four subclasses, this time characterized by (ξ,ξ,υ), (υ,ξ,ξ), (ξ,υ,υ) and (υ,υ,ξ), and again prescriptions can be given for the type of bilinear products of form factors which occur in each of these subclasses.

For composite reactions, the subclass structure can be constructed from the subclass structure of the irreducible constituents. In turns out, for instance, that for a reaction which is a composite of two irreducible constituents, there are 8 subclasses for the observables in Class I, and 8 subclasses for the observables in Class II. Each of these subclasses are formed by two of the products of the subclasses of the two constituents. For a composite of three irreducible constituents we have 8 subclasses, in each of the four classes, etc. The character of the products of form factors which appear in each of these subclasses can also be deduced from the character of the productsets in the constituent subclasses.

It turns out that the subclasses for the observables L^{++} and the observables L^{--} are the same.

Except for very pathological cases, the number of observables in a given subclass is always larger than the number of products in the corresponding productset. Thus, in terms of the bilinear products of the form factors, the observables within a given subclass are not independent of each other, but there are a certain number of linear relationships among them. These relationships are in general different for the L^{++} observables and for the L^{--} observables, and thus they give an experimental way of distinguishing between L^{++} and L^{--}. Thus, if we assume that in a given reaction the intrinsic parities of all but one of the particles are known, these relations permit a determination of this unknown parity. It can be shown, in fact, that these relations give <u>all</u>

such parity experiments[1,2,5,7,10] which can be carried out in the absence of dynamical information about the particles.

The relationships among a certain set of observables will also depend on the spins of the particles involved in the reaction, so that these relations can also be used for the determination[5,10] of an unknown spin in the reaction.

Finally, the subclass structure is helpful in deciding which experiments provide new information about the reaction, and what the nature of this new information is. It may be possible, for instance, with the help of the subclass structure, to select a set of experiments fitting certain experimental requirements which together completely determine the form factors, or conversely, to decide what the easiest experimental circumstances are under which a complete determination of form factors can be carried out.

Explicit illustrations for the above outlined observable structure have been given in the literature[4,6,11] for such reactions as $1/2 + 1/2 \to 1/2 + 1/2$, $1/2 + 1 \to 1/2 + 0$, $\gamma + 1/2 \to s + 1/2$, $\gamma + 1/2 \to s + 3/2$. We will now give as another illustration, the structure of the reaction

$$0 + 1 \to 0 + 1 \tag{17}$$

Quantities related to this reaction will bear the subscript 0. This reaction will be composed of the two irreducible constituents

$$0 + 1 \to 0 + 0 \tag{18}$$

and

$$0 + 0 \to 0 + 1 \tag{19}$$

whose quantities will bear the subscripts 1 and 2, respectively. Then, using $S \equiv S_{[1]}(1,0)$, $S' \equiv S_{[1]}(0,1)$, and $T_{[1]}(\{p\}^r) \equiv T(\ell)$, $T(m)$, or $T(n)$, we have

$$M_2^+ = a_2\ T(m):S' \tag{20}$$

$$M_2^- = a_1\ T(\ell):S' + a_3\ T(n):S' \tag{21}$$

$$M_3^+ = b_2\ T(m):S \tag{22}$$

$$M_3^- = b_1\ T(\ell):S + b_3\ T(n):S \tag{23}$$

and

$$M_1^+ \ (=)\ M_2^+ \otimes M_3^+ + M_2^- \otimes M_3^- \tag{24}$$

$$M_1^- \ (=)\ M_2^+ \otimes M_3^- + M_2^- \otimes M_3^+ \tag{25}$$

so that

$$\begin{aligned} M_1^+ = c_{22}\ T(m):S\ T(m):S' &+ c_{11}\ T(\ell):S\ T(\ell):S' + c_{13}\ T(n):S\ T(\ell):S' \\ &+ c_{31}\ T(\ell):S\ T(n):S' + c_{33}\ T(n):S\ T(n):S' \end{aligned} \tag{26}$$

where we made the correspondence

$$a_i b_j \to c_{ij} \tag{27}$$

Similarly, we have

$$M_1^- = C_{21}\, T(\ell){:}S\ T(m){:}S' + C_{23}\, T(n){:}S\ T(n){:}S' + C_{12}\, T(m){:}S\ T(\ell){:}S' + C_{32}\, T(m){:}S\ T(n){:}S' \qquad (28)$$

Now we can write down the observables and pseudo-observables for reactions (2) and (3). For the former, they are given in Table I. In this table $L(x)$ is a shorthand notation for $L_2(0,x;0,0)$, where $L(a,b;c,d)$ denotes an observable with the spin states of the first initial, second initial, first final, and second final particles characterized by a, b, c, and d, respectively. The table gives the coefficients of the bilinear combinations of form factors in the various observables.

The observables for reaction 3 are identical with those of reaction 2, except that

1. $L(x)$ now denotes $L_3(0,0;0,x)$
2. All a_i's in Table I should be changed to b_i's.
3. The signs of all coefficients for $L(m)$, $R(\ell)$, and $R(n)$ have to be reversed.

Now we turn to the observables of reaction 1, which can be constructed using Table I, and Eq. (15), (16), and (27). The general structure of the subclasses for reaction 1 is shown in Table II. The coefficients within the subclasses are given in Table III. In these tables observables involving mm do not appear, since they are not independent from those involving $\ell\ell$ and nn, but they can be computed easily using

$$L(\ldots\ell\ell\ldots) + L(\ldots mm\ldots) + L(\ldots nn\ldots) = 0 \tag{29}$$

Thus we get some additions to the subclasses which are shown in Table IV, but these are observables which depend on the previous ones through Eq. (29).

Now we can turn to finding the parity experiments in these subclasses.

Subclass I-1. Since there are 9 observables, and at least 4 bilinear combinations of form factors, there should be 9 - 4 = 5 independent parity experiments here. One possible set is

$$L^{\pm\pm}(0,mm) - L^{\pm\pm}(mm,0) = \left\{\begin{matrix} 0 \\ \neq 0 \end{matrix}\right\} \tag{30}$$

$$L^{\pm\pm}(0,0) + L^{\pm\pm}(mm,mm) = \left\{\begin{matrix} \neq 0 \\ 0 \end{matrix}\right\} \tag{31}$$

$$L^{\pm\pm}(0,0) - L^{\pm\pm}(mm,mm) = \left\{\begin{matrix} 3/4 \\ -6 \end{matrix}\right\} [L^{\pm\pm}(mm,0) + L^{\pm\pm}(0,mm)] \tag{32}$$

$$L^{\pm\pm}(0,\ell\ell) - L^{\pm\pm}(0,nn) = \left\{\begin{matrix} 3 \\ -3/2 \end{matrix}\right\} [L^{\pm\pm}(mm,\ell\ell) - L^{\pm\pm}(mm,nn)] \tag{33}$$

$$L^{\pm\pm}(\ell\ell,0) - L^{\pm\pm}(nn,0) = \left\{\begin{matrix} 3 \\ -3/2 \end{matrix}\right\} [L^{\pm\pm}(\ell\ell,mm) - L^{\pm\pm}(nn,mm)] \tag{34}$$

Subclass I-2. There are 3 - 1 = 2 independent parity experiments:

$$L^{\pm\pm}(\ell n,0) = \left\{\begin{matrix} 3 \\ -3/2 \end{matrix}\right\} L^{\pm\pm}(\ell n,mm) \tag{35}$$

$$L^{\pm\pm}(\ell n,\ell\ell) - L^{\pm\pm}(\ell n,nn) = \left\{\begin{matrix} \neq 0 \\ 0 \end{matrix}\right\} \tag{36}$$

Subclass I-3. Again two experiments:

$$L^{\pm\pm}(m,0) = \begin{Bmatrix} 3 \\ -3/2 \end{Bmatrix} L^{\pm\pm}(m,mm) \tag{37}$$

$$L^{\pm\pm}(m,\ell\ell) - L^{\pm\pm}(m,nn) = \begin{Bmatrix} \neq 0 \\ 0 \end{Bmatrix} \tag{38}$$

Subclass I-4.

$$L^{\pm\pm}(0,\ell n) = \begin{Bmatrix} 3 \\ -3/2 \end{Bmatrix} L^{\pm\pm}(mm,\ell n) \tag{39}$$

$$L^{\pm\pm}(\ell\ell,\ell n) - L^{\pm\pm}(nn,\ell n) = \begin{Bmatrix} \neq 0 \\ 0 \end{Bmatrix} \tag{40}$$

Subclass I-5.

$$L^{\pm\pm}(\ell n,\ell n) = \begin{Bmatrix} \neq 0 \\ 0 \end{Bmatrix} \tag{41}$$

$$L^{\pm\pm}(m,m) = \begin{Bmatrix} \neq 0 \\ 0 \end{Bmatrix} \tag{42}$$

Subclass I-6.

$$L^{\pm\pm}(m,\ell n) = \begin{Bmatrix} \neq 0 \\ 0 \end{Bmatrix} \tag{43}$$

$$L^{\pm\pm}(\ell n,m) = \begin{Bmatrix} \neq 0 \\ 0 \end{Bmatrix} \tag{44}$$

Subclass I-7.

$$L^{\pm\pm}(0,m) = \begin{Bmatrix} 3 \\ -3/2 \end{Bmatrix} L^{\pm\pm}(mm,m) \tag{45}$$

$$L^{\pm\pm}(\ell\ell,m) - L^{\pm\pm}(nn,m) = \begin{Bmatrix} \neq 0 \\ 0 \end{Bmatrix} \tag{46}$$

In each of the Class II subclasses, there will be one parity experiment.

Subclass II-1.

$$L^{\pm\pm}(\ell,\ell) = \begin{Bmatrix} +4 \\ -4 \end{Bmatrix} L^{\pm\pm}(nm,nm) \tag{47}$$

Subclass II-2.

$$L^{\pm\pm}(n,\ell) = \begin{Bmatrix} -4 \\ +4 \end{Bmatrix} L^{\pm\pm}(\ell m,nm) \tag{48}$$

Subclass II-3.

$$L^{\pm\pm}(\ell m,\ell) = \begin{Bmatrix} +1 \\ -1 \end{Bmatrix} L^{\pm\pm}(n,nm) \tag{49}$$

Subclass II-4.

$$L^{\pm\pm}(nm,\ell) = \begin{Bmatrix} -1 \\ +1 \end{Bmatrix} L^{\pm\pm}(\ell,nm) \tag{50}$$

Subclass II-5.

$$L^{\pm\pm}(\ell,n) = \begin{Bmatrix} -4 \\ +4 \end{Bmatrix} L^{\pm\pm}(nm,\ell m) \tag{51}$$

Subclass II-6.

$$L^{\pm\pm}(n,n) = \begin{Bmatrix} +4 \\ -4 \end{Bmatrix} L^{\pm\pm}(\ell m,\ell m) \tag{52}$$

Subclass II-7.

$$L^{\pm\pm}(\ell m,n) = \begin{Bmatrix} -1 \\ +1 \end{Bmatrix} L^{\pm\pm}(n,\ell m) \tag{53}$$

Subclass II-8.

$$L^{\pm\pm}(nm,n) = \begin{Bmatrix} +1 \\ -1 \end{Bmatrix} L^{\pm\pm}(\ell,\ell m) \tag{54}$$

We mentioned at the beginning that the relationships between observables in the same subclass can also be used to determine the spin of one of the participating reactions. An example for this application is now given.

For $0 + 1 \rightarrow 0 + 1$ the following relationship holds

$$L^{+}(m,0) = 3L^{+}(m,mm) \tag{55}$$

while for the reaction $0 + 1 \rightarrow 0 + 2$, the relationship between these observables is

$$L^{+}(m,0) = -L^{+}(m,mm) \tag{56}$$

In neither case are these two observables independent of each other, but their ratio depends on the spin of the final state particle and hence measurements of these observables can serve to determine this spin.

Apart from the parity and spin experiments, we can deduce other interesting information from the subclasses. For example, since Class II has only observables in which both the initial and the final particles are polarized, we conclude that in order to determine the form factors completely, one has to carry out at least one experiment in which the initial and final particles are simultaneously polarized. Furthermore, such an experiment must involve polarization directions other than m.

It can also be shown, for instance, by a rather simple inspection of the subclass tables, that for the L^{--}'s, all form factors can be completely determined without having to resort to experiments of the type (x,yz), (xy,z) or (xy,zw), where x, y, z, and w = ℓ, m, or n.

The purpose of this talk was to exhibit some of the advantages of the phenomenological description of reactions in terms of their invariant amplitudes. I believe that this method will gain in usefulness in the near future as experimental techniques continue to develop. In particular, not only do we make progress in the well-advertised direction of higher energies, but the techniques of measuring spin-wise more complicated observables are also advancing. The medium energy accelerators with very high currents, projected for the near future, should also be of great help in this respect. As more and more type of experiments become feasible experimentally, more and more of the power of the formalism I have discussed can be put to practical use, particularly in the case of reactions at intermediate energies, involving particles of substantial spins.

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Table I

Observables L and pseudo-observables R for the reaction $0 + 1 \rightarrow 0 + 0$. For notation, see the text. The observable subclass $(\upsilon,\upsilon,\upsilon)$ is empty in this case.

OBSERVABLES

++		—	
$\lvert a_2\rvert^2$	Subclass 1. (ξ,ξ,ξ)	$\lvert a_1\rvert^2$	$\lvert a_3\rvert^2$
+1	L(0)	+1	+1
$+\frac{1}{3}$	L(ℓℓ)	$-\frac{2}{3}$	$+\frac{1}{3}$
$+\frac{1}{3}$	L(nn)	$+\frac{1}{3}$	$-\frac{2}{3}$
++		—	
—	Subclass 2. (ξ,υ,ξ)	Im $a_1 a_3^*$	
0	L(m)	-2	
++		—	
—	Subclass 3. (υ,ξ,υ)		
0	L(ℓn)	$-\frac{1}{2}$	

Table I (cont'd)

PSEUDO-OBSERVABLES

+-		-+
$a_2 a_3^*$	Subclass 1 (υ,ξ,ξ)	$a_3 a_2^*$
-i	R(ℓ)	+i

+-		-+
$a_2 a_1^*$	Subclass 2 (ξ,ξ,υ)	$a_1 a_2^*$
+i	R(n)	-i

+-		-+
$a_2 a_1^*$	Subclass 3 (υ,υ,ξ)	$a_1 a_2^*$
$-\frac{1}{2}$	R(ℓm)	$-\frac{1}{2}$

+-		-+
$a_2 a_3^*$	Subclass 4 (ξ,υ,υ)	$a_3 a_2^*$
$-\frac{1}{2}$	R(nm)	$-\frac{1}{2}$

Subclass structure for the observables of the reaction 0 + 1 → 0 + 1. For notation, see the text.

	Constituent sub-classes		Observables	Productsets ++	Productsets —
-1	1	1	$L_1(0,0), L_1(0,\ell\ell), L_1(0,nn), L_1(\ell\ell,0), L_1(\ell\ell,\ell\ell), L_1(\ell\ell,nn), L_1(nn,0), L_1(nn,\ell\ell), L_1(nn,mm)$	$\lvert C_{22}\rvert^2, \lvert C_{11}\rvert^2, \lvert C_{33}\rvert^2$ $\lvert C_{13}\rvert^2, \lvert C_{31}\rvert^2$	$\lvert C_{12}\rvert^2, \lvert C_{21}\rvert^2, \lvert C_{23}\rvert^2, \lvert C_{32}\rvert^2$
-2	3	1	$L_1(\ell n,0), L_1(\ell n,\ell\ell), L_1(\ell n,nn)$	$\mathrm{Re}\, C_{11}C_{31}^*, \mathrm{Re}\, C_{13}C_{33}^*$	$\mathrm{Re}\, C_{12}C_{32}^*$
-3	2	1	$L_1(m,0), L_1(m,\ell\ell), L_1(m,nn)$	$\mathrm{Im}\, C_{11}C_{31}^*, \mathrm{Im}\, C_{13}C_{33}^*$	$\mathrm{Im}\, C_{12}C_{32}^*$
-4	1	3	$L_1(0,\ell n), L_1(\ell\ell,\ell n), L_1(nn,\ell n)$	$\mathrm{Re}\, C_{11}C_{13}^*, \mathrm{Re}\, C_{31}C_{33}^*$	$\mathrm{Re}\, C_{21}C_{23}^*$
-5	3 2	3 2	$L_1(\ell n,\ell n)$ $L_1(m,m)$	$\mathrm{Re}\, C_{11}C_{33}^*, \mathrm{Re}\, C_{13}C_{31}^*$	— —
-6	2 3	3 2	$L_1(m,\ell n)$ $L_1(\ell n,m)$	$\mathrm{Im}\, C_{11}C_{33}^*, \mathrm{Im}\, C_{13}C_{31}^*$	— —
-7	1	2	$L_1(0,m), L_1(\ell\ell,m), L_1(nn,m)$	$\mathrm{Im}\, C_{11}C_{13}^*, \mathrm{Im}\, C_{31}C_{33}^*$	$\mathrm{Im}\, C_{21}C_{23}^*$
I-1	1 4	1 4	$L_1(\ell,\ell)$ $L_1(nm,nm)$	$\mathrm{Re}\, C_{33}C_{22}^*$	$\mathrm{Re}\, C_{23}C_{32}^*$
I-2	2 3	1 4	$L_1(n,\ell)$ $L_1(\ell m,nm)$	$\mathrm{Re}\, C_{13}C_{22}^*$	$\mathrm{Re}\, C_{12}C_{23}^*$
I-3	3 2	1 4	$L_1(\ell m,\ell)$ $L_1(n,nm)$	$\mathrm{Im}\, C_{13}C_{22}^*$	$\mathrm{Im}\, C_{12}C_{23}^*$
I-4	4 1	1 4	$L_1(nm,\ell)$ $L_1(\ell,nm)$	$\mathrm{Im}\, C_{33}C_{22}^*$	$\mathrm{Im}\, C_{32}C_{23}^*$
I-5	1 4	2 3	$L_1(\ell,n)$ $L_1(nm,\ell m)$	$\mathrm{Re}\, C_{31}C_{22}^*$	$\mathrm{Re}\, C_{21}C_{32}^*$
I-6	2 3	2 3	$L_1(n,n)$ $L_1(\ell m,\ell m)$	$\mathrm{Re}\, C_{11}C_{22}^*$	$\mathrm{Re}\, C_{12}C_{21}^*$
I-7	3 2	2 3	$L_1(\ell m,n)$ $L_1(n,\ell m)$	$\mathrm{Im}\, C_{11}C_{22}^*$	$\mathrm{Im}\, C_{12}C_{21}^*$
I-8	4 1	2 3	$L_1(nm,n)$ $L_1(\ell,\ell m)$	$\mathrm{Im}\, C_{31}C_{22}^*$	$\mathrm{Im}\, C_{32}C_{21}^*$

Table III

Subclass coefficient tables for the reaction 0 + 1 → 0 + 1. For notation, see the text.

++						--			
$\lvert c_{22}\rvert^2$	$\lvert c_{11}\rvert^2$	$\lvert c_{33}\rvert^2$	$\lvert c_{13}\rvert^2$	$\lvert c_{31}\rvert^2$	Subclass I-1	$\lvert c_{12}\rvert^2$	$\lvert c_{21}\rvert^2$	$\lvert c_{23}\rvert^2$	$\lvert c_{32}\rvert^2$
+1	+1	+1	+1	+1	L(0,0)	+1	+1	+1	+1
$+\frac{1}{3}$	$-\frac{2}{3}$	$+\frac{1}{3}$	$+\frac{1}{3}$	$-\frac{2}{3}$	$L(0,\ell\ell)$	$+\frac{1}{3}$	$-\frac{2}{3}$	$+\frac{1}{3}$	$+\frac{1}{3}$
$+\frac{1}{3}$	$+\frac{1}{3}$	$-\frac{2}{3}$	$-\frac{2}{3}$	$+\frac{1}{3}$	L(0,nn)	$+\frac{1}{3}$	$+\frac{1}{3}$	$-\frac{2}{3}$	$+\frac{1}{3}$
$+\frac{1}{3}$	$-\frac{2}{3}$	$+\frac{1}{3}$	$-\frac{2}{3}$	$+\frac{1}{3}$	$L(\ell\ell,0)$	$-\frac{2}{3}$	$+\frac{1}{3}$	$+\frac{1}{3}$	$+\frac{1}{3}$
$+\frac{1}{9}$	$+\frac{4}{9}$	$+\frac{1}{9}$	$-\frac{2}{9}$	$-\frac{2}{9}$	$L(\ell\ell,\ell\ell)$	$-\frac{2}{9}$	$-\frac{2}{9}$	$+\frac{1}{9}$	$+\frac{1}{9}$
$+\frac{1}{9}$	$-\frac{2}{9}$	$-\frac{2}{9}$	$+\frac{4}{9}$	$+\frac{1}{9}$	$L(\ell\ell,nn)$	$-\frac{2}{9}$	$+\frac{1}{9}$	$-\frac{2}{9}$	$+\frac{1}{9}$
$+\frac{1}{3}$	$+\frac{1}{3}$	$-\frac{2}{3}$	$+\frac{1}{3}$	$-\frac{2}{3}$	L(nn,0)	$+\frac{1}{3}$	$+\frac{1}{3}$	$+\frac{1}{3}$	$-\frac{2}{3}$
$+\frac{1}{9}$	$-\frac{2}{9}$	$-\frac{2}{9}$	$+\frac{1}{9}$	$+\frac{4}{9}$	$L(nn,\ell\ell)$	$+\frac{1}{9}$	$-\frac{2}{9}$	$+\frac{1}{9}$	$-\frac{2}{9}$
$+\frac{1}{9}$	$+\frac{1}{9}$	$+\frac{4}{9}$	$-\frac{2}{9}$	$-\frac{2}{9}$	L(nn,nn)	$+\frac{1}{9}$	$+\frac{1}{9}$	$-\frac{2}{9}$	$-\frac{2}{9}$

Table III (cont'd.)

++			--
Re $C_{11}C_{31}^*$	Re $C_{13}C_{33}^*$	Subclass I-2	Re $C_{12}C_{32}^*$
- 1/2	- 1/2	$L(\ell n,0)$	- 1/2
+ 1/3	- 1/6	$L(\ell n,\ell\ell)$	- 1/6
- 1/6	+ 1/3	$L(\ell n,nn)$	- 1/6

Im $C_{11}C_{31}^*$	Im $C_{13}C_{33}^*$	Subclass I-3	Im $C_{12}C_{32}^*$
- 2	- 2	$L(m,0)$	- 2
+ 4/3	- 2/3	$L(m,\ell\ell)$	- 2/3
- 2/3	+ 4/3	$L(m,nn)$	- 2/3

Re $C_{11}C_{13}^*$	Re $C_{31}C_{33}^*$	Subclass I-4	Re $C_{21}C_{23}^*$
- 1/2	- 1/2	$L(0,\ell n)$	- 1/2
+ 1/3	- 1/6	$L(\ell\ell,\ell n)$	- 1/6
- 1/6	+ 1/3	$L(nn,\ell n)$	- 1/6

Re $C_{11}C_{33}^*$	Re $C_{13}C_{31}^*$	Subclass I-5	——
+ 1/8	+ 1/8	$L(\ell n,\ell n)$	0
- 2	+ 2	$L(m,m)$	0

Im $C_{11}C_{33}^*$	Im $C_{13}C_{31}^*$	Subclass I-6	——
+ 1/2	+ 1/2	$L(m,\ell n)$	0
- 1/2	+ 1/2	$L(\ell n,m)$	0

Im $C_{11}C_{13}^*$	Im $C_{31}C_{33}^*$	Subclass I-7	Im $C_{21}C_{23}^*$
+ 2	+ 2	$L(0,m)$	+ 2
- 4/3	+ 2/3	$L(\ell\ell,m)$	+ 2/3
+ 2/3	- 4/3	$L(nn,m)$	+ 2/3

Table III (cont'd)

++		—
Re $C_{33}C_{22}^*$	Subclass II-1	Re $C_{23}C_{32}^*$
+ 1	$L(\ell,\ell)$	- 1
+ 1/4	$L(nm,nm)$	+ 1/4
Re $C_{13}C_{22}^*$	Subclass II-2	Re $C_{12}C_{23}^*$
- 1	$L(n,\ell)$	+ 1
+ 1/4	$L(\ell m,nm)$	+ 1/4
Im $C_{13}C_{22}^*$	Subclass II-3	Im $C_{12}C_{23}^*$
- 1/2	$L(\ell m,\ell)$	+ 1/2
- 1/2	$L(n,nm)$	- 1/2
Im $C_{33}C_{22}^*$	Subclass II-4	Im $C_{32}C_{23}^*$
- 1/2	$L(nm,\ell)$	+ 1/2
+ 1/2	$L(\ell,nm)$	+ 1/2
Re $C_{31}C_{22}^*$	Subclass II-5	Re $C_{21}C_{32}^*$
- 1	$L(\ell,n)$	+ 1
+ 1/4	$L(nm,\ell m)$	+ 1/4
Re $C_{11}C_{22}^*$	Subclass II-6	Re $C_{12}C_{21}^*$
+ 1	$L(n,n)$	- 1
+ 1/4	$L(\ell m,\ell m)$	+ 1/4
Im $C_{11}C_{22}^*$	Subclass II-7	Im $C_{12}C_{21}^*$
+ 1/2	$L(\ell m,n)$	- 1/2
- 1/2	$L(n,\ell m)$	- 1/2
Im $C_{31}C_{22}^*$	Subclass II-8	Im $C_{32}C_{21}^*$
+ 1/2	$L(nm,n)$	- 1/2
+ 1/2	$L(\ell,\ell m)$	+ 1/2

Table IV

Additional (dependent) observables for the reaction 0 + 1 → 0 + 1. For the notation, see the text.

++						—			
$\|C_{22}\|^2$	$\|C_{11}\|^2$	$\|C_{33}\|^2$	$\|C_{13}\|^2$	$\|C_{31}\|^2$	Subclass I-1	$\|C_{12}\|^2$	$\|C_{21}\|^2$	$\|C_{23}\|^2$	$\|C_{32}\|^2$
-2/3	+1/2	+1/3	+1/3	+1/3	L(0,mm)	-2/3	+1/3	+1/3	-2/3
-2/3	+1/3	+1/3	+1/3	+1/3	L(mm,0)	+1/3	-2/3	-2/3	+1/3
-2/9	-2/9	+1/9	-2/9	+1/9	L(ℓℓ,mm)	+4/9	+1/9	+1/9	-2/9
-2/9	+1/9	-2/9	+1/9	-2/9	L(nn,mm)	-2/9	+1/9	+1/9	+4/9
-2/9	-2/9	+1/9	+1/9	-2/9	L(mm,ℓℓ)	+1/9	+4/9	-2/9	+1/9
-2/9	+1/9	-2/9	-2/9	+1/9	L(mm,nn)	+1/9	-2/9	+4/9	+1/9
+4/9	+1/9	+1/9	+1/9	+1/9	L(mm,mm)	-2/9	-2/9	-2/9	-2/9

++			—
Re $C_{11}C_{31}^*$	Re $C_{13}C_{33}^*$	Subclass I-2	Re $C_{12}C_{32}^*$
-1/6	-1/6	L(ℓn,mm)	+1/3

Im $C_{11}C_{31}^*$	Im $C_{13}C_{33}^*$	Subclass I-3	Im $C_{12}C_{32}^*$
-2/3	-2/3	L(m,mm)	+4/3

Re $C_{11}C_{13}^*$	Re $C_{31}C_{33}^*$	Subclass I-4	Re $C_{21}C_{23}^*$
-1/6	-1/6	L(mm,ℓn)	+1/3

Im $C_{11}C_{13}^*$	Im $C_{31}C_{33}^*$	Subclass I-7	Im $C_{21}C_{23}^*$
+2/3	+2/3	L(mm,m)	-4/3

PHOTOPRODUCTION OF N^* RESONANCES IN THE QUARK MODEL

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I must apologize for changing the title of the talk. It was originally meant to be on phase shift analysis but Dr. Roper has covered that so thoroughly that it seemed to be the best thing to try and make some comment on resonances which he just discussed. The comment I shall make is a model dependent comment, and the model is the quark model for elementary particles. I shall discuss particularly the resonances s_{11}, d_{13}, s_{11}, and d_{15} which Dr. Roper talked about and which have got negative parity and $T = \frac{1}{2}$. The quark model is a very simple-minded model: three quarks are in a potential well in a nonrelativistic way. As pointed out by Dalitz,[1] these resonances fit rather neatly into a quark model in which the quarks have orbital angular momentum, L = 1. Each quark would have half-spin so the spin configuration can be $S = 1/2$ or $S = 3/2$. To maintain Fermi statistics when one has a wave function of the form (spin × (unitary spin) ×(space), the multiplets of Fig. 1 occur. On the left-hand side of Fig. 1 are the quartet P states having total angular momentum 1/2-, 3/2-, 5/2- . The doublet P states have 1/2- or 3/2- total angular momentum. We can see that we have two $T = \frac{1}{2}$ states in the octet state which we will concentrate on. We have two s_{11} states, which appear to be filled, a d_{15} state which appears to be filled, and we are a little embarrassed by having two d_{13} states, having found only one d_{13} resonance. However, it could easily have been missed if it were very inelastic. (The S_{31} state fits into a decuplet as a 1/2-state.) There are not too many states left over to be discovered.

Now let us consider photoproduction of the resonances: $\gamma + N \to N^*$.

First of all, for a resonance we are not discussing, namely the $N^*(1236)$ (the P_{33} resonance), Becchi and Morpurgo have shown[2] that in the non-relativistic quark model, the E_2 transition vanishes and process (1) vanishes, leaving only the M1 transition. This is in accord with observation and is one of the reasons why some people are disposed to take the non-relativistic quark model rather seriously.

Now let us discuss the electromagnetic transitions — $\gamma + N \to N^*$, $2S_{1/2} \to {}^4P_J$ when the N^* are the ^4P states of the quark model which includes the d_{15} state. The interaction operator inducing this transition can be split into two parts:

1. The part involving the interaction with the electric charge of the quarks.
2. The part involving the magnetic moment of the quarks, i.e., the operator $\Sigma\mu_i\underline{\sigma}_i e^{ikr_i}$ when μ_i is the magnetic moment of the i^{th} quark.

Since we are inducing a ${}^2S_{1/2} \to {}^4P_{J=1/2,3/2,5/2}$ transition involving a change from quark spin 1/2 to quark spin 3/2, it is obvious that 1 must vanish as it does not involve any quark spin operator. Explicit evaluation with the correctly anti-symmetrized wave-functions shows also that 2 must vanish.[3]

Two papers have appeared[4] giving a peak in η photoproduction near the threshold — one paper surmises that this corresponds to the lowest

s_{11} states for s-wave $N\eta$. It is at around 600 MeV pion kinetic energy. Thus in the quark model we can ascribe the lowest s_{11} and the d_{13} resonance to the ^2P quartet and say that the other, ^4P, s_{11}, d_{13}, d_{15} resonances are not photo-produced.

1. R. H. Dalitz, Proc. Oxford International Conf. on Elementary Particles, p. 157 (Rutherford Laboratory 1965).
2. C. Becchi and G. Morpurgo, Phys. Letters 17, 352 (1965).
3. R. G. Moorhouse (SLAC preprint - to be published).
4. C. Bacci *et al.*, Phys. Rev. Letters 16, 157 (1966); R. Prepost *et al.*, Stanford preprint HEPL-384 (1965).

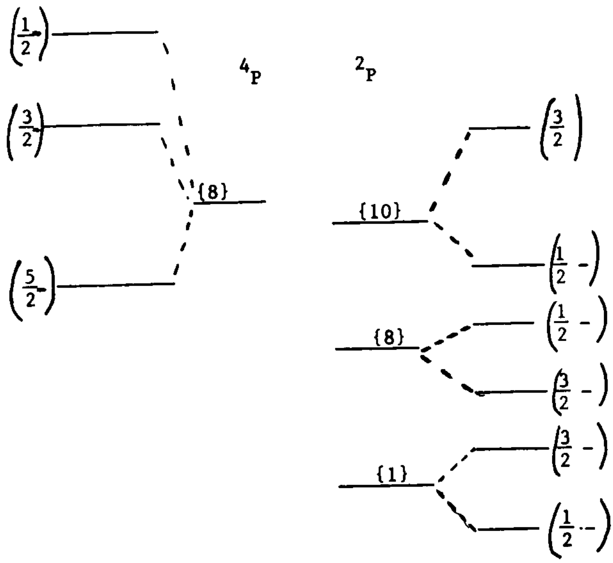

Figure 1. Quark Model multiplets in Dalitz diagram. The baronic multiplets with negative parity expected to occur for an L = 1 space wave function with mixed symmetry.

MORAVCSIK: You can't say anything about the masses without assuming something abou. symmetry breaking.

MOORHOUSE: The usual assumption is that the splitting is by an L-S coupling and then, according to the sign of the L-S coupling, you have one of the other of the states. I don't think that it is possible to say anything else with reasonable assumptions, because the forces between the quarks that produces these states must be extremely complicated.

ROPER: Is this the model where you have two D_{13} states, but do not have a P_{11}?

MOORHOUSE: P_{11} does not exist in these states which I have written on the board but the quark model is so flexible that one can accommodate though with a little difficulty. Dalitz, for instance, assigns it to a symmetrical space wave function as against the barion octet and decaplet which is assigned to an antisymmetrical space wave function.

ROPER: There are two D_{13} states that you have here and only one observed.

MOORHOUSE: Yes.

ROPER: I noticed in the Sacly results that they have a strange behavior toward one Bev in the D_{13} state.

MOORHOUSE: Yes, it is quite possible that there is a second D_{13}, a displaced resonance with background.

A COMPARISON OF RESONANCE FORMULAS FOR THE (1236 MeV, 3/2) Pion Nucleon Resonance (*)

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SUMMARY - To obtain information on the energy dependence of relativistic particle resonances, a comparison among six commonly used expressions for the total cross-section in the reaction $\pi^+ + p \rightarrow \pi^+ + p$ has been made in the energy region between 38 and 307 MeV (pion lab kinetic energy). The parameters in these expressions were systematically varied until the best fit (minimum chi-square) to fifty points was achieved for each formula. It was found that a significant statistical difference exists among some of the formulas used.

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(**) NASA Predoctorial Fellow

1. — INTRODUCTION.

The phenomenon of resonance exists in many different areas of physics in a number of different guises. One of its most recent manifestations is in the field of particle physics, where, since 1952, many so-called particle resonances have been discovered. From the example of low energy nuclear physics it would be entirely reasonable for such a resonance, if it existed, to appear as a peak in the total cross-section as a function of energy in some appropriate particle reaction. It is not our purpose here to enter into a discussion and critique of the criteria that might be used to determine whether a resonance phenomenon exists in a particle reaction. Rather, we are concerned with raising the questions of the shape of a particle resonance, given that one exists, or more explicitly, with discussing the energy dependence of the total cross-section of a particle reaction in the vicinity of a resonance. The point is that the description of resonance phenomena in a number of different areas of physics has a solid theoretical base. This is especially true in nuclear physics where non-relativistic quantum mechanics can be used to justify the Breit-Wigner formulas(1) to describe nuclear resonances. We are not aware of the existence of a theory with comparable generality and well-defined basis which describes the energy dependence of relativistic particle resonances.(2) As is to be expected, therefore, several different formulas have been proposed for such a purpose.

For at least two reasons it is of interest to compare the success of some of these formulas in describing particle resonances. First, formulas that fail to do the job can be dropped from further use. Second, any formula or class of formulas that gives a reasonably decent description of particle resonances provides at least that much insight into what a well-formulated theory of the process will be required to produce.

In pursuit of the above objectives, six expressions for the total cross-section have been compared with data on the reaction $\pi^+ + p \rightarrow \pi^+ + p$ in the energy region between 38 and 307 MeV (pion lab kinetic energy). It is in this energy range that one of the best known particle resonances, and the first one to be discovered, is found. This resonance occurs in a pure spin 3/2, i-spin 3/2 nucleon state. We have chosen to work with this particular resonance for three basic reasons. First, there is an abundance of experimental data for this resonance.(3) Second, the scattering is elastic so that complications from other channels are minimized. Third, the background contributions from states other than the resonating state are quite small, and one can treat this background as essentially constant when the incident laboratory kinetic energy of the pion is less than 300 MeV.(4)

2. — THE FORMULAS.

The formulas used are listed in Table I. where we have chosen $\hbar = c = 1$. In the last column for the fifth formula it should be noted that γ_λ^2 is determined when E_r, Γ_r and a are known. For future reference the six expressions will be referred to by the number associated with them in the table. The meanings of the symbols are listed below.

γ_λ^2	----- a reduced half width
a	----- channel radius
m	----- mass of pion
E_r	----- resonant energy
γ	----- dimensionless reduced width
B	----- background (treated as a parameter)
Γ_r	----- energy independent width at resonance energy E_r
E	----- total center of mass energy
η_r	----- pion center of mass momentum at the resonance energy E_r
η	----- pion center of mass momentum
M	----- mass of proton
A	----- proportionality constant
f^2	----- coupling constant (dimensionless)
X	----- 350 MeV

(1) $$\rho(E) = \frac{(E + M)^2 - m^2}{E^2}$$

(2) $$\sigma = \frac{8\pi}{\eta^2} \frac{\left(\frac{\Gamma}{2}\right)^2}{(E - E_r)^2 + \left(\frac{\Gamma}{2}\right)^2}$$

(3) $$\sigma(\Gamma_r) = \frac{8\pi}{\eta^2} \frac{\left(\frac{\Gamma}{2}\right)^2}{(E - E_r)^2 + \left(\frac{\Gamma_r}{2}\right)^2}$$

(4) $$\sigma' = \frac{8\pi}{\eta^2} \frac{E_r^2\, \Gamma^2}{(E^2 - E_r^2)^2 + E_r^2 \Gamma^2}$$

3. - DISCUSSION OF THE FORMULAS.

I. The first formula that appears in Table I is a Breit-Wigner formula. Expressions of this general form have had much success in atomic [5] and nuclear [1] physics. In 1954, following the work of Bruckner, [6] Gell-Mann and Watson [7] used this form to fit π^+ p scattering data up to 400 MeV.

II. The second formula is also a Breit-Wigner formula. It is in form the same as I. but one parameter has been fixed. This parameter was fixed by Glashow and Rosenfeld [8] on the basis of experimentally measured partial widths for the γ -octet and δ -decuplet of SU(3).

III. The third expression is the one used by W. M. Layson. [9] It is interesting to note that it is possible to insert the expression for Γ into the total cross-section formula and obtain

$$\sigma = \frac{8\pi}{\eta^2} \frac{(\Gamma'/2)^2}{(E_r^2 - E^2)^2 + \left(\frac{\Gamma}{2}\right)^2} \tag{5}$$

where

$$\frac{\Gamma'}{2} = 2 \; \gamma m M \; \frac{(\eta a)^3}{1 + (\eta a)^2} \tag{6}$$

Here we see the total cross-section has been expressed in terms of the Mandelstam variable E^2.

IV. This equation comes from the Chew and Low expression for the resonance we are considering. It was proposed by consideration of an effective range treatment of the scattering of pions by nucleons in a static meson theory.[10] The same expression has been derived using a dispersion theory approach.[11] We have used the form of this equation reported by Nishijima[12] for the purpose of fitting.

V. This expression has been obtained by modifying the Breit-Wigner formula, I., with the additional requirement that the Γ that appears in the denominator of the expression for σ be replaced by Γ_r, the value of Γ at the resonance. The reason for doing this was to determine whether or not there was a significant difference between I. and V.

VI. The sixth expression, given by Jackson,[13] was derived by an approach partly based on perturbation theory. We see that the cross-section σ' is written in terms of the Mandelstam variable E^2, and in this respect it is similar to the empirical expression used by Layson.

4. — TREATMENT OF EXPERIMENTAL DATA.

The compilation of data by Klepikov et. al. [3] was used in the analysis with the following criterion for selection of points. All points were excluded that were given zero weight by Klepikov. These were mainly experimental results that were later generally believed to be inaccurate. Of the remaining points those were chosen that had a standard deviation of the total cross-section not greater than 8 millibarns, and a standard deviation of the mean kinetic energy not greater than 4 MeV. In the energy region between 35 and 310 MeV there were 55 points that met the above requirements.

The data were fitted to an expression of the form

$$\sigma_{total} = \sigma + B \qquad (7)$$

for each of the formulas, where σ and B are defined in section 2. In each case the parameters were systematically varied until a best fit (chi-square minimum, χ^2_{min}) was achieved. The parameters that were varied are indicated in Table I. In all six cases at least five points fell approximately three standard deviations from the curve with minimum χ^2. (The same five points were associated with formulas I. through IV. and different sets of five with V. and VI.) In Fig. 1 the three sets of five points are indicated. For each formula the corresponding five points were discarded and the parameters were again systematically varied until a best fit was obtained for the remaining 50 points. The results of this work are summarized in Table II and in Fig. 1 and Fig. 2.

5. — DISCUSSION AND CONCLUSIONS.

Examination of Table II and Fig. 2 shows that the difference between I. and III. can not be distinguished. The values of 52 for χ^2_{min} are not unreasonable especially in view of the many different experiments that were involved. Since I. and II. have the same form, there should be no difference between the two if X is allowed to vary. Clearly, however, the best value for X would not be found to be 350 MeV by this method. The parameters E_r, α, and χ^2_λ are given by Gell-Mann and Watson[7] as 1238 MeV, 0.88 ($\hbar$/mc), and 58 MeV respectively. It can be seen that our values are not in good agreement with theirs. This may be attributed to the fact that they did not have as much data to work with and we have included B in the expression for σ_{total}. In I. there is a strong correlation between γ_λ^2 and α. By this we mean that to increase (decrease) α and decrease (increase) γ_λ^2 would cause only a slight increase in chi-square. This strong correlation also causes the final values obtained for the parameters to be unusually sensitive to the data points.

In III. we find the same correlation, mentioned above, between γ and α. Layson[9] gives 1238 MeV, 0.71 ($\hbar$/mc) and 0.37 for E_r, α and γ respectively. The most striking difference between these results and the ones in Table II is the value for γ. Once again this could be due to the strong correlation and the sensitivity to the data used.

The χ^2_{min} associated with IV. is 90; however, one should be aware of the fact that, neglecting B, there are only two adjustable parameters in the expression for the total cross-section, while in I. and III. there are three. The value quoted by Chew and Low[10] for f^2 is 0.08. A discussion is given by Bernardini[14] of various determinations of f^2. He concludes that the best value is 0.0813 ± 0.0035. It should be pointed out that this value was obtained by an extrapolation method from a Chew-Low plot, while we have fitted the data to a total cross-section formula with a background term.

There is clearly a significant difference between I. and V. as can be seen by the corresponding values for χ^2_{min}. Apparently one should not use V. to describe this resonance, since the proper asymmetry to the right of the maximum can not be achieved with this form.

It appears that the energy dependence of Γ in VI. is the source of the relatively poor fit obtained. We note that over the energy region involved the factor $\rho(E)$ is a slowly varying function, and the expression for Γ varies approximately as $\Gamma_r \left(\frac{q}{q_r} \right)^3$ which can be compared with (6). Here, as in IV., we have a theoretical expression with one less adjustable parameter than in the empirical cases.

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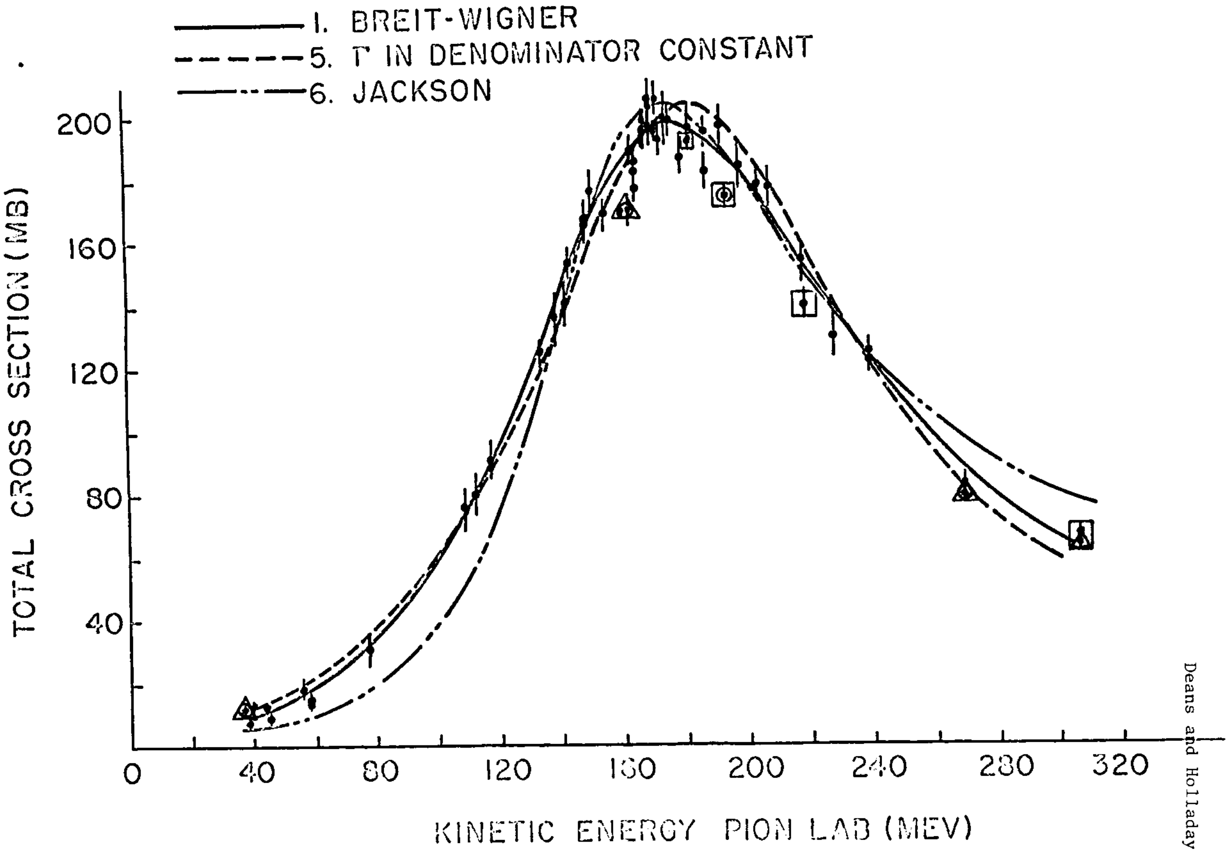

Fig. 1. — Best fits for I., V. and VI. using 50 points. The 55 original points are shown. The encircled points were dropped for I. through IV., those inside a rectangle were dropped for V., and those inside a triangle were dropped for VI.

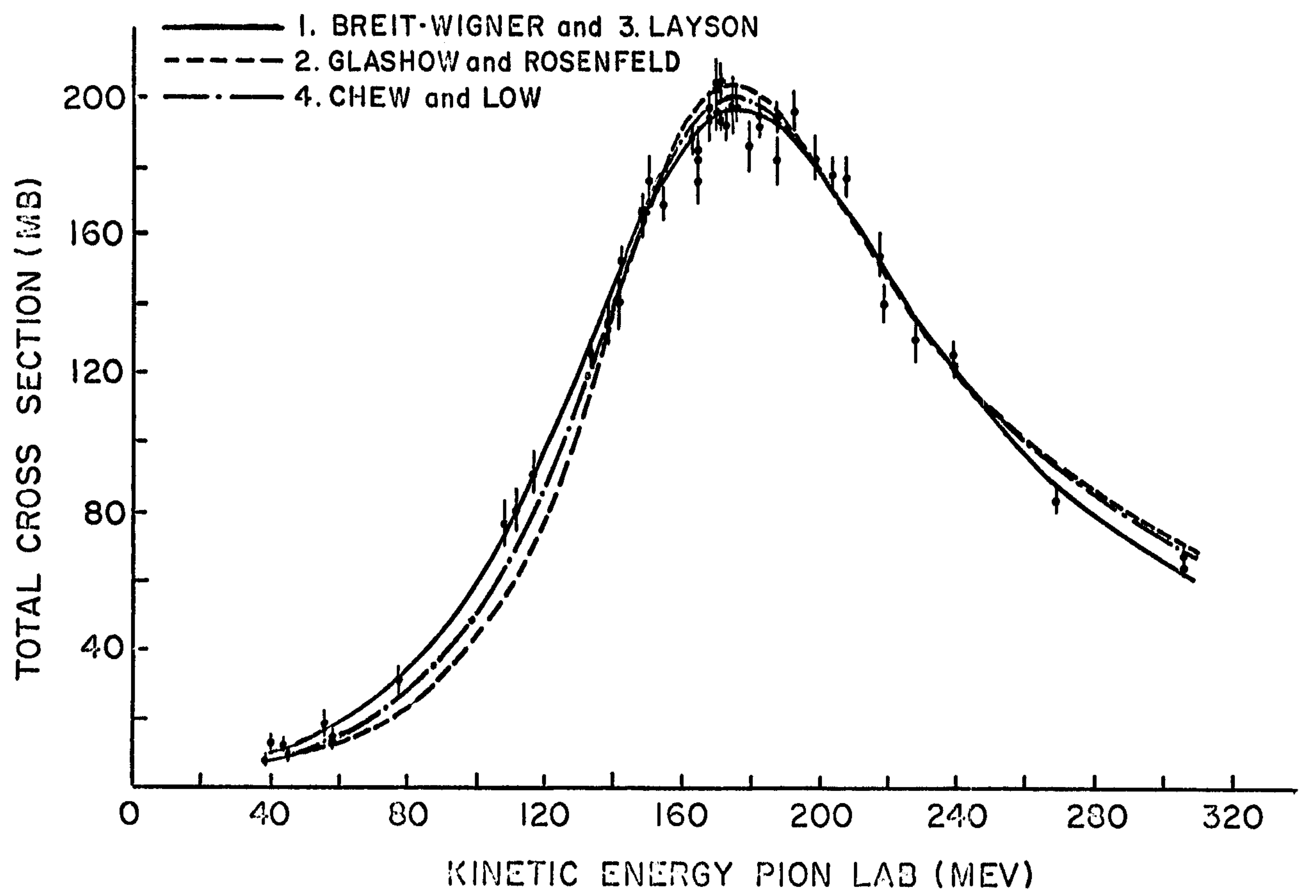

Fig. 2. — Best fits for I. through IV. using 50 points.

TABLE I. - The Formulas

Identification	Total Cross Section	Energy Dependent Width	Parameters
I. Breit-Wigner (7)	$\sigma+B$	$\Gamma = 2\gamma_\lambda^2 \dfrac{(\eta a)^3}{1+(\eta a)^2}$	$E_r, \gamma_\lambda^2, a, B$
II. Glashow-Rosenfeld (8)	$\sigma+B$	$\Gamma = A \dfrac{\eta^2}{\eta^2+x^2} \dfrac{\eta}{E_r}$	E_r, A, B
III. Layson (9)	$\sigma+B$	$\Gamma = \dfrac{4mM\gamma}{(E+E_r)} \dfrac{(\eta a)^3}{1+(\eta a)^2}$	E_r, γ, a, B
IV. Chew-Low (10)	$\sigma+B$	$\Gamma = \dfrac{8m}{3} f^2 \eta^3 \dfrac{(E_r-M)}{(E-M)}$	E_r, f^2, B
V. Γ Constant in Denominator	$\sigma(\Gamma_r)+B$	$\Gamma = 2\gamma_\lambda^2 \dfrac{(\eta a)^3}{1+(\eta a)^2}$	$E_r, \Gamma_r, \gamma_\lambda^2, a, B$
VI. Jackson (13)	$\sigma'+B$	$\Gamma = \Gamma_r \left(\dfrac{\eta}{\eta_r}\right)^3 \dfrac{\rho(E)}{\rho(E_r)}$	E_r, Γ_r, B

TABLE II. - The parameters obtained for the curves with minimum χ^2 when 50 points were used. In the last column the calculated width at resonance is given.

Identification	E_r (MeV)	γ_λ^2 (MeV)	γ	a ($\hbar/mc$)	A (MeV)	f^2	B (mb)	χ^2_{min}	$\overline{\chi^2}$ (*)	$P(\chi^2 > \chi^2_{min})$	Γ_r (MeV)
I. Breit-Wignor	1236	71		0.81			4	52	46	0.26	121
II. Glasbow-Rosenfeld (8)	1232				2054		6	151	47	$\ll$ .0001	112
III. Layson(9)	1236		0.78	0.74			4	52	46	0.26	121
IV. Chew-Low(10)	1234					0.072	4	90	47	.0001	118
V. Γ Constant in Denominator	1224	27		1.42			0	99	46	$<$ .0001	100
VI. Jackson (13)	1232						4	208	47	$\ll$ .0001	120

(*) $\overline{\chi^2}$ is the expectation value of χ^2.

MORAVCSIK: In your last paper, in an absolute sense, none of the fits were spectacularly good, if I read the values right. None of these curves really fit too well.

DEANS: It is my opinion that the data are somewhat self inconstant. We obtain data from a great many different experimentors. It is my opinion that this is one of the reasons for the essentially poor fit obtained. One should not look at the absolute values in the chi square column but at relative values in the chi square column.

WOLFENSTEIN: Do I understand correctly that you assume a constant background? The fact that various other partial waves begin to grow is not taken into account at all.

DEANS: We have assumed a constant background and we have restricted our energy region to be less than 310 Mev, so that we could essentially assume a constant background. If one goes higher, one would expect that one might have to add in velocity dependence or energy dependence, or something of that sort.

WOLFENSTEIN: Since there are phase shift analysis that go down this low, one should have some idea of what that that background cross section is. Furthermore, knowing roughly that it shouldn't be chosen as a constant but varies as q or q^3 or something like that, the cross section in the other partial waves could be treated in a more sensible fashion than saying that the sum of the other partial waves contributing to the cross section is just a constant.

DEANS: That is true. However, it was shown in a paper by Olson, Physical Review Letters, 14, 1965 that over the energy region in which we are considering, the background contribution was approximately 1 millibarn. Consequently, since the lines on our graphs are at least that wide, we saw no trouble in dropping the possible change.

PHILLIPS: From a number of years of experience in doing phase shift analysis at very much lower energies, it seems to me perhaps that Professor Wolfenstein's comments are quite to the point, that what one really has to do to test the energy dependence of cross sections or phase shifts is to extract out just the particular one that's resonating. However, if the other phase shifts are all essentially zero, then your arguments are sound.

n-n S-wave Scattering Length from the Neutron Spectra of the Reaction $\pi^- + d \rightarrow 2n + \gamma$. *

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A determination has been made of the neutron-neutron S-wave scattering length giving a_{nn} = -16.47F ± 1.27F. Comparison with measured values of a_{pp} and $^1a_{np}$ (where the coulomb forces in a_{pp} have been extracted using potential theory[1]) indicates charge symmetry of nuclear forces but not charge independence. The experiment recorded the neutron time-of-flight and angle spectra from the reaction $\pi^- + d \rightarrow 2n + \gamma$. Negative pions from the Berkeley 184 inch cyclotron were stopped in a liquid deuterium target. γ-ray detection in coincidence with an incoming pion and succeeded by two (only) neutrons within 300 nanoseconds was required. The P-wave contribution was minimized by restricting n-n relative momenta in their center-of-mass system to q < 50 MeV/c. (According to theoretical predictions[2] this contribution should be < two per cent.) Preliminary results[3,4] gave a larger uncertainty in a_{nn} than quoted above. The analysis of the data has been extended to include an independent determination of a_{nn}, using the neutron angle spectra. Also, extensive data processing

*Work supported in part by the U. S. Atomic Energy Commission and the National Science Foundation.

including Monte Carlo computer synthesis of the experiment and χ^2 fitting program has permitted further improvement in results.

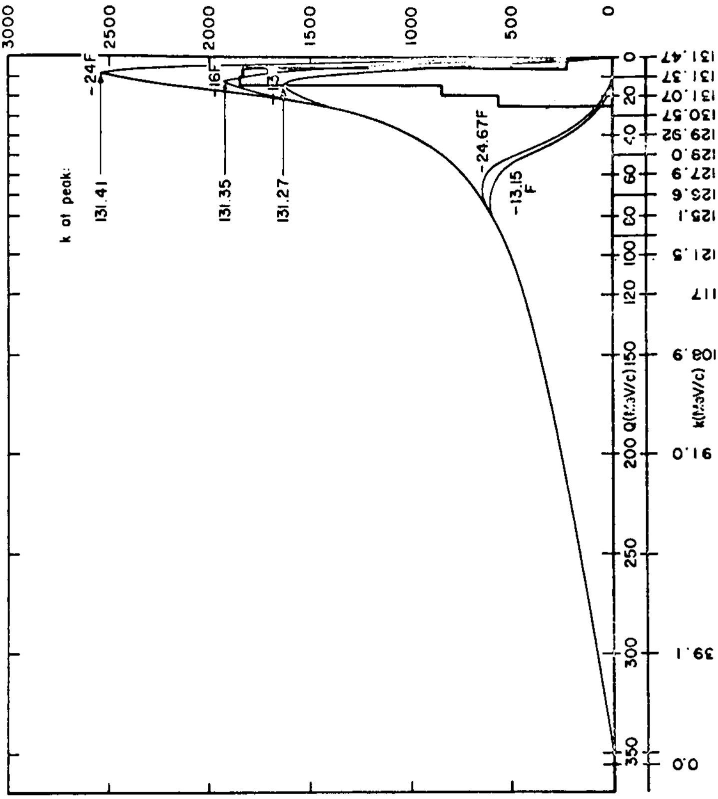
The parameter a_{nn} is representative of the purely nuclear forces between two neutrons and is important in the verification of the principles of Charge Symmetry and Charge Independence enumerated some 30 years ago by Breit, and others.[5] The p-p and n-p scattering lengths a_{pp} and a_{np}, have been accurately known for many years although there are uncertainties in the process for extracting the coulombic effects upon a_{pp} and in separating the triplet and singlet spin states in a_{np}. The respective values thus determined indicate violation in Charge Independence by several standard deviations. The question is, "Does this discrepancy indicate a breakdown of Charge Independence or was the process for determining a_{pp} and a_{np} wrong?" As we will see later, Charge Independence does indeed appear to be violated, while the principle of Charge Symmetry does appear to be confirmed.

a_{pp} and a_{np} were experimentally determined by scattering protons or neutrons, respectively, on free hydrogen. Equivalent targets of free neutrons have not been attained, being orders of magnitude short of a sufficient concentration for reasonable counting statistics. Thus, the only avenues available are those of scattering neutrons from nuclei such as deuterons or tritons, or in creating a di-neutron and measuring the distribution of its decay products. The former course has been investigated principally by the Yugoslavian group of Ilakovac, and others.[6] This method suffers principally from the presence of other strongly interacting particles in the final state interactions (besides the two neutrons). The latter technique, first suggested by Watson[7] has been used by Phillips and Crowe[8] and by Ryan.[9] In both of these experiments the γ-ray spectrum from the reaction $\pi^- + d \to n + n + \gamma$ was measured by a pair spectrometer. Fig. 1 shows this spectrum, which shows theoretical predictions for a_{nn} = -24F, -16F, and -13F. Note that only in the region of low relative neutron-neutron, center-of-mass momentum (Q less than 25 MeV/c) is the distribution sensitive to different a_{nn} hypotheses. This is tantamount to the γ-ray carrying away better than 96 percent of the kinetic energy in the reaction. Note the two curves plotted in Slide 1 are transpositions of theoretical spectra determined by Ryan for a_{nn} = -24.67F and -13.15F with his spectrometer resolution folded in. We see that when the resolution of the γ-spectrometer is included the γ-ray spectrum is relatively insensitive to a_{nn} and that the effect of a_{nn} is spread out over a much larger range of γ-energies so that a considerable contribution from P-wave effects may be present.

McVoy[10] and later, Bander,[2] have considered this problem theoretically. Bander predicts that if the relative neutron-neutron center-of-mass momentum is restricted to values of less than 50 MeV/c, then the P-wave contribution should be less than two percent. The overall theoretical uncertainty in deducing a_{nn} for Q < 50 MeV/c was estimated to be ± 1F for the above reaction.

In our experiment, following McVoy's suggestion, we measured the spectra of the two neutrons instead. Negative pions produced by an internal Be target in the Berkeley 184 inch cyclotron were collimated

FIGURE I



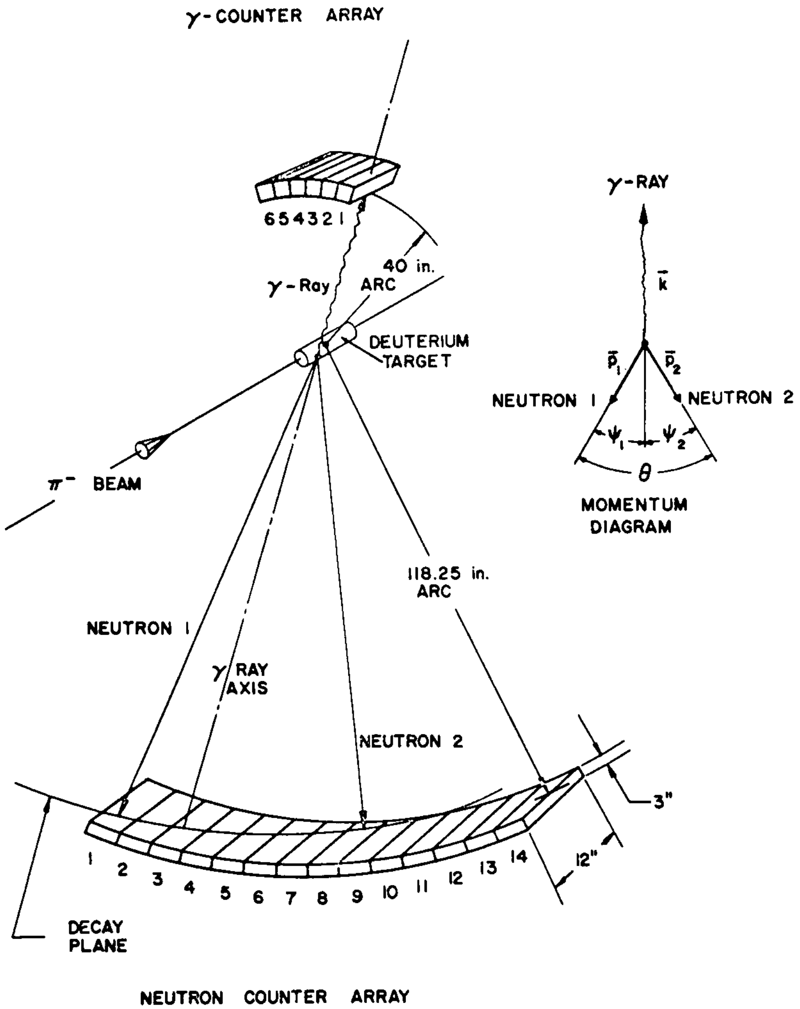
THEORETICAL γ-MOMENTUM SPECTRUM FOR $\pi^- + d \rightarrow 2n + \gamma$ REACTION.

by quadrupole magnets and momentum-selected by a bending magnet and focused on a liquid deuterium target. The energy of pion beam was reduced by a copper degrader so adjusted to yield maximum atomic capture by the deuterium. As seen in Figure 2, all three particles were detected; the γ-ray in one of six lead-and-plastic scintillators, the neutrons in two of fourteen Ne224 liquid scintillator detectors. Three plastic scintillators in triple coincidence detected the pion passage. This signal in coincidence with one (only) γ-counter pulse and followed in 300 nanoseconds by two (only) neutron detector signals formed the signature requirement for the 2n-γ event. Imposition of this constraint made background effects negligible.

The energies of the two neutrons were obtained from time-of-flight measurement over a ten-foot flight path. Two LRL time-to-height converters, using the γ-ray pulse as a start signal, were coupled to a Nuclear Data dual analog-to-digital converter and recorded in digital form by a DEC PDP-5 computer. This computer was used to produce raw data tapes and also do real-time, preliminary processing of events on an event by event basis, displaying such things as time spectra and a two-dimensional signal-to-noise representation of the cumulative goodness-of-fit of events to requirements imposed by conservation of momentum and energy. The kinetic energy available in the final state interaction is known to about ± .02 percent from current values of n,d and pion masses and the deuteron binding energy.

A back-up system simultaneously recorded on film, oscilloscope pictures of counter pulses against a calibrated sweep rate. Correlation

FIGURE 2



TARGET AND COUNTER ARRANGEMENT

between the two time-of-flight determinations was quite accurate. The film system also provided a capability for pulse height analysis of the counter signals.

It will be noted in Fig. 2 that the experimental arrangement also permitted determination of the angles among the three reaction products, thereby permitting in turn determination of the kinematics independent of the time-of-flight data. An iteration between these two data sources was performed on a event-by-event basis on IBM 7094 computers yielding an extremely sharp distribution of "good events." The peak centering around the nominal reaction energy of 136.07 MeV for channels of .01 MeV is shown in the center figure of Figure 3. The spreading of this distribution is primarily from scattering of the neutrons by deuterium in the target, resolution effects having been minimized by the iteration program.

Fig. 4 depicts the theoretical 2n-γ distribution of the energy of one neutron (E) and its angle with γ-ray, (ψ) (see Slide 2). This distribution was computed for a_{nn} = -16F. Note the relative proportion of events of γ-energy between 130.9 MeV and 131.47 MeV (maximum possible). Also note the E vs ψ distribution as a function of γ-energy.

Two independent determinations of a_{nn} were made -- one employing time-of-flight data, one using angle data. Events verified by the iterative program previously described were employed but on an uncorrected basis. Expected distributions were computed assuming the three particle phase space to be enhanced corresponding to various assumed values of a_{nn}. Bander's method[2] was used which is essentially

FIGURE 3

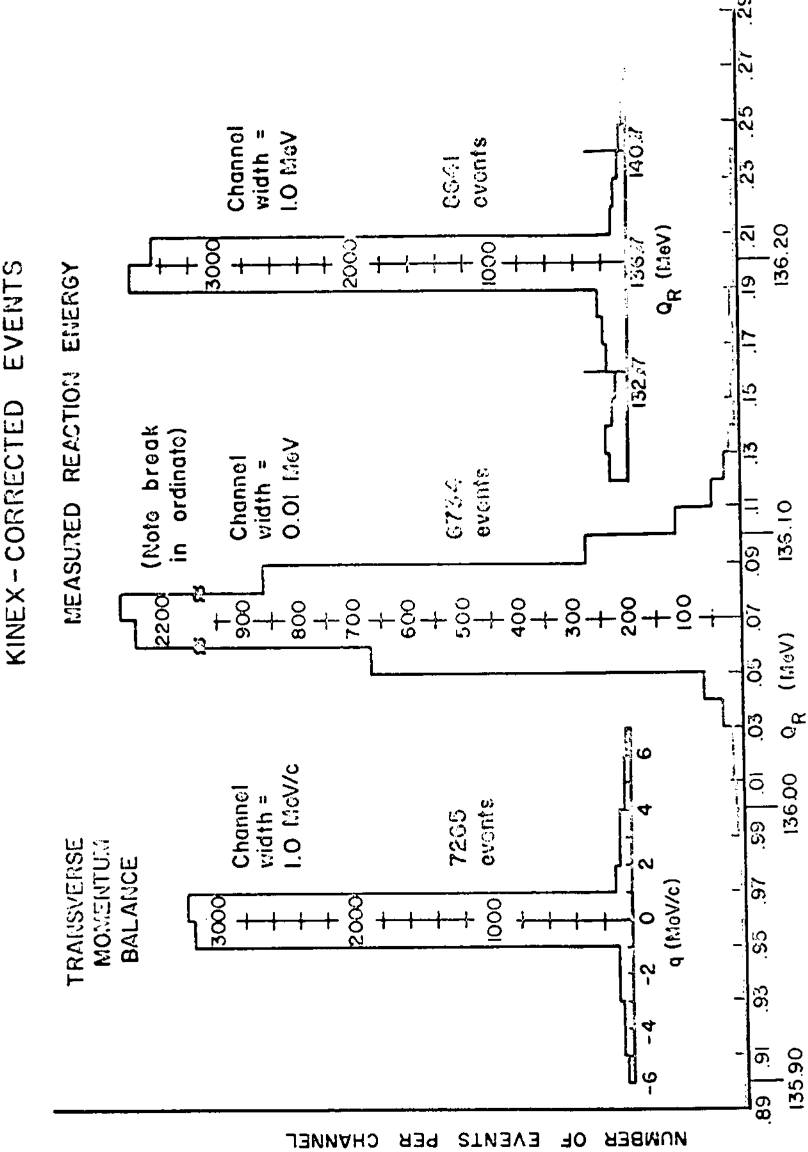
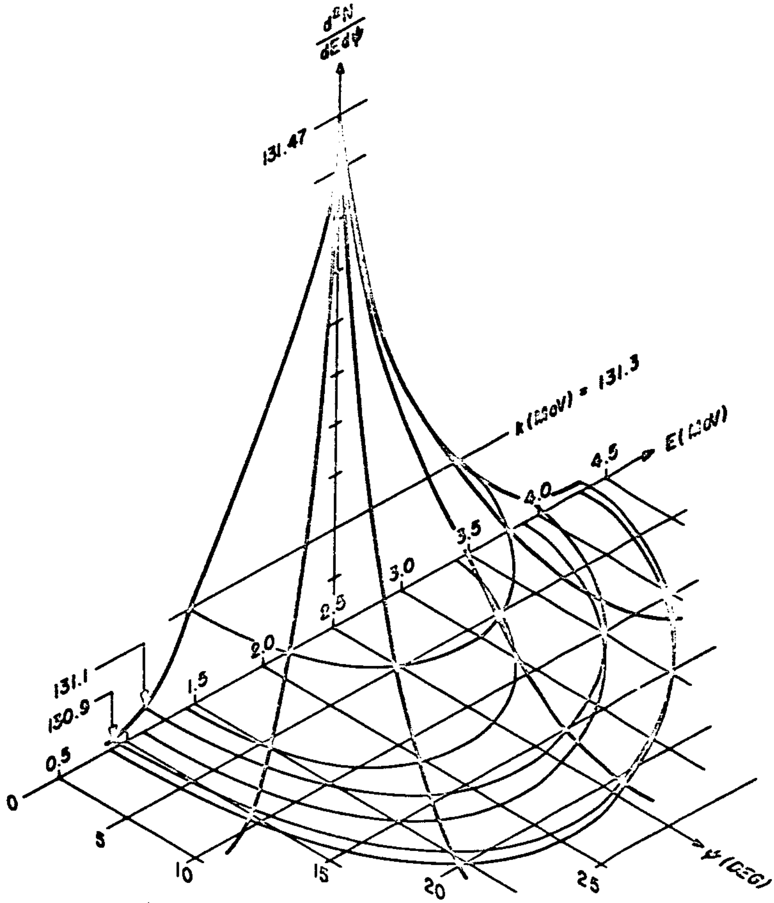

KINEX - CORRECTED EVENTS
TRANSVERSE MOMENTUM BALANCE
Channel width = 1.0 MeV/c
7265 events
q (MeV/c)
MEASURED REACTION ENERGY
(Note break in ordinate)
Channel width = 0.01 MeV
6734 events
Q_R (MeV)
Channel width = 1.0 MeV
6641 events
Q_R (MeV)
NUMBER OF EVENTS PER CHANNEL

FIGURE 4



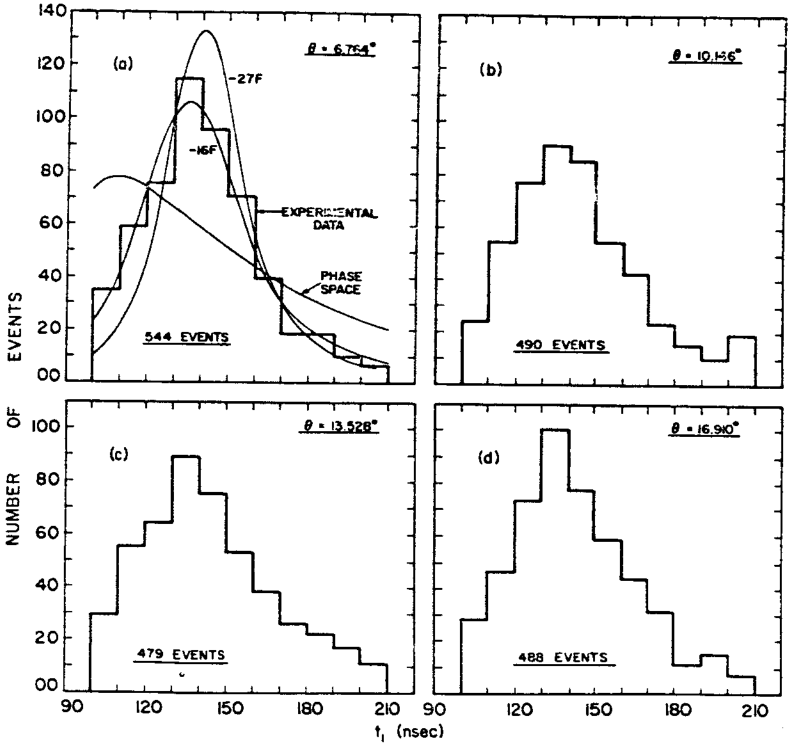
THEORETICAL $\pi^- + d \rightarrow 2n + \gamma$ DISTRIBUTION vs. ONE NEUTRON ENERGY (E) & ITS ANGLE WITH THE γ-RAY (ψ). SHOWN ARE CONTOURS OF CONSTANT γ-RAY ENERGY (k).

equivalent to the effective range approximation but with a correction for the proper transition amplitude terms including effects of the two polarizations of the γ-ray. This correction changes less than 1-1/2 percent over the range of γ-energy used.

The shape of the neutron energy spectra of the 2n-γ reaction is relatively insensitive to the angle between the two neutrons. Thus the variation in angle resolution of (say) counters 6 and 9 (in Slide 2) compared to 6 and 8 or 6 and 10 is not great and the corresponding effect on the energy spectrum negligible. Thus histograms of experimental neutron energy spectra were compared directly with theoretical distributions computed for variously assumed values of a_{nn}. A histogram of experimental data vs theory is shown in Fig. 5 for a 2-counter separation ($\theta = 6.7°$). Note the distinct fit at $a_{nn} = -16F$ compared to $a_{nn} = -27F$.

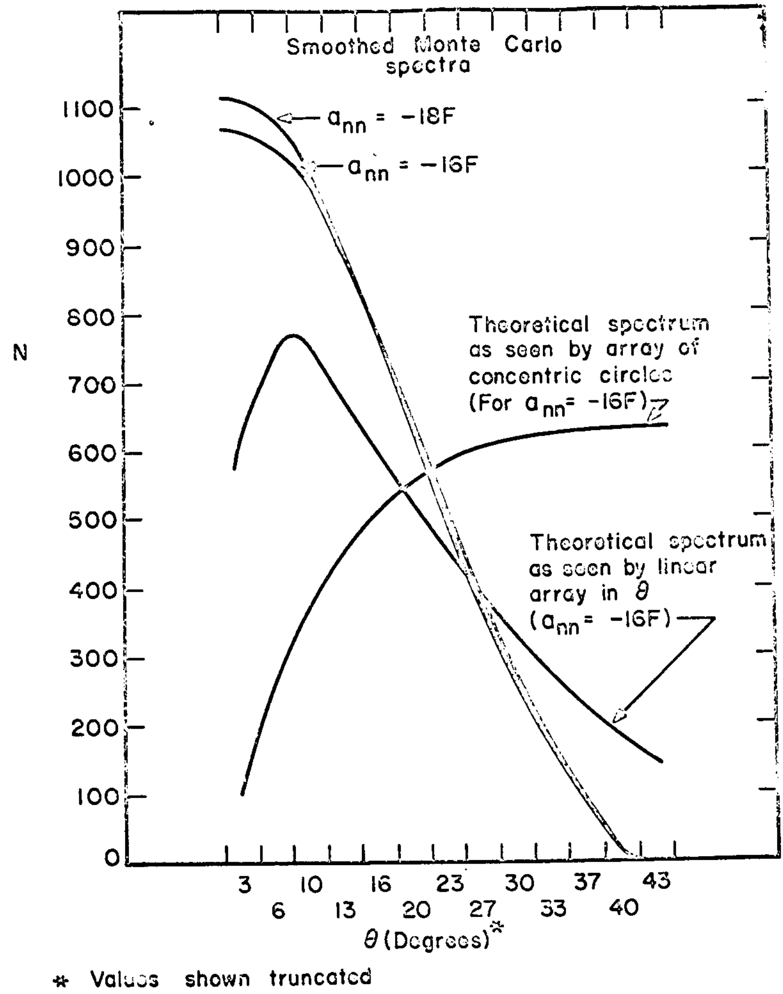
The angle spectrum for selected neutron energies is very sensitive to geometry but independent of the energy dependent efficiency of the neutron counter. Fig. 6 shows spectra expected for either a one-dimensional or two-dimensional array of neutron counters. Spectra of the experiment fell somewhat intermediate between these two situations. An analytical solution of this problem was untenable, so a Monte Carlo synthesis of the entire experiment was performed on computers starting with randomly selected production of events at various points in the target according to the initial pion beam distribution. Smoothed Monte Carlo predicted spectra for $a_{nn} = -16F$ and $-18F$ are shown (unnormalized)

FIGURE 5



HISTOGRAMS OF TIME-OF-FLIGHT SPECTRA.

FIGURE 6

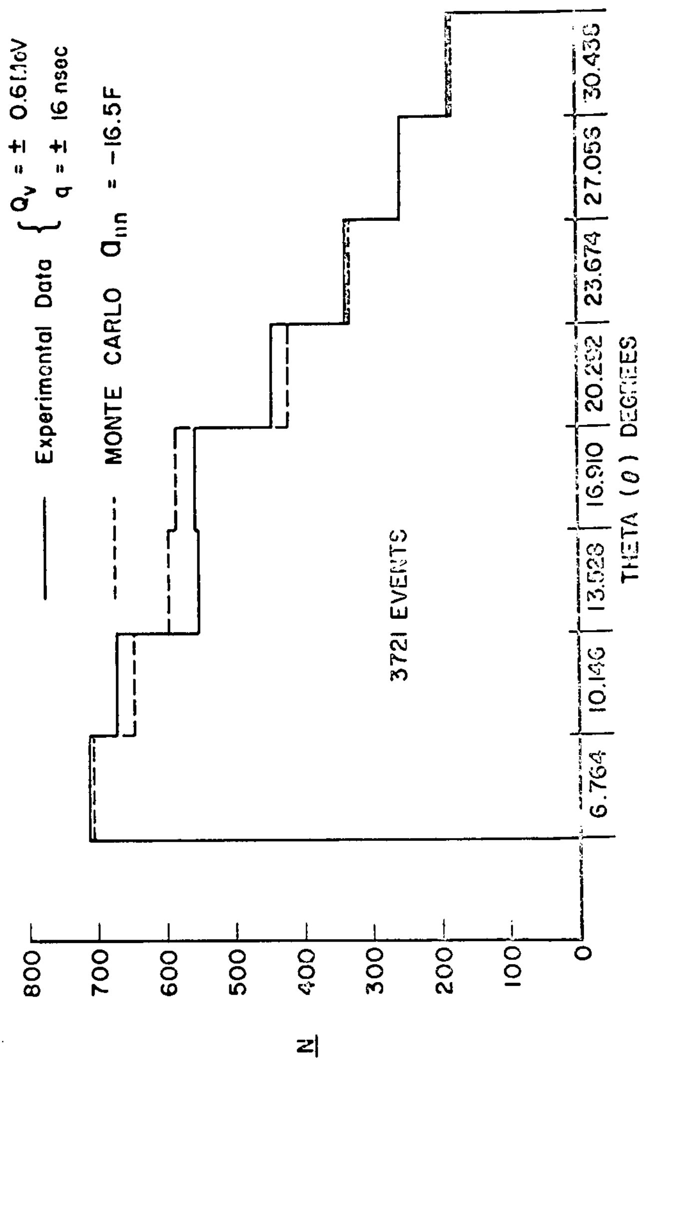


SMOOTHED MONTE CARLO THETA (θ) SPECTRA FOR a_{nn} = -16F & -18F. ALSO SHOWN ARE THEORETICAL SPECTRA IN θ.

in Fig. 6. Actual comparison between experiment and Monte Carlo is given in Figure 7.

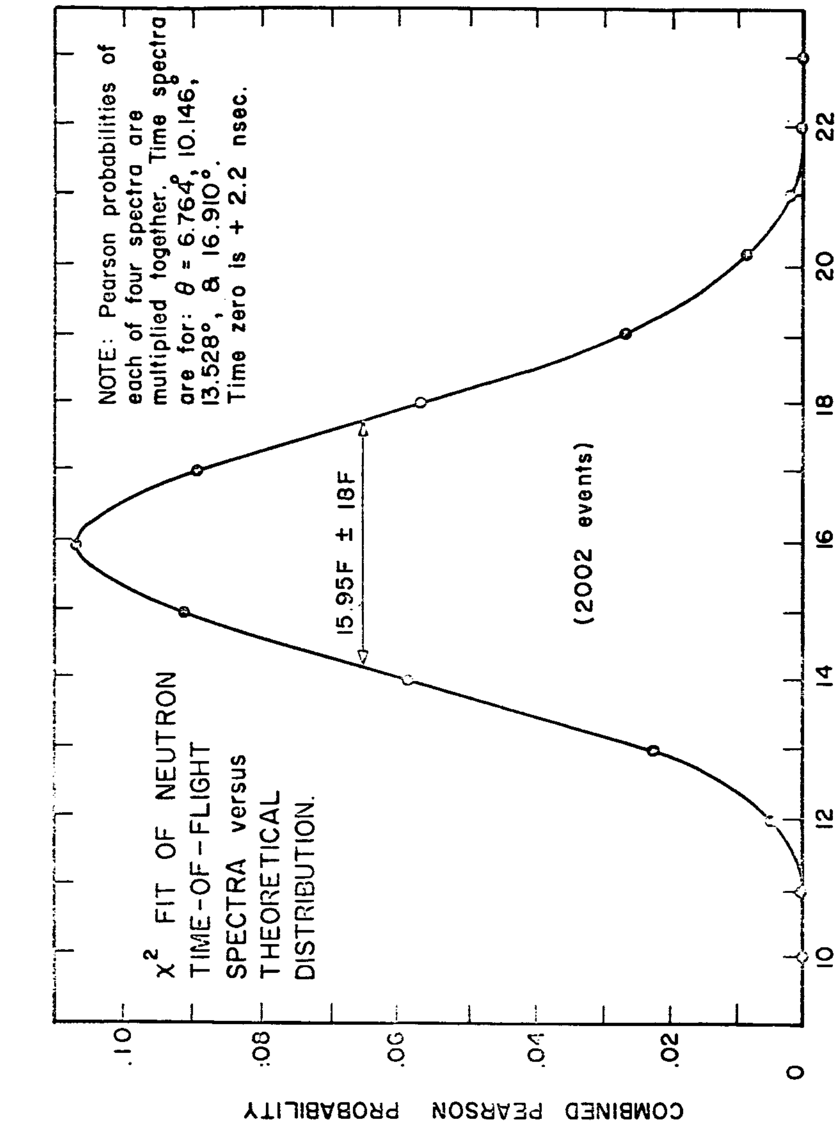
χ^2 fits of theory to experiment were also made by a computer program. The theoretical hypothesis was normalized to the number of events in the experimental energy and angle spectra separately and the χ^2 evaluated. Then the hypothesis was changed and the process repeated. A typical print out had hundredths of separately computed χ^2 summations. In this way the Pearson probability distributions were easily determined. The resulting curves were nearly gaussian and the moment of the distributions were computed accordingly. Fig. 8 shows the Pearson Probability distribution for the time-of-flight fit and Fig. 9 for the angle spectrum fit. Systematic corrections were then made to the mean values and the standard deviations were increased for effects not included in the theoretical hypothesis used for χ^2 fitting. Value of a_{nn} = -16.40F ± 1.85F and a_{nn} = -16.52F ± 1.75F were obtained for the time-of-flight and angle spectra respectively. Fig. 10 describes the nature of the effects included in this fashion. The magnitude of the effects were estimated by an independent χ^2 fitting procedure where the hypothesis was a particular value of a_{nn} and the resolution, background, efficiency, etc., were allowed to vary independently. Because time-of-flight and angle fits are independent determinations of a_{nn} the final answer for a_{nn} of a_{nn} = -16.47 ± 1.27F is a weighted mean. We plan to test the result with a more general fitting program, but we do not expect the result to change appreciably. It should be pointed out that the time-of-flight and angle fit give

FIGURE 7



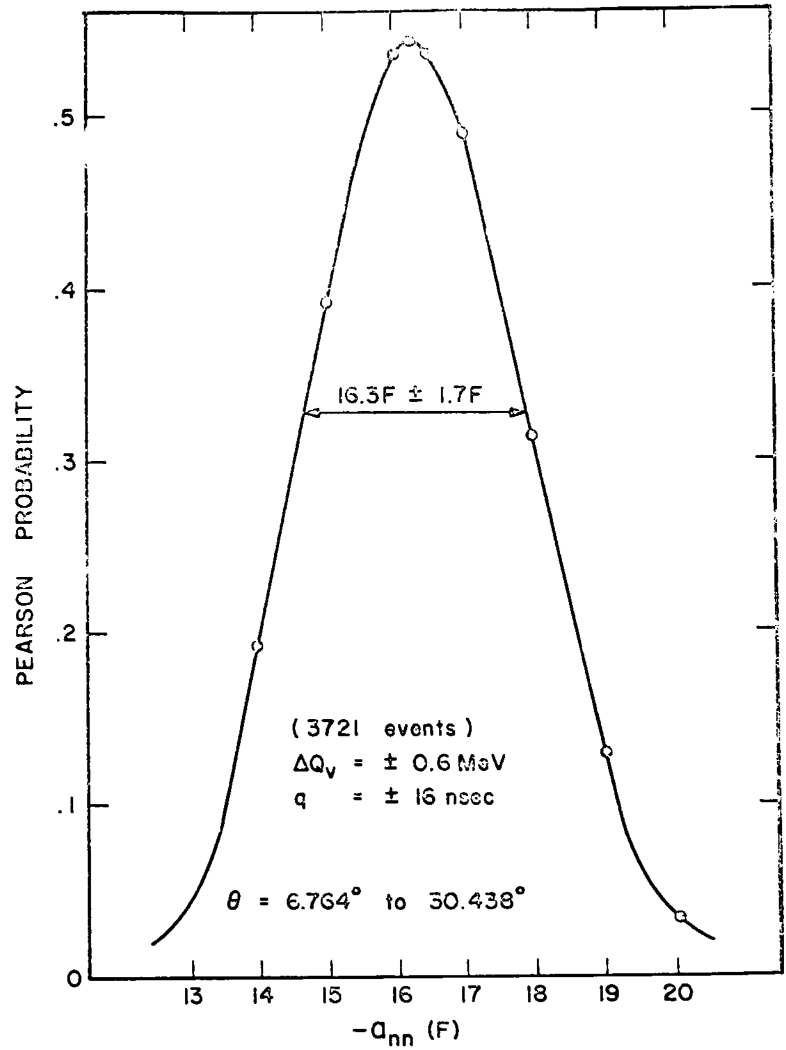
COMPARISON OF MONTE CARLO & EXPERIMENTAL θ DISTRIBUTIONS.

FIGURE 8



χ² FIT OF NEUTRON TIME-OF-FLIGHT SPECTRA versus THEORETICAL DISTRIBUTION.
NOTE: Pearson probabilities of each of four spectra are multiplied together. Time spectra are for: θ = 6.764°, 10.146°, 13.528°, & 16.910°.
Time zero is + 2.2 nsec.
15.95F ± 18F
(2002 events)
COMBINED PEARSON PROBABILITY
.10
.08
.06
.04
.02
0
10
12
14
16
18
20
22
−a_nn (F)

FIGURE 9



χ^2 FIT OF θ SPECTRUM vs. MONTE CARLO.

TABLE 1

FACTORS AFFECTING a_{nn} ACCURACY AND UNCERTAINTIES INTRODUCED THEREFROM

Factor[(i)]

The t_i Spectrum	Change in a_{nn}	Uncertainty
Raw value of a_{nn}	-15.95	± 1.8F
Neutron Counter Efficiency f(E)	- 3.0F[(ii)]	± 0.4F
Background	- 0.25F[(iii)]	± 0.1F
Timing Resolution	0.45F[(iv)]	± 0.1F
Corrected value	-16.40	± 1.85F
The θ Spectrum		
Raw value of a_{nn}	-16.3F	± 1.7F
Experiment Geometry	0.0F	± 0.02F
Background	0.0F[(iii)]	± 0.3F
Target Outscattering	0.22F[(vi)]	± 0.2F
Neutron Counter Efficiency, Relative	0.0F	± 0.2F
Corrected value	-16.52	± 1.75

Final value

(i) Factors due to time-of-flight length uncertainty, time slewing, preferential outscattering as f(E), angle resolution uncertainty in t hypothesis were found to introduce negligible effects (see Text). As discussed previously, theoretical uncertainties are about ± 1F. In addition, r_o has not been measured here. A change of r_o by ± .3F will change a_{nn} by ± .1F.

(ii) Change from constant efficiency as function of energy to actual energy dependent efficiency.

(iii) Change from no background to that estimated.

(iv)Change introduced when a five percent FWHM gaussian time spread is inserted into hypothesis

(v)No change needed since Monte Carlo used for hypothesis

(vi)Change from no outscattering, effects due to energy dependence of neutron counter efficiency are included.

nearly the same result, indicating that neutron counter efficiency and geometric resolutions are included in a consistent fashion. Also, the uncertainty in a_{nn} is nearly all statistical and could be improved provided the theoretical uncertainties are likewise improved.

The comparative value of a_{pp}, with coulombic effects extracted using potential theory of Blatt and Jackson[11] (and setting the $-e^2/r$ term = 0) or as predicted by Heller, and others[12] in a recent estimate of $-16.9F \le (a_{pp})$ nuclear $\le -16.6F$ indicates that Charge Symmetry does indeed hold. On the other hand, the value of ${}^1a_{np}$ remains at $-23.678F \pm .028F$ (with $(r_o)_{np} = 2.51F \pm .11F$).[13] Comparison of a_{nn} and ${}^1a_{np}$ clearly indicates Charge Dependence.

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MORAVSCIK: I presume the error that you give does not include the theoretical error.

HADDOCK: The theoretical error, as far as I understand it, except for this sort of calculation, would be around ± 1 F.

MORAVSCIK: I was going to say that my recollection of Bander's paper is that he gives ±1F. There are some people who think that this is somewhat optimistic, considering that a number of assumptions go into the theoretical scheme that relates the spectrum to the scattering length. I just wanted to say that if you add that to it, you probably get a larger error, and, in fact, I think your experiment comes pretty close to the limit of what is worth while doing experimentally at the present time in view of the fact that there is this uncertainty in the theory.

HADDOCK: The object of the experiment was to go to what the theorists thought they could calculate.

TELEGDI: I would like to offer this in the line of a comment, also. The reaction $\mu^- + d \rightarrow n + n + \gamma$ would be very worthwhile to investigate as a possible source of information the muon interaction in a rather understandable situation. The reaction question has been considered by such theorists as Wolfenstein, Uberall and, most recently, by Dr. Bietke at Cal. Tech. and myself. The grand finale is simply that if you know the scattering lengths, then all the strong interaction final state effects factor out, and the weak interaction is left alone. The experimental idea is that whereas the capture cross-section in deuterium is extremely small, you can use the coincidence between the two neutrons and the relative time delay between them to fish out this reaction as against background whereas in the case of mu capture in hydrogen there is only a single neutron. So, the factoring and this information should enable one to do an attractive experiment for muons. It's a method of intensity, of course, but this work, in some sense, makes that experiment very attractive. The techniques would also be very similar. Of course, there would be no neutrino counter.

BREIT: Is the value that you gave us a suitably weighted mean of the two ways of determining the scattering length or is it to be considered to be an independent value?

HADDOCK: It's a straightforward procedure. What you do is to change the hypothesis, which in this case is a_{nn}, fixing the effective range at some reasonable value. I should say that the error due to the effective range is negligible. You then go through and χ^2 both of the distributions find the overall minimum in the two independent distributions and that, then, gives you your result. When you're dealing with χ^2, there's always some uncertainty about what the Pearson probability really means. What I was attempting to show on Slides 8 and 9 was our version of it. That is, if you plot the Pearson probability corresponding to the χ^2 for the given number of degrees of freedom, you get some sort of Gaussian curve, which we interpreted in a Gaussian fashion.

BREIT: Thank you. Another thing I wanted to ask is if someone knows what's the matter with that Russian determination of the scattering length. It was reported at the Paris conference.

HADDOCK: Well, I'm not really familiar with that work. I should say that this is not the only experiment of this nature. There was one done at Liverpool, I believe, in which just the gamma ray and one neutron was detected.

BREIT: That was a low energy experiment and therefore more subject to question There was more theoretical calculation involved than in your experiment.

HADDOCK: Right.

BREIT: I wonder why they (the Russians) got a larger absolute value.

HADDOCK: Well, my understanding is that you have opened up a sort of wound, if you like. People have looked at, for example, the n,d reaction where you get two neutrons and a proton out. Ivo Shauss, who is at UCLA, has considered this problem. He does not understand why you get an answer which is near the neutron-neutron singlet scattering length. However, when you do the experiment in a slightly different way, where you exchange the projectiles and the reaction products, in some cases you do get an answer which is quite close. I believe it's the d,t reaction where you end up with two neutrons; you do get an answer which is -18 fermis.

BREIT: Yes, I reported in my talk this morning.

HADDOCK: I have no idea why that happens.

IGO: I'd like to ask an experimental question please. Could you tell me about the counting rates you get in this experiment?

HADDOCK: In the overall neutron array, summing all signals, it was around a megacycle. The gamma ray counters were turning over at about 30 cycles/second.

IGO: I'd like to know about your triples.

HADDOCK: The counting rate was one count/minute.

SIGNELL: I would like to make a comment on this. A hard core model is not, of course, the only potential. One can take in place of the hard core plus a strong attraction, the Baker transform and get a momentum dependent, weak potential which matches the same p,p phase shifts at all energies. The first yields a_{nn} from -16.6 to -16.9F, and the second yields -19.3F for a_{nn}. So, in this simple-minded kind of a calculation, there is a distinction between these.

PHILLIPS: I've been for years interested in what happens to systems that end up with three particles in the final state. I think the one just reported by Haddock is apparently one of the cleanest ones that we know of. I would just like to be the devil's advocate, though, and just raise the simple question, do we really know the physical mechanism here. I don't propose to give the answer, but the mechanism assumed for the theory is that pion is captured upon the proton, turning it into a neutron, and emitting a gamma ray. The two neutrons are then left close together in configuration space in very strong interaction. Now an alternative mechanism would be that the nucleon is excited to some state. The two nucleons separate in space and then the free excited nucleon emits a gamma ray. Now the amplitude for that latter process must be added to the former and the cross-section will be the square of the sum of these. And so there could be interference terms there, possibly if the second mechanism that I propose has a non-zero amplitude, so I'd appreciate any comments that any of our theoretical friends have on that.

HADDOCK: I'm glad you put it that way.

BREIT: I may be wrong, but since this is a discussion, an error is perhaps permissable. If the lifetime of the excited state is short enough, then it will not make a serious difference because the primary thing is the amplitude of the neutron-neutron relative motion wave function and that would be determined by the longer range interaction. So it will all hook itself onto the lifetime of the excited state. The ordinary lifetime is pretty short. It's short enough so that they should separate. But since you deal with an interference term there is some uncertainty.

MEASUREMENTS OF THE DIFFERENTIAL CROSS-SECTION AND POLARIZATION IN PROTON-PROTON SCATTERING AT ABOUT 143 MEV

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We have recently made some new measurements of the differential scattering cross-section and polarization in proton-proton scattering at an energy of about 142 MeV. This work was performed using the Harwell synchrocyclotron.

Before describing these measurements I would like to explain why the work was considered necessary, in view of the fact that sets of cross-section and polarization data are already available from no fewer than three laboratories (Harwell, Harvard and Orsay), all three sets referring to an energy close to 150 MeV[1]).

Basically, the motivation arises from the fact that these published data have suffered the customary eroding influence of old age. The Harwell and Harvard measurements of the cross-section and polarization at 142 and 147 MeV were published about 8 years ago and since that time a complete set of triple-scattering measurements has been made at both laboratories. The most recent phase-shift analyses of the data yield unique solutions and the values of the phase-shifts are known with quite high precision. However, the same analyses demonstrate disagreements between the three sets of cross-section data. Thus, MacGregor[2]) has found it necessary to discard both the Harwell and Harvard cross-sections and to use only the Orsay data --- despite the fact that the energy to which it refers was somewhat high (156 MeV) compared to that at which all the other data was obtained. It is clearly desirable to demonstrate experimentally that MacGregor's procedure was permissible. A separate problem is the determination of the absolute value of the differential cross-section data. Here one finds more disagreements but the accuracy of the determinations is poor -- the most precise determination (± 4%) being obtained at Orsay. In practice, this normalization is best made indirectly through the Harvard total cross-section measurements[3]), which were accurate to about ± 1%. An independent check on these measurements would be desirable.

In contrast to these disagreements, the published polarization data from the three laboratories were in reasonable accord, although they each refer to a somewhat different energy. However, this agreement is quite illusory as we have recently found[4]) that the absolute scales of these data are considerably in error owing to their normalization to a single, and incorrect, determination of the polarization in p-carbon scattering.

For these reasons we felt it necessary to repeat the measurements with inproved accuracy. The present cross-section data were obtained to a relative precision of better than ± 0.5% and the absolute scale was determined to ± 0.8%. The polarization data were determined to a relative precision of about 1% of the maximum value and the absolute scale to ± 0.85%.

Although about 8 years have elapsed since the first proton-proton scattering work was done at Harwell, the experimental techniques available to us for cross-section measurements have remained essentially unaltered. Thus, in the present work the protons scattered from the hydrogen target were detected as usual in a counter telescope using several scintillation counters in fast coincidence. The main difference from the earlier measurements is that very thin (20 mil) plastic scintillators were used to minimize losses due to scattering and absorption in these counters. In contrast to this situation , however, the measurement of polarizations has benefited from the developement of the solenoid to reverse the direction of polarization of the incident polarized beam. When properly used, this technique makes the relative determinations almost trivial.

Fig. 1, shows the general layout of the experimental area. The proton beam was extracted from the synchrocyclotron by scattering from an internal target. This internal target was tungsten to give an unpolarized beam of intensity about 10^8 protons/sec and aluminum to give a polarized beam of about 10^7 protons/sec -- the polarization being known from earlier work to be 47.2 ± 0.4%. The important point to notice is that these two beams were obtained along almost identical beam paths so that no repositioning of equipment was needed when changing beams.

The solenoid was used to give a precession of the direction of polarization by ± 180°. This solenoid was very carefully aligned such that the beam direction and position at the experimental area was unaffected by whether or not the solenoid was being used to change the polarization direction. This alignment was made possib by the use of split-ionization chambers to detect small changes in the position of the center of gravity of the beam spot.

The beam intensity monitoring device consisted of a 0.5 mm. thick sheet of polythene placed in the path of the beam upstream of the target position and two counters were set at 44° on opposite sides of the beam to record coincidences from (p-2p) events in the polythene. The linearity of the monitor with beam intensity was assured by the low counting rates used.

The momentum analyzed beam was not used for the main experiment as a poor beam focus was obtained for full intensity - and beam intensity was too precious to squander.

The differential cross-section measurements were made by two methods which were independent except in relation to the calibration of the beam monitor. In the first method , a polythene target was used and scattered and recoil protons were recorded in coincidence. Sufficient absorber to define a threshold energy of 120 MeV was placed in the scattered proton telescope arm in order to reduce the background due to (p,2p) events in the carbon of the polythene target. Backgrounds were taken as usual with a dummy carbon target. The attenuation due to the absorber in the scattered arm was measured in the following manner. Absorber was placed in the recoil arm of sufficient thickness to define the 120 MeV threshold. Measurements were then made with the absorber in and out of the scattered arm, the resulting ratio -- corrected for backgrounds -- gave the required attenuation factor directly. The chemical composition of the polythene target was analyzed by a slow neutron technique -- using the accurately known n-p and n-c total cross-section values-- the result of which indicated the composition to be CH_2 to within 1/3%. Chemical analysis gave a similar result but with rather less accuracy.

The second technique used for the cross-section work required the use of a liquid hydrogen target. Two quadruple counter telescopes were used to record left and right scatters. Absorbers were again used to reduce backgrounds, which in turn were measured with the target evacuated. This was also the arrangement for the polarization work. Only the angular region outside the minimum due to Coulomb interference was investigated due to the considerable difficulty experienced in the small angle range ($<8^{\circ}$lab) from the rapidly increasing backgrounds and from counter resolution problems. At the angles investigated the backgrounds were in general only a few %. The attenuation due to the absorbers was measured by absorber in and absorber out measurements with the telescope in the direct beam, reduced to the appropriate energy. Repetition of some of these attenuation measurements in the momentum analyzed beam demonstrated that the low energy component (<120 MeV) in themain beam was < 1/3%. Small corrections too numerous to detail were made before the final relative cross-sections were obtained. The hydrogen target volume was determined by measurements with a traveling microscope with the target full of liquid nitrogen.

The two independent sets of cross-section data were in satisfactory agreement.

The beam intensity monitor was calibrated by the use of a two counter telescope placed in the direct beam at the position of the hydrogen target. By using a coincidence circuit and scaling units capable of recording at about 50mc/s (the cyclotron r.f. frequency being only 20mc/s) it was possible to calibrate the monitor directly with a beam intensity of about 10^6 protons/sec. For this it was essential to use the long-duty-cycle facility of the cyclotron which could give a macroscopic duty-cycle of about 80%. This calibration was demonstrated to be reproducible to within the statistical uncertainty of $\pm$ 0.5% over a period of about 4 months.

Fig. 2 shows the cross-section data and the curve represents the predictions of a recent phase-shift analysis (by J. K. Perring) which included the present data. The shape of the cross-section curve is characterized by a fall in cross-section of about 5% from 45° to 90° cm. This is in good agreement with the Orsay data but is larger than the value given by the Harvard results and is in direct contradiction with the rise given by the previous Harwell measurements. Thus, we have in a manner justified the data selection made by MacGregor.

The differential cross-section data were integrated graphically to give a total cross-section between 12° and 90°cm. of 24.0 $\pm$ 0.2 mb. This is precisely the same value as one obtains from an interpolation between the experimental measurements of Goloskie and Palmieri[3]. We have, consequently, obtained the desired check on the absolute normalization.

Fig. 3 shows the polarization measurements. The curve is again obtained from the analysis by Perring. This analysis demonstrates a large measure of consistency among the data now available, an overall X^2 of 197 being obtained for a 14 phase-shift search in which 203 pieces of data were fitted. The contribution to X^2 from the present results was about 0.8 per data point, which is gratifying in view of the fact that the errors on the present data are on average rather smaller than one half of those on the corresponding earlier data -- and so the present data should surely dominate the analysis. Finally, the normalization constants found in the search were 0.998 for the cross-section data and 0.999 for the polarization data.

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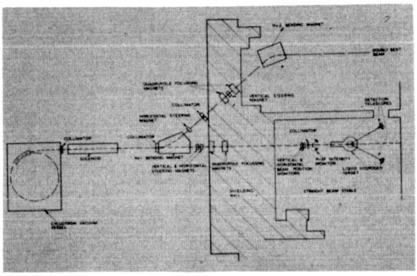

Figure 1 - General layout of the experimental area.

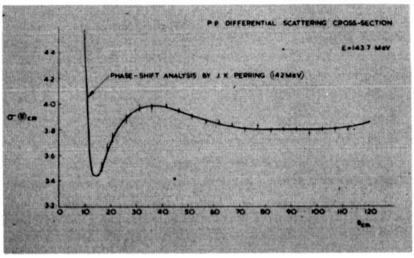

Figure 2

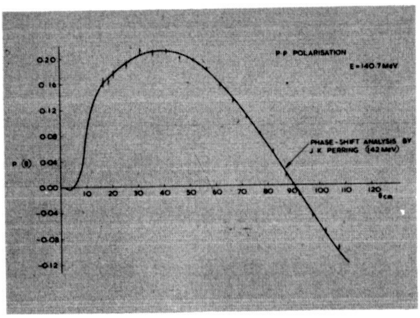

Figure 3

BREIT: These results are, of course, most welcome to data analysis and so far as Yale is concerned are especially timely because we have been troubled considerably by the Orsay P(Θ) at 138 Mev. The characteristic trouble that has been bothering us is absent in the data just shown. It would be a great help if somehow one could ascertain just how those 138 Mev measurements at Orsay were made. Is it that they were bothered in some way by a contamination of their target? The reason why this occurs to me is that the low angle points do not give any trouble, but those above 90^o are not consistent with those just below 90^o which just doesn't make sense for p-p scattering.

JARVIS: You're talking about the polarization data now and I believe you're giving a good description of a false asymmetry in the measurements.

SESSION E

NUCLEON INTERACTIONS

Chairman, S. Barnes

N66 32774

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EXPERIMENTAL ASPECTS OF NUCLEON-NUCLEON SCATTERING AND POLARIZATION BELOW 1 BEV

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Abstract

The present state of nucleon-nucleon data is reviewed. There has been a resurgence of interest in the problem at 300-700 MeV, partly stimulated by the availability of polarized targets. Some of the new data is clearing away old discrepancies and some revealing new ones. The precision of existing data is rarely as high as claimed, and much greater precision will be needed if unique energy independent analyses are to be carried out at the higher energies. Twice as many experimental papers are published on the p-p as on the n-p system. It is proposed that more attention be devoted to the neutron proton system with neutron beams, in preference to quasi-free experiments using the deuteron as a neutron target. The problems of doing so are discussed. Recent results on nuclear bremsstrahlungen are briefly mentioned.

Introduction

Today I shall take as my primary assumption that the principal experimental objective in studying the nucleon-nucleon system is to supply data that is capable of producing unique and accurate phase shift analyses at any energy. It seems that such a parametrization of the experimental data is physically meaningful and is a convenient phenomenological half way house between the experimentalist and the basic theorist. It is necessary therefore that the data itself should be accurate, and we must not assume that the data is correct just because a unique phase shift analysis is possible. I shall therefore be spending a considerable portion of my talk in the comparison of experimental data. I shall put a rough lower limit at a laboratory energy of 100 MeV, though I shall go below this limit where it appears interesting to do so.

Having got a good phase shift analysis one may then be interested in its predictive powers, - e.g. how well does it predict the values of as yet unmeasured quantities - or, more interestingly, how well can it be used to predict the properties of the proton-deuteron interactions. I shall spend some time on this problem.

Alternatively, one may study the nucleon-nucleon system to see to what extent the basic conservation laws are confirmed - parity conservation, in strong interactions in particular. There has been virtually no work on this subject since ~ 1958 and the normal methods of phase shift analysis have this assumption built in. There is, I understand, some work in progress at Rochester which has not yet reached fruition, so there will be nothing more to say except to encourage others to consider it as a worthy field of study.

Finally, the nucleon-nucleon forward scattering amplitudes are being used to attempt to determine coupling constants for the heavier mesons through the use of dispersion relations, and very important contributions to the various integrals come from the energy range of interest to this conference. The data so far seems sufficiently imprecise to allow anyone to find the particular answer he is seeking. However the forward scattering amplitudes themselves are presumably most accurately determined through phase shift analyses, for then one can use more data than just the differential cross section - but much greater precision than has been obtained so far is going to be needed.

I shall discuss the experimental problems involved in improving the precision of neutron-proton work, and finally, as a complete change of topic, hope to say a few words about nuclear bremsstrahlung.

Present state of data

Until about a couple of years ago, there had been a continuous study of the nucleon-nucleon system below 220 MeV at various laboratories, at ~ 650 MeV at Dubna and at ~ 1 GeV at Birmingham. These studies had been pursued largely by the methods traditional to the energy range. Then two experimental developments began to make themselves felt. One of these was the application of what had previously been considered as really high energy techniques to synchrocyclotron physics - very large arrays of counters or spark chambers or both. The second was the development of polarized targets. These two developments led to a resurgence of experimental interest in the nucleon-nucleon problem and has resulted in a great quantity of data. Meanwhile those using tradition techniques have refused to be intimidated. They have refined them with considerable success, resulting in data of an accuracy which the newer techniques have yet to match.

p-p data

Beginning at the lower energies and working upwards, we have new measurements at Harwell at ~ 140 MeV of $d\sigma/d\Omega$ and P using traditional methods [1]) and of C_{nn} using a polarized target [2]). As there is a contributed paper on the former pair of measurements I shall just show in Figure 1 the new cross section data with the previous data on for comparison, together with curves derived from a recent analysis by Perring [26]). There are two points to note. One is that the new data has the same general shape as that measured at neighbouring energies at Harvard and Orsay, and that there remains no doubt whatsoever that the old Harwell data had the wrong general shape. The other is that the new data has much greater absolute (as well as relative) precision (~ 0.7%) than the old. That this claim is justified is illustrated in Figure 2, where the cross section integrated from $12^{\circ} - 90^{\circ}$ c.m. is compared with total cross-sections of Goloskie and Palmieri [3]) measured over the same angular range. The latter data is also in very good agreement with the equally precise data of Young [4]) at 68 MeV.

Similarly the absolute precision of the new polarization data (0.8%) is much better than that of the previous data in the region. This has no direct cross check, though we may note that the polarization of the Orsay beam [5] as measured either by double scattering at ~ 150 MeV or by comparison with the recent Harwell results on p-C scattering [6] gave agreement within the errors of 2%. This cross check between (a) laboratories and (b) p-H and p-Nucleus polarization is particularly important and is, in my view, not sufficiently practised. The need for it will become more apparent later on.

As a result of this new data some of the phases have their errors reduced by almost a factor of two. The experiment on C_{nn} was made using a polarized target and measurements were made of C_{nn} at 90° and 60° (c.m.) at 143 MeV and also of $C_{nn}(90^\circ)$ at 98 MeV and 73 MeV. The target polarization was determined from the asymmetry in scattering an unpolarized beam from the polarized hydrogen, and the biggest difficulty in determining the absolute values of C_{nn} lay in the fact that the target suffered severe radiation damage - and hence loss of polarization - during the runs. This is illustrated in Figures 3 and 3½, where we see that the fall off to half polarization occurs after the passage of $\sim 10^{12}$ protons, or about 10 hours running with the polarized beam. The net result is that the ratios $(C_{nn}(60^\circ)/C_{nn}(90^\circ))_{143\ \mathrm{MeV}} = 0.826 \pm 0.03$, $(C_{nn}(98\ \mathrm{MeV})/C_{nn}(143\ \mathrm{MeV}))_{90^\circ} = 0.69 \pm 0.04$ and $(C_{nn}(73\ \mathrm{MeV})/C_{nn}(143\ \mathrm{MeV}))_{90^\circ} = 0.25 \pm 0.06$ are all determined without much dependence on the law assumed for the decay of target polarization with energy, and are limited by statistics. However, the absolute value $C_{nn}(90^\circ, 143\ \mathrm{MeV})$, which is close to unity, may depend upon the decay law assumed in the analysis, and may therefore have an uncertainty additional to the statistical uncertainty of ~ 4%.

The next new data is coming in at ~ 200 MeV from Rochester [7] - also by traditional methods - where they are engaged in precision measurements of the differential cross section into the small angle region - typically 1% absolute and relative error is hoped for. Their preliminary cross-section data is shown on Figure 4 and their values of $(P/\sin\theta\cos\theta)$ in Figure 5. The absolute value of the polarization is not, as I understand it, being remeasured. This seems to me a pity because the original determination of the polarization of the Rochester beam was made many years ago.

Rochester is also engaged in a tidying up operation by remeasuring some values of the triple scattering parameters A and D that were rather scornfully rejected by the phase shift analysis and also had some internal inconsistencies in the measurements.

Some new data has come in at 300 MeV from Berkeley on polarization - the first for about 10 years. These are experiments of Cheng [8]) who produced data from 300-700 MeV and of Betz who measured polarization at 300 and near 700 MeV. The experiment of Betz used a polarized target and was not troubled by radiation damage. That of Cheng used what one may call synchrotron techniques, as demonstrated in the Figures 6 and 7 where we see about fifty assorted counters and several thousand tons of concrete.

Cheng produced his polarized beam by scattering from carbon at 6^{o} and determined its polarization by rescattering at the same angle. Betz determined the polarization of his target by solid state techniques.

The situation at - 300 MeV is illustrated in Figure 8. It is clearly not very satisfactory, for though the data is in reasonable agreement, the accuracy both relative and absolute is low for the data of Betz and the old data of Chamberlain, and there is clearly some additional relative error in Cheng's data beyond the statistical errors ascribed, as one can see by their displacement from a smooth curve drawn by eye through the data. The feeling of uncertainty about the absolute precision of the data is engendered by the fact that the measurements of p-C polarization at this energy are far from being in agreement as we shall see later. This leads to a difficulty in principle, for those experiments which produce a polarized beam by scattering from carbon effectively determine the p-C polarization and the beam polarization from the same measurement. If the p-C polarization is wrong then it must be purely fortuitous if the beam polarization is right, and of course if the beam polarization is in error then so is any polarization determined from it.

The data at 400 MeV has recently been reinforced by Roth [10], who has produced data on P, D, R, A and A'. The latter is a real collector's item, being the only measurement at any energy of this parameter. The polarization, is in good agreement with that of Cheng - but the original measurement of the beam polarization at Chicago was made a very long time ago. There is also in addition a coupling with the 200 MeV data, because the spark chambers used in the measurement of the triple scattering parameters were calibrated in part on the Rochester polarized beam. This is possibly only an academic point because the statistical precision of the data is not high, but it should nevertheless be appreciated by those who make energy dependent phase-shift analyses. They may be energy dependent in a different way from that normally meant.

Near 600-700 there is a great deal of new data on polarization, coming from Dubna, CERN and Berkeley. At 700 MeV the situation looks quite satisfactory(Figure 9). Five different experiments-Ashgirey [11] at 667 MeV, Betz [9] at 679 MeV, Dost [12] at 680 MeV, Cheng [8] at 700 MeV, McManigal [13] at 725 MeV and Betz [9] at 736 all agree fairly well, though not to quite the precision claimed. The absolute accuracies vary from 3-6% for the various data plotted in the figure. Betz and Dost used the same polarized target and measured the polarization by the same solid state methods - so I presume one should not consider them completely independent. McManigal scattered an unpolarized beam from hydrogen and analysed the polarization in a second scattering from carbon, with all the attendant difficulties of resolution from inelastic events. He produced accurate data only over a small angular region because of the reduction of energy of protons in scattering from hydrogen at larger angles, and the fact that the analysing power of carbon is not well enough known as a function of energy. This is illustrated in Figure 10. (Note that two precision experiments near 700 MeV are in disagreement).

Only Ashgirey has the simple answer of producing a polarized beam by scattering from hydrogen, from which inelastic events are very easily removed by magnetic analysis, and rescattering from hydrogen.

That this technique is not, however, foolproof is illustrated in the work of Dost, who also produced a polarized beam in this way in order to measure C_{nn} with his polarized target. Without magnetic analysis after his first scattering, he found that about 6% of his beam came from his target walls and that the resulting beam had a mean polarization of 0.44 compared with ~ .51 to be expected from p-p scattering at that energy— a result which would seem to imply that the mean polarization of protons scattered from the target walls was ~ - 0.5.

At 600 Mev, the situation is much less satisfactory (Figure 11) The results of Cheng [8]) and Coignet [14]) from CERN/Orsay are systematically different, whilst the old data of Mescheryakov [15]) is about 33% too low. The large absolute error on Betz' data at this energy would allow it to agree with either the Cheng or Coignet data.

If one now looks at the maximum polarization in p-p scattering as a function of energy(Figure 12), it is apparent that, almost certainly, the 1 GeV Birmingham point is low - for the higher energy data is also taken with the Berkeley polarized target which has been seen to give agreement, certainly within about 10%, with other methods at energies in the range 300-700 MeV.

Certainly looking back on some of the older work, it is clear that the difficulties of measuring the polarization of a beam were underrated, and the measurement of a polarization with an ascribed error of ± .01 would be dismissed in a single sentence. It would seem also that they have not been fully realized in some of the recent work.

I have reservations, possibly because I don't fully understand the method, about solid state techniques for measuring the polarization of hydrogen targets. In principle, they measure an average polarization throughout the target which may be different from the average value the proton beam sees. I therefore should consider it important that a precision measurement be made of p-p polarization by conventional techniques if only to check this point. At present, at 600-700 MeV it seems to me that Ashgirey's 3% measurement should be the most accurate we have.

As mentioned briefly earlier, Dost [12]) has made measurements of C_{nn} at ~ 600 MeV - using a polarized target and a polarized beam produced by scattering from hydrogen. In addition Coignet [14]) has also measured C_{nn} with a polarized target, and a polarized beam produced by scattering from carbon. These results (Figure 13) differ in scale by almost 40% and it would be rather unexpected to have such a rapid energy variation. The situation is rendered rather murky by the fact that you will recall that Coignet's measurement of p-p polarization is rather lower than Cheng's at 600 MeV, and in addition his measurement of p-C polarization, from which presumably his beam polarization was determined, was much higher than Cheng's value at 600 MeV. One is therefore inclined to doubt the absolute values of Coignet, since Cheng's values seem to agree with the majority on p-p polarization at 700 MeV (though it is of course no guarantee that his data at 600 MeV is also correct).

Figure 14 shows the variation with energy of $C_{nn}(90)$ and it is now reasonably completely established. The open circles represent the 'predictions' of phase shift analyses by Kazarinov [16]).

Finally, I should mention that work is going on at Birmingham at 1 GeV on both the p-p differential scattering cross section, and the depolarization parameter.

The great recent experimental interest in C_{nn}, particularly in the higher energy region, is not because it is a particularly important quantity but merely because it is now possible to measure it and because it provides an excuse for playing with a new experimental technique. I hope however that it will lead to a detailed study of the p-p system at several different high energies - including precision measurements of all the different quantities that will be necessary to make accurate energy independent phase shift analyses possible - not forgetting that rather dull quantity, the differential cross section.

As an aside, because I have been concentrating rather on data accuracy, I would remind you of the accuracy claimed in some recent work on the total p-p cross section in the energy range above ~ 440 MeV taken on Nimrod (Figure 15). Here an absolute accuracy of 0.3% is claimed, with relative errors of 0.1%. The claim here is about a factor of 3 greater precision than has so far been obtained in the energy region of special concern to us.

My own guess is that something like an order of magnitude improvement on present accuracies of data is going to be needed if a phase shift analysis round about 600-700 MeV is going to be meaningful in the same sense as those from 200 MeV downwards. Whether you have any chance of achieving it, or whether you really want it, is another matter.

And now before turning to the neutron proton work, there is one point to make about energy determination. It is usual to determine medium proton energies from range curves, and the standard curves used are those of Sternheimer modified [17]. A recent report from McGill [18] shows that at ~ 100 MeV these curves gave an energy ~ 1 MeV low. (Figure 16). It would seem to be important to check these results - as energy errors of this amount are beginning to be of importance in the analysis of the p-p data.

n-p data

Turning now to the free n-p system, it is obvious that there is far less data, and what there is is generally very inaccurate, compared to its p-p counterpart.

Up to 150 MeV there is a fair amount of total cross section, differential cross section and polarization data and a few measurements of triple scatterin parameters. Recent work includes the following measurements. A precision experiment by Groce et al [19]) has been made between 20-28 MeV of the np total cross section to rather better than 1/2%, whilst Measday and Palmieri [20]) have remeasured the total cross section at several energies below 150 MeV to an accuracy of ~ 2%. These results(Figure 17) tend to confirm the higher points amongst the previous data. Measday [21]) has also measured the relative differential cross sections at 129 and 150 MeV(Figure 18) over the angular range from 50-180° c.m. with results of greater precision than had pr viously been obtained, but only in marginal disagreement with previous data. Measday's measurements were made using a 'monkinetic' neutron beam obtained from the d(p,n) reaction at 0°. Langsford et al [22]), using a pulsed neutron time-of-flight system, have measured the polarization as a function of energy (Figures 19, 20) from about 20-120 MeV over the full angular range-demonstrati that the old 77 MeV data was too high. However the most technically difficul experiment on the free n-p system has probably been the Los Alamos experiment in which C_{nn} was measured using a polarized target. The usual difficulty wit a polarized target of lanthanum magnesium nitrate is to identify the scatteri from the protons against the scattering from all the other rubbish surroundi them, and in the case of a p-p scattering experiment this is achieved by kinematic means - usually by measuring the two protons in coincidence with sometimes a range or energy discrimination included. In the case of (n-p) scattering, however, if one is looking near to 180° cm, the proton energy is very close to that of the primary neutron and hence it proved sufficient to identify the recoil protons by an E : dE/dx method and to discriminate on its energy in order to achieve a relatively low background (~ 15%).

This technique should only be possible at lowish energies, where the (p,n) threshold for the majority of the constituents of the target is a substantial fraction of the primary neutron energy.

Apart from a total cross section from Dubna at ~ 630 MeV to ~ 3%, the above represent all the data on the free n-p system that has been produced over the past two years, or is being produced.

d(p; pn) data

The data on the free n-p system has been supplemented by data taken with proton beams incident on deuterium targets, and regarding the neutron in the deuteron as more or less free. The higher the energy the more plausible the argument becomes that the small binding energy of the neutron will not affect seriously the interpretation of the results as being something closely approximating to free n-p scattering.

The argument is made somewhat more sophisticated by making an experimental comparison between the free p-p parameters and quasi-free p-p parameters as measured with a deuterium target. Then if the theory can account for such differences as exist, it becomes plausible to use the theory to correct the quasi-free (p-n) parameters.

The most detailed experiments to make these comparisons were made at Harvard some years ago [24]. In these experiments, $d\sigma/d\Omega$, P, R and A were compared for p-p and quasi p-p. The agreement between theory and experiment was not bad but of only limited precision. For example, in Figure 21, taken from the paper of Cromer and Thorndike [25], the difference between the polarization in free p-p and quasi-free p-p scattering is plotted against the opening angle between the counters. Although for the lower curve the theoretical curve passes reasonably through one set of points, it ignored those at large included angle which the experimentalists obtained on a different run and which they have more faith in on experimental grounds. In the upper curve there appears to be a steady disagreement of ~ .02, which is almost 15% of the value of the polarization at that scattering angle. The comparisons with the triple scattering parameters were also of rather limited accuracy - inevitably for these are difficult experiments with low counting rates-and at some angles large corrections are needed. The agreement with 'predictions' from a recent phase shift analysis by Perring [26]) were within the accuracy of the measurements i.e. to ± 0.1 (Figure 22). Similarly, experiments at Rochester [27]) on the polarization in pp and quasi-pp scattering at ~ 200 MeV were certainly no better than 10% experiments.

An alternative test is to compare free n-p polarization with quasi-free p-n and there are experiments at two different energies where this has been done. At 140 MeV the maximum values of the polarization in the two experiments differ by almost 20% compared with a combined error of ~ 8%. This is illustrated in the Figure 23, where we have free n-p data at 127 MeV [28]) and corrected data at 140 MeV [29, 30]) compared with quasi-free pn data at 143 MeV [31]) after correction [25]). The quasi-free data seems consistently too low and to have the wrong general shape. The disagreement of the quasi p-n data from the general trend is emphasised in the Figure 23½. However Perring's recent analysis renormalizes the 140 and 127 Mev data downwards by about 10% so the situation is not as clear cut as once appeared.

A further comparison has been made between the 310 MeV quasi free n- data of Chamberlain [32]) et al and the free n-p data of Siegel [33]). Alth the general shape of the two sets of data are in agreement, the agreement in absolute magnitude must be fortuitous because (a) we know that the polarization of the Chamberlain beam is now in doubt because his value f p-C polarization at that energy is in disagreement with Cheng's (presuma more accurate value and (b) the polarization of Siegel's neutron beam wa determined by a rather dubious method. The beam was produced by the C(p reaction and analysed by the C(n,p) reaction. It was assumed that the analysing power in the C(n,p) reaction was equal to the polarization of neutron-producing reaction, so that the beam polarization was essentiall the square root of the final asymmetry. Now we know from a study by

Jarvis [33]) that, using the same technique at ~ 160 MeV, Harding [34]) derived a beam polarization that was ~ 30% too low. Consequently one can have little faith in the absolute value of the polarization ascribed by Siegel to his np scattering data at 350 MeV.

With this as background, therefore, one is a little uncertain of the value of the recent data of Cheng [8]) who studied the polarization in quasi-free pn scattering at energies from 300-700 MeV. He also compared the polarization in free and quasi-free p-p scattering. Figure 24 shows one such comparison from his data, and again the experimental check is no better than to 10%. Figure 25 shows his data at 600 MeV on 'p-n' compared with earlier Russian work, which used a polarized beam which gave a wildly wrong answer for the p-p polarization at that energy. Figure 26 shows the comparison of his data on quasi p-n at 300 MeV with the data of Chamberlain and Siegel mentioned earlier. It seems to me purely fortuitous that the agreement between their three data sets is as good as it is. Undoubtedly this data will be of interest to a study of the deuteron. I think it remains to be proved that it is more than a rough guide in the study of the neutron-proton interaction.

For completeness I should mention that work is also in progress on the quasi-free n-p differential scattering cross section at Birmingham at ~1 GeV. The corresponding work at Rochester is to be subject of one of the contributed talks later this morning.

Test of Predictive Powers of P.S.A.

It is of course always a matter of interest to the experimentalist to see whether phase shift analyses have any predictive value - whether they are able to predict either the value of a previously unmeasured quantity, or of another quantity which has previously been measured and for which more accurate values become available. We have seen already that phase shift predictions for C_{nn} were quite accurately borne out at 140, 100 and 70 and near 20 MeV. Figure 27 shows such a comparison for the recent 140 MeV pp data - and clearly the Livermore analysis does very well indeed, much better than the Yale. However the latter was struggling to accommodate the old Harwell differential cross section which we now know to have the wrong shape, whilst Livermore ignored it - in other words, an analysis can be quite badly 'pulled' by one set of bad data.

An alternative method of testing phase shift analyses is to use the derived phases plus the impulse approximation to predict the spectra and transfer polarization in the d(pn) reaction. Some work has been done on this at 50 MeV at the Rutherford Laboratory [36], at 95 and 143 MeV at Harwell [37] and at ~ 200 MeV at Rochester which is being discussed in a contributed paper.

In the work at 50 MeV the transfer polarisation was studied - a polarized proton beam was directed on to a liquid deuterium target and the polarization of the forward neutrons studied with a liquid helium analyzer. The results are shown in Figure 28 - neutrons in the peak are indeed polarized and the transfer polarization, ~ - 0.34, agrees within errors with that predicted by Phillips [38] from the Livermore phase shifts. The neutrons below the peak have the opposite sign of polarization.

The experiments at 95 and 143 MeV were made to study the spectrum at 0^{o} using the time-of-flight spectrometer; and to normalize these spectra by measuring the absolute differential cross-section using an external proton beam incident on a heavy wax target and measuring the neutron flux produced at 0^{o} by counting proton recoils from a polythene radiator. In effect, the latter experiment measured the product $\sigma(d(p,n)0^{o}) \times \sigma((n,p)\ 180^{o})$.

The results at both energies agree well with theoretical values. In Figure 29 we see the 95 MeV data with a theoretical fit to the spectrum shape, suitably spread by the experimental resolution. The spectrum is well fitted to 14 MeV below the peak - where the calculation stopped. The peak value of 16 ± 2 $mb.sr^{-1}.MeV^{-1}$ (after removing the instrumental resolution) is to be compared with values of 17-18 $mb.sr^{-1}MeV^{-1}$ from various phase shift solutions. The results at 143 MeV were equally well fitted and gave the same cross section. The various phase shift analyses all gave 17-18 $mb.sr^{-1} MeV^{-1}$ with the exception of the Livermore EI analysis which gave 14.5 mb sr^{-1} MeV^{-1} (Figure 30).

It is also possible to test some M(12) predictions for p-p scattering. In a recent publication Freund and Lo [48]) have predicted that A = - R', A' = R, C_{kp} = 0 and C_{nn} (90°) = 2 D(90°) - 1. The first of these is reasonably well satisfied at 140 MeV though not at 210 MeV where the data is more accurate, and the last is wildly wrong at 140 MeV since D is small and C_{nn} ~ 1. Furthermore, C_{kp} is consistent with zero only at 50 MeV and R $\neq$ A' at 430 MeV so it is very hard to see what relationship these predictions have to reality.

Possibilities of improving data

The various analyses in general are in very good agreement at low energies particularly in the pp system. In the np system though there is general agreement, differences do occur, and for example the Livermore ED and EI analyses differ by ~ 12% in the 140 MeV (n-p) differential cross section at 180°.

This lack of precision in the predictions or the phase shifts is of course due to the lack of precise n-p data. However for the past eight years or so there have been only half as many experiments on the n-p (including quasi free p-n) as on the p-p system, and there is no indication of any recent change in this habit. However the p-p data is in so much better shape - even though above 250 MeV considerable discrepancies remain to be cleared up, - that there should really be a considerable switch of effort to the n-p system and I feel that the time has come to make a real attempt to do these experiments with free neutrons rather than in the quasi-free system, which is basically a study of the deuteron, or of the impulse approximation.

The experimental problems are, of course, considerable. First one must have either a neutron beam of known energy and fairly narrow energy spread, or else one in which one can identify the energy of individual neutrons by time-of-flight, or by determining the energy of recoil protons. To measure absolute polarizations, one must have a method of determining the polarization of a neutron beam. Let us consider each of these problems briefly.

To produce a reasonably monokinetic neutron beam, one may use the reaction d(pn) or Li (pn) with monokinetic protons on a fairly thin target. The difficulty with a liquid deuterium target such as that at Harvard is that it has to be used with an external proton beam and therefore loses in intensity because of the relatively low extraction efficiency. The neutron fluxes produced are about an order of magnitude less at the same energy resolution as in the corresponding Harwell time of flight spectrometer. However this method is potentially very useful at higher energies where much thicker deuterium targets could be used without sacrificing energy spread. Alternatively one could regenerate a proton beam on to an internal target and so avoid so much loss of intensity and the energy spread resulting from mutliple traversals. The stripping of deuterons is likely to be satisfactory only at high energies[39] though even down at 400 MeV, one would expect 200 ± 34 MeV neutrons from this process, which is not bad, particularly if it is combined with some energy discrimination on, for example a recoil proton.

The alternative process, which is to use a wide neutron spectrum but to identify the energy of each neutron, has been used at Harwell with the time of flight spectrometer, but is in principle applicable at any energy if the energy and direction of the recoil protons are detected. The time of flight method gets progressively less useful as the energy is increased, as does the determination of the energy of recoil proton with scintillation counters or range telescopes, but the determination of the energy of recoil protons via spark chambers and magnetic fields gets easier with increased energy On the other hand, the acquisition of very good statistics from spark chambers represents a data processing problem which has not yet been solved.

To measure absolute diferential cross sections at small neutron scattering angles is relatively straight forward, because by using the same counter alternatively to count the beam and the scattered neutrons, one can avoid the problem of determinating its absolute efficiency. For large neutron angles where one detects the protons it is necessary to know the neutron flux - and therefore one must be able accurately to calibrate a neutron counter. This has been done up to ~ 100 MeV [40]) by comparing the counting rate in two scintillators with different hydrogen content, and relating the difference to the total hydrogen cross section. There seems no reason why this should not be pushed to higher energies - apart from the fact that the n-p cross section is not very well determined. For this purpose it is of course absolutely essential to measure the n-p cross section using free neutrons, in order to avoid the endless discussion over the coulomb and Glauber corrections which arise if the $\sigma(p\text{-}d) - \sigma(p\text{-}p)$ difference is used.

Another possibility is to use an activation method - such as $C^{12}(n, 2n)C^{11}$ to determine the flux, provided one can correct the fairly well known value of the $C^{12}(p,pn)C^{11}$ cross section to give that of the corresponding (n, 2n) cross section.

Three methods have been proposed for the production of polarized neutron beams and two used. The first simply looks at neutrons produced by the (pn) reaction from almost any target at an angle different from 0°. Polarizations of up to 30% have been obtained. The second, used only at Harvard, is to produce an unpolarized neutron beam and to scatter it at ~ 15° from carbon. This produces a beam polarization ~ 43%. The third is to use neutrons produced at 0° from the bombardment of deuterons with polarized protons [38]) when a sizeable transfer of polarization should occur.

The first and second methods both result in very wide neutron spectra, and the effective width of the neutron spectrum is set almost entirely by the neutron detector. Typically they have resulted in energy spread of 60 MeV base width at 140 MeV mean energy, unless used with a time of flight spectrometer when the resolution can be very much better than this. The second has in addition a beam typically an order of magnitude less intense because of the two stages in the production process.

The third method, tried only so far at 50 MeV, does not look promising at present because of the opposite polarization of the tail of the neutron spectra. It also has a low yield because an incident polarized proton beam is always low in intensity compared with an unpolarized beam. In addition, the polarization transfer coefficient seems to be typically between - 0.25 and - 0.5 [38]) - at least below 250 MeV - and therefore unless very highly polarized proton beams are used, the neutron beam will be of low polarization as well as low intensity.

Having produced a polarized neutron beam one has to measure its polarization. Schwinger scattering, double scattering from carbon and appeals to charge independence have all been used. The theory of Schwinger scattering has not been tested experimentally to an accuracy of more than about 20%. Double neutron scattering, which has been used twice to measure the polarization of the Harvard neutron beam, yielded discordant polarization values for neutron-carbon scattering and therefore one must suspect the derived values of beam polarization. The appeals to charge independence have to be made rather carefully and in no case have been accurately checked.

It seems therefore unlikely that any neutron polarization value is known to better than 10%, so there remains plenty of scope for careful and accurate experimental work along the existing lines. Alternatively, the use of a polarized target, in which a solid state method of determining the target polarization can be first checked by proton scattering, may lead to an increase of accuracy. Certainly, the increased intensities which will become available when the various synchrocyclotron conversion projects are completed should be a great help to increasing the precision of the neutron-proton work.

Bremsstrahlung

Finally I should like to make a brief reference to a closely related subject, that of nuclear bremsstrahlung which is the only possible inelastic process at low energies. There have already been published reports from Harvard [41]) and Winnipeg [42]) on p-p bremsstrahlung, which showed that the production was at least an order of magnitude less than was to be expected from a calculation of Sobel and Cromer [43]). We shall be hearing more on this topic from contributed papers later in the session.

Edgington and I [44]) have also been looking for bremsstrahlung from proton bombardment of hydrogen and of deuterium. In distinction from the other workers, we have looked simply at the photons with a lead glass Cerenkov counter, without requiring a proton in coincidence. We found only an upper limit for p-p bremsstrahlung - typically a differential cross section of - 5 $\pm$ 8mμp sr^{-1} at 90^{o} lab for photons greater than 40 MeV, and a total cross section of - .06 $\pm$ .05 μb - though we observed strong radiation from the p-d interaction. The results are shown in Figure 31.

The integrated cross section for p-d was - 4. μb for an energy greater than 40 MeV. We can make arguments, based on our study of p-nucleus bremsstrahlugen and on the theory of Beckham [45]) that the free p-n bremsstrahlung should be about twice that observed from deuterium - namely - 8 μb, which is in good agreement with early estimates - e.g. Cutkosky [46]).

Recently a preprint by Ueda [47]) estimates p-p bremsstrahlung production at 200 and 160 MeV. If we take the lower of his estimates and extrapolate to 140 MeV we have the line given on the slide, which is still at least a factor four above our upper limit. In his report Ueda also gives a preliminary value from Rochester of $.035^{+.04}_{-.015}$ μb.$ster^{-1}$ at 90^{o} lab for photons > 35 MeV. Our extrapolation of Ueda's calculation suggests that the cross section would be only one third of this at 140 MeV - namely 12^{+13}_{-5} mμb $ster^{-1}$ to compare with our experimental value of - 3 $\pm$ 13 mμb $ster^{-1}$. So perhaps our experimental numbers are not in disagreement with the Rochester preliminary experimental values. However the theoretical values still seem much higher than the experimental values for the pp system, whereas for the n-p system they seem roughly correct.

Summary

To summarize briefly:

(a) the low energy p-p system is in reasonably good shape and there is a little cross checking between laboratories on the precision measurements.

(b) the high energy p-p system is full of inconsistencies which need careful work to eliminate.

(c) the np system is in only moderate health at low energies and there is only qualitative data at higher energies.

(d) much more experimental effort is needed on the n-p system - more complete instrumentation and more patience in collecting data.

(e) order of magnitude experiments on p-p bremsstrahlung show marked disagreement with theory, though n-p bremsstrahlung is probably of the right order of magnitude.

I shall end with a slightly bowdlerized quotation from an article by Jesse Dumond in a recent 'Physics Today', applying it to a different context than that of the author.

"I cannot emphasize too strongly the importance of much more widespread duplication, using many different approaches by many different groups, because here we are dealing with the foundations of nuclear physics .... ".

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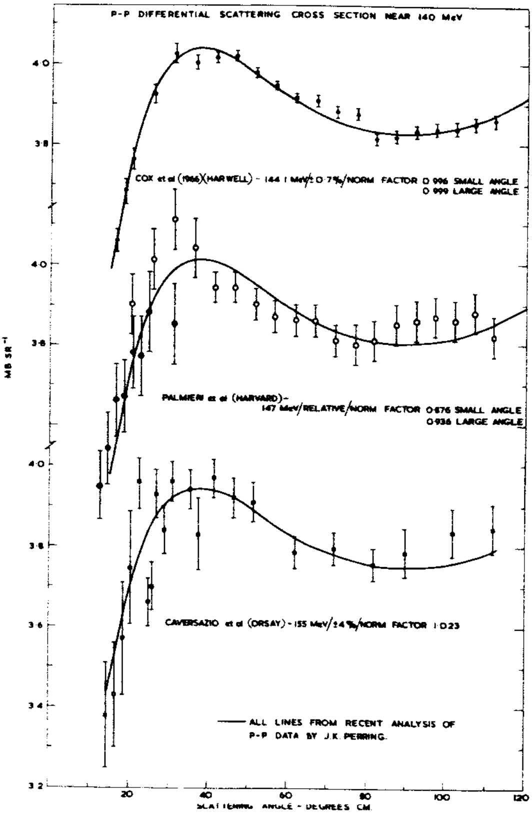

P-P DIFFERENTIAL SCATTERING CROSS SECTION NEAR 140 MeV
COX et al (1966)(HARWELL) - 144 1 MeV/±0 7%/NORM FACTOR 0 996 SMALL ANGLE
0 999 LARGE ANGLE
PALMIERI et al (HARVARD)-
147 MeV/RELATIVE/NORM FACTOR 0·876 SMALL ANGLE
0·936 LARGE ANGLE
CAVERSAZIO et al (ORSAY) - 155 MeV/±4%/NORM FACTOR 1·023
ALL LINES FROM RECENT ANALYSIS OF
P-P DATA BY J.K. PERRING.
MB SR⁻¹
4·0
3·8
4·0
3·8
4·0
3·8
3·6
3·4
3 2
20
40
60
80
100
120
SCATTERING ANGLE - DEGREES CM.

Figure 1

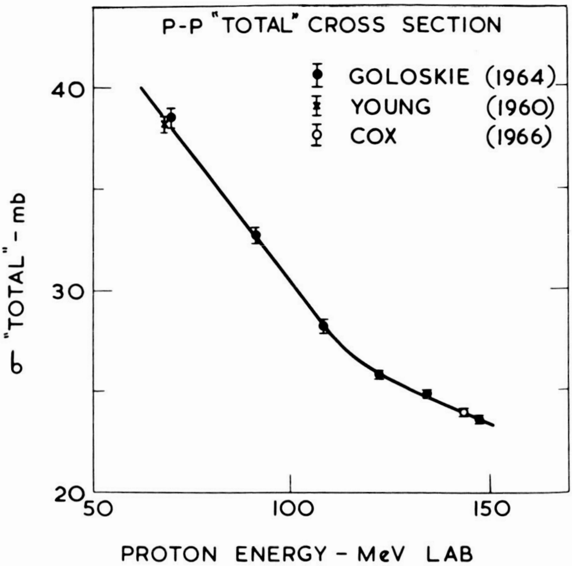


Figure 2

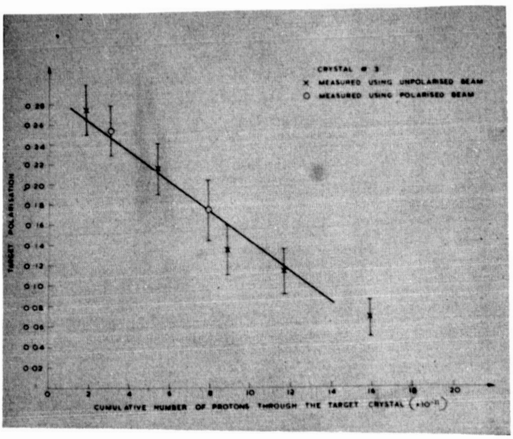


Figure 3 - Target polarization vs cumulative number of protons through target crystal.

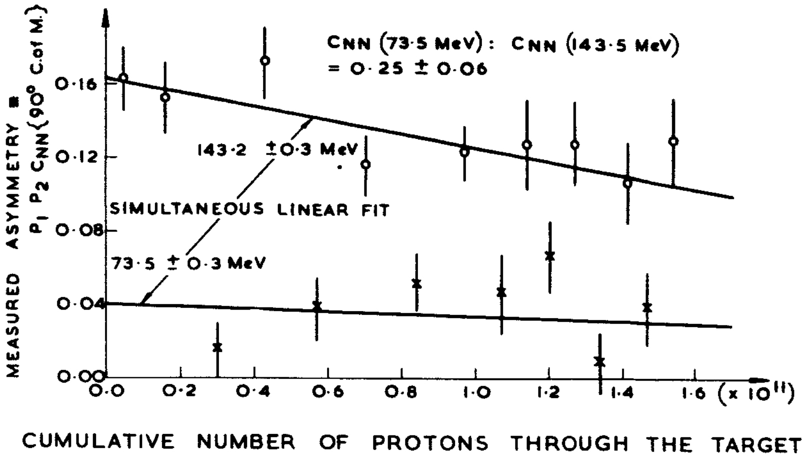


Figure 3½
Measured Asymmetry or protons through the target.

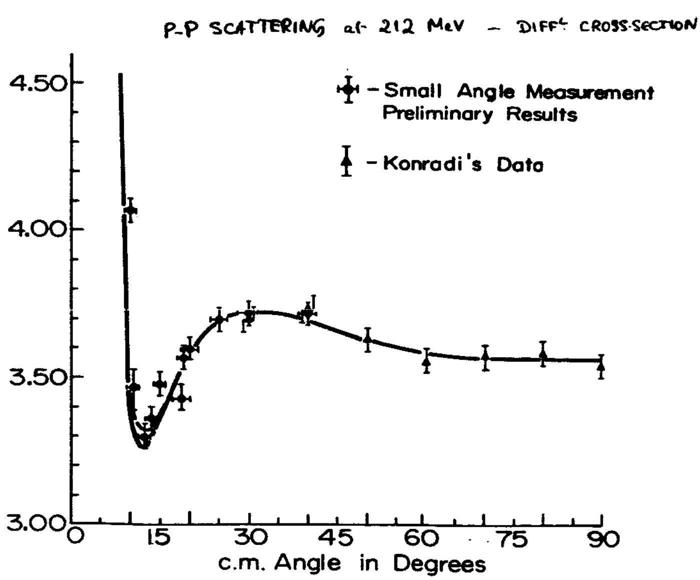


Figure 4

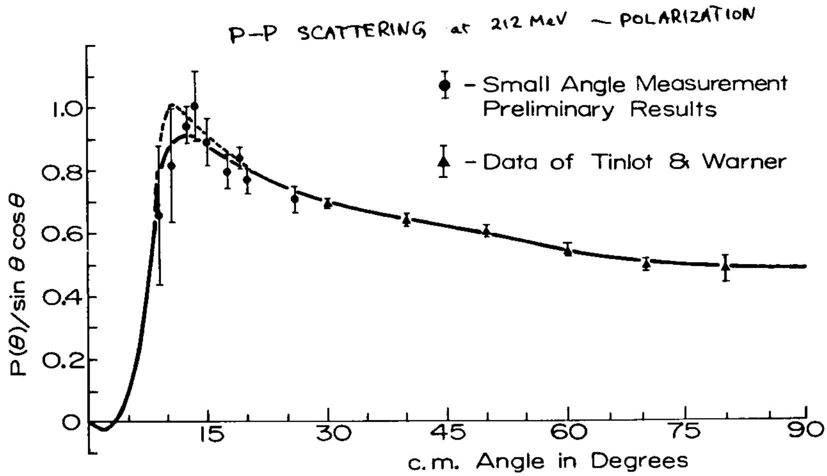


Figure 5

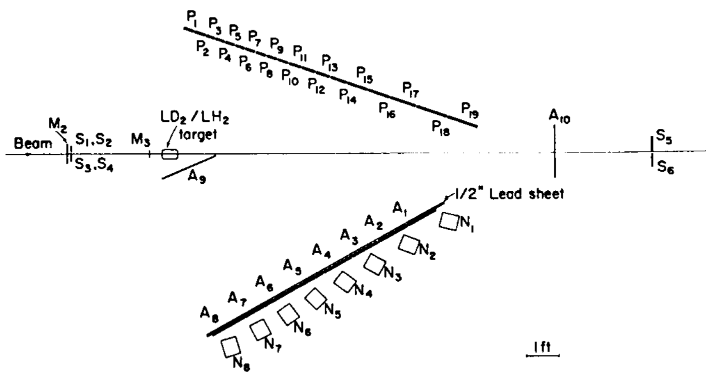


Figure 6 - Counters around the LD_2/LH_2 target.

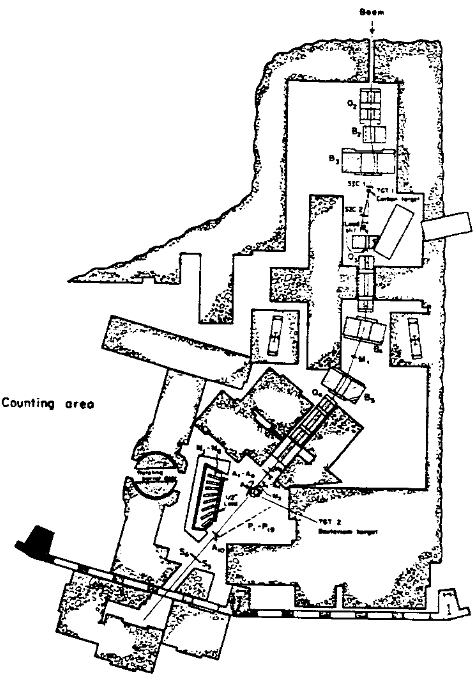


Figure 7 - Plan view of the experimental set up.

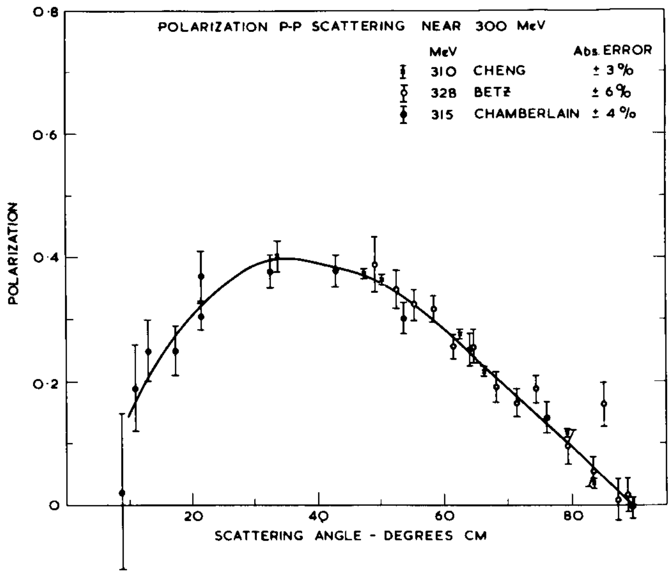


Figure 8

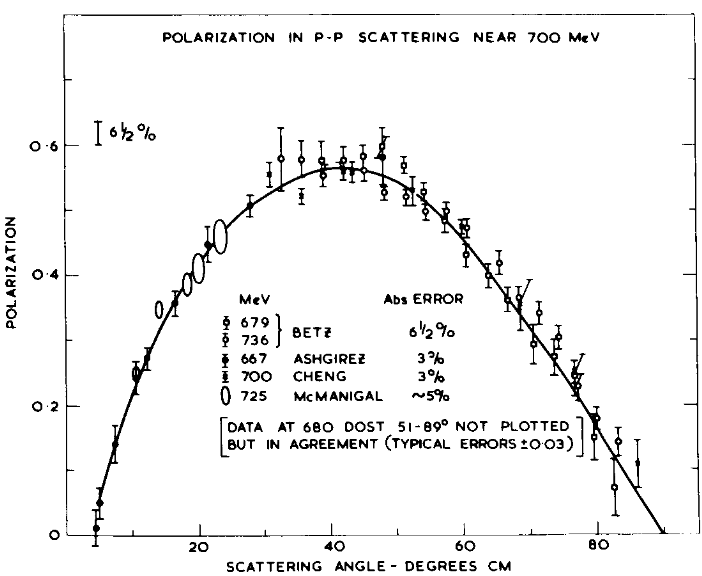


Figure 9

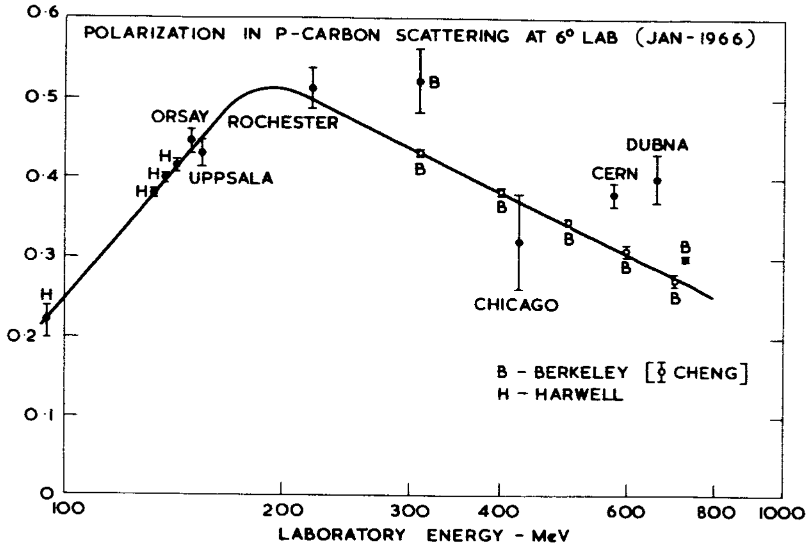


Figure 10

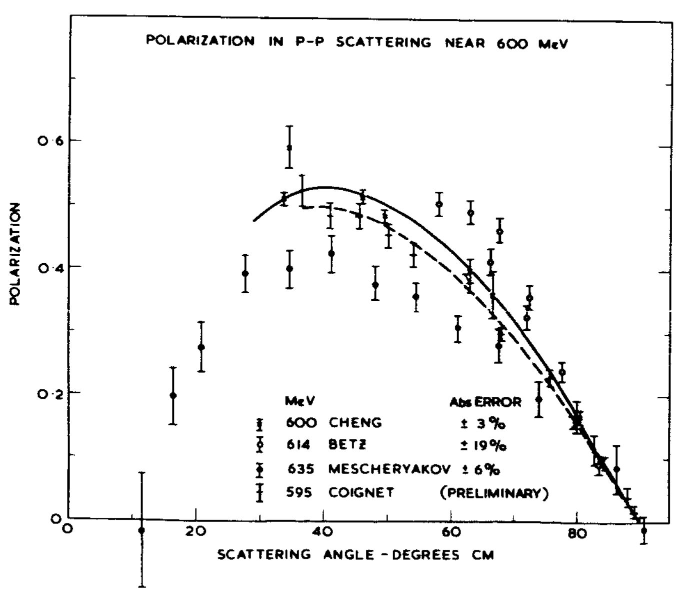


Figure 11

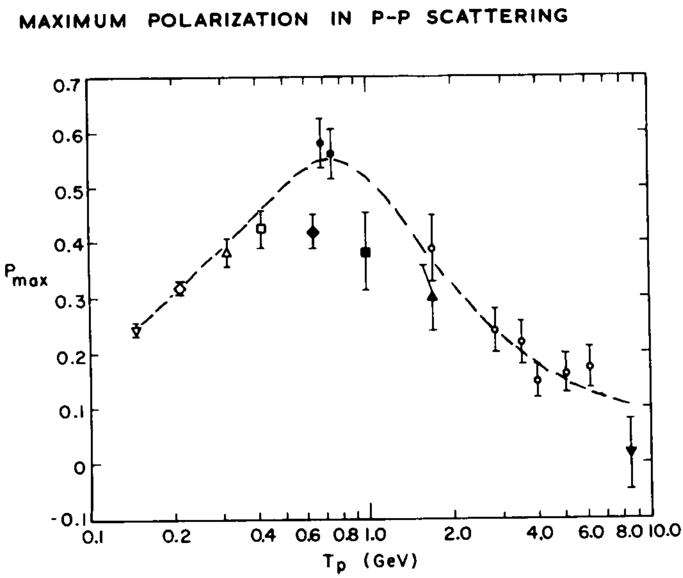

MAXIMUM POLARIZATION IN P-P SCATTERING
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
-0.1
P_{max}
0.1
0.2
0.4
0.6
0.8
1.0
2.0
4.0
6.0
8.0
10.0
T_p (GeV)

Figure 12

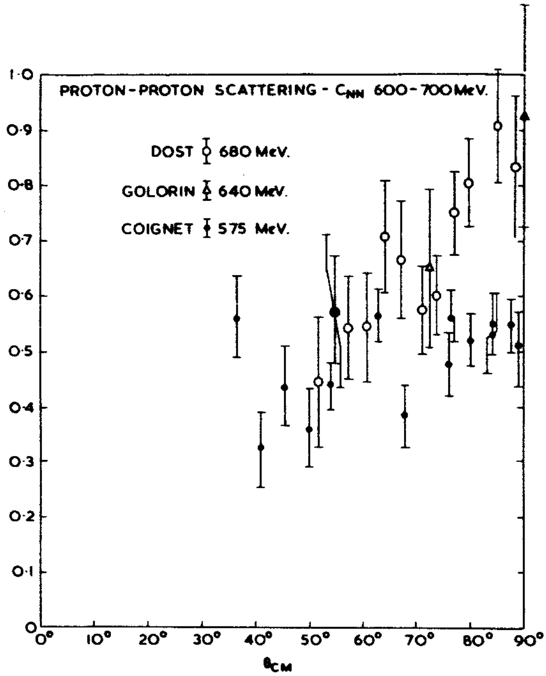


Figure 13

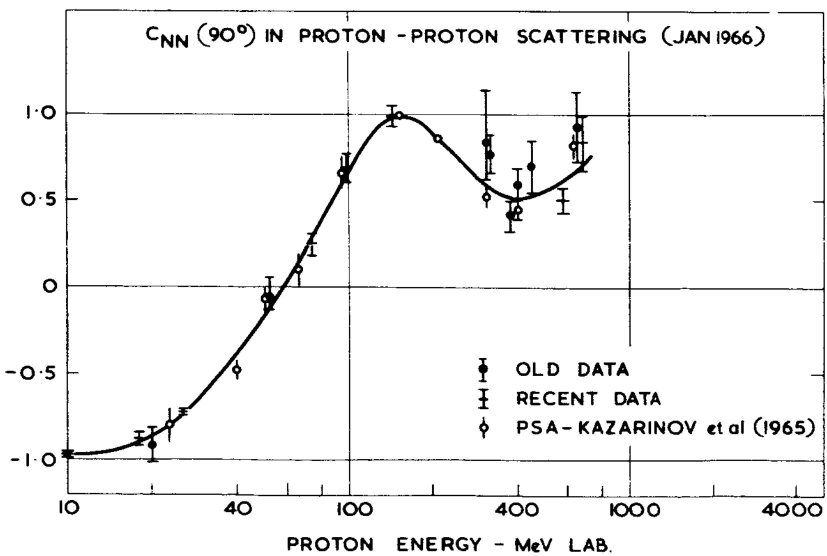


Figure 14

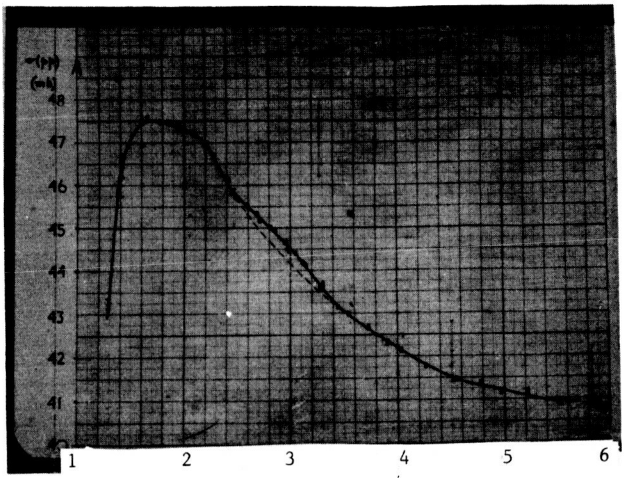
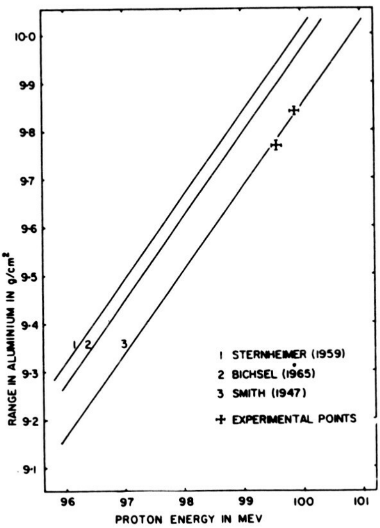


Figure 15 - Total p-p cross section or energy in 100's of Mev.



Comparison between present experimental range–energy values and theoretica range–energy curves.

Figure 16

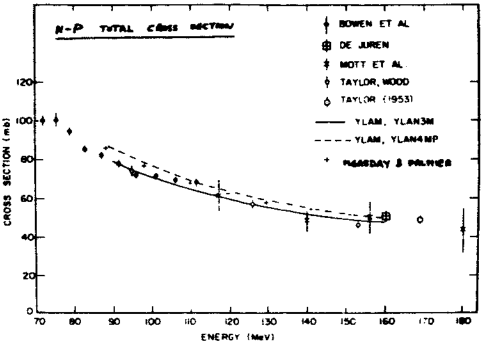


Figure 17

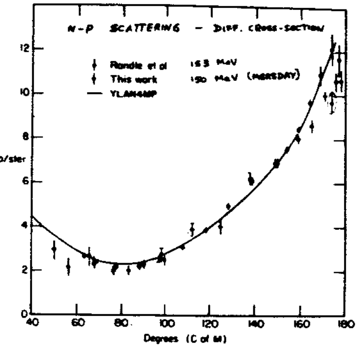


Figure 18

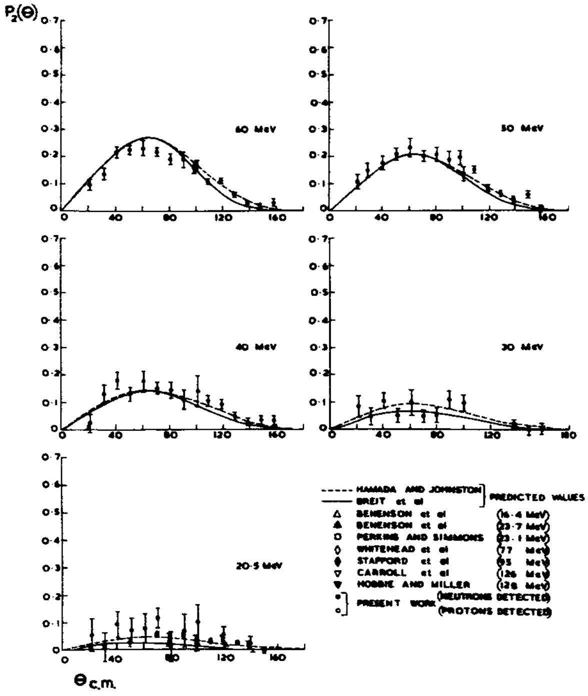


Figure 19 - Polarization or a function of angle and energy.

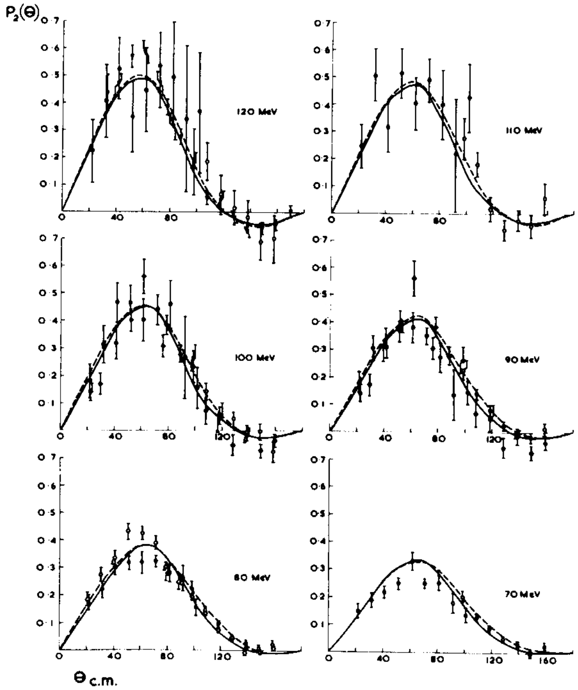
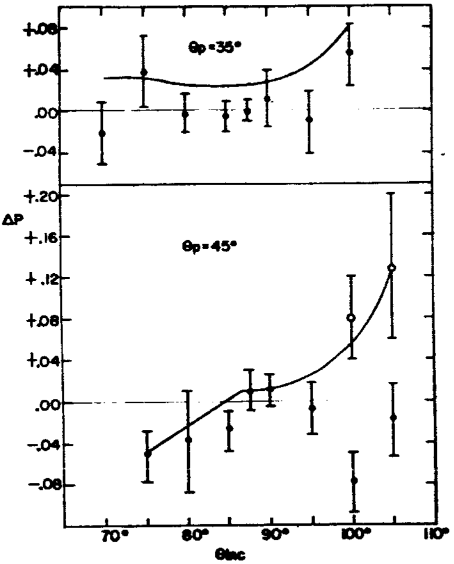


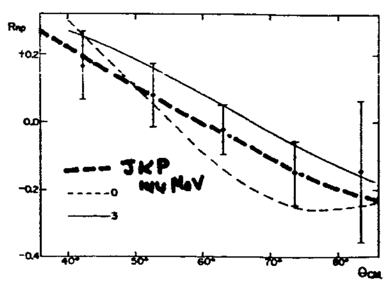
Figure 20

Polarization or a function of angle and energy.



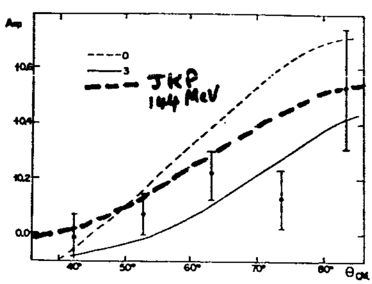
ΔP (the difference between the free *p*-*p* polarization and quasifree *p*-*p* polarization) as a function of θ_{inc}, the angle between the two protons, for two proton scattering angles ($\theta_p = 35°$ and 45°). The points are measurements of KWC, Ref. 4. The curves are values for ΔP calculated from Eq. (2.7). See text for explanation of the two sets of experimental points for $\theta_p = 45°$, large θ_{inc}.

Figure 21



Inferred value of *K* for free *n*-*p* scattering at 137½ MeV. The errors indicated are quadratic combinations of random and systematic errors. The curves are predictions of phase-shift solutions of Hull *et al.*, Ref. 20, at 137 MeV.

Figure 22



Inferred value of *A* for free *n*-*p* scattering at 135½ MeV. The errors indicated are quadratic combinations of random and systematic errors. The curves are predictions of phase-shift solutions of Hull *et al.*, Ref. 20, at 137 MeV.

Figure 22½

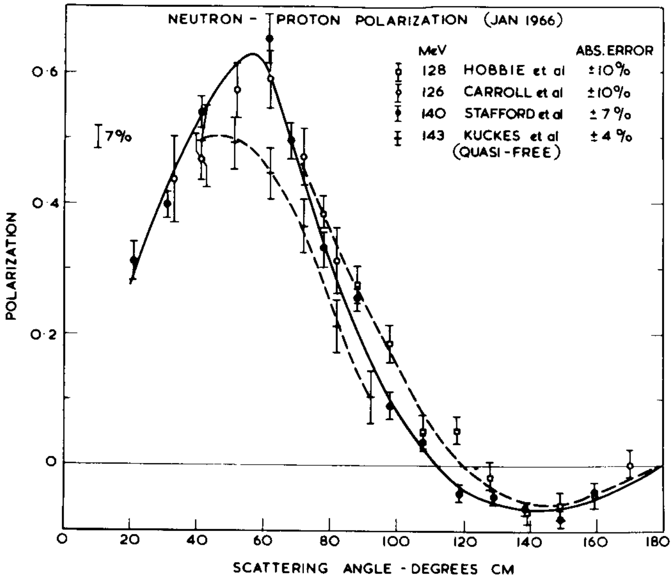


Figure 23

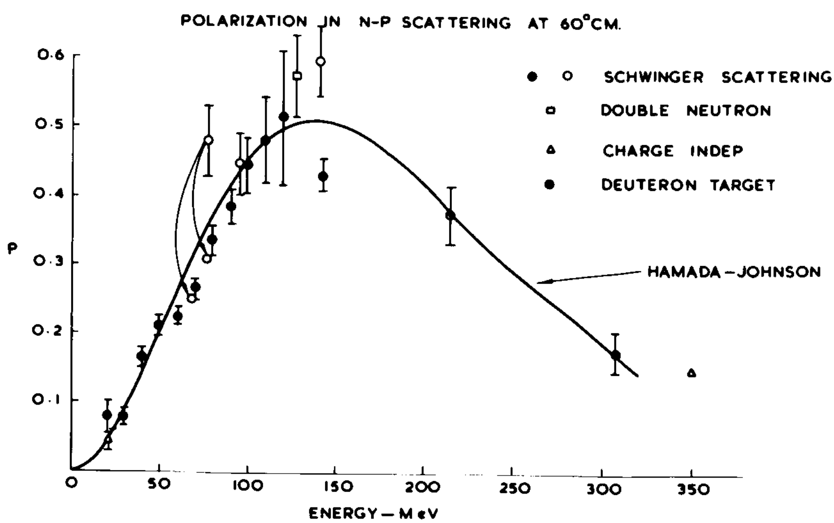
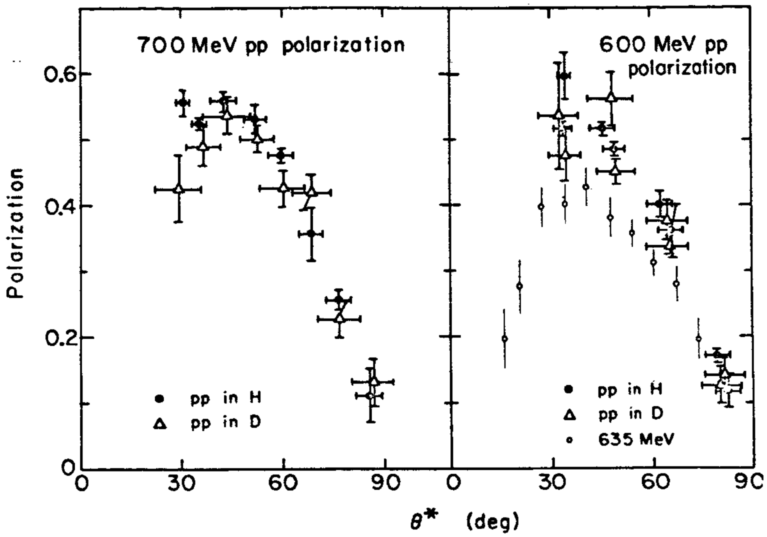


Figure 23½



(a) (b)

Figure 24

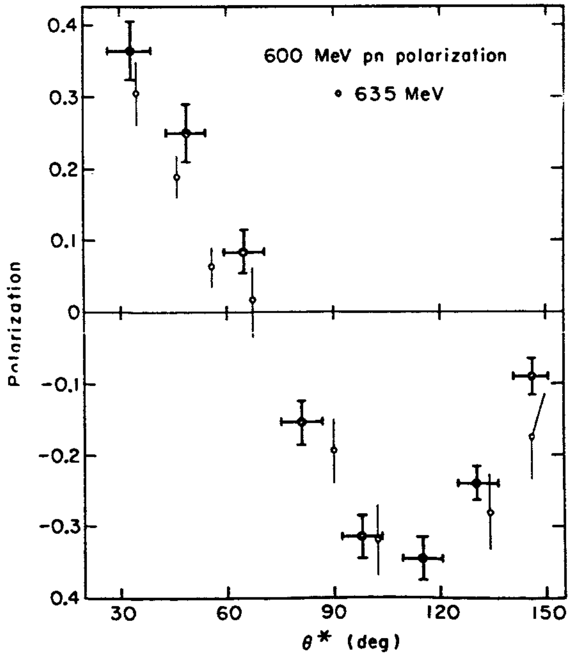


Figure 25

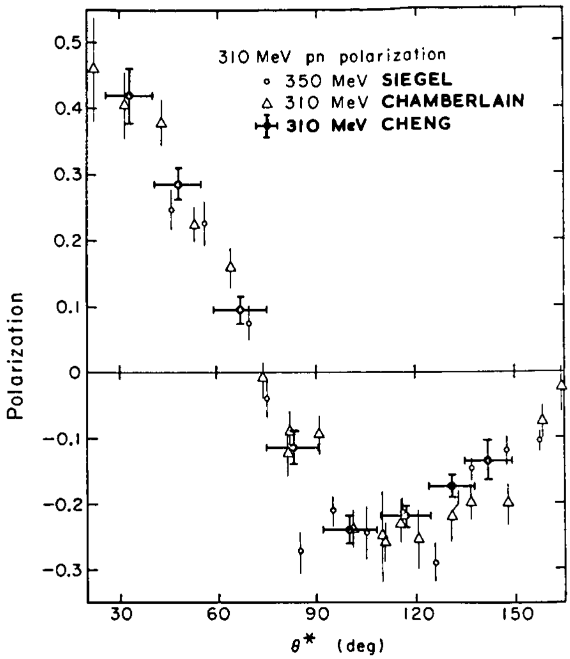


Figure 26

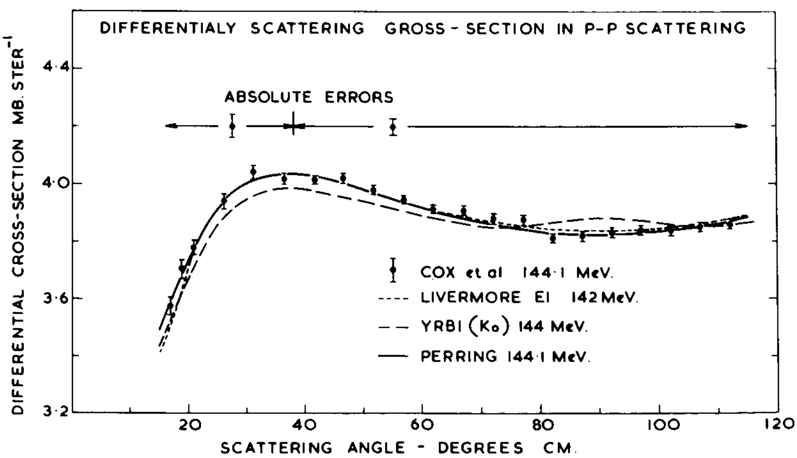


Figure 27

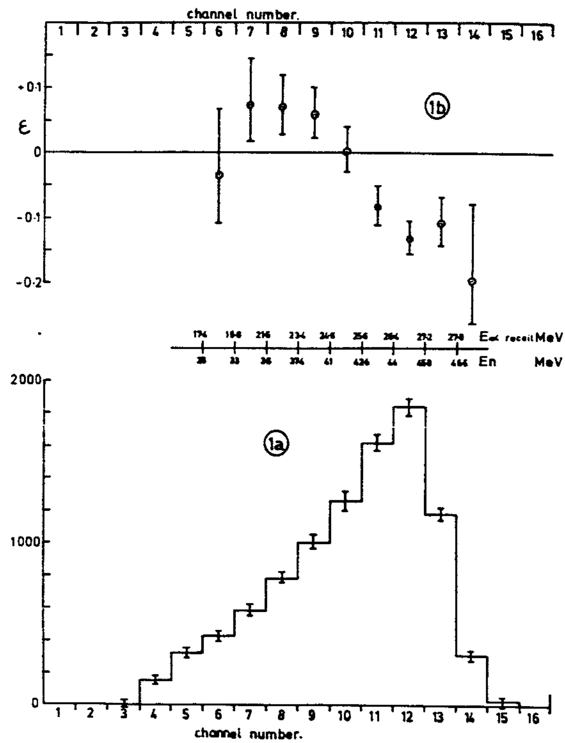


Figure 28 – Transfer polarization of neutronz from liquid deuterium target.

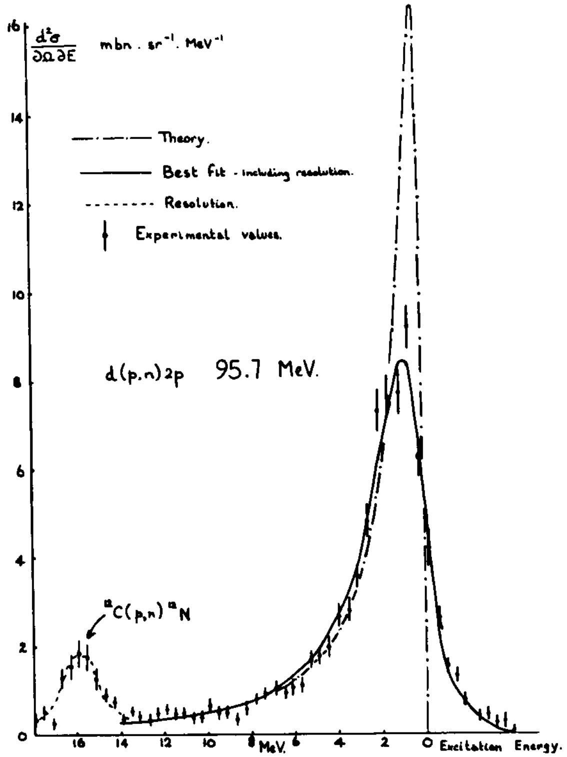


Figure 29 - d(p,n2p reacting at 95.7 MeV.

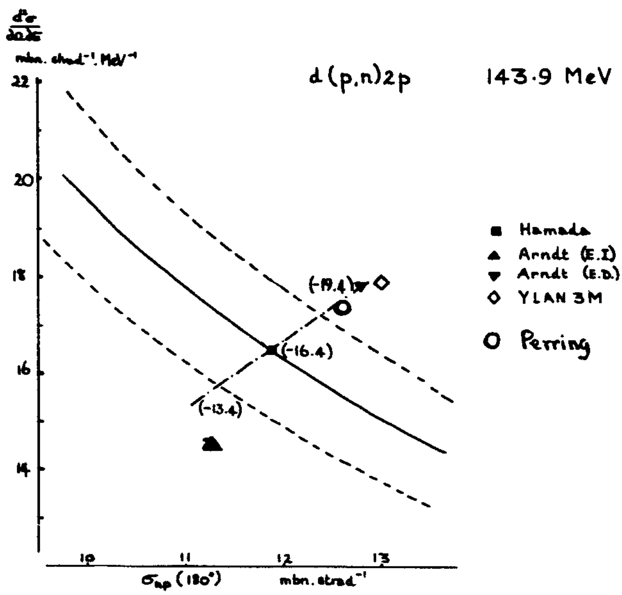


Figure 30 - d(p,n)2p reacting at 143.9 MeV

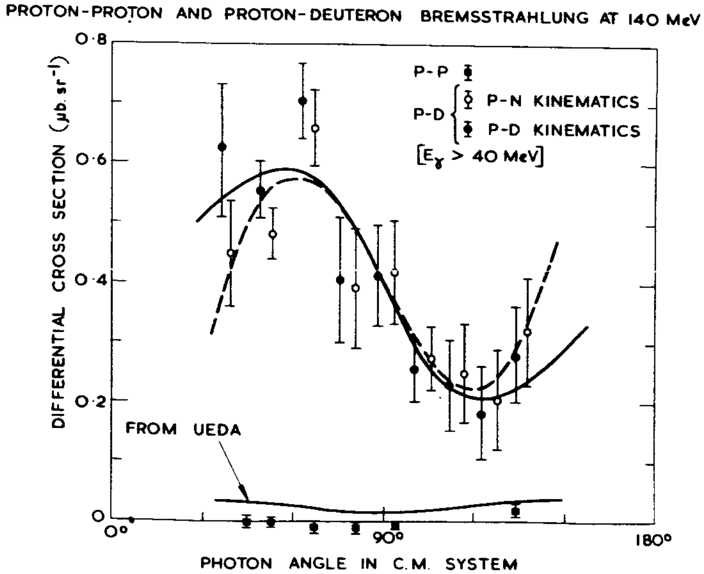


Figure 31

IGO: Is the curve shown through the points on your last graph, in the n-p system, a theoretical fit also?

ROSE: No, it was a polynomial fit to $\cos^2\theta$ - just a guide to the eye.

IGO: Is there any theory for that which would produce a discrepancy?

ROSE: No, I believe the number corresponding to that for free n-p should be multiplied by about 2. We don't get it from the study of the p-d system, but from the general study of the p-nuclear bremsstrahlung and then relate it to the theory of Beckham from a UCRL report. He stated that the bremsstrahlung from p-Be collisions at 90^o is approximately half what you'd expect from p-n at the same angle, due to the effect of the exclusion principal on the scattered neutrons inside this nucleus. Now we found that Beckham's theory did fairly well in describing our bremsstrahlung results from muclei and we are inclined to believe his factor of 2 here. This is the cross section per neutron in Be compared with free neutrons. So we applied the same factor of 2 to the measured value of 4 and we get about 8, therefore greater than 40 Mev. Of course, the spectrum is such a steep one with energy that the cross section is highly dependent on the cutoff.

MORAVCSIK: I have two questions both pertaining to experimental techniques. One, you mention several sets of data that you concluded would have to be disregarded for the present time because the mean polarization wasn't known was wondering whether these data could not be salvaged by simply re-working them in view of new measurements of the calibrating reaction which we now know are different or have a different value now than when the people measured it originally?

ROSE: I suspect that some of the old data might be salvaged, but I think it would be much better to remeasure it.

MORAVCSIK: The second question pertains to the coulomb interference region. You showed some new measurement in the coulomb interference. Out experiences at Livermore with various analyses have been that in the past much of the data, particularly differential in cross section data in the coulomb interference region, had to be thrown away because it was impossible to fit it, no matter what you did. It might be that one of the reasons for this was that in that region you have to measure the angle accurately since the difference of cross section drops very rapidly with angle. I was wondering what the limitations are in measuring angles in this respect, and are there any advances made in this particular field?

ROSE: Well, you may have noticed that in the data that N. Jarvis showed yesterday, we didn't go into this region, for precisely the reasons that you have stated. We were unhappy about multiple scattering correction. Backgrounds were getting up to about 40% of the effect and since we are aiming at 1% measurement, we are very doubtful about subtracting such large backgrounds. You don't have to only measure the angle accuracy; you've got to fold in the counter resolution. Generally we thought that with present techniques there was simply no point in pursuing the data any further. We think that perhaps the Rochester people are going to have to justify themselves rather hard in the data that they have presented - to satisfy me, anyway.

BREIT: In our work in the region of 600 KeV to 1.8 MeV, it always looked exceedingly nice to have a check through the coulomb interference because one dealt with an angular variation that one knew and one had at least one term that one could absolutely rely on. I remember especially in the work of Herb and collaborators who tried to be more accurate that it helped very much to use that region - get the bugs out by seeing why things did not agree in the preliminary form of the work.

ROSE: Yes, I think at higher energies, the hardest part of the experiment is the small angle region. At low energies the techniques are different in that you can use gas targets. You don't have any walls to your detectors. Altogether it's much cleaner than higher energies.

BREIT: Of course, the other pasture always looks greener. With gas targets and with the slit systems used you really have horrible things to compute and make corrections for. I think even now they don't know how the slits really work on account of slit penetration. But this is for very accurate work - much more accurate than that with which I have been concerned. And the geometry you use is, in a way, better - more clean.

MORAVCSIK: May I make a quick comment on this? Of course, the big difference between low energy and high energy coulomb interference work is that at high energy the effect is at very small angles. In the low energy region that you mention, it goes up to 30 or 40^{o}. There are no problems like the ones described - so there is no problem there experimentally.

BREIT: It went considerably lower than 30 or 40^{o}. It went to about 10^{o} with a gas target, and that kind of slit system is much more difficult than with the present geometry. The reason I am making the comment is that is seems it should be very good to have a check on something that one knows. I just wanted to point that out. If you throw that out, I think you will be losing something valuable.

ROSE: Do you really know coulomb scattering that well? I felt that this was not really all that clear.

BREIT: I can point out a case in which the Yale fits are better than the Livermore fits.

ROSE: There is appreciable discrepancy between the energy dependent and the energy independent predictions for the peak cross sections for the d(p,n)2p reactions. The energy dependent and the energy independent differ by about 4 milibarns in 18 which is really rather a lot, I think.

BREIT: One point you brought up very briefly, which seems to me a very imporrant point experimentally is the situation on the range-energy curves. In other words, the 1947 fit seems to be better than the more recent ones. At lower energy, namely at about 10 or 15 MeV, we've also had this kind of difficulty. It seems that this is one measurement that ought to get straightened out pretty soon.

ROSE: Yes, I would agree. Perhaps it ought to be straightened out by two quite different techniques. At McGill, they use floating wire techniques, and I think one could do very well by time-of-flight, too, in this energy region.

N65-32775

p-p Bremsstrahlung at 160 MeV

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W. J. Shlaer and K. H. Wang

Harvard University

Bremsstrahlung is a rather simple inelastic p-p interaction (in fact, the only one we have at low energies) so that one hopes that one might obtain information on the off-energy-shell interaction from it. We have been measuring this reaction at 160 MeV incident energy. Now, since there are three bodies in the final state, and since these experiments are fairly new so that there are as yet no established conventions for defining cross-sections, I feel I will have to spend some time explaining the geometry and technique of our experiment. I will then be able to summarize the results in a few minutes.

The principle of our measurement (which is different from that of the Rochester measurement about which you will hear later) is that we rely solely on the kinematic relations between the two protons to establish that bremsstrahlung took place. We have a gamma ray counter but it is not included in the coincidence requirement; it is merely called, you might say, as a witness in appropriate cases. That is, during the data analysis the computer may say to the gamma counter: "These protons say that a gamma ray went your way. Did it?" and the gamma counter will say: "Maybe, I don't know.", or sometimes, "Yes.", or perhaps take the fifth amendment. It's not a very good gamma counter because it is very hard to design one with both good efficiency and energy resolution at these energies.

Since the reaction leaves three bodies with no internal states it suffices to measure five quantities to determine the final state completely. If you measure six, you can use the redundancy of the last to see whether the reaction was bremsstrahlung or not. In this specific case, assume that we know the proton angles - four quantities. Then one proton energy should be observed to be a definite function of the other. A simplified diagram of the experiment is shown in the first figure. We use a liquid hydrogen target. Two counters determine the energy of the protons; they are set at angles such that elastic coincidences are a priori excluded. These counters form a trigger, which causes their pulses, and also the pulse from the gamma detector (a Cerenkov counter) to be analyzed and recorded. However, I wish to emphasize once more that the gamma counter is not in the trigger requirement.

Figure 2 shows the expected kinematic relations between the two proton energies, for three values of the proton scattering angle θ. (The two protons are detected at equal angles to the beam; this choice is convenient but not necessary.) From a point lying anywhere on these loci, the gamma direction and energy can be inferred. In other words, these measurements are completely differential in all the kinematically free variables, except spins, on which we have no information. These T_R, T_L plots can be viewed as generalizations of the elastic case; that is, the rings, as θ grows larger, recede to a point when $\theta = 43\text{-}3/4^{\circ}$, the angle for symmetric elastic scattering at 160 MeV. The important feature of these rings, experimentally speaking, is that the energy region of interest is bounded. For instance, at $\theta = 40^{\circ}$ a "bremsstrahlung" proton will have a maximum energy of 75 MeV, whereas an elastic proton has about 90 MeV at this angle. Therefore, elastic protons can be excluded in each arm individually. This is crucial, since it eliminates the enormous background rate of elastic-elastic randoms which would otherwise occur.

Figure 3 shows a detailed block diagram. I would like to emphasize just two points here. First, the counter geometry is so arranged that the telescopes do not see the target walls in coincidence. This eliminates a large background of quasi-elastic (p,2p) events. The single telescopes do see the walls and, therefore, some non-bremsstrahlung low-energy protons; random coincidences between two such protons limit the beam intensity we can use at present. Second, all protons of interest stop in counters 4 whose pulses are analyzed; counters 5 veto elastic protons. A fraction of a percent of the elastic protons fail to reach 5 because of a nuclear interaction in 4, and these would still cause a high single-telescope rate. Therefore, the fast coincidence circuits are timed to reject elastic protons on the basis of time-of-flight between counters 1 and 3. Thus we are using two criteria - range and time-of-flight - to discriminate against elastics. We feel that we have eliminated essentially all of them; of course, this merely reduces the background and does not sensitively affect the final cross sections I'll present.

One other comment is that we record both reals and randoms (that is, prompt and delayed coincidences) simultaneously, so that we have a continuous measure of the residual random-coincidence contamination.

Figure 4 shows a plan view of the apparatus. The table is five by ten feet. The counters are shielded against scattered particles from odd angles. The singles rates in the counters are quite high; that is, megacycles.

Figure 5 shows the Cerenkov counter temporarily placed behind one of the proton telescopes, where it was timed. This was done by introducing a little scintillator to produce a clean pulse, then looking at the recoil protons from elastic scattering. Just to give you an idea of its size, it involves 16 gallons of CCl_4 which, for a 160 MeV machine, is a fairly large detector.

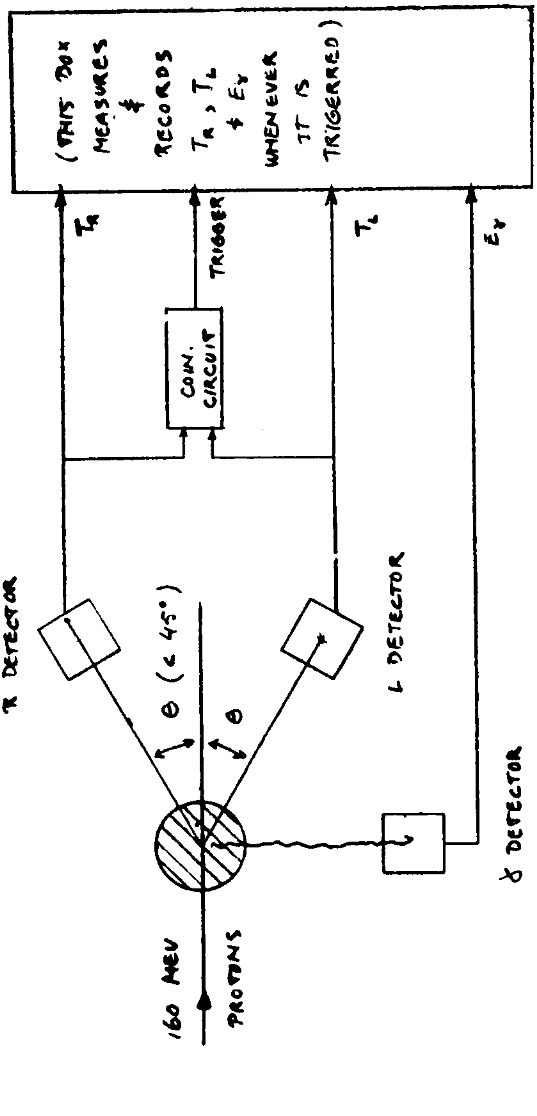
A PDP-1 computer was used in the time-sharing mode to collect the data. Figure 6 shows what happened when we asked it for a scatter plot of events while an independent group was reconstructing some spark-chamber event. Time-sharing occasionally leads to interference of this sort.

Figure 7 shows the observed scatter plots. Randoms have been "subtracted" by annihilating the "prompt" event nearest each "delayed" event in the T_R, T_L plane. These plots fit the kinematic expectations very well. (The energy calibration of the #4 counters was determined by using degraders to produce protons of known energies.) In addition to the bremsstrahlung one sees a systematic clump of events in the upper right-hand corner of each plot; these are quasi-elastic d(p,2p)n events owing to the deuterium contamination of the liquid hydrogen. Actually, they are quite useful since they verify the energy calibration and tell you what the energy resolution is; they also give you a rough check of the absolute cross section.

Since going around the ring essentially corresponds to varying the gamma-ray angle, one can infer the gamma-ray angular distribution by plotting an appropriate function of the density as one goes around the ring; such plots are seen in Figure 8. The distribution is sensibly uniform, except perhaps for $\theta = 40^\circ$. The dotted lines correspond to a theory of Sobel and Cromer, reduced by a factor of four; that is, it predicts much too high an absolute cross section. In this case one can also see that it predicts the wrong angular distribution, except perhaps at 40°. I should mention that a Monte Carlo simulation of the experiment shows that our effective gamma angular resolution is about 20°; that is, one histogram bin; it is somewhat worse at $\theta = 40^\circ$.

If one sums all the events on a given ring one obtains the cross
:tion integrated over all gamma-ray angles; Figure 9 shows the results.
: squares are renormalized data from a preliminary run; they were
.ginally off a factor of two in absolute value, but the ratio between
' and 40^{o} checks pretty well. The crosses show the Sobel-Cromer theory,
:in reduced by a factor of four; it gives the variation with θ very well.

The final figure shows the evidence from the gamma counter. I
nk the scatter plots leave little doubt that we are seeing bremsstrahlung
not much else. Figure 10 is a plot of the gamma counter response versus
gamma-ray angle as inferred from the proton data. It shows that when the
tons say that a gamma came out at the particular angle at which the
enkov counter was placed, this counter indeed shows an enhanced response.
s is not so clear at $\theta = 40^{o}$, but here we are dealing with very low-energy
mas for which the counter is not efficient. The 35^{o} picture is missing
ause the Cerenkov counter had not been put into service yet when these
a were taken.



N.B. THE γ - PULSE IS NOT PART OF THE TRIGGER REQUIREMENT. HENCE γ-COUNTER EFFICIENCY DOES NOT ENTER THE CROSS-SECTION MEASUREMENT.

SIMPLIFIED BLOCK DIAGRAM OF PPB EXPERIMENT

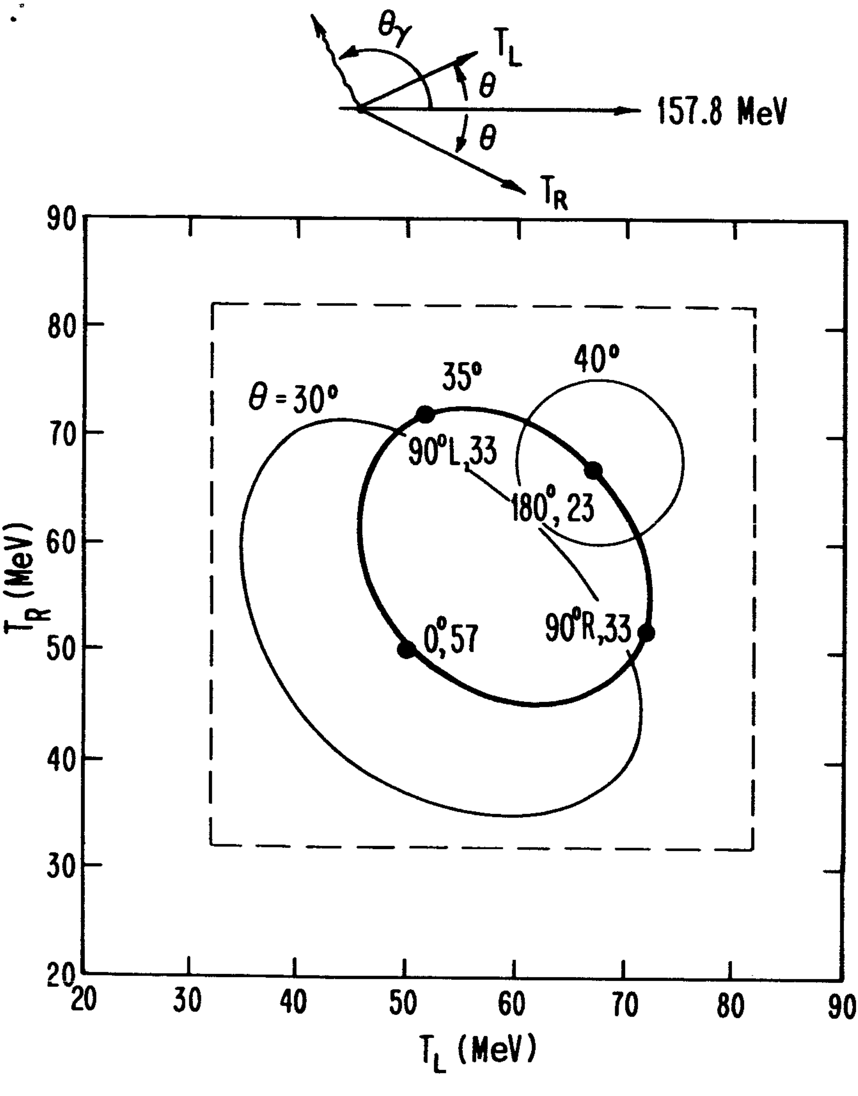


Figure 2. Kinematic relation between the two outgoing proton energies, at various scattering angles.

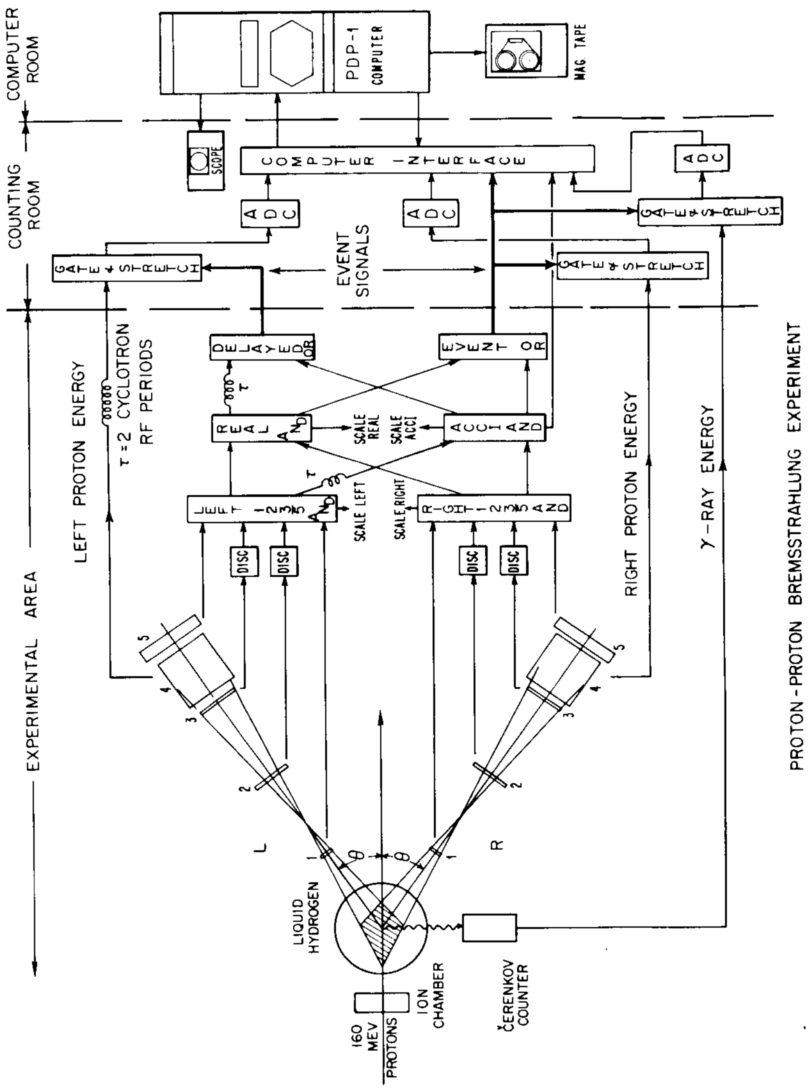


Figure 3. Detailed block diagram.

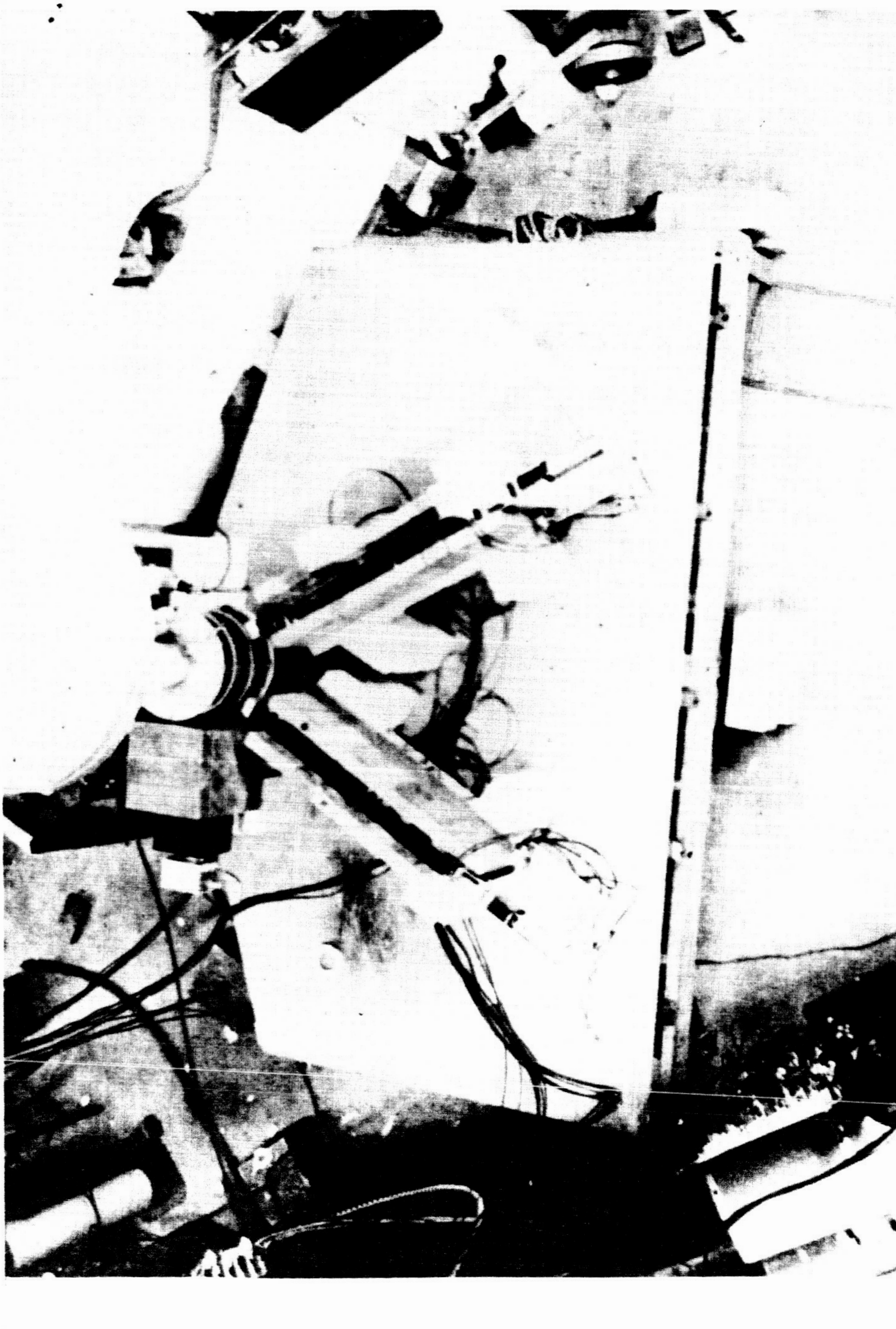
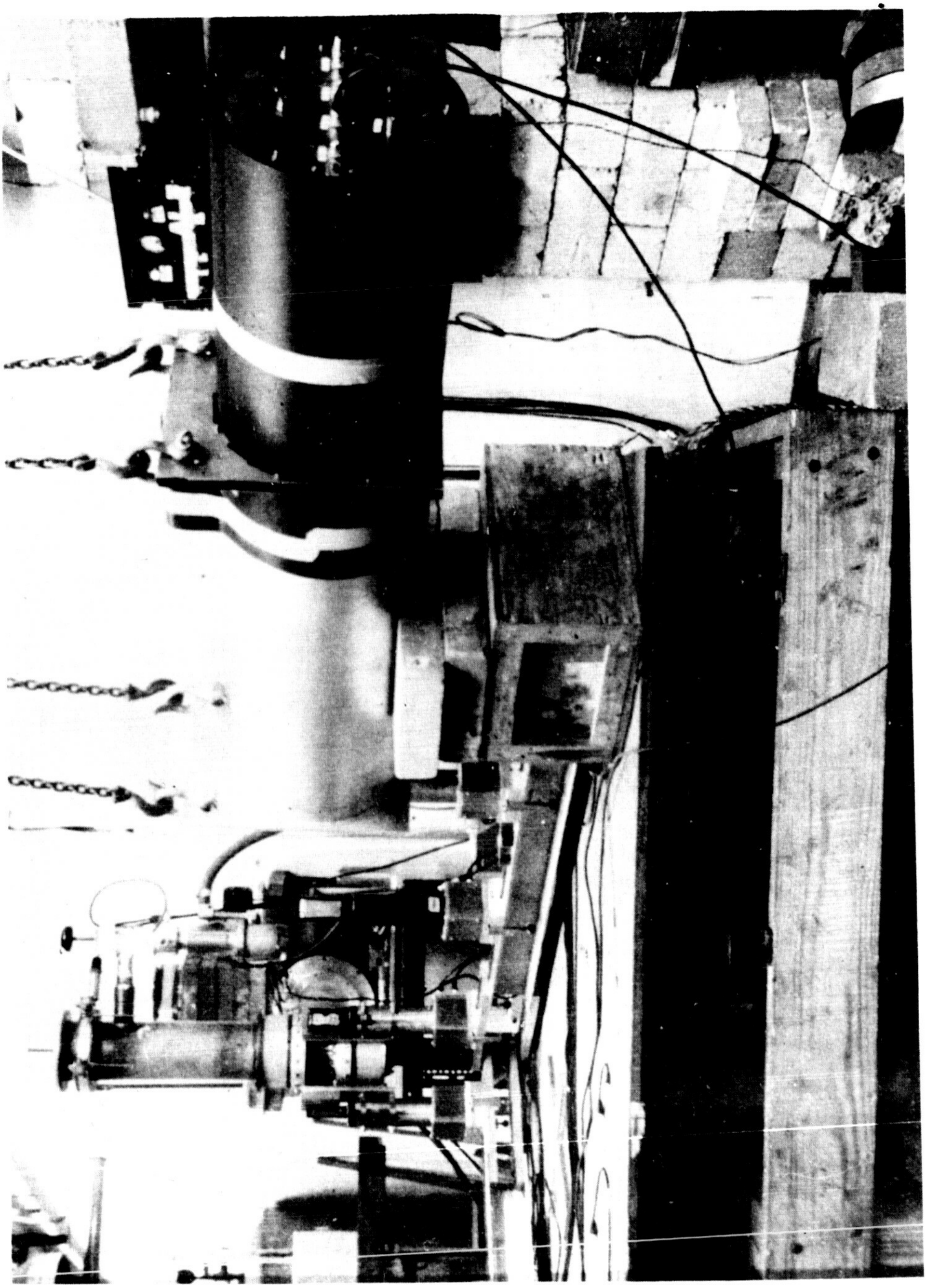


Figure 4. Plan view of the apparatus.



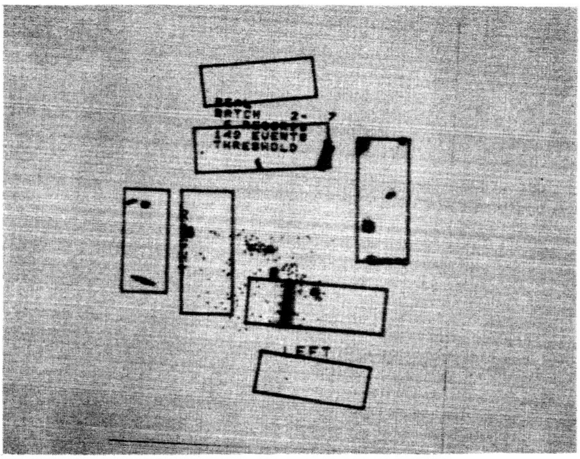


Figure 6. Sample of computer output during run.

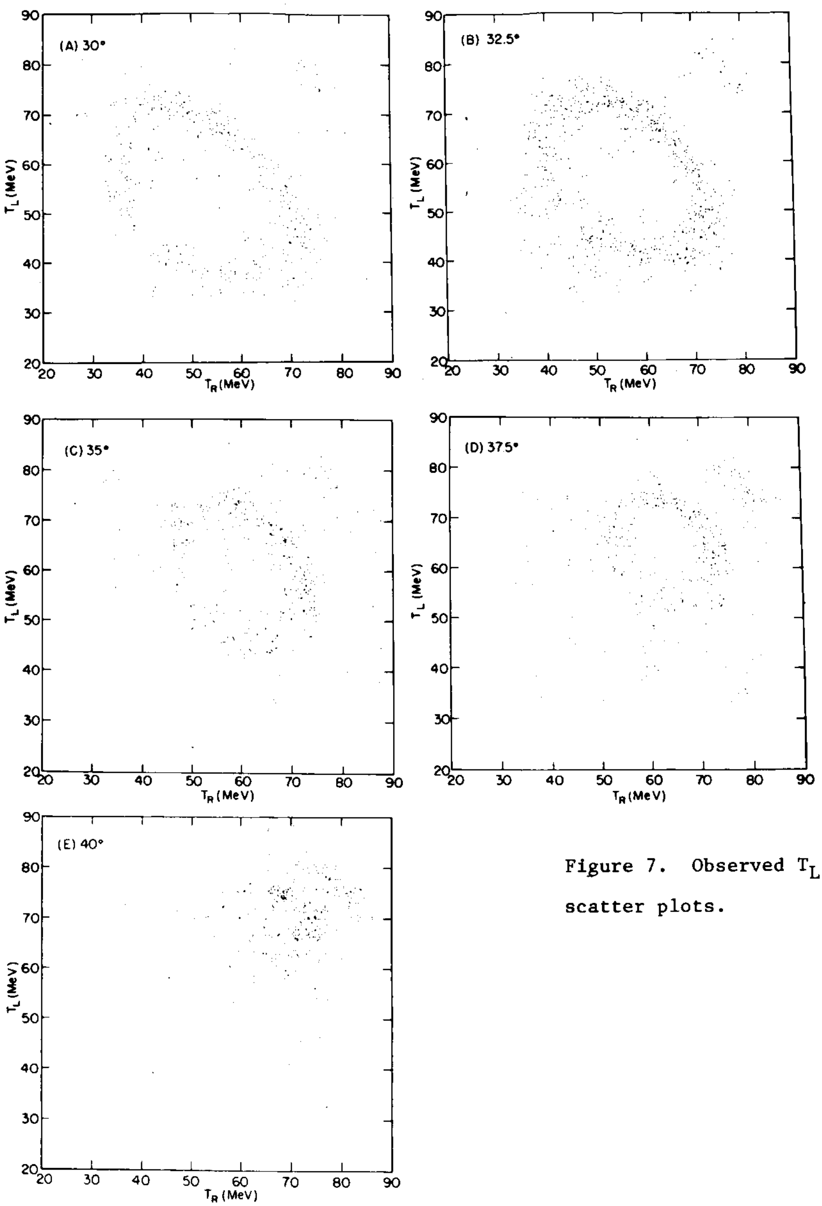


Figure 7. Observed T_L, T_R scatter plots.

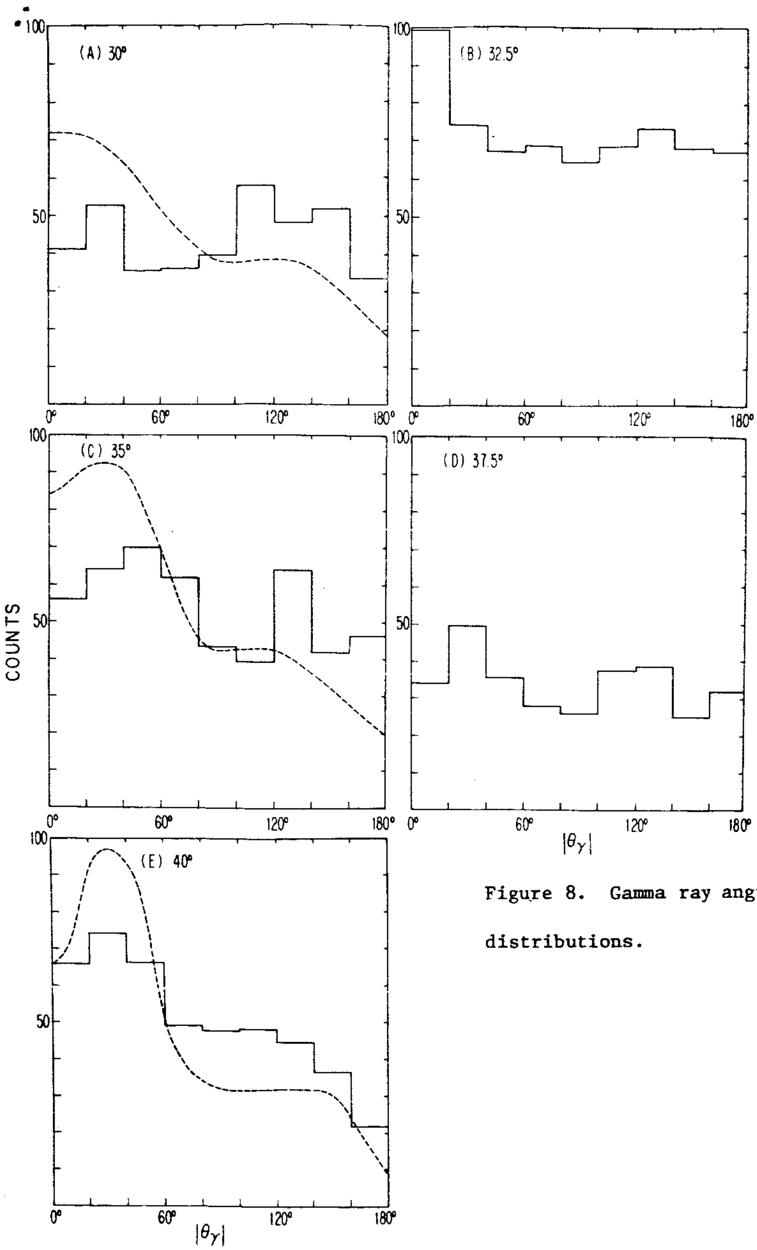


Figure 8. Gamma ray angular distributions.

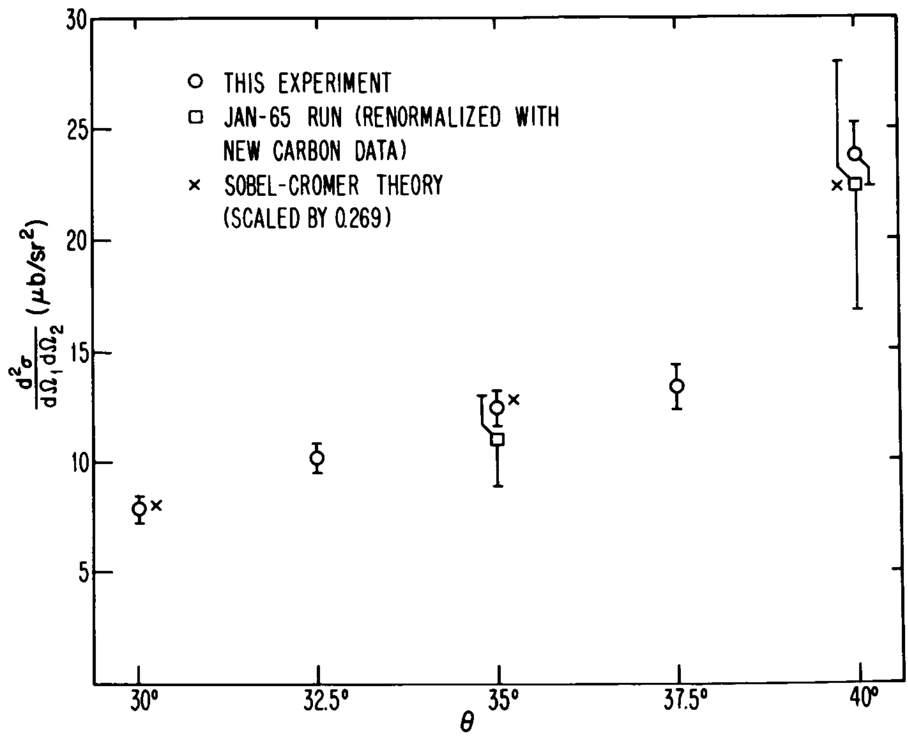


Figure 9. Cross sections integrated over all gamma-ray angles, at various scattering angles.

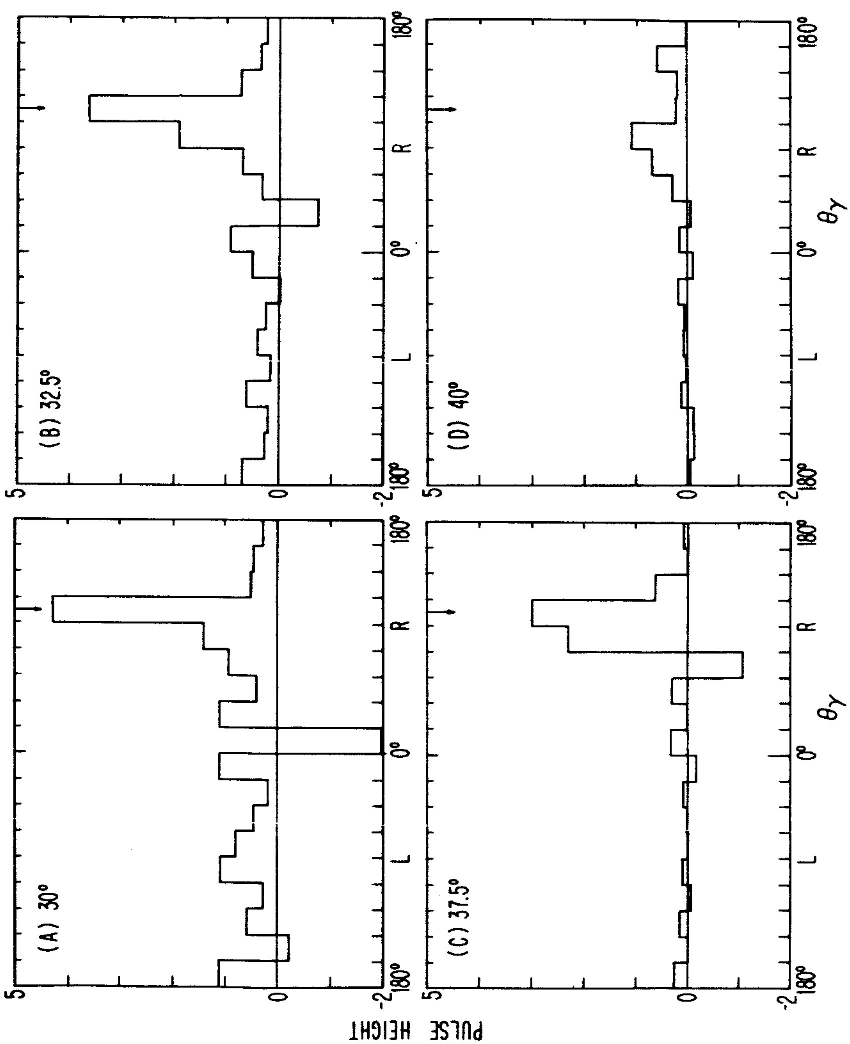


Figure 10. Cerenkov counter output vs. gamma-ray angle inferred from proton data.

JARVIE: What was the effective speed of your coincidence time resolution?

GOTTSCHALK: To do the time resolution we had to separate protons with three-quarters of a nanosecond difference in time-of-flight. The right-left circuits did not have to be particularly sharp; I forget what they were. But in each arm the criterion was very sharp. We worked them conservatively. At 40^{o} there is so little difference that we got very little good out of it, but at 30^{o} we got considerable reduction of the background rate.

IGO: I'd like to ask about the theory. Your first sentence was that you learned something about off-the-energy shell interactions. As the theory disagrees so badly with this data, what is the situation on your understanding of the problem?

GOTTSCHALK: The situation is this. Both the experiments and the theory are relatively young. The consensus of experiments that have now been done at three energies is fairly good; they all give considerably lower values than the theory. The theory is a potential model of the interaction. There is no a priori reason that such a model should be a good description, although, because of nuclear matter calculations and such things, one would perhaps be surprised if it were a very poor description. So, I don't know. One could say that, right now, there is no very strong reason the theory should give a good answer. It's hard to say.

ROTHE: I'd just like to add a little to that. Ueda's calculation, to which Dr. Rose was referring, was done on a one-pion-exchange in a photoproduction vertex, which is somewhat different from the potential model. You don't expect it to be right, and it isn't either.

SOBEL: I just wanted to give a number on the distance from the energy shell. The ratio of the final center of mass energy to initial center of mass energy is about three or four in these experiments. This is quite far from the energy shell. Possibly this is involved in the discrepancy of the potential prediction.

KOLTUN: Just on whether to expect effects from off-the-energy shell or not, a reminder that some years ago there were calculations on high-energy photo-disintegration of the deuteron by Marshak and deSwaart, which is very off-energy shell and very much potential model and which works rather well. So, if the discrepancy remains, I suspect there will be a lot of hard work in finding its source.

BREIT: There was work at Yale on the same problem. There are deviations which show up as you go to higher energies. Now, of course, one tends to attribute them to meson production or being close to the meson production threshold. But then, the comparison of such calculations is complicated by the fact that in p-p bremsstrahlung, the very large effect of the electric dipole, (which, while not the dominant thing at high energies, is dominant at low energies) is absent altogether. So you depend on more complicated things. And, of course, there is also the electromagnetic form factor to consider, which will be more important for E2 and M1.

N66-32776

P-P BREMSSTRAHLUNG CALCULATIONS

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Michigan State University

and

D. Marker

Hope College

We would first like to mention the recent measurements near 50 MeV at Manitoba (Werner) and at UCLA. Warner found $d\sigma/d\Omega_1,d\Omega_2 = 2.1 \pm 0.4\ \mu b/st^2$ for $\theta=30°$ in the Harvard-geometry notation, while a preliminary value from UCLA is $3.4 \pm 1.4\ \mu b/st^2$.

The original idea in carrying out PPγ experiments was to try to decide between different potential models. What different kinds are there? We think the following list is sufficiently exhaustive:

(1) hard core + strong attraction (Hamada-Johnston, Yale)

(2) long-range finite core + weak attraction (Bressel-Kerman-Lomon)

(3) weak, momentum-dependent (Green)

(4) non-local, separable (Tabakin, Amado)

There have been statements in the literature, recently, to the effect that the hard core is produced by vector boson exchange. If this is true, we do not need to consider the other models. The only recent one-boson-exchange potential is that of Bryan and Scott. We have plotted their potential as a function of radius (Figure 1). Notice the contrast between the Hamada-Johnston (HJ) and Bryan-Scott (BS) curves for the $^1S_0$ state. The one-boson-exchange potential has a hard core radius of less than a tenth of a Fermi! It is essentially non-existent. It had long been supposed that the exchange of vector bosons would produce a strong short-range repulsion. This is true, but in the case at hand it has been all but wiped out by the strong attractive ρ-nucleon tensor coupling. The latter is demanded by the isovector anomalous gyromagnetic ratio of the nucleon. The triplet even state (Figure 2) reverses the sign of the strong tensor contribution, so one has an almost completely repulsive potential. Exit the deuteron. We conclude that there is no evidence for the "physical" hard core from one-boson-exchange potentials.

We have calculated the predictions for PPγ from potentials of each of the four types listed above. We used the two-potential formalism of Gell-Mann and Goldberger, which means that one treats the strong force potential correctly (to all orders) while retaining only first order electromagnetic terms. Other speakers here refer to this kind of calculation as the "Sobel-Cromer theory" but of course it is not a theory. It is just the correct way of using the potential scattering formalism to compute PPγ predictions. In doing a calculation of this kind, one computes three terms (Fig. 3a). The blobs are the exact off-energy-shell strong-force scattering amplitudes computed from the potential model. Sobel put a great deal of effort into calculating the double scattering term, the third figure in the diagram. He found this term to be negligible compared to the other two so we have neglected it. An unknown but hopefully small error is present in both Sobel's and our calculations due to the neglect of an amplitude contribution which vanishes on-energy-shell but may be finite off-energy-shell. Sobel is at present investigating this term.

Our results for the 50 calculations are that: (a) the old Brueckner-Gammel-Thaler (BGT) potential predicts 40 $\mu b/st^2$, and (b) the other hard-core potentials and the other three classes of potentials all predict 25-30 $\mu b/st^2$. Why is BGT so much higher? It is well-known that the BGT potential is a much poorer χ^2-fit to the elastic scattering data than are the more recent models.

One can get a better feeling for the discrepancy by looking at individual phase shifts. One of the most important phases for PPγ is the $^3P_2$ (Figure 4). The "experimental" points shown are from phase shift analyses. It is immediately obvious why the BGT PPγ result is so much larger. A similar situation occurs in the $^1D_2$ state (Figure 5). It is obvious that the BGT potential should be omitted from all future calculations and discussions.

One then comes to the mysterious grouping displayed by the cross sections from the diverse kinds of potentials. One first notes that the $^1S_0$ state is of great importance for PPγ because of the low energy of the final two protons; higher-wave interactions must be comparatively weak there. We have examined the off-energy-shell K-matrix element for the $^1S_0$ state for each of the four potential classes. The predictions of three very different types of potentials are shown in Figure 6. The horizontal scale is (p/k) so that on-energy-shell has the value unity. If one "eyeballs" the curves into on-energy-shell agreement, the off-energy-shell predictions over the range of interest for PPγ are all very close. It is thus not surprising that the several types give close predictions for the PPγ cross section.

What does one make of the discrepancy? Yennie has noted that in some nuclear calculations the double scattering term exactly cancels the single scattering terms to lowest order in the photon momentum. So if Sobel made a gross error in estimating the double scattering term, we might yet be saved. We are currently checking this term via a separable potential. Koltun has pointed out that the two-potential formalism omits the emission of the γ while bosons are in flight between the nucleons. Such a term could be comparable to the terms already included, possibly resulting in the desired partial cancellation.

Finally, we would like to mention Ueda's dispersion theory calculation.

The equivalent diagrams for PP scattering are shown in Figure 3b. The two-pion exchange diagram gives a particularly large contribution because of the resonating N*'s in the nucleon blobs. One would not expect the one-pion exchange contribution to have any solo relevance at all for the $^1S_0$ state, and of course calculation bears this out. The corresponding diagrams for ppγ are shown in Figure 3c. Here, again, one would not expect the one-pion-exchange diagram to be relevant for the $^1S_0$ state. Ueda calculated only that term so although his calculation is very interesting, it is only a beginning. It should only be compared to a peripheral experiment, not to the experiments which have been reported so far.

A remark. It is conceivable that the absence of E1 transitions in PPγ leaves us with a residue which can not be correctly calculated from potentials. In this case, NPγ might turn out to give reasonable agreement between experiment and potential theory. We would then understand why what worked for photodisintegration of the deuteron does not work for PPγ, and the relevance of PPγ for nuclear physics calculations would be less than that of NPγ.

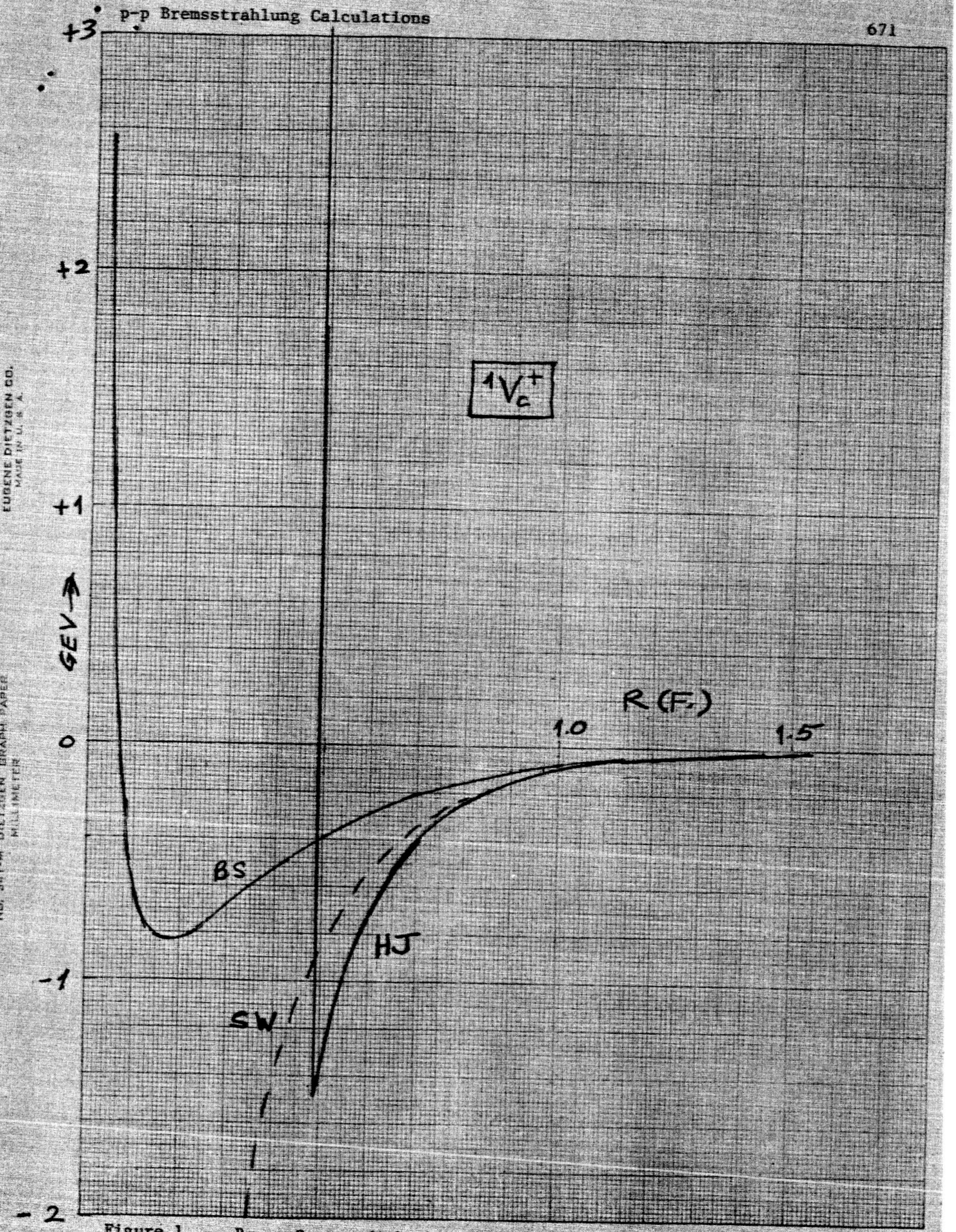

Figure 1 - Bryan-Scott, (BS) and Hamada-Johnston (HJ) potential vs radius in fermis.

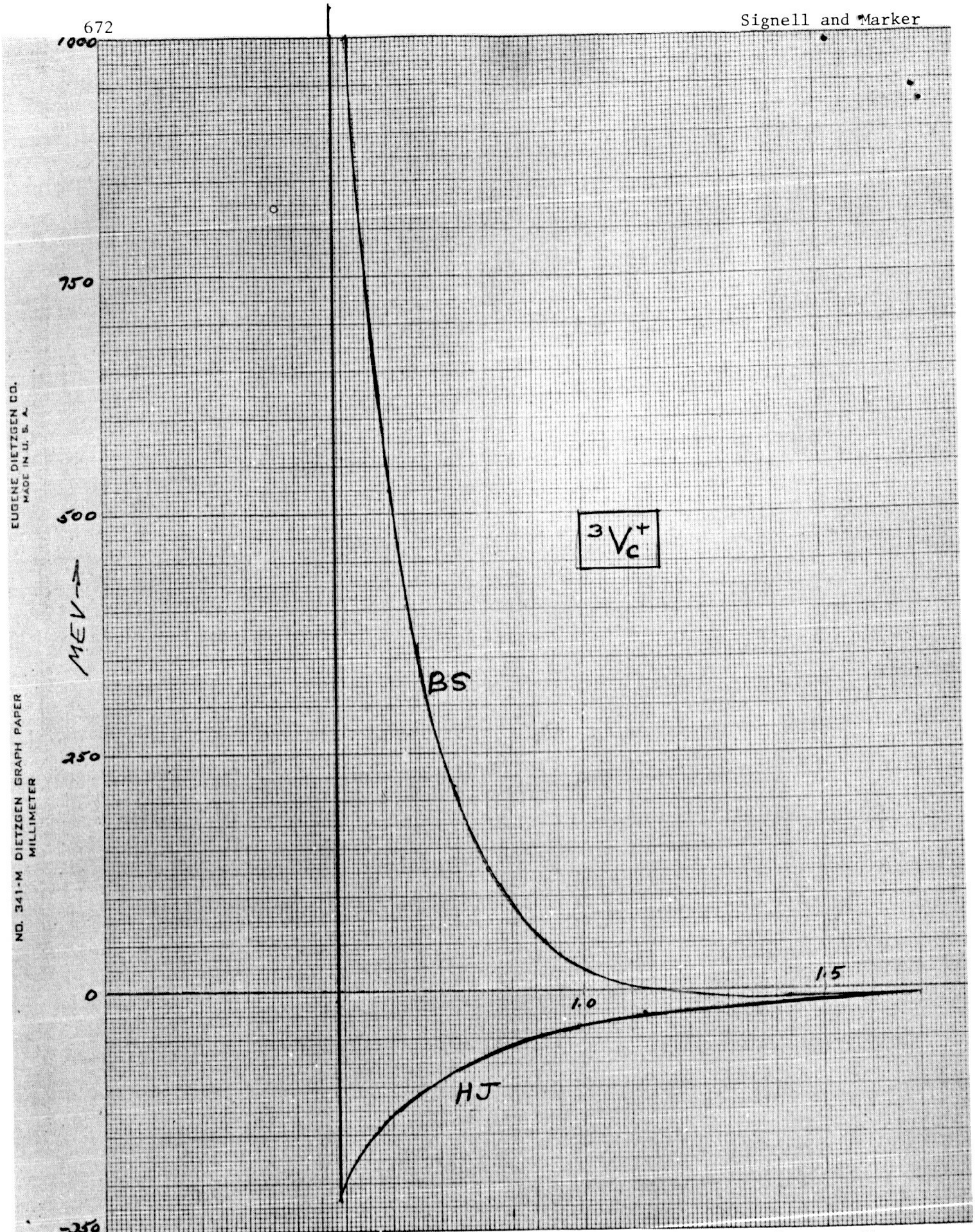

Figure 2 - Bryan-Scott (BS) and Hamada-Johnston (HJ) potentials vs radius in fermis for triplet state.

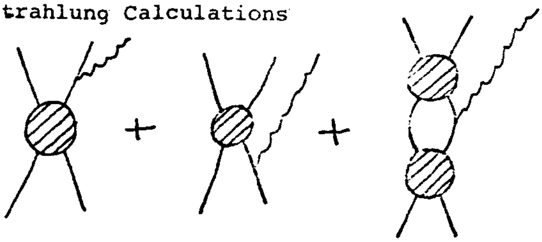

Figure 3a.

Feynman diagrams used to calculate p,pγ cross sections.

p,pγ calculation, Sobel.

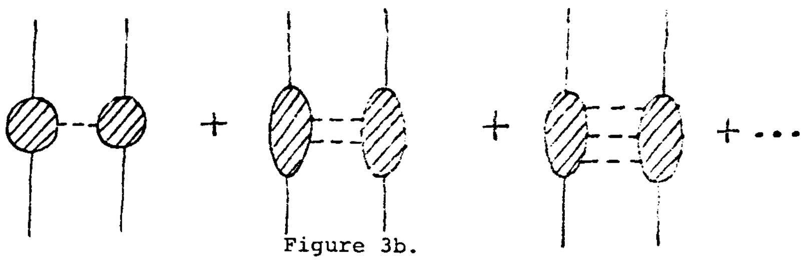

Figure 3b.

pp calculation, Ueda-type

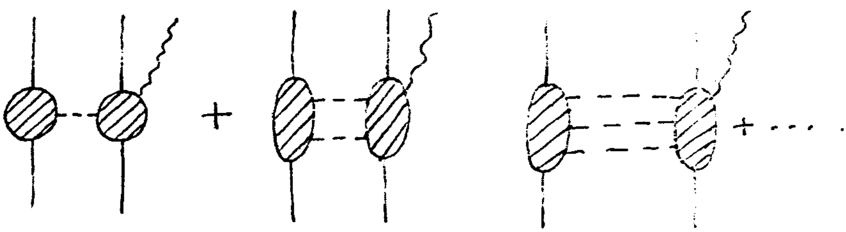

Figure 3c.

p,pγ Calculation, Ueda.

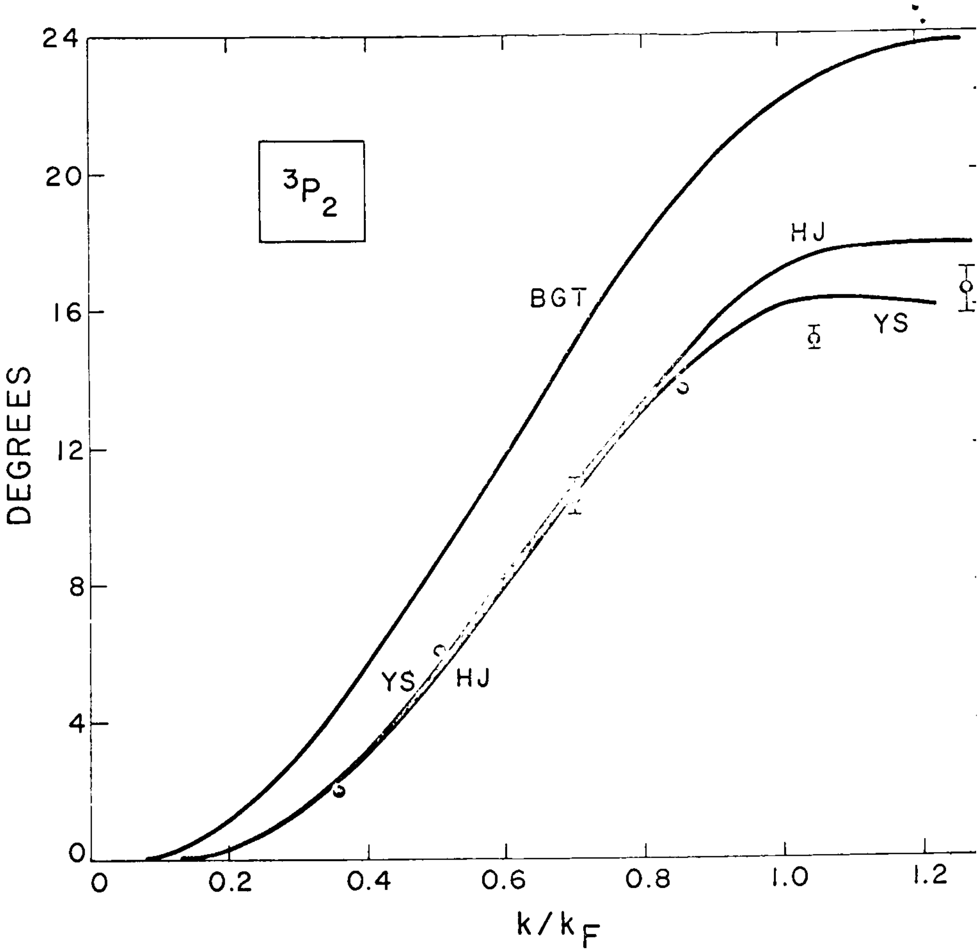

Figure 4. $^3P_2$ Phase shifts in degrees.

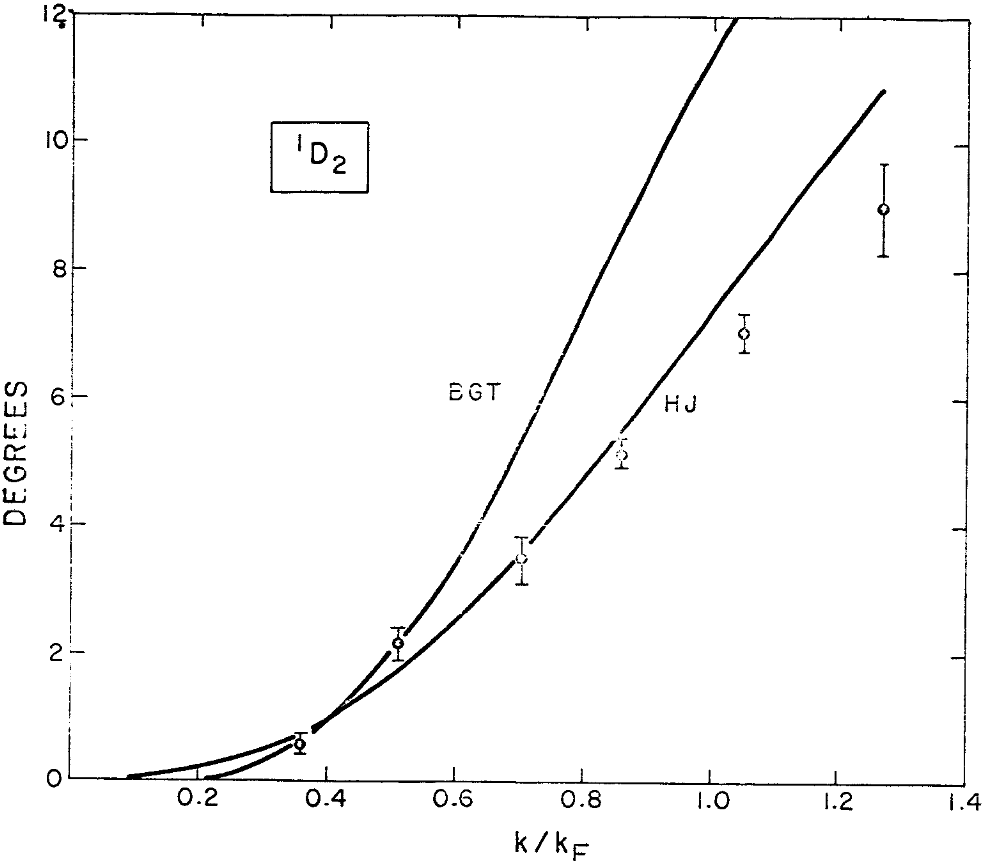

Figure 5. $^1D_2$ Phase shifts in degrees.

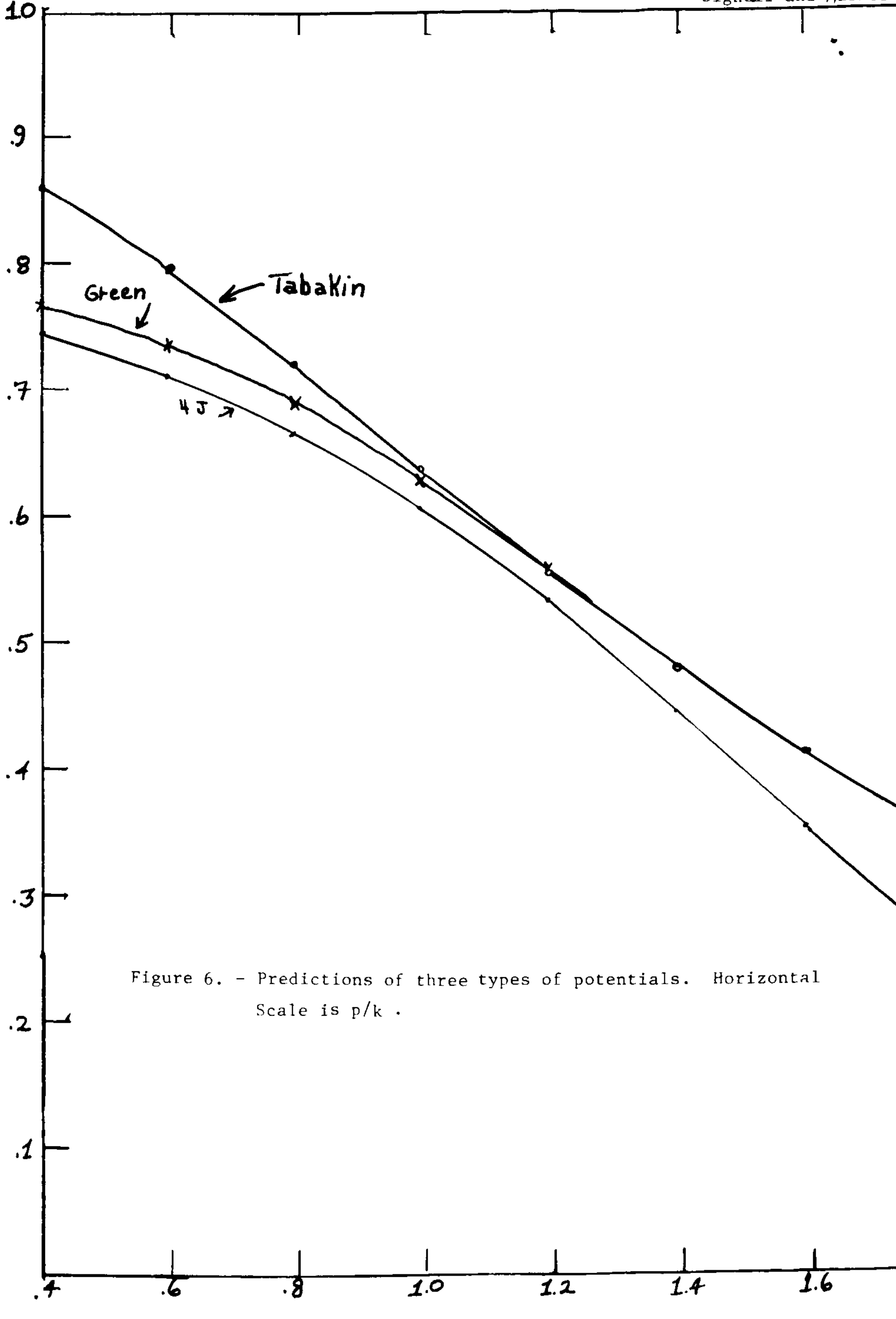

Figure 6. - Predictions of three types of potentials. Horizontal Scale is p/k .

N66 32777

Nucleon-Nucleon Bremsstrahlung at 200 MeV

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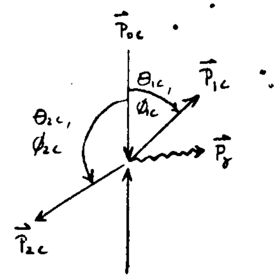
This talk is concerned with two quite separate experiments one of which has been completed (the ppγ work) while the other is still in progress (the pdγ work). Both experiments have been run at Rochester's 130" Cyclotron at energies near 200 MeV. The object of the work is to examine in as much detail as possible the behavior of off mass shell nucleon-nucleon scattering.

The approach used in the ppγ experiment is illustrated in Fig. 1. An incident beam of polarized 204 MeV protons strikes a 3" hydrogen target; the resulting γ-ray is detected in coincidence with two protons. This coincidence triggers the spark chambers. Kinematic information on eight of the final state variables is obtained leaving only the gamma energy to be inferred. This overdetermination of the reaction kinematics allows rapid rejection of background events.

Since the two protons in the final state do not come off back to back in the three-body center of mass system one has to describe the scattering in terms of some average c.m. angles. We have chosen to define them as momentum averaged angles by considering the following geometry:



$\hat{i}$ is along beam

$\hat{j}$ is perpendicular to the beam, in the plane containing the γ ray.

$$\hat{k} = \hat{i} \times \hat{j}$$

$$\cos \theta_c = \frac{(\vec{P}_{1C} - \vec{P}_{2C})}{|\vec{P}_{1C} - \vec{P}_{2C}|} \cdot \hat{i} \;; \qquad \cos \phi_c = \frac{(\vec{P}_{1C} - \vec{P}_{2C})}{|\vec{P}_{1C} - \vec{P}_{2C}|} \cdot \hat{j} \sin \theta_c$$

Here $\phi = 0$ is defined by the γ-ray ϕ, while $\theta_c = 0$ corresponds to the incident beam direction. θ_c and ϕ_c together with θ_γ, ϕ_γ, and E_γ are the five variables we have chosen to look at as a physically meaningful combination. E_γ measures the extent to which the reaction is off the mass shell while in the limit as $E_\gamma \rightarrow 0$ θ_c and ϕ_c become the elastic c.m. angles.

Let us turn now to the $pp\gamma$ results. Data were taken at $\theta_{\gamma lab} = 45^\circ$, 90°, and 135°. Fig. 2 shows the observed angular dependence of ϕ_c at 90°. Isotropy is clearly ruled out. Cos ϕ_c, and $\cos^2\phi_c$ both provide reasonable fits. Fig. 3 shows the same at 135° and Fig. 4 that at 45°. Taken as a group the total χ^2 for a $\cos^2\phi_c$ fit is 22, for $\cos\phi_c$ is 33 while the expected χ^2 is 16. What this means is that the protons prefer to come out in the plane formed by the γ-ray and the incoming protons, or, otherwise said, the gamma prefers to come off in the plane of the final state protons.

Next we look at the θ_c dependence. Fig. 5 shows the 135° results.

(Elastic scattering is flat in cos θ_c as it is in ϕ_c). Here the asymmetric effect is not so pronounced although as figures 6 and 7 show there is definitely a tendency to pile up events in the $0.5 \geq |\cos \theta_c| \geq 0$ rather than in the $1 \geq |\cos \theta_c| \geq 0.5$ region. Averaging over all θ_γ angles, roughly two thirds of the events lie in the central region, with one third in the peripheral region.

Let us look now at the gamma ray spectra. Fig. 8 shows the 90° energy spectrum. The essential feature of this and the spectra which follow is their constancy until the highest allowed gamma energies are reached. This is repeated at 135° and 45° as shown in Figs. 9 and 10. These spectra agree very well in shape but are a factor of two lower in magnitude than the predictions of Ueda[1] who used a one pion exchange and photoproduction vertex to compute the cross section. To conclude the pp data I would like to present the cross sections integrated over gamma energy $(d\sigma/d\Omega_\gamma)_{E_\gamma \geq 35 \text{ MeV}}$ in the c.m.:

$\theta_{\gamma cm}$	$(d\sigma/d\Omega_\gamma)_{cm}$	Ueda
59° (45°lab)	38±7 nb/ster	111 nb/ster
72° (90°)	39±3	86
34° (135°)	73±3	143

It seemed desirable to obtain n-p bremsstrahlung measurements to complement the pp measurements. In the absence of a sufficiently high intensity, monoenergetic neutron beam, we turned to deuterium for a "neutron

* The vertical scale in Fig. 8, 9, and 10 should be reduced by factor 2.

target". Unfortunately, there is quite a collection of γ ray producing reactions initiated by protons on deuterium. They are listed in Fig. 11.

We have performed a survey experiment, in which we measured the cross section for production of γ rays, and obtained rough branching ratios for the 5 processes listed. Our experimental setup is shown in Fig. 12. Protons strike a liquid deuterium target, γ rays are detected in the γ counter, (25 MeV threshold), while charged particles may count in the scintillation counters 5, 6, 7, 8, which subtend large solid angles.

The cross section for γ production was found to be:

θ_γ^{lab}	45°	90°	135°	Total
$\frac{d\sigma}{d\Omega_\gamma}$ (μb/ster)	7.6±0.8	2.9±0.3	1.1±0.1	48 μb

Our γ ray production cross section is high compared to measurements of Edgington and Rose, at Harwell.[2] In particular, when we degraded our beam to an energy of 148 MeV, we obtained 26 μb, while they obtained 3.2 μb at 146 MeV. Our γ threshold was 25 MeV, theirs was 40 MeV. It seems unlikely the difference in thresholds can explain all of the discrepancy.

There were more charged particle coincidences with the counter (5 or 6) on the side away from the γ counter than on the side towards the γ counter. The excess of counts was found to be predominantly coming into the small solid angle region appropriate for the pickup reaction. Attributing this excess to the pickup reaction, we find the surprisingly large cross section of (19±3)μb for it. (This compares with 11 μb expected via detailed balance from photodisintegration of the deuteron.)

γ 56 coincidences in excess of the number expected from pp bremsstrahlung were interpreted as pd bremsstrahlung. An efficiency program, based on the rash assumption that the angular dependences of pd elastic scattering and pdγ are the same, extrapolated from the γ 56 counts the pdγ contribution to the single charged particle coincidence rate, $\gamma 5$ or $\gamma 6$, independent of γ direction. A total cross section of 9 μb was obtained for pdγ .

If we interpret those $\gamma 5$ and $\gamma 6$ events not already explained, as np bremsstrahlung events, and if we further make the rash assumption that n-p elastic scattering and n-p bremsstrahlung have the same angular dependence, then we obtain an np bremsstrahlung cross section near 8μb.

Our previous measurements showed the pp bremsstrahlung total cross section to be near 1/2 μb. Further, a separate measurement indicated the capture reaction did not exceed 1 μb. Thus our charged particle coincidence measurements coupled with some extrapolations have accounted for some 37 μb out of the total of 48 μb.

It is not clear how to obtain a free n-p bremsstrahlung cross section from our numbers. It certainly should not be smaller than the quasifree n-p bremsstrahlung cross section which we have estimated at 8 μb. On the other hand, it should not exceed the total γ-ray production cross section (48 μb) minus the free n-p capture cross section (11μb). Hence we obtain 8 μb $\lesssim \sigma_{np\gamma} \lesssim$ 37μb. Recall that $\sigma_{pp\gamma} \approx$ 0.5μb. Hence np bremsstrahlung is a factor near 40 larger than pp bremsstrahlung.

References
1. Y. Ueda, Thesis, University of Rochester, 1965, not published.
2. AERE - PR/NP, 8 p. 42 (1965).

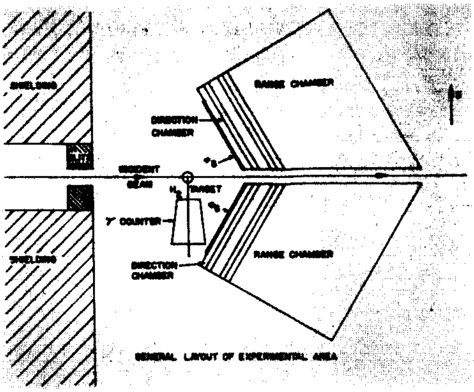


Figure 1. ppγ Experimental Setup.

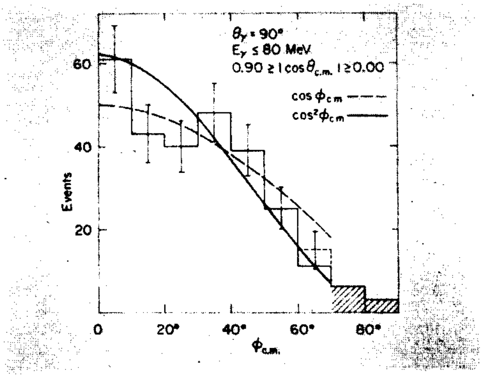


Figure 2. Angular Distribution, No. of events vs. ϕ_c for $\theta_\gamma = 90°$.

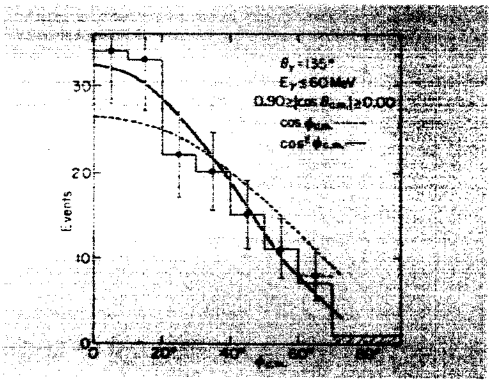


Figure 3. Angular Distribution, No. of events vs. ϕ_c for $\theta_\gamma = 135°$

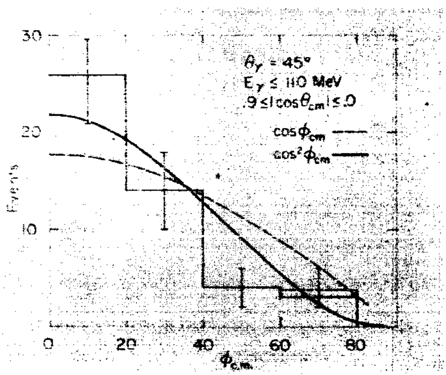


Figure 4. Angular Distribution, No. of events vs. ϕ_c for $\theta_\gamma = 45°$

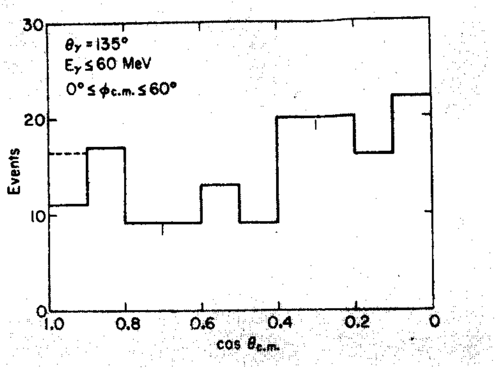


Figure 5 - Angular Distribution, No. of events vs cos θ_c for θ_γ = 135°

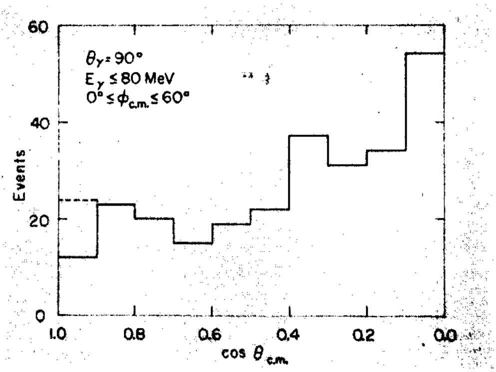


Figure 6. Angular Distribution, No. of events vs. cos θ_c for θ_γ= 90°

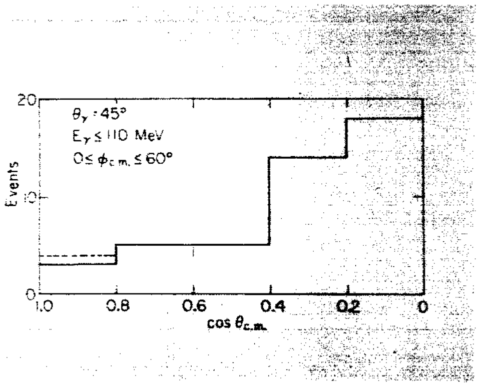


Figure 7. Angular Distribution, No. of events vs. cos θ_c for $\theta_\gamma = 45°$

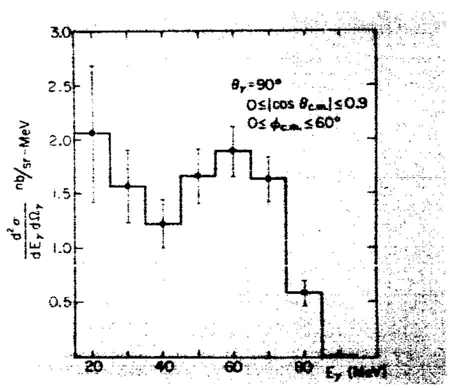


Figure 8. Gamma Energy Spectrum, $\theta_\gamma = 90°$
Vertical scale should be reduced X 2.

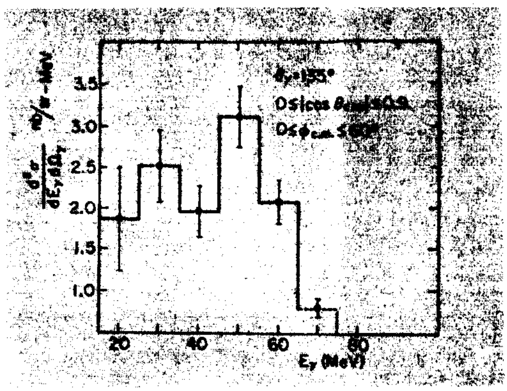


Figure 9. Gamma Energy Spectrum, $\theta_\gamma = 135°$
Vertical scale should be reduced X 2.

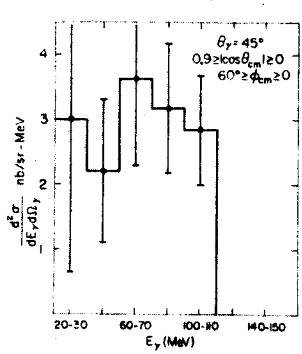


Figure 10. Gamma Energy Spectrum, $\theta_\gamma = 45°$
Vertical scale should be reduced X 2.

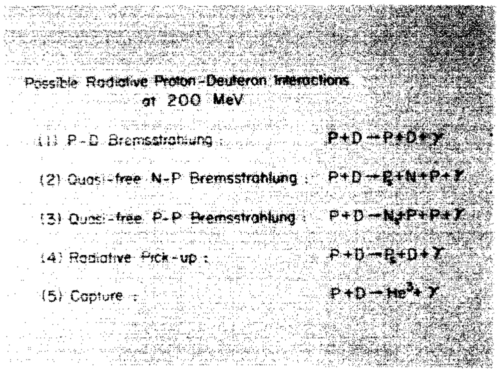


Figure 11. Possible Reactions for pdγ

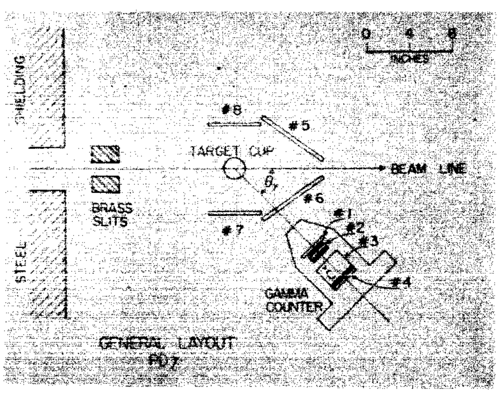


Figure 12. pdγ Experimental Setup.

GOTTSCHALK: Were the gamma energies on your graphs in the lab or cm system?

ROTHE: They were in the lab. However, we cut off the cross section at 35 Mev in the center of mass.

GOTTSCHALK: I would just like to make a point. If they were in the lab, then your assertion that you were further off the energy shell than we were is not correct because you have to take into account that your energy is higher to begin with. We went to about a 60 Mev gamma at 160 Mev proton energy. You went to 90 Mev at 200 Mev proton energy.

ROTHE: The point that I should have made in my talk is that one reason which we consider unlikely but a possibility causing our numbers to be high, is that in this reaction there was strong tendency for the three final state bodies to be coplanar. In connection with your talk, you do show such a tendency but not very strong...The numbers that Dr. Signell quoted for Dr. Warner are probably somewhat off due to his finite counter size and the $\cos^2 \phi$ dependence.

ROSE: I think you said you had a cross section for a pick up reaction something like 19 microbarns. In our experiments we measured a gamma ray spectrum, albeit very crudely, and we saw no evidence whatsoever of the peak which in our case would have been around 70 Mev and which would correspond to such a capture process.

ROTHE: We are going to look into this subsequently with spark chambers.

ROSE: The other point was that you mentioned your cross section was much higher than our cross section leaving me uncertain as to how much higher. I will put in the factor of 3 at least between our measurements and yours because of this difference of threshold between 20 Mev and 40 Mev based on the assumption that the spectrum below 40 Mev is approximately exponential. It happened that the fit we used had the same exponential fall off as we observed.

SIGNELL: The numbers I quoted for Warner are his latest numbers in which he attempted to increase the error bars and so on to take into account possibilities of even something as strong as the...

GOTTSCHALK: The thing that I am talking about is that if you look at gamma ray and integrate over a large counter you have a $\cos^2 \phi$ variation. If he did that he beat us all out by predicting a dependence that I don't understand.

SIGNELL: No, I meant to say that he did increase his error bars quite a bit over his original numbers when he realized what you had been talking about in your paper. I did want to say that the object of p-p bremsstrahlung experiments did seem to start out by trying to decide between these different kinds of potential models. When we found that they all gave about the same thing when we made the match on the energy shell, we looked at the off-energy shell matrix elements. It no longer seems to be deciding between these various potential shapes.

THORNDIKE: I'd like to get a bit more quantitative on the effect of the observed $\cos^2 \phi$ on these measurements. It will not affect Warner's results by more than a factor of 2 but I will be surprised if it affects it by less than a factor of 1.5. One assumes the factor to be something like 1.75. It will raise his cross section so that they now fall below the theory of Sobel and Cromer by about the same amount as the measurements of Gottschalk fall below the theory of Sobel and Cromer. With this experimental correction thrown in, Sobel and Cromer's theory scales properly with energy.

MORAVSCIK: I would like to see a plot of some sort of the discrepancy between the p-p Bremsstrahlung seen here and in the experiment as a function of the amount that you are off on the energy shell. If you can blame all this on the potential, the discrepancy presumably will somewhere disappear as you go back to the energy shell.

ROTHE: Well, as long as you sit at a given lab gamma angle then the amount that you are off in the energy shell is simply a function of the gamma energy. What you are saying is that as you go down in gamma energy, the agreement possibly should get better and as you go up you possibly get worse. In fact, with respect to shape it is identical with what is predicted. The thing that is different is the normalization - you have to bring everything up a factor of 2 at all angles.

GOTTSCHALK: Just a very quick point. One of the graphs I showed which is a plot of the integrated cross section versus angle is in a sense such a plot as you asked for because at each set of proton angles the gamma energy does not vary too much and it increases. Therefore the good fit of the Sobel theory to those point is in a sense a fit versus offness but the fit may be fortuitous.

MORAVSCIK: From these two pieces of information I would then conclude that it is probably not the potential that is to be blamed for all this; there is something else. It does not seen to be an effect which increases as one goes more off the energy shelf.

N66 32778

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Neutron-Proton Interactions at 205 MeV

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We have studied the neutron-proton interaction by bombarding deuterons with 205 MeV polarized protons, and observing high energy neutrons recoiling into forward angles. The parameters P and R_t have been measured.

The incident proton beam polarization was manipulated with a solenoid, so that it lay in a horizontal plane and normal to the beam direction. Its sign was reversed by reversing the current direction through the solenoid.

The polarization parameter P was obtained by measuring the up-down asymmetry of the recoiling neutrons. The neutrons were detected in a counter consisting of a polyethylene converter and a multi-element range telescope. Measurements were made at neutron laboratory angles between 10° and 30°. By reversing the solenoid current, many systematic errors were eliminated. Statistical errors were typically ±0.017.

The triple scattering parameter R_t differs from the conventional parameter R in that one analyzes the spin of the target particle instead of the incident particle; that is, the polarization transferred between the particles is investigated. The definition of R_t is shown in Fig. 1.

$\langle \vec{\sigma}_b \rangle_f$ is the final polarization of the target particle; $\langle \vec{\sigma}_a \rangle_i$ is the initial polarization of the incident particle. The equation assumes that the target is unpolarized, and that the incident beam has components of polarization only in the $(\vec{n}_t \times \vec{k})$ direction.

Our R_t experiment, then, consisted of directing a proton beam with polarization in the $(\vec{n}_t \times \vec{k})$ direction onto a liquid deuterium target, and measuring the $\vec{S}_t$ component of polarization of recoiling neutrons. The experimental layout is shown in Fig. 2. Neutrons recoiling from the deuterium target at angle θ_2 in the horizontal plane pass through the anti-coincidence counters, 0, 1 and onto a liquid hydrogen target used for spin analysis. By measuring the asymmetry of neutron-scattered protons recoiling into angle $\theta_3^{lab}(=25^\circ)$ in the vertical plane, the neutron polarization is determined. The measured asymmetry is a product of incident beam polarization P_1, analyzing power of the n-p scattering in the hydrogen P_3, and R_t. P_1 is known, P_3 is the free n-p scattering polarization parameter, determined from our own measurements, those of others, and phase shift analyses.

Measurements of R_t were made at neutron laboratory angles between 0° and 20°, to an accuracy of typically ± 0.09.

Because the target neutron is bound in a deuteron, a theoretical treatment is necessary to describe our reaction and relate it to neutron-proton scattering. We have performed an impulse approximation calculation which includes the s-wave final state interaction of the two protons. A similar approach worked well for quasifree p-p scattering, and for "slightly inelastic" p-d scattering. The calculation is intuitively described as follows. A proton (represented by a plane wave) is incident on a deuteron (represented by a triplet spin, ground state deuteron wave function). The incident proton and target neutron have an interaction (represented by the free n-p scattering matrix M_{np}) with the neutron recoiling into small angles (plane wave) and with the two protons emerging with relative momentum k (the p-p continuum wave function $\psi_{pp}(k)$). All states in $\psi_{pp}(k)$ except the s-state are described by a plane wave, while the s-wave final state interaction is included by using a square well potential with parameters chosen to fit the effective range and scattering length. Coulomb effects are ignored.

Our result is shown in Fig. 3.

An expression for R_t is obtained from the second equation by replacing P by R_t wherever it appears.

The coefficients a and b are form-factor-like quantities, $\int \psi_f \quad e^{iq.r} \psi_i$. I_o^{np} , P^{np} are the free np differential cross section and polarization parameters. "Ces" refers to charge exchange singlet. The "ces" parameters I_o^{ces}, and P^{ces} are obtained from the scattering matrix M^{ces} . (Λ^s and Λ^t are singlet and triplet spin projection operators).

The predicted neutron spectrum for 5^olab is shown in Fig. 4. Singlet scattering dominates, and is sharply peaked. The spectrum for 20^o lab is shown in Fig. 5. The broader peak of the free n-p scattering is now dominant. Our experimental conditions were varied with angle so as to include almost all of these peaks.

The results of the polarization measurement are shown in Fig. 6.

The curves are the predictions of phase shift solutions YLAN of the Yale[1] group, (0, 3, 3M, 4M), and the energy independent solution of the Livermore[2] group, (A-M). Solutions 3, 3M, 4M, and A-M give quite acceptable fits. Solution 0, does not fit, and solutions 1, 2, and 2M, not shown, lie above 0 and fit even worse.

The results of the R_t measurement are shown in Fig. 7. Solutions 3M, 4M, and A-M give good fits. Solutions 3 and 1 do not fit. Solutions 0, 2, and 2M, not shown, are worse fits than solution 1.

Of the 6 original Yale phase shift solutions, only 3M (the preferred one at that time) fits our data. The most recent modification of it, 4M, also fits our data, as does the most recent Livermore solution (A-M). Since our data were not used as input for any of these phase shift searches, the good agreement suggests solutions 3M, 4M, and A-M are essentially correct, and further changes in them will be small.

References

1. M.H. Hull Jr., K.E. Lassila, H.M. Ruppel, F.A. MacDonald, and G. Breit, Phys. Rev. 122, 1606 (1961), and private communications from Professor Breit.

2. Richard A. Arndt and Malcolm MacGregor, Phys. Rev. 141, 873 (1966).

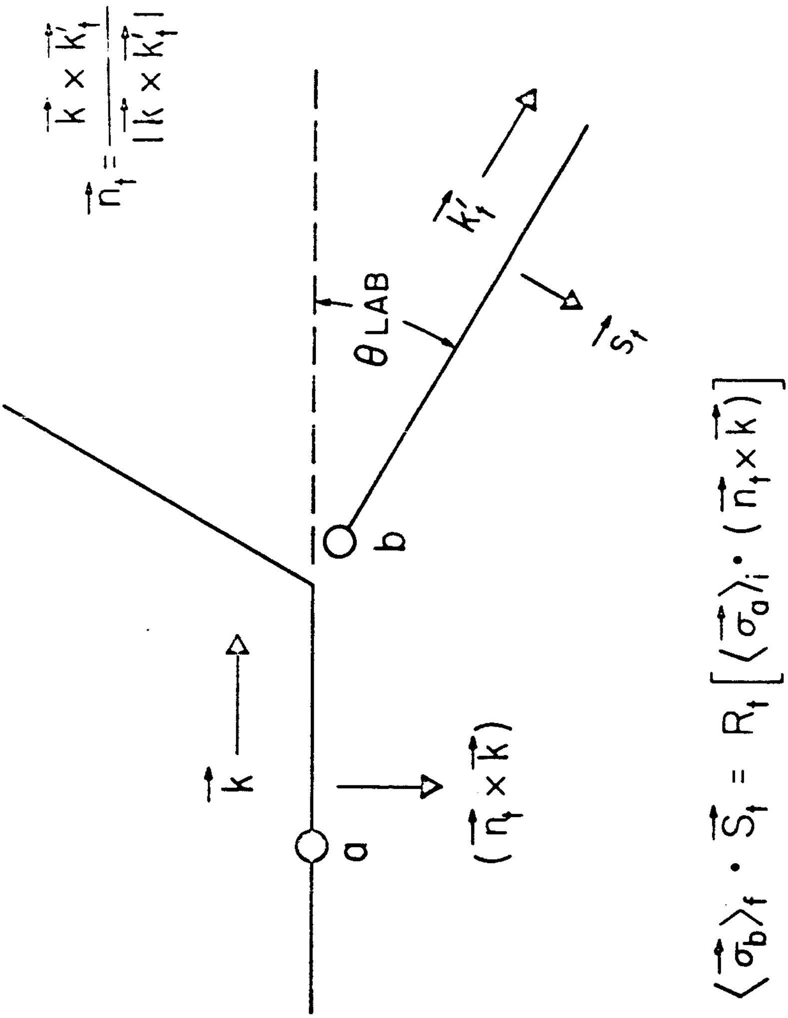


Figure 1. Scattering parameter expressions.

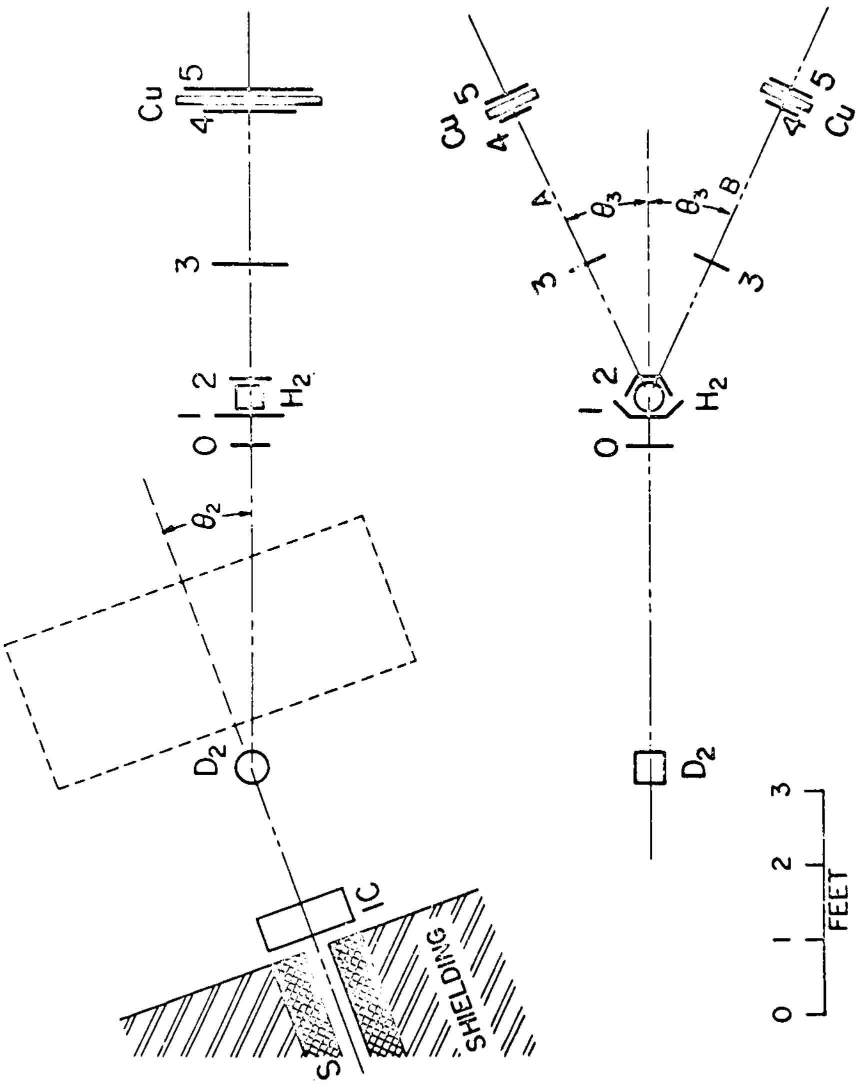


Figure 2
Experimental layout.

$$\frac{d^2\sigma}{d\Omega_n dE_n} = a(E_n)\, I_o^{np} + 2b(E_n)\, I_o^{ces}$$

$$P(E_n)\,\frac{d^2\sigma}{d\Omega_n dE_n} = a(E_n)\, I_o^{np}\, P^{np} + 2b(E_n)\, I_o^{ces}\, P^{ces}$$

$$M^{ces} = \Lambda^s(1,3)\; M_{np}(1,2)\; \Lambda^t(2,3)$$

Figure 3. Impulse approximation calculation for differential cross sections.

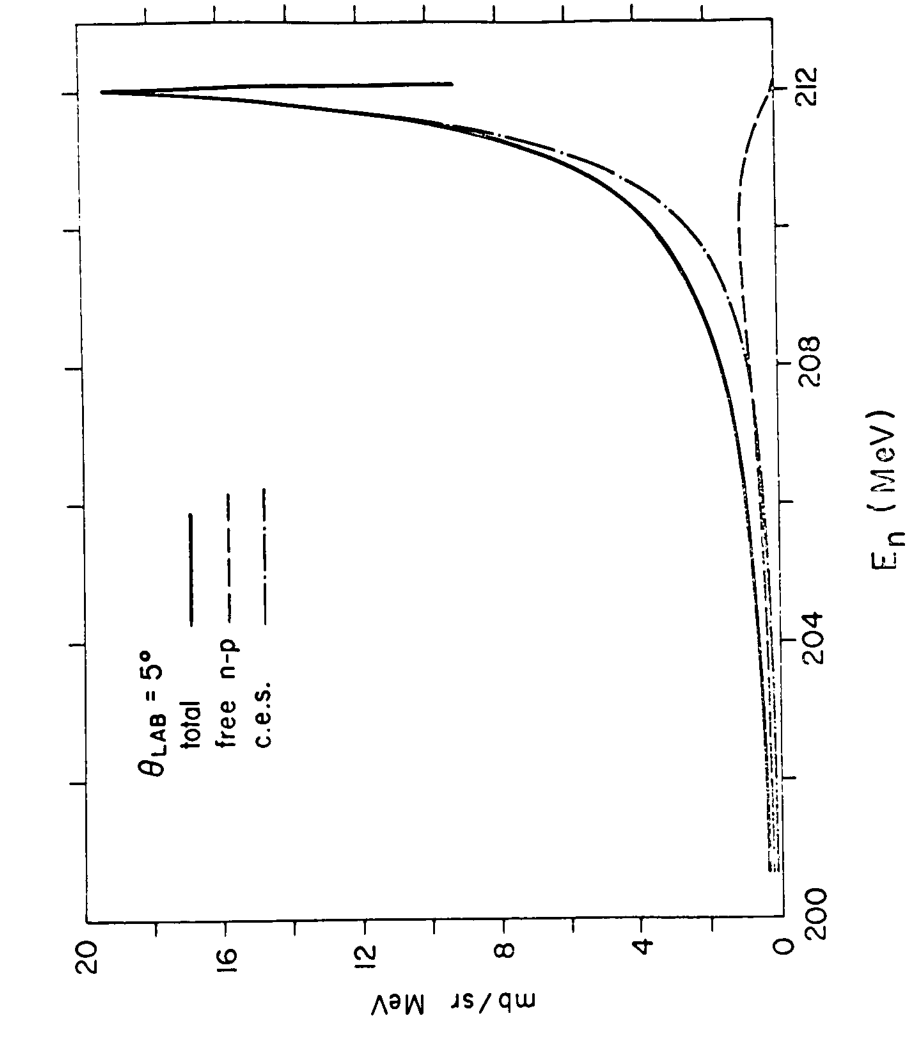


Figure 4. Predicted neutron spectrum for 5° lab. angle.

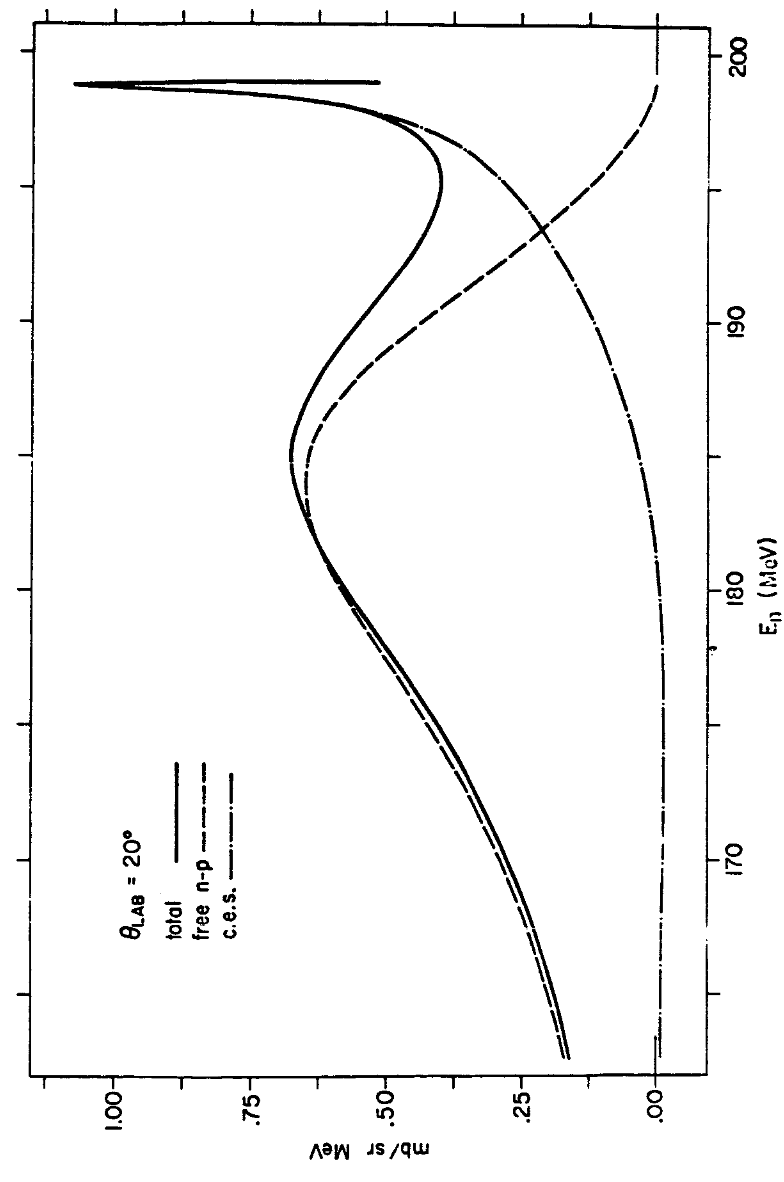


Figure 5. Predicted Spectrum for 20° lab. angle.

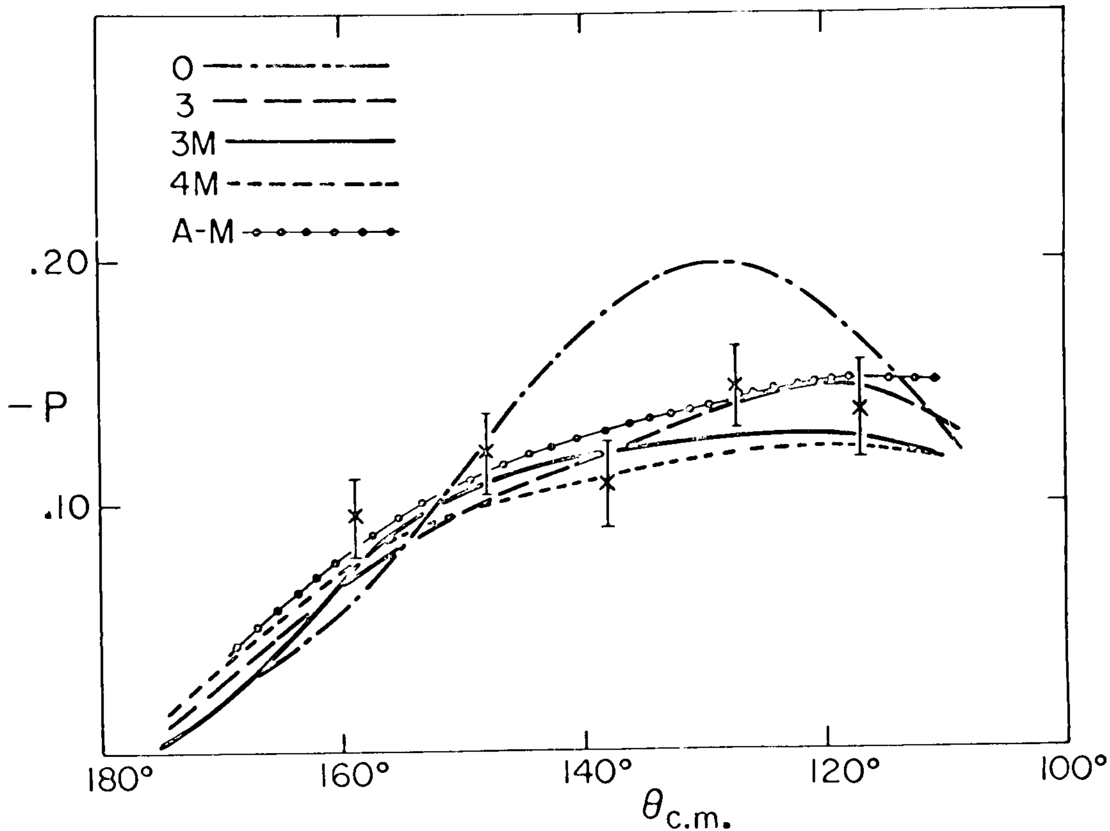


Figure 6. Polarization measurements vs. scattering angle. Also shown are theoretical curves of Yale and

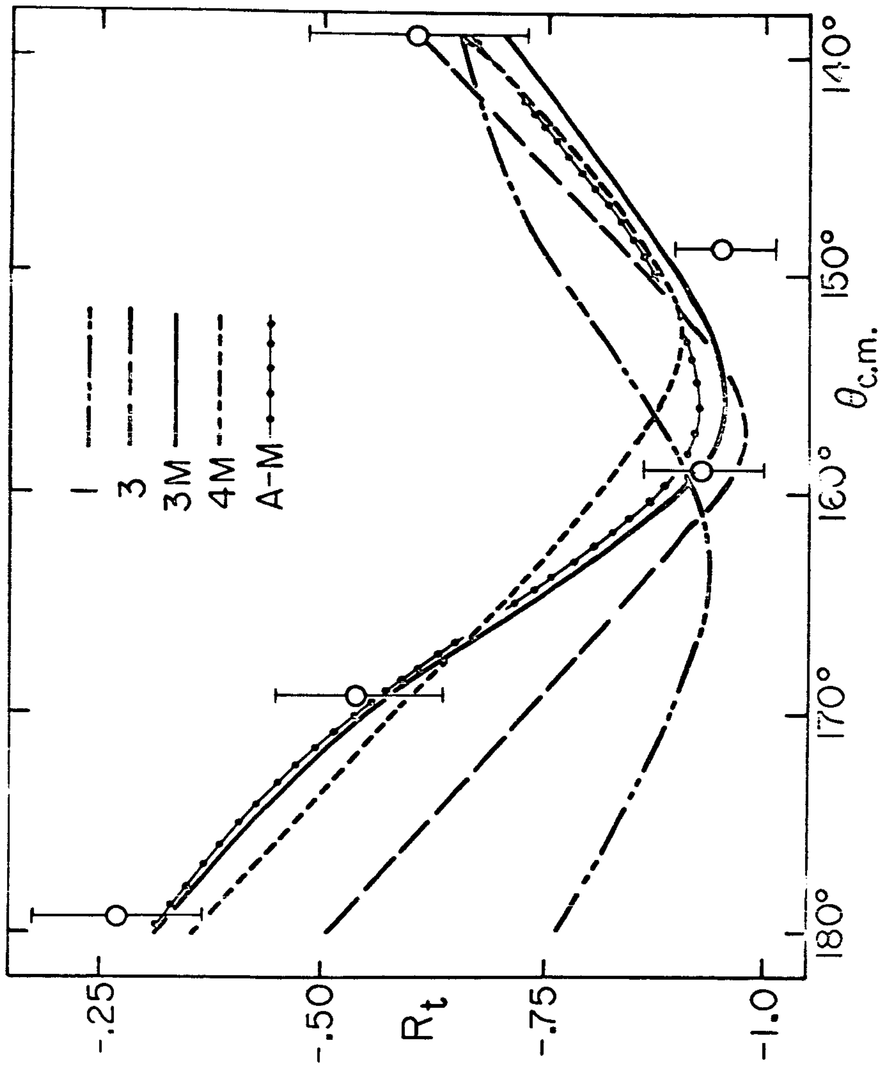


Figure 7. Experimental R_t vs. scattering angle. Yale and Livermore Theoretical are also shown.

BREIT: Is there a plan to compare these phase shifts with the Kazarinov et. al. phase shifts? They are similar to our old YLAN3 regarding the coupling parameter between S1 and D1. It is not identical with it and it would be perhaps helpful if one knew how it agrees with your data.

N66 32779

Quasi-Free Proton Scattering at 160 MeV

B. Gottschalk

Northeastern University

W. J. Shlaer, K. Strauch, and K. H. Wang

Harvard University

The title of this talk was a deliberate hedge since I hoped to have some quasi-free proton-deuteron scattering results available, but I don't as yet so this will be about the (p,2p) reaction. This is a fairly old sort of experiment; the modern version dates back about ten years to work at Uppsala which first showed that expectations on the basis of shell model were at least qualitatively verified. An excellent review of the field by Jacob and Maris has just appeared in the Reviews of Modern Physics; they are old hands at this experiment and I recommend the article.

I won't go into details of the theory but the measurements I shall describe were designed to test a particular aspect of it; namely, how closely does the reaction actually behave like a knock-out reaction? Despite the fact that experiments have been going on for about ten years, this point has not yet been sensitively checked. The formalism is shown in Figure 1. On top is a somewhat simplified version of the standard formula used in interpreting these experiments. The cross-section (on the left) is a function of the two proton solid angles and must be taken at a given excitation of the residual nucleus to define the shell-model state one is looking at. The expression, which follows from a plane-wave impulse-approximation treatment of the problem, states that the cross-section equals a product of three factors: a) a kinematic function; b) an effective cross-section for the primary p-p interaction; and c) the probability of finding a target proton with the appropriate initial momentum to yield an event in the phase-space incremental volume being studied.

Thus, if one believes the knock-out approximation made in deriving this relation one can infer nuclear momentum distributions from these measurements. The object of the present experiment is to find out how well in fact the cross-section depends only on P(q). In other words, is it a function of $|q|$? Perhaps I should define q more rigorously than I have: it is the recoil momentum of the residual nucleus after the interaction, which, in the impulse approximation, equals the negative of the target proton momentum before the interaction.

Figure 2 shows a scatter plot of events in the T_1, T_2 (energy) plane; such plots are familiar by now. This one represents measurements on a carbon target at 160 MeV incident energy; each proton telescope is set at 42½ degrees to the beam. One expects a minimum cross-section for p shell target protons if the energy is equally shared between the outgoing protons, because this corresponds to q = o and the p momentum-space wave function goes to zero here. The standard "coplanar-symmetric" experiment uses detectors biased to accept only equal-sharing events; P(q) may then be deduced from the angular distribution of such events. Our experiment, as the figure shows, also accepts protons of unequal energy sharing. They are divided up according to energy-sharing between the protons (as shown by the oblique lines); we then ask whether these "asymmetric" events obey the same momentum-description of the cross-section. (By the way, notice that the s state events are lumped near the center; this is also predicted by knockout since these protons have a high probability of zero momentum.)

Figure 3 is essentially a sum of the scatter plot in the diagonal direction; namely, a binding-energy distribution of events (summed over a limited momentum interval). The binding-energy resolution is about 3 MeV FWHM. The p and s proton peaks are clear. Obviously, the lower limit for the s protons is somewhat arbitrary since this state is very wide; therefore, the absolute s proton cross-sections will have to be taken with a grain of salt.

Figure 4 shows cross-sections as a function of angle (the two proton angles are equal for all these measurements). The lines are to guide the eye to points of the same energy sharing (five categories are used corresponding to the bins shown in the first slide). The dip for equal-sharing p-state events is deeper than that observed in the earlier experiments of Garron et al. probably because the angular resolution is better. The absolute value agrees very well with Garron and also with independent results of Gooding and Pugh. The main point about these cross-sections is that they form a very confused picture.

In Figure 5 we have plotted P(q) as calculated from these cross-sections according to the "knock-out" equation. This brings the whole picture into focus and shows that the momentum description of the cross-section indeed works to a considerable extent. There is one exception--an area of systematic discrepancy between events of different sharing. This occurs, for each category of events, just at the point where the solution of the conservation equations for that sharing is about to disappear; given the energy sharing, there is a minimum $|q|$ which can be observed and the P(q) discrepancy occurs at this point. This may be an angular-resolution effect; it turns out that, in order to observe a "consistent" P(q) here, one would have to see a discontinuity in the angular distribution; such a discontinuity would be "washed out" by the finite resolution. Such a resolution effect is very difficult to calculate quantitatively; a Monte Carlo method might work but this has not yet been done. All I can say is that it is our feeling that these discrepancies may well be due to resolution. Incidentally, this figure shows that the description works well even for the deep-lying s proton shell.

Another useful test of the knockout model is that the momentum distribution P(q) defined in Figure 1 should not depend on the incident energy provided one takes out the kinematic factors correctly. Figure 6 compares our P(q) with that calculated from data of Tyren et al. at 460 MeV--a substantially different incident energy. Overall agreement is not bad. Tyren's results are symmetric about q = o as of course they must be if one is truly measuring a momentum distribution--ours are not. The two sets of points agree quite well in the left-hand wing corresponding to tail-on collisions in the primary interaction but in the right-hand wing (head-on collisions) one observes a discrepancy which (going back to the angular distribution) increases with the proton scattering angle. In fact, it almost appears as though our P(q) were obtained by taking Tyren's by the tail and stretching it. We feel (although this has not been substantiated numerically) that this effect could be accounted for using realistic parameters if one took into account the refraction of the proton waves leaving the nucleus--this effect also increases with the proton angle. I believe that this "bending" of the trajectory has been neglected in most distorted-wave calculations.

I'd also like to comment that the normalization of Tyren's data is arbitrary though in principle it shouldn't have to be--absolute cross-sections were given. The necessity for it is somewhat surprising. Because of kinematic effects and the fact that absorption of outgoing protons is less at high energy one would expect the cross-section measured at 460 MeV to be much higher than at 160 MeV; in the event, it appears to be about the same!

(Higher means at least a factor of four, so there appears to be a real anomaly here.)

Figure 7 shows some preliminary results of a similar nature for oxygen; the p3/2 and p1/2 states are easily resolvable in the binding-energy spectra and we have examined the cross-sections and P(q) separately. The dip in the cross-section at about 42° (for carbon) is absent in the oxygen p1/2 results and rather shallow in the p3/2 case, even though the angular resolution was about the same as in the carbon run. Figure 8 shows the momentum description of the p1/2 events which again brings the results into focus rather well. The systematic discrepancies are of the same sort as in the carbon results, again leading one to believe that this might be a sort of experimental effect and have nothing to do with the nucleus as such. Figure 9 shows P(q) for the p3/2 events; if one takes knockout and the shell model quite literally, this should be the same as P(q) for the carbon p3/2 protons, and indeed the shapes of the left-hand maxima agree rather well. At 460 MeV Tyren et al. saw a striking difference between the p1/2 and p3/2 angular distributions which we do not observe--the reason is not understood.

"KNOCKOUT" MODEL, PLANE WAVES :

$$\left.\frac{d^5\sigma}{d\Omega_1\, d\Omega_2\, dT_1}\right|_{E_B} = \frac{4m}{\hbar^2}\,\frac{k_1 k_2}{k_0}\,\sigma(\theta)\,P(q)$$

WHERE

$\hbar \underset{\sim}{k_0}$ = INCIDENT PROTON MOMENTUM

$\hbar \underset{\sim}{k_1}, \hbar \underset{\sim}{k_2}$ = OUTGOING PROTON MOMENTA

$\underset{\sim}{k_0} = \underset{\sim}{k_1} + \underset{\sim}{k_2} + \underset{\sim}{q}$

$\sigma(\theta)$ = EFFECTIVE CROSS SECTION OF PRIMARY INTERACTION

$P(q) \simeq$ PROBABILITY OF TARGET PROTON WITH MOMENTUM $\hbar \underset{\sim}{q}$

(ACTUALLY, $P(q)$ = MOMENTUM PROBABILITY DENSITY = $|\phi(q)|^2$)

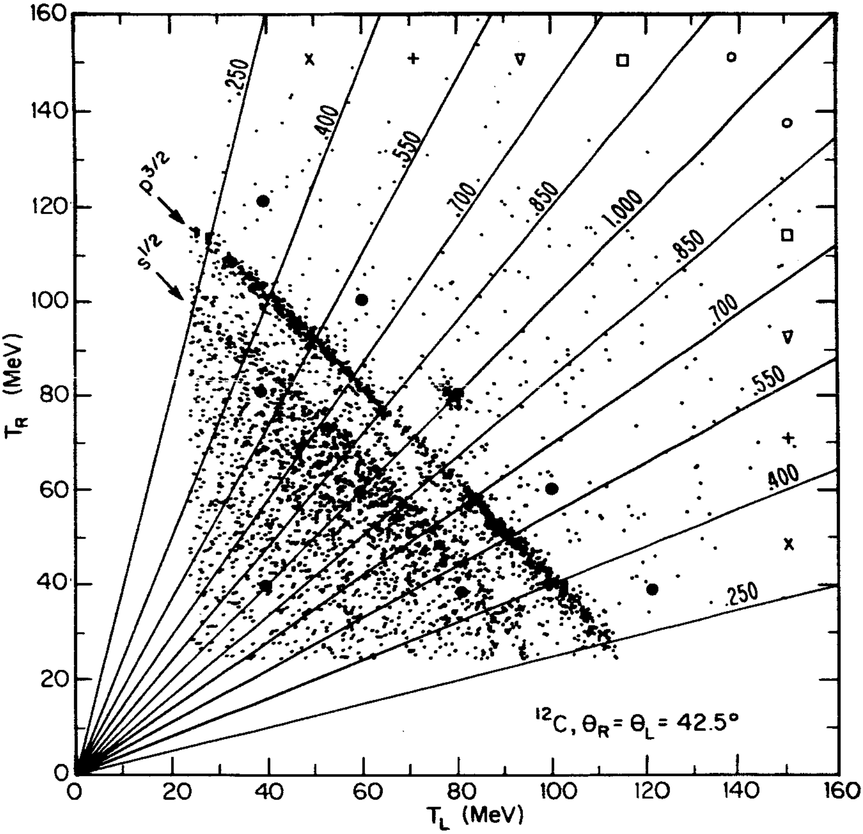


Figure 2. T_R, T_L scatter plot from carbon at 160 MeV incident energy.

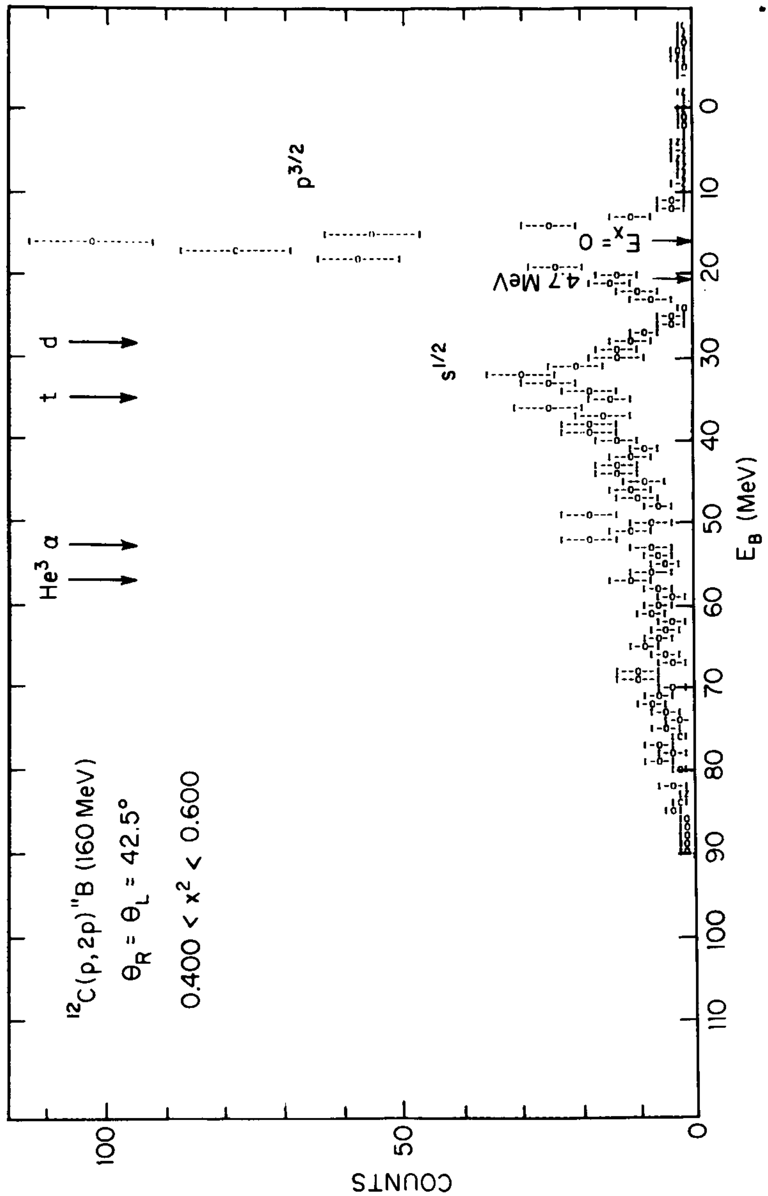


Figure 3. Binding-energy spectrum of carbon.

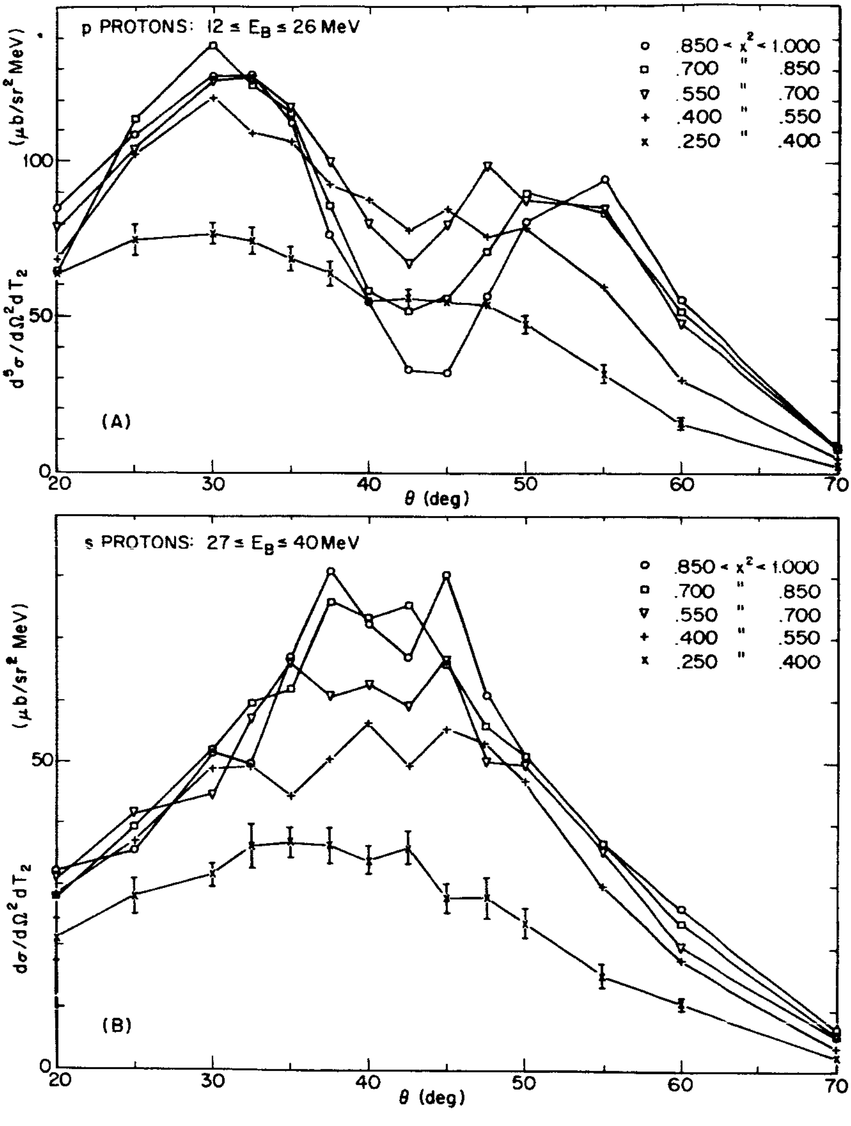


Figure 4. Cross sections of carbon p and s state events vs. scattering angle.

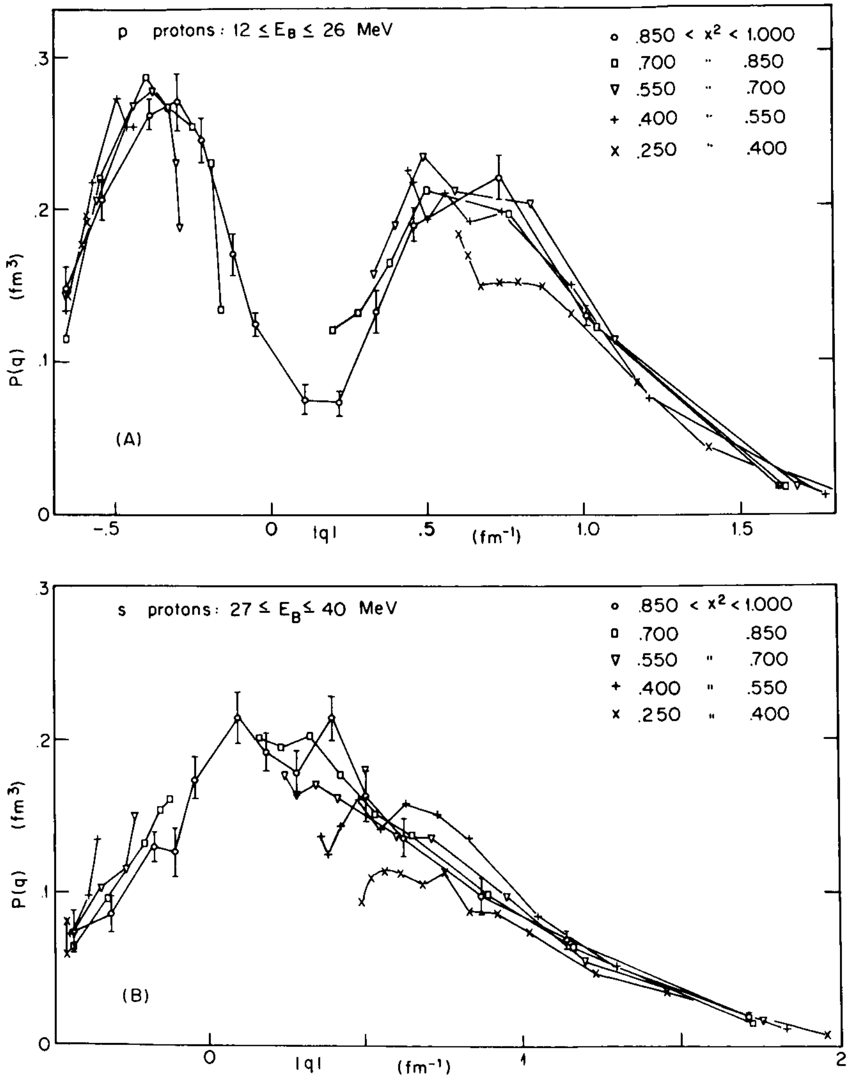


Figure 5. Distorted momentum distributions of carbon p and s state events.

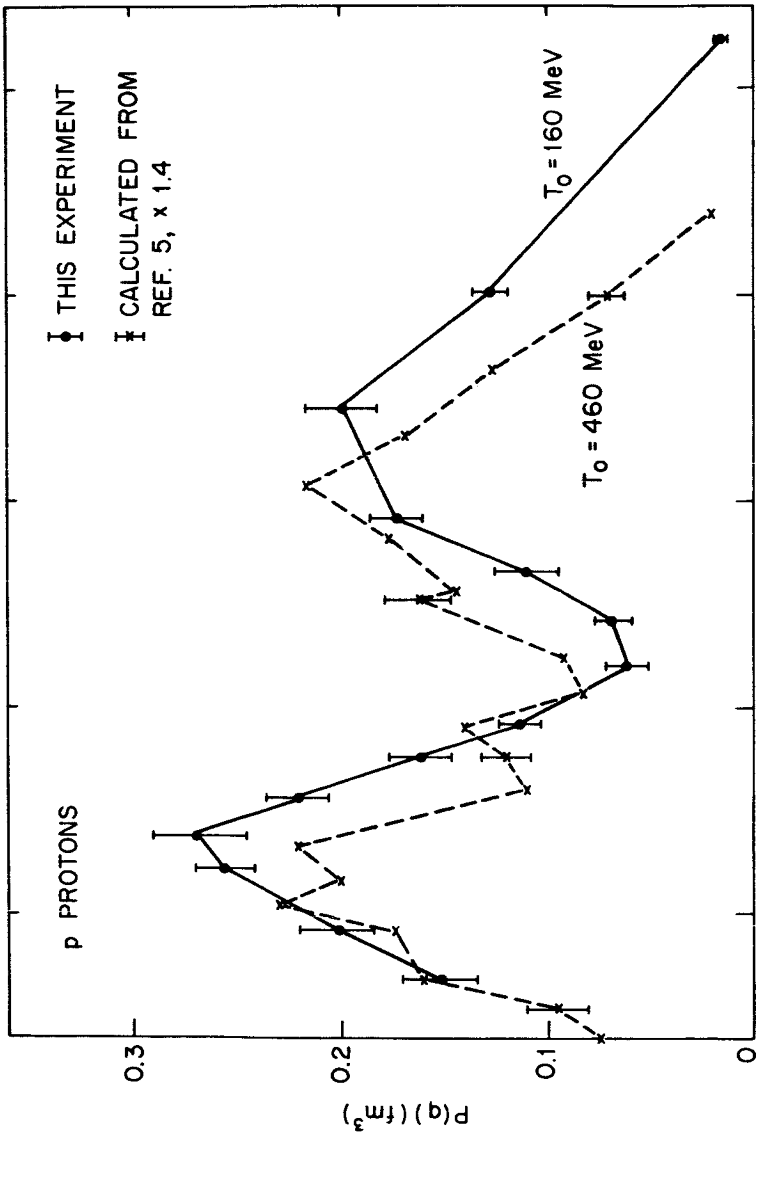


Figure 6. Comparison of the distorted momentum distributions measured at 160 and 460 MeV.

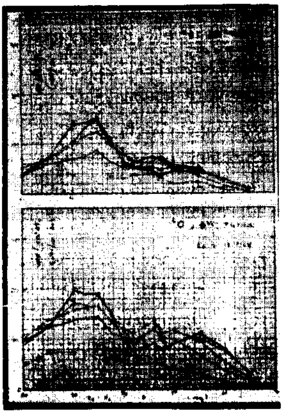


Figure 7. Preliminary results: cross sections for p3/2 and p1/2 state events from ^{16}O.

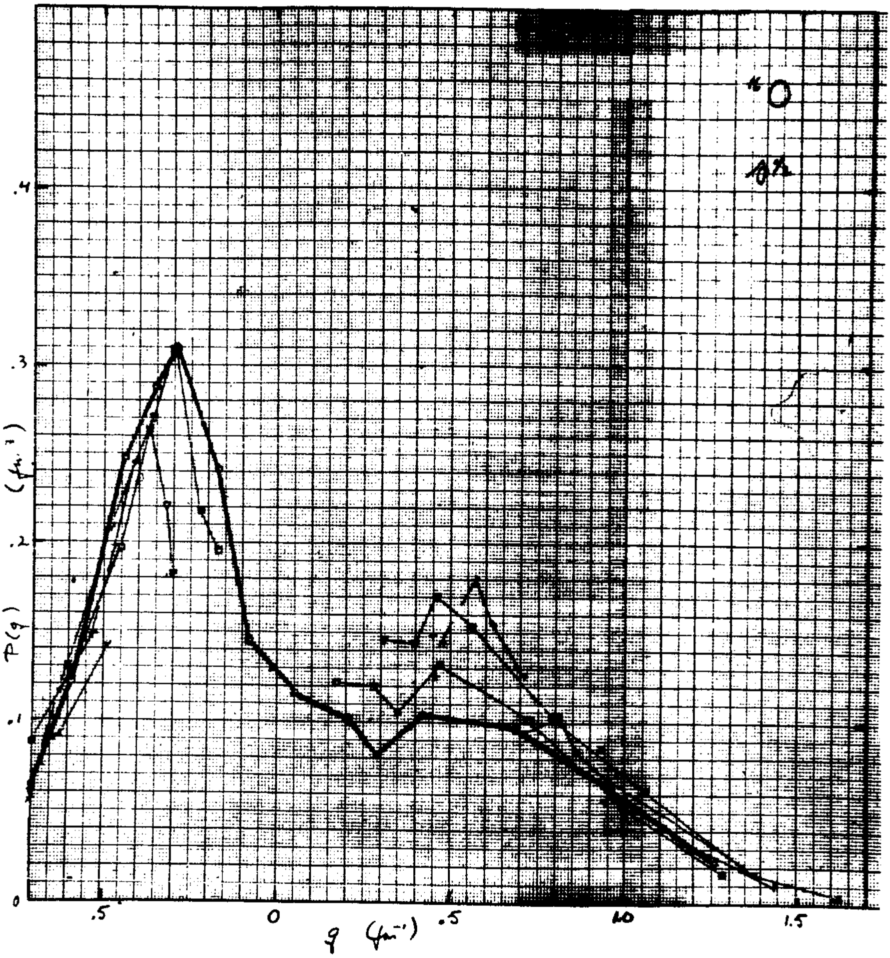


Figure 8. Preliminary results: distorted momentum distribution of p1/2 events from ^{16}O.

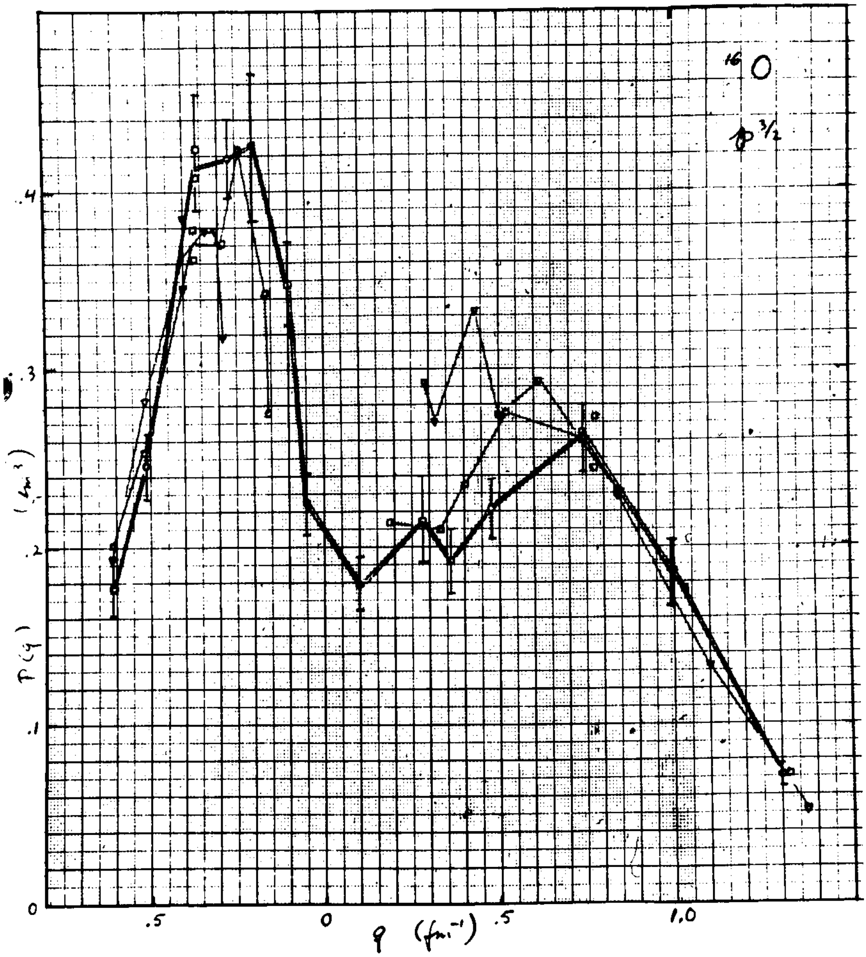


Figure 9. Preliminary results: distorted momentum distribution of p3/2 events from ^{16}O.

ELTON: You mentioned that the diffraction effects might be important but as far as I could tell, your analysis was in terms of plane wave approximation. There is no distortion analysis of any kind.

GOTTSCHALK: You are absolutely right. We took the extremely simple-minded picture and tried to see how far it would carry us in order to see what the distortion effects might be.

ELTON: One effect of distortion, of course is that the formula gets much more complicated and no longer factorizes so you can make such a simple analysis. The fact that your simple analysis gives such beautiful results indicates that distortion effects are not all that important over all, although they are particularly important in filling in the minimum in the p-wave proton scattering One other point is that distortion effects remove the symmetry between the two sides of q positive and negative q . The fact that at 160 Mev there seems to be an asymmetric result while at a 460 Mev the result was symmetric may simply mean that the plane wave approximation was good for 460 and not quite so good at 160 Mev.

WILETS: Relative to the distortion effect, I gather detailed calculations haven't been made in general. Have people considered the final interaction which, in addition to distorting the outgoing wave, could also lead to a subsequent excitation of the nucleus? This would also effect the final energy of the proton. Would this be small?

GOTTSCHALK: May I comment on that. Such reactions are pretty much experimental - for instance, only one excited state of the residual nucleus could possibly have contributed.

WILETS: What I had in mind wouldn't show up so much as a distinct peak as a broadening, - a degrading of the energy leading to an asymmetry of the energy of the peak.

GOTTSCHALK: The events for which I constructed the momentum distribution are events of a well-defined energy. We know that these came from the ground state or the first excited state of the residual nucleus.

WILETS: What resolution did you have?

GOTTSCHALK: About 2 Mev. The next state is easily resolvable.

ELTON: As long as you stick to carbon and oxygen, this is true. If you take other nucleii, the energy levels are closer. Secondly, even if they are resolved, there may, of course, be a coupled channel effect which in other fields have been found to be quite important, so I think Wilets point is very valid.

WALL: Relative to Dr. Wilets point there are the experiments of Pugh, et. al., at Berkeley at a much lower energy. Here the various excited states of B^{11} are well resolved. If one looks at the ground state transition, one sees something which looks like a rather clean knock-out process. However, if you look at the excited state which one can't get a simple knock out process, the angular distribution looks quite different.

A SHELL-MODEL CALCULATION OF THE QUASI ELASTIC SCATTERING OF PROTONS FROM COMPLEX NUCLEI

N. S. Wall
University of Maryland

I'm afraid I'm backtracking historically and somewhat intellectually in that the quasi-free scattering which I would like to talk about involves a much simplier experimental arrangement and possibly some ideas which are a bit more naive than we have just heard from Dr. Gottschalk. The only thing that I will say in its behalf is that the experiments are quite simple.

If one examines the scattering of an intermediate energy proton from a medium weight nucleus, in a single counter experiment, not a (p,2p) experiment, at the incident energy one would see an elastic peak. With adequate energy resolution one could also see a few discrete excited states. As the observed particle energy decreases one then generally sees a large broad peak in the spectrum. The peak location of this broad peak occurs at approximately the incident energy, times the cosine squared of the scattering angle, θ. Neglecting the center of mass effect, the elastic peak stays at the same outgoing energy as do the peaks corresponding to the scattering from discrete states. However, the broad low energy peak does shift. The reason this peak is called a "quasi-elastic" is that we imagine the scattering of the incident protons is by the nucleons in the nucleus. The broad peak, therefore, reflects the total momentum distribution of all of the nucleons in the nucleus. If we had a nucleon as our target, and it was at rest, then the observed energy, nonrelativistically would be given by just the $\cos^2\theta$ factor.

Figure 1 shows spectra obtained in an experiment at 160 MeV with the Harvard Cyclotron by Dr. Roos and myself about two years ago. We plot the differential cross section as a function of the outgoing proton energy. We have not plotted the elastic scattering peak, although there is a remnant of it in the 30^{o} spectrum. Noticeable is the peak shifting to lower energy in angle with an increasing angle. One can also see, at about 5 MeV excitation, some of the effects of inelastic scattering to discrete states. We have, in fact, averaged over this. Results such as these date back, I think, to an experiment in 1952 by Cladis, Moyer, and Hess with an analysis originall due to Wolff. The analysis is a plane wave impulse approximation calculation. The essential points are that the differential cross sections, $d^2 \sigma/d\Omega dE$, is proportional to $d\sigma/d\Omega$ for the nucleon-nucleon scattering, some kinematic factors and an integral over the momentum distribution of the $i^{\underline{th}}$ type of nucleon summed over the individual nucleons. In the early analysis, one just replaced the momentum distribution with some sort of a Gaussian with a characteristic width of something of the order of 15 MeV. The bounds on this integral essentially go from some lower momentum, K_{min} to some very high momentum high compared to what one expects in the nucleus. If a free scatter had occurred to an angle θ, then $E = E_o \cos^2 \theta$. If we observe a proton with energy higher than E then within this impulse approximation it could have occurred because the nucleon had some momentum in the nucleus. The minimum momentum necessary to produce a proton at a given energy and at a given angle is k_{min}.

In our analysis we have taken, essentially, the same description but have derived the momentum distribution for the nucleons in the nucleus from an extreme shell model point of view. I think in the next paper, we will hear about the charge distribution in Ca^{40} as derived from a realistic poten What we have done is to take parameters which were at least some time ago consistent with Dr. Elton's parameters for the shell model potential, derived the single particle states in that potential, Fourier transformed them and put them into the following equation for the cross section:

$$\frac{d^2\sigma}{d\Omega\, dE} = \frac{2m}{\hbar^2} \frac{\sqrt{E}}{\sqrt{E_0}\, |\bar{P}_0 - \bar{P}|} \sum_i N_i \int_{K_{MIN}}^{\infty} |\psi_i(k)|^2 \frac{d\sigma_i(\theta)}{d\Omega}_{N-N} k\, dk$$

In other words we really take the shell mode at face value. We know from the (e,ep) experiments of Amaldi, et al, that at least the 1s binding energy in a nucleus like Calcium is more tightly bound than the bottom of a shell model potential which fits the (p,2p) high lying states. We assumed in the calculation, that the momentum distribution for the 1s state is not too different from that given by the local non-energy dependent potential. In the evaluation of K_{min} we have put in an estimate of 75 MeV for the binding energy.

Figure 2 shows the energy at the peak as a function of the scattering angle.

Figure 3 shows the detailed spectrum at 20°. I think you should disregard the last two experimental points.

Figure 4 shows the same calculation now at 30°. Again the peak location, which corresponds to low internal momentum, is given quite well.

Figure 5 shows the 50° situation. At energies corresponding to the order of 20 MeV residual energy one finds a cross section which is two to three times greater than the predicted cross section, even though the predicted cross section is a factor of three too high.

We have not taken absorption into account. It should distort the spectrum. With respect to the excess of protons at high energies let me point out that 140 MeV, the minimum average momentum necessary to scatter a proton through 50°, corresponds to 1.4 F^{-1}. In a very clear paper Gottfried pointed out that when one gets to this large a momentum transfer the short range behavior of the nucleon-nucleon interaction should begin to be quite important causing the impulse approximation to go bad, in part because of the short range correlation in the nucleus. This has not been taken into account in our extreme single particle calculation.

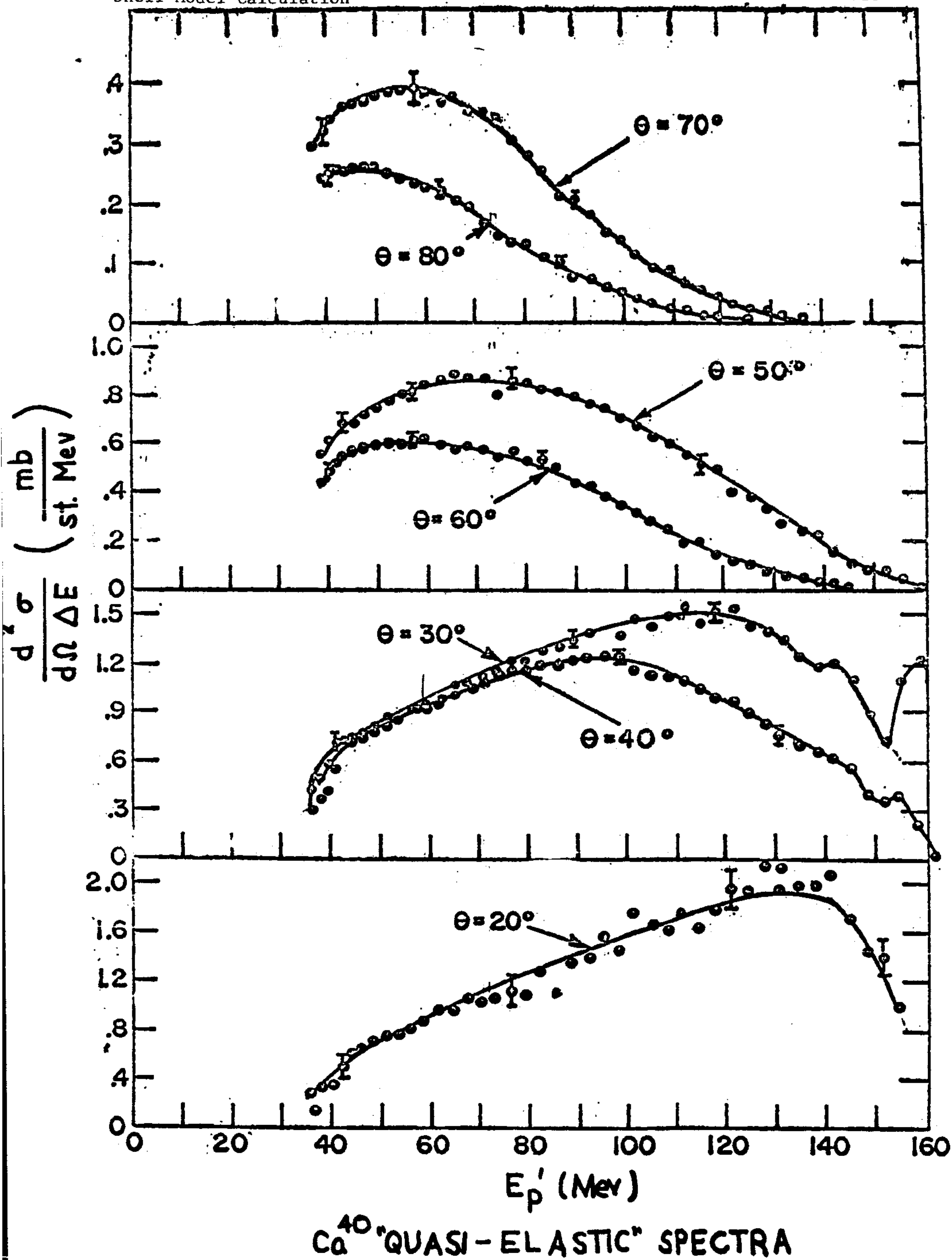
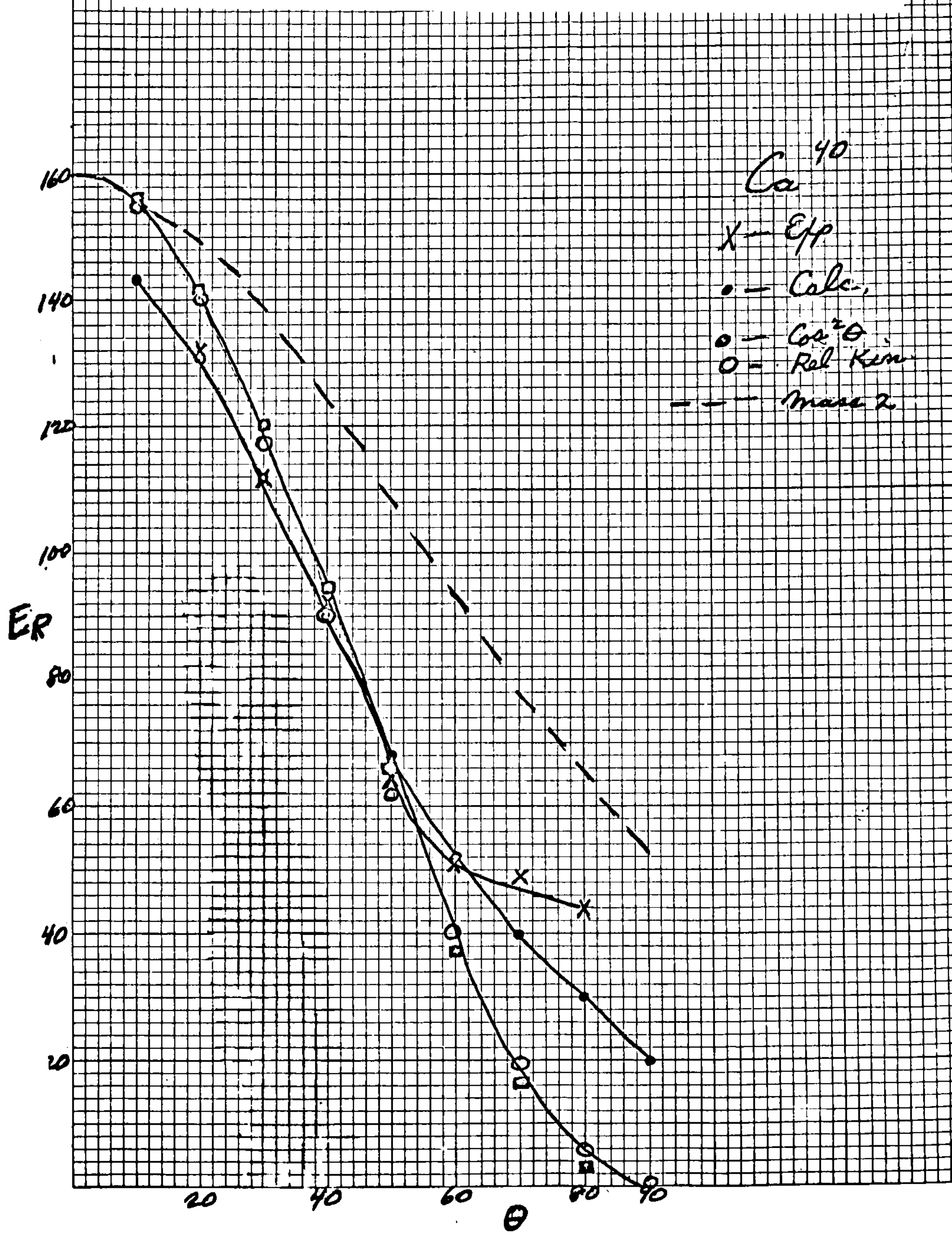

θ = 70°
θ = 80°
θ = 50°
θ = 60°
θ = 30°
θ = 40°
θ = 20°
d²σ / dΩ ΔE (mb / st. Mev)
E_p' (Mev)
Ca40 "QUASI-ELASTIC" SPECTRA

Figure 1.

Figure 2 - Quasi-elastic scattering peak location as a function of angle.



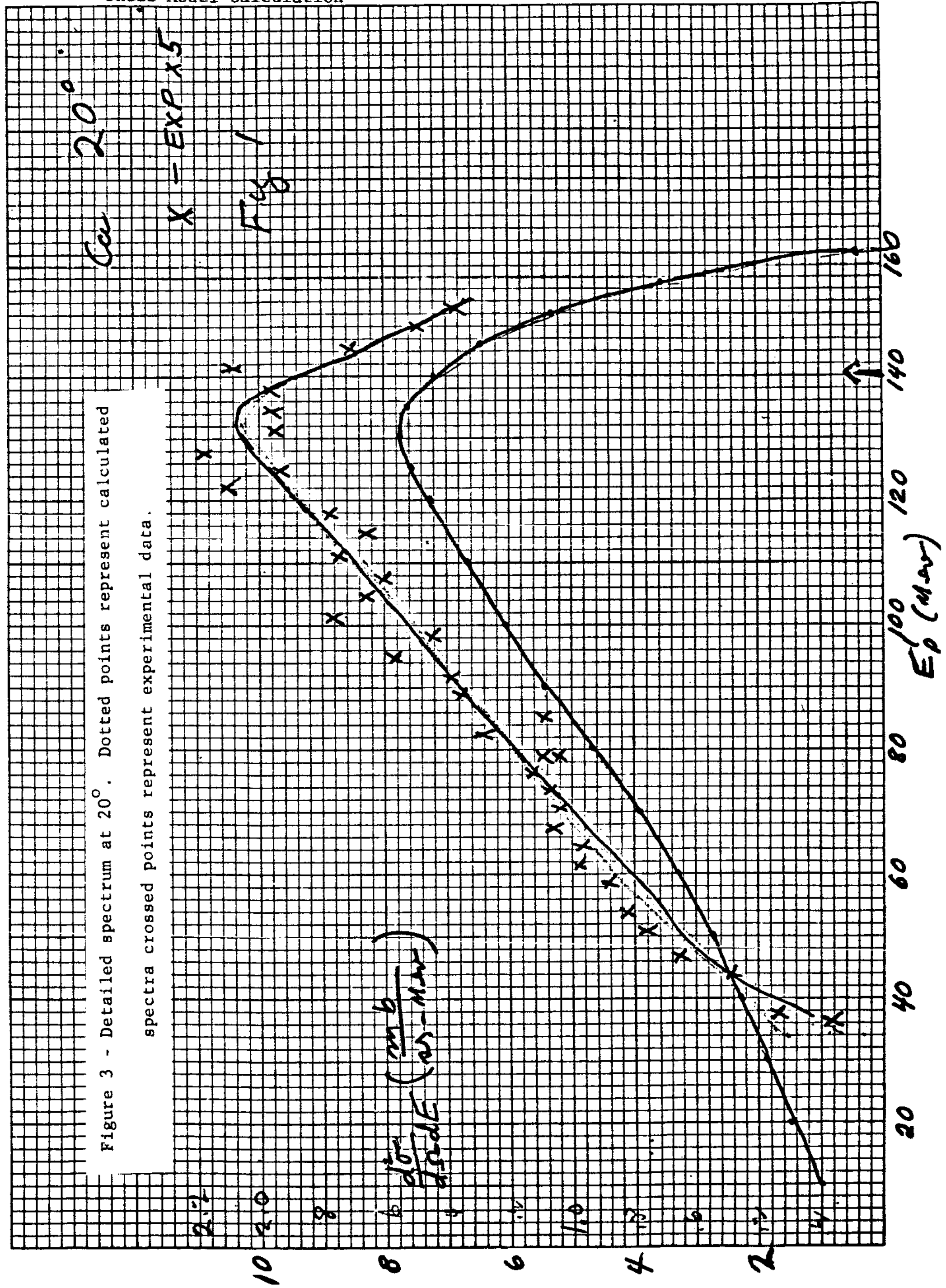


Figure 3 - Detailed spectrum at 20°. Dotted points represent calculated spectra crossed points represent experimental data.

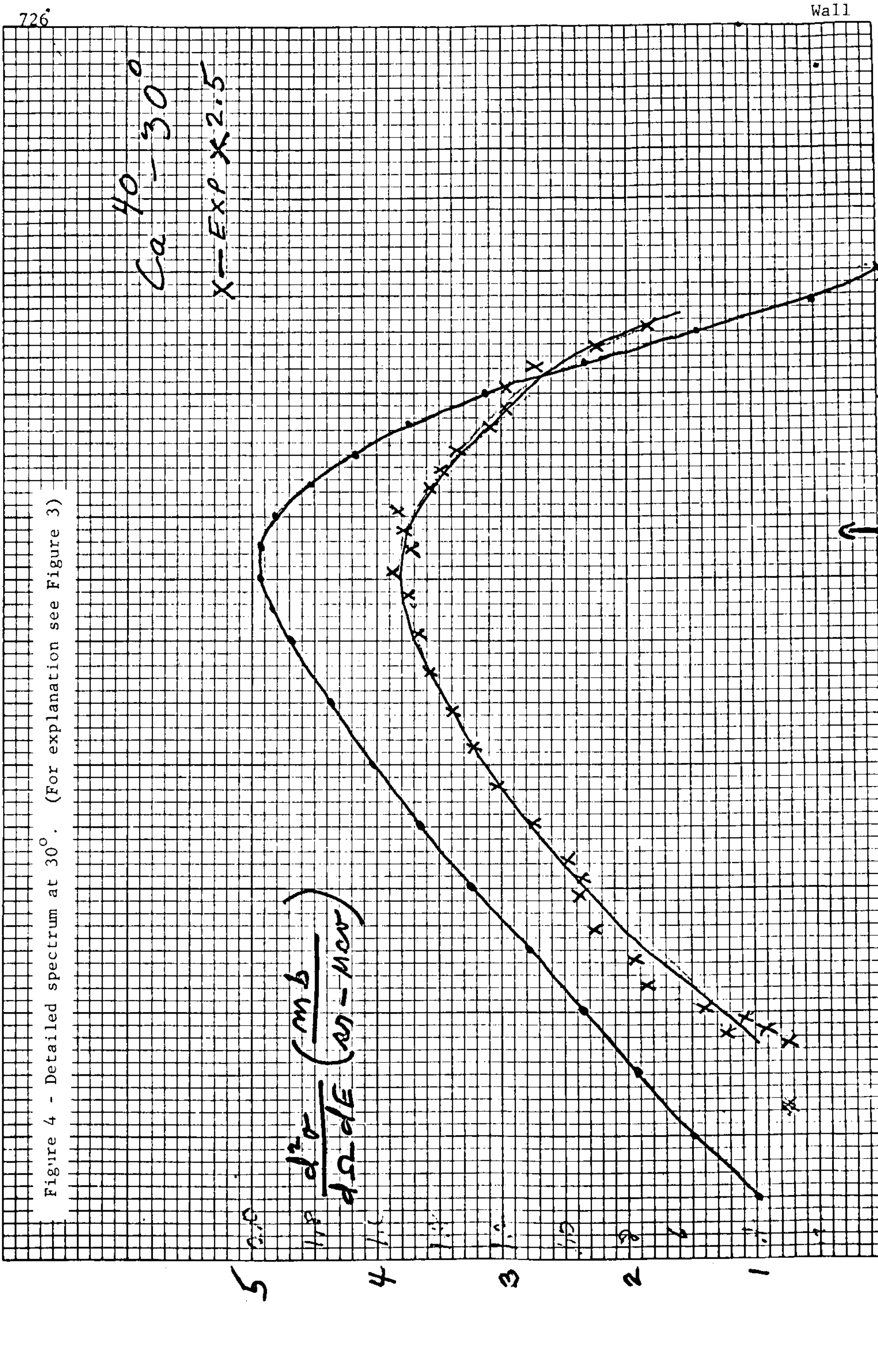


Figure 4 - Detailed spectrum at 30°. (For explanation see Figure 3)

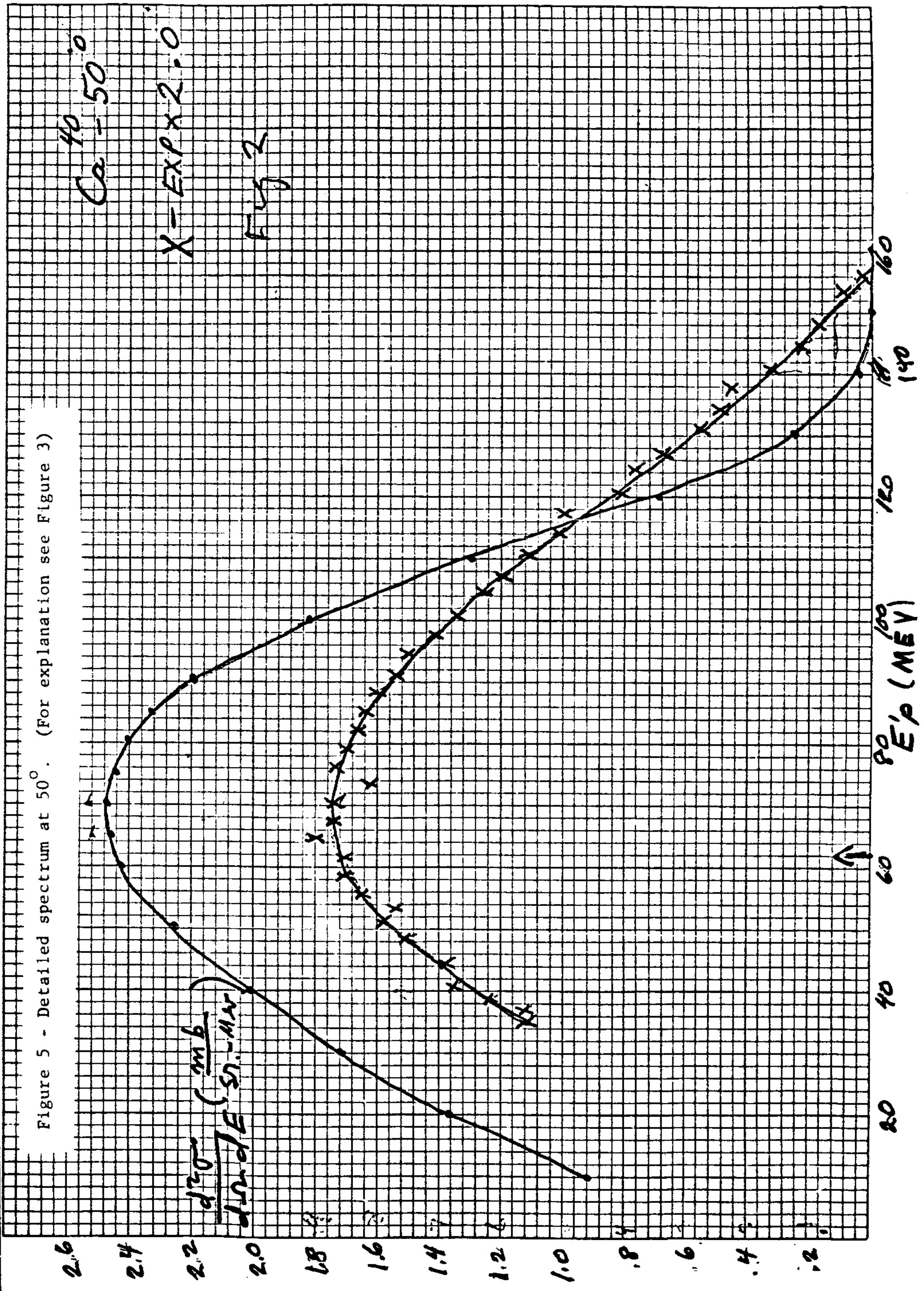


Figure 5 - Detailed spectrum at 50°. (For explanation see Figure 3)

QUESTION: Could the extra counts be inelastic scattering?

WALL: We know that there are strong states observed in Ca^{40} inelastic scattering at, for example, the well-known 3^- at 3.73 Mev, the 5^- at 4.48 Mev. On one of the slides I showed there was some indication of these states. The cross section for these states is about a factor of 3 less than what we observed in the 20 Mev excitation region. The point here is the inelastic scattering at a large angle seems to be extremely weak, in fact in these experiments we only have an upper limit for it. Furthermore, the 3.73 and 4.48 Mev states are known from inelastic and scattering experiments to use up a very large fraction of the transition strength - something of the order of 2/3 for the octopole transition strength. Therefore we believe that what we see here is not just a result of averaging over a large number of discrete inelastic states. The only point I'd like to make there is the states you are speaking of, where you know the cross sections, are essentially direct interaction states. What I was speaking of was nuclear evaporation spectra. The evaporation part of the spectrum would be expected at a much, much lower energy, but some of it would be up high. I suspect to get anything significant that it would require abnormally high nuclear temperatures - at the nuclear reactions it's a mixture. There are some evaporation type experiments of Fox and Ramsey going back to about 1958 or so.

GOTTSCHALK: I want to make a point that is almost frivolous in its simplicity. One takes your experimentally measured reduction factor of 3 or 4 and squares it getting a result not inconsistent with predicted and measured reduction factors in (p, 2p) experiments.

WALL: This has been observed.

WILETS: When you get to the measurement of the high momentum components you mentioned which come from the strong nucleon-nucleon interaction, does not this correspond to the short range correlations? Isn't this also then a region where you would expect the two body correlation structure to enter so that you are essentially scattering from two nucleons rather than one?

WALL: Did you notice on the kinematic curve, the kinematics for a mass 2? You could have done the same sort of calculation that we've done but pretend that there are mass 2 particles bound in the nucleus. If I take something of the order of 10% of the 40 nucleons in the nucleus and put them into mass 2,I could construct a curve which would have just the required shape.

FALLIEROS: You happen to know what would be the effect of improving the treatment of the 1s state? That is, if you choose a different well, would you reproduce the right binding for the shell?

WALL: I have not been able to do this for that particular level as yet. We've done a similar calculation for Be by changing the parameters of the $1p^{3/2}$ single particle state. By changing radius of the well by about 10% one finds relatively small correction to the predicted spectrum.

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CHANGES IN RADII BETWEEN NEIGHBOURING NUCLIDES

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The properties of neighbouring nuclides, and in particular of different sotopes of a given element can be used to investigate (a) bulk properties of uclear matter and (b) specific shell effects. It is important to keep these nvestigations separate, for nuclides which are suitable for one of these in-estigations are obviously unsuitable for the other.

Some years ago it was shown[1,2] that, under very simple assumptions radii of nuclei along the line of maximum stability followed the law

(1) $$R = (5/3)^{\frac{1}{2}} \langle r^2 \rangle^{\frac{1}{2}} = 1.123\, A^{1/3} + 2.352\, A^{-1/3} - 2.070\, A^{-1},$$

where the constants are fitted to electron scattering data, and this law has recently been confirmed through evidence from μ-mesic atoms[3], as is shown in Figure 1. This law should not of course be applied to the detailed variations between neighbouring nuclei, but approximate compliance with it of a group of neighbouring nuclides is a good indication that these nuclides will give information of type (a), while gross departures from it may indicate shell structure effects. A good example of this concerns the isotopes Ca^{40} and Ca^{44}, for which the increase in root mean square radius was found to be only about 0.8 percent [4,5], instead of over 3 percent, as predicted by (1). This result, as well as a good fit to the electron scattering data[6] can be obtained from proton distributions, based on single-particle wave functions in a Saxon-Woods well,[7] when account is taken of the larger binding of the last proton in Ca^{44} compared with that in Ca^{40}. The well parameters are given in Table 1, the fit in Figure 3. It is seen that the critical surface region is almost the same for the two nuclei, although, because of the greater central density of Ca^{40}, the conventionally defined surface thickness parameter is smaller in Ca^{44}, as was also found by fitting a Fermi distribution to the data.[6)]

We now turn to nuclides for which shell effects are unimportant. We define the following quantities:

(2) $$\gamma_A = \frac{3A}{R}\frac{dR}{dA}\,,\quad \gamma_N = \frac{3A}{R}\frac{\partial R}{\partial N}\,,\quad \gamma_Z = \frac{3A}{R}\frac{\partial R}{\partial Z}\,,$$

where γ_A is defined only along the line of maximum nuclear stability. Then considerations of nuclear stability[8)] lead to the expression

(3) $$\gamma_Z - \gamma_N = \frac{3A}{2Z}\;\frac{4E_C}{KA + E_C}$$

where K is the coefficient of nuclear compressibility and[2)]

(4) $$E_C = 0.715\ Z^2\ A^{-1/3}\text{MeV}$$

is the nuclear Coulomb energy. For infinite nuclear matter, the compressibility coefficient is then given by[9)]

(5) $$K_\infty = K + K_S\ A^{-1/3}$$

where the surface coefficient $K_S \simeq 200$ MeV. For heavy nuclides, isotope shift measurements[10)] together with the use of expression (1) yield[11)]

(6) $$\gamma_N = 0.65\pm0.10,\ \gamma_Z = 1.36\pm0.21,\ K = 81^{+61}_{-25}\ \text{MeV},\ K_\infty \simeq 120\ \text{MeV},$$

while, for A = 58, it has been possible[11)] to determine γ_N and γ_Z directly from elastic electron scattering by Fe^{56}, Ni^{58} and Ni^{60},

(7) $$\gamma_N = 0.71\pm0.16,\ \gamma_Z = 1.20\pm0.25,\ K = 59^{+240}_{-\ 27}\ \text{MeV},\ K_\infty \simeq 110\ \text{MeV}.$$

Because of the dependence of K on $(\gamma_Z - \gamma_N)^{-1}$, quite small errors in γ_N and γ_Z can lead to very large uncertainties in K.

Measurement of the energies of x-rays due to the $2p_{3/2}$-$1s_{1/2}$ transition in μ-mesic atoms have yielded values of R both for different isotopes of the same element and -or elements (natural isotopic mixtures only so far) with neighbouring Z. The latter give γ_A directly, while the former give γ_N. The differences between the measured energies are generally much better known than the energies themselves, and this reduces the uncertainties in γ_A and γ_Z. Thus, from a measurement[12] of Mo^{96} and Mo^{98} and that[13] of the natural isotopic mixtures of Mo and Rh we find

$$\gamma_A = 1.25\pm0.40,\ \gamma_N = 0.82\pm0.09,\ \gamma_Z = 1.80\pm0.60, \quad K = 35^{+100}_{-15}\ \text{MeV},\ K_\infty \simeq 80\ \text{MeV}. \tag{8}$$

To evaluate the error bracket on γ_A, we estimated the part of the energy difference which was due to the size effect only, which came to 33 keV, and assumed that the uncertainty in this was the same as that quoted for the total experimental energy difference, 271.0 ± 10 keV. The rest of the energy difference is of course due to the extra proton in rhodium.

The above results show that, within the large error brackets, the experiments are entirely consistent with the simple theory, but yield values of K_∞ that appear to be somewhat lower than the value $K_\infty \simeq 170$ MeV, obtained from more fundamental considerations.[14] More accurate measurements on μ-mesic x-rays from separated isotopes would settle this point.

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Table 1.

Well Parameters and Energy Levels of $Ca^{40,44}$

The parameters r_o, V_o a and λ refer to a Saxon-Woods well with spin-orbit coupling, and E refers to the single particle energies in this well. (Distances in fm, energies in MeV.)

Ca^{40}

$r_o = 1\cdot30$, $a = 0\cdot60$, $\langle r^2\rangle^{\frac{1}{2}} = 3\cdot39$

Level		V_o	λ	E	E_{expt}	Reaction
protons	$1s_{1/2}$	85	-	62·9	-	
	$1p_{3/2}$	60	90	32·1	-	
	$1p_{1/2}$			24·5	24·5	(p,2p)
	$1d_{5/2}$			15·2	15·1	(p,2p)
	$2s_{1/2}$	53	40	10·1	10·9	(d,He^3), (p,2p)
	$1d_{3/2}$			8·5	8·3	(γ,p), (p,2p)
neutrons	$1d_{5/2}$			22·6	21·9	(p,d)
	$2s_{1/2}$	53	40	17·6	18·2	(p,d)
	$1d_{3/2}$			16·0	15·6	(γ,n), (p,d)

Ca^{44}

$r_o = 1\cdot30$, $a = 0\cdot60$, $\langle r^2\rangle^{\frac{1}{2}} = 3\cdot41$

Level		V_o	λ	E	E_{expt}	Reaction
protons	$1s_{1/2}$	85	-	64·1	-	
	$1p_{3/2}$	60	90	33·3	-	
	$1p_{1/2}$			26·4	-	
	$1d_{5/2}$			18·5	-	
	$2s_{1/2}$	55	40	13·2	-	
	$1d_{3/2}$			12·1	12·2	(γ,p)

TABLE II

Saxon - Woods Well Parameters and Energy Levels
(Preliminary Results)

Nuclide	Level	V_o	r_o	λ	a	E	E_{expt}	Reaction	(r
Li^6	$1s_{1/2}$	56·0	1·42	-	0·65	22·7	22·7	(p,2p)	2·
	$1p_{3/2}$	49·5	1·48	30	0·65	4·9	4·9	(p,2p)	
Li^7	$1s_{1/2}$	58·0	1·38	-	0·65	25·5	25·5	(p,2p)	2·
	$1p_{3/2}$	58·0	1·38	40	0.65	10·1	9·9	(t,α)	
C^{12}	$1s_{1/2}$	59·5	1·36	-	0·55	33·9	34·2	(p,2p)	2·
	$1p_{3/2}$	55·5	1·36	30	0·55	16·2	16·0	(t,α),(p,2p)	
O^{16}	$1s_{1/2}$	68·0	1·41	-	0·65	43·8	44·0	(p,2p)	2·
	$1p_{3/2}$ }	51·5	1.41	45	0·65	18·4	18·6	(t,α),(p,2p)	
	$1p_{1/2}$ }					12·0	12·1	(t,α),(p,2p)	
Si^{28}	$1s_{1/2}$	81·0	1·39	-	0·65	59·1	~60(Al^{27})	(e,ep)	3·
	$1p_{3/2}$ }	65·0	1·39	70	0·65	35·4	36 ?	(p,2p)	
	$1p_{1/2}$ }					27·7	28 ?	(p,2p)	
	$1d_{5/2}$ }	59·0	1·39	25	0·65	17·5	18	(p,2p)	
	$2s_{1/2}$ }					13·3	14	(p,2p)	
P^{31}	$1s_{1/2}$	84·0	1·33	-	0·65	61·3	-		3
	$1p_{3/2}$ }	66·0	1·33	60	0·65	35·3	-		
	$1p_{1/2}$ }					28·5	28 ?	(p,2p)	
	$1d_{5/2}$ }	51·0	1·33	60	0·65	13·5	13·9	(p,2p)	
	$2s_{1/2}$ }					7·4	7·3	(p,2p)	
S^{32}	$1s_{1/2}$	80·0	1·38	-	0·55	60·0	~70	(e,ep)	3
	$1p_{3/2}$ }	60·0	1·38	75	0·55	33·1	33·2	(p,2p)	
	$1p_{1/2}$ }					26·6	26·6	(p,2p)	
	$1d_{5/2}$ }	49·5	1·38	75	0·55	16·0	16·1	(p,2p)	
	$2s_{1/2}$ }					8·3	8·4	(d,He^3)	
Ca^{40}	$1s_{1/2}$	85·0	1·30	-	0·60	62·9	-		3
	$1p_{3/2}$ }	60·0	1·30	90	0·60	32·1	-		
	$1p_{1/2}$ }					24·5	24·5	(p,2p)	
	$1d_{5/2}$ }	53·0	1·30	40	0·60	15·2	15·1	(p,2p)	
	$2s_{1/2}$ }					10·1	10·9	(d,He^3),(p,2p)	
	$1d_{3/2}$ }					8·5	8·3	(γ,p),(p,2p)	

Figure 1. Experimental values of R $A^{-1/3}$, as obtained from μ-mesic atoms, compared with equation (1) which has been fitted to the electron scattering results.

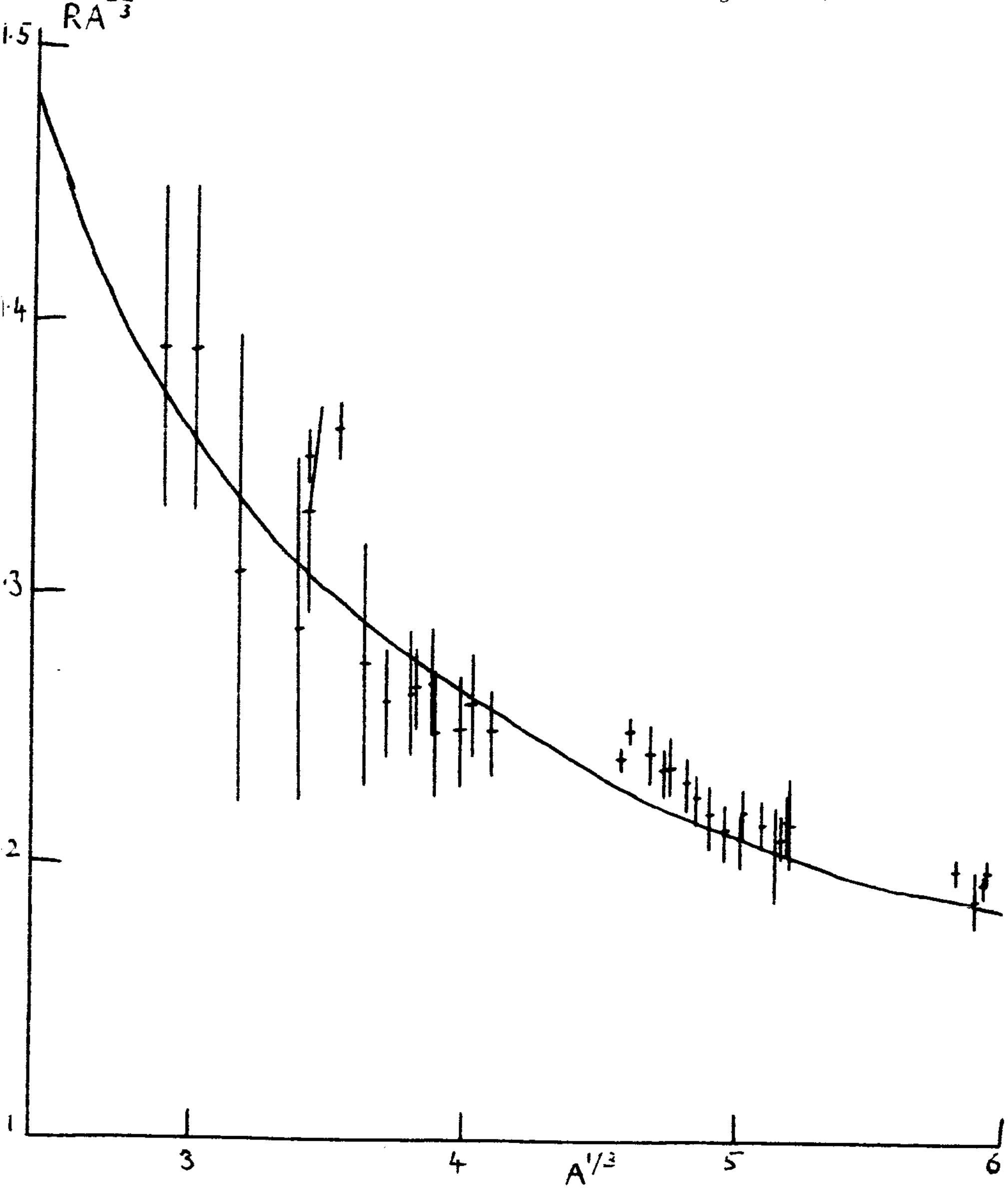
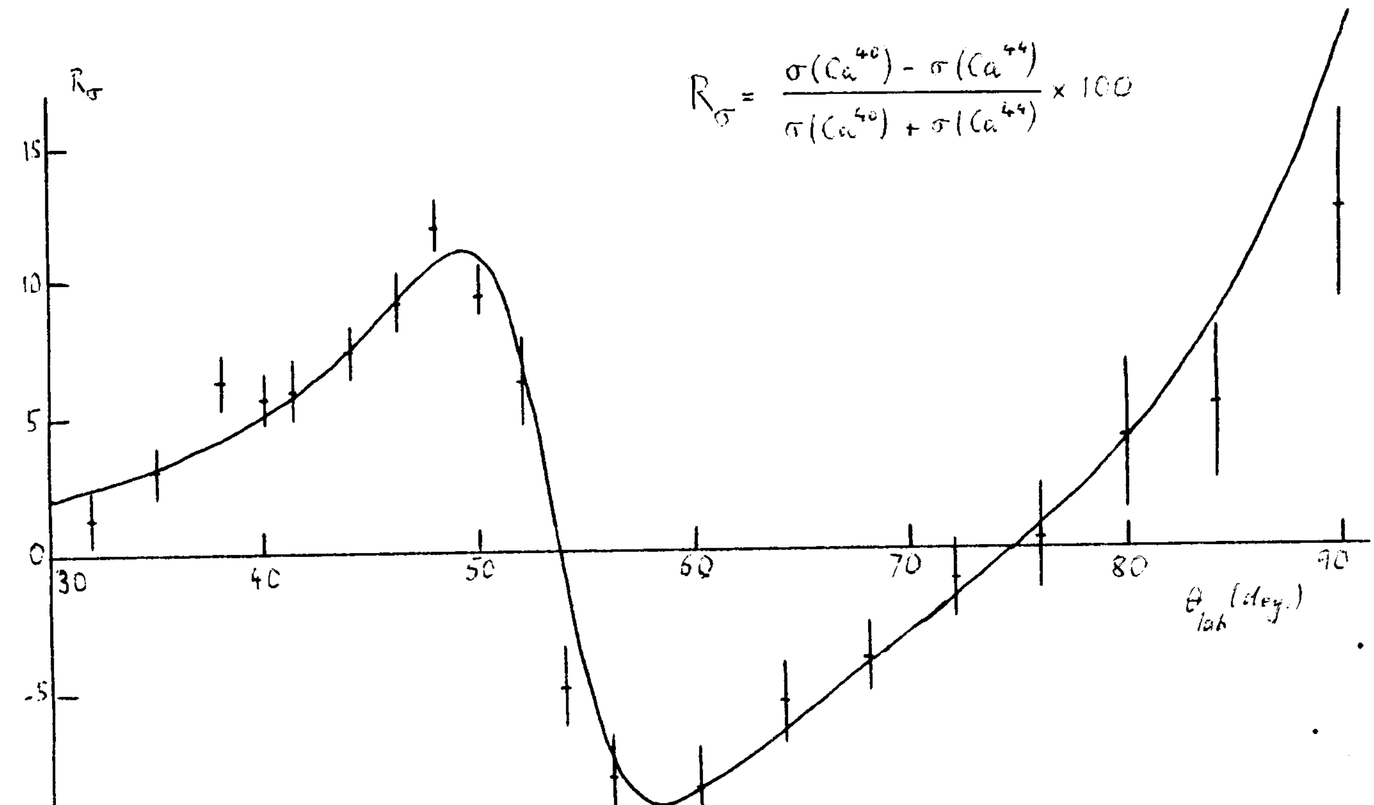


Figure 2 - Isotope difference of electron scattering from Ca^{40} and Ca^{44} at 250 MeV. The curve is based on the parameters of table 1.



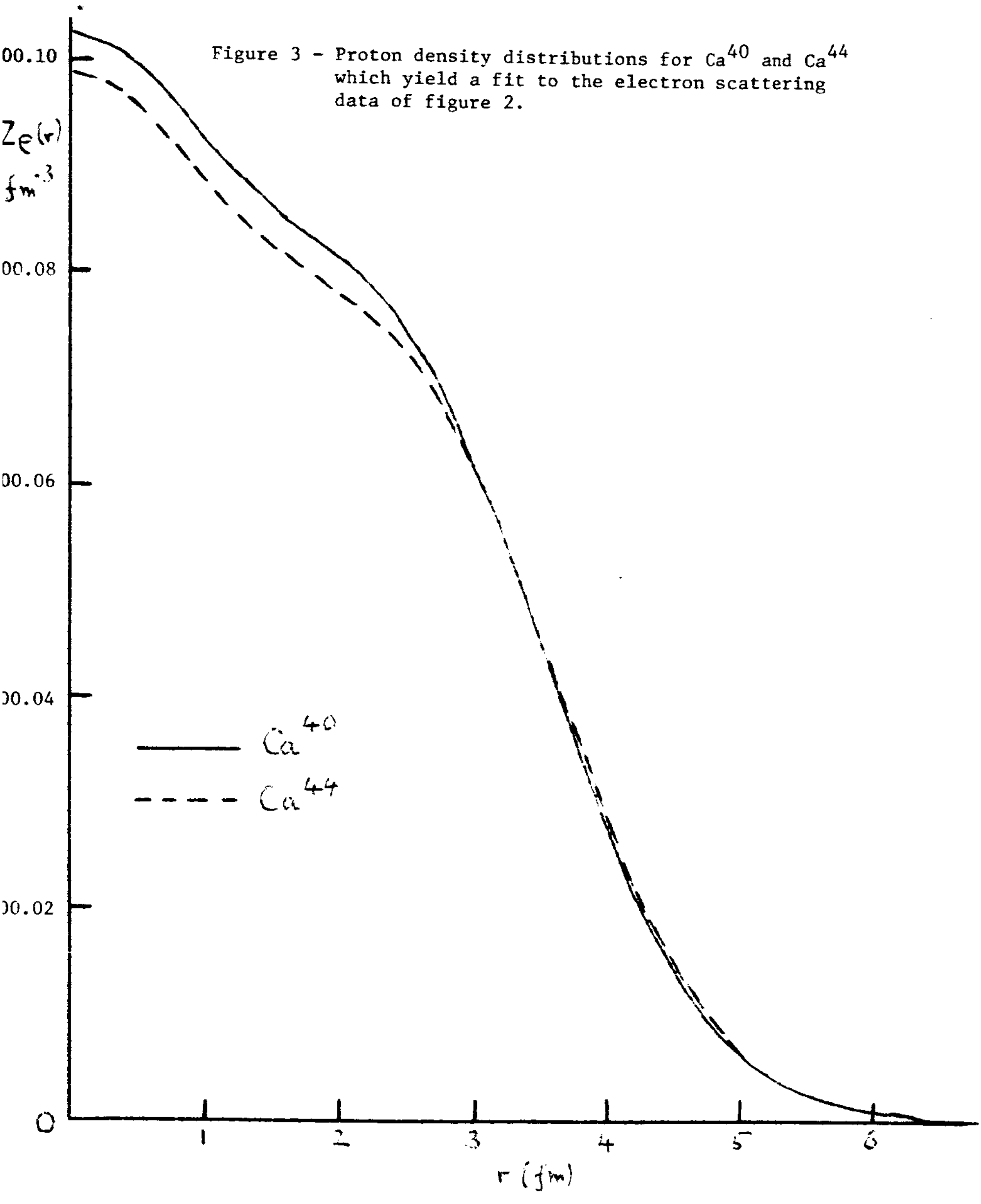


Figure 3 - Proton density distributions for Ca^{40} and Ca^{44} which yield a fit to the electron scattering data of figure 2.

WILETS: The numbers that you obtained for compressability for infinite matter seem to be on the low side in general from what many-body calculations would give. This may be right, but I would just like to point out that there is another effect which can contribute to the reduction of γ_N, the increase of nuclear radius with adding neutrons. In the compressability model one assumes that the neutrons and protons stick together - that as one adds neutrons or protons one has a uniform increase. There are calculations which qualitatively show that as you add neutrons - in fact, Ca^{40} was one case in point - that the neutrons tend to stick outside of the protons. This is more than just a shell effect. It should be a systematic effect. One can use an old argument of Johnson and Teller about 10 years ago showing that neutrons should lie outside of protons. Well, this argument doesn't stand by itself anymore. We know that neutron and proton distributions are very similar, but the argument was based upon the fact that the neutrons with higher kinetic energy in the nucleus climb up higher in the shell model potential well. Now if you increase neutrons from a distribution where the neutron-proton distributions are similar, the neutrons will tend to climb up the well faster. So this is a finite surface thickness effect. I think your compressability estimates would go up if one had a contribution like this.

RAVENHALL: I would just like to mention first that there is work of a kind that Dr. Elton described on the Ca^{40}-Ca^{44} isotopes by Perey and Schiffer. It was done to obtain charge distributions from putting protons into a potential well. I and some students have also done work which I presume involved similar parameter variations also in the Ca^{40}, Ca^{44}, and Ca^{48} isotopes but Dr. Elton has only just received that data.

ELTON: To do this fit it is absolutely essential to use the separation energy data as well, otherwise a unique fit most certainly will not be obtained.

WALL; If one accepts your charge distribution for Ca^{40} and Ca^{44} there is an α scattering experiment designed to look at the difference in the nuclear radius that was reported in the Paris Conference which indicates that Ca^{44} is significantly larger, though not by an $A^{1/3}$ increase, than Ca^{40}. This might suggest our old friend the neutron skin because we should be examining in the α scattering just the tail of the nuclear matter distribution.

ELTON: I think Ca^{44} almost certainly will have a neutron skin. Of course, what we are measuring here is the charge distribution. If we switch off the coulomb potential and work out the wave function for the neutrons, we get a neutron skin.

KOLTUN: I just wondered whether the magnetic parts of the electron scattering are sensitive enough to tell you something about neutrons skins as opposed to charge distributions?

ELTON: I should be very surprised.

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COHERENT NEUTRON PROTON-HOLE EXCITATIONS IN NUCLEI*

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This discussion is closely connected with the paper presented yesterday by Drs. Walecka and Uberall. In the first figure, we illustrate the familiar situation of the light nucleus O^{16}. The shaded region represents the occupied shells in this nucleus, and we know that the muon capture results in the creation of what we will call a neutron-proton hole pair, (to be referred as n-$\bar{p}$ pair); which brings us over to the nucleus N^{16}. The various possible configurations that can be formed this way interact with each other. The appearance of a coherent n-$\bar{p}$ state with angular momentum $J=1^-$ is a result of this interaction. This state will be excited strongly in the μ^--capture process[1)], and, as is well known, it is the isospin counterpart of the giant dipole resonance of O^{16}. The relative shift between these 2 levels represents the Coulomb energy difference between the 2 neighbouring nuclei.

The purpose of this work is to examine the possible presence of such excitations in heavier nuclei. We summarize our results as follows:

A - The existence of the coherent n-$\bar{p}$ excitation is expected also in this case, while the existence of the giant dipole resonance is familiar.

B - Both states can still be described by definite values of the isotopic spin quantum number. However, they are no longer members of the same isospin multiplet.

We illustrate the situation in the case of Zr^{90} in Figure 2. The shaded region is a simplified representation of the ground state of Zr^{90}; 2 typical n-$\bar{p}$ configurations are shown; the interaction between the various configurations is illustrated diagrammatically and the relevant nuclear force component responsible for this interaction is also shown.

Detailed calculations of the odd-parity states in Y^{90} were performed[2)] using this picture. From the large number of levels found this way, we select, for the purpose of this talk, a coherent ($J=1^-$) level which is shown in the third figure. The energy of this state was found to be approximately 8 Mev, i.e., 1 Mev above the threshold for neutron emission. The radiative width of E1 transition from this state to the low-lying 2^+ state has also been calculated and found to be of the order of the corresponding Weisskoff estimate.

It is worth noting here that the isotopic spin of the ground state of the Zr^{90} is $T = \frac{N-Z}{2} = 5$ while the Y^{90} state under consideration has isotopic spin $T = 6$. It follows that the analogs of the various states of Y^{90} are expected to appear in Zr^{90} at an excitation energy determined by the characteristic Coulomb energy difference which in this case is $\sim$11 Mev. The analog of the coherent n-$\bar{p}$ state is then predicted to lie at $\sim$21 Mev in agreement with previous estimates[3)]. This energy should be compared with the energy of the giant dipole resonance which is known to lie at about 16 Mev(Fig.3). The giant resonance is not the isospin counterpart of any state in Y^{90} and is thus characterized by an isotopic spin $T = 5$. We find that about 20% of the electric dipole sum rule is associated with the 21 Mev state, while the normal giant dipole state absorbs most of the dipole strength. This is an illustration of the splitting of the dipole strength in a specific nucleus with $T \neq 0$.

The fragmentation of the dipole strength into 2 components of different isotopic spin is expected to occur in all nuclei. A qualitative idea of the distribution of this dipole strength can be obtained from the following graph. (Fig. 4)

What is plotted is the relative value of the reduced E1 transition rate in arbitrary units normalized to 1 as a function of the number of excess neutrons in a nucleus. For an N = Z nucleus, all the strength is concentrated in the familiar T = 1 component; as N-Z increases, the relative strength of the component with isotopic spin equal to that of the ground state gradually increases and tends to unity when N becomes much larger than Z.

We should emphasize again that it is the T+1 component which is the analog of the 1^- state excited in μ capture. Thus, the form factors of these 2 states will be essentially the same where as the form factors of the state T which is the normal dipole resonance can be quite different.

* Supported by the Atomic Energy Commission and the U. S. Office of Naval Research under Contract Nonr-3777(00).

† Presently supported in part by the National Research Council of Canada.

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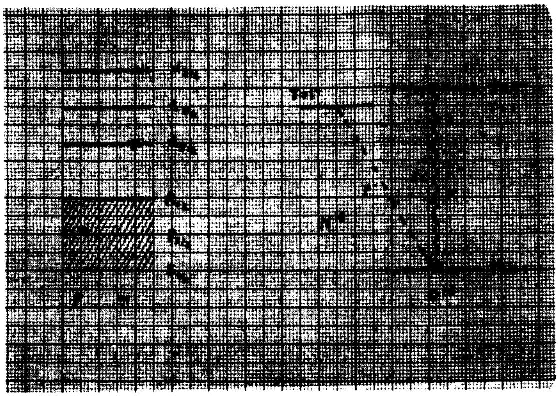

Figure 1 - Particle and hole states in O^{16}

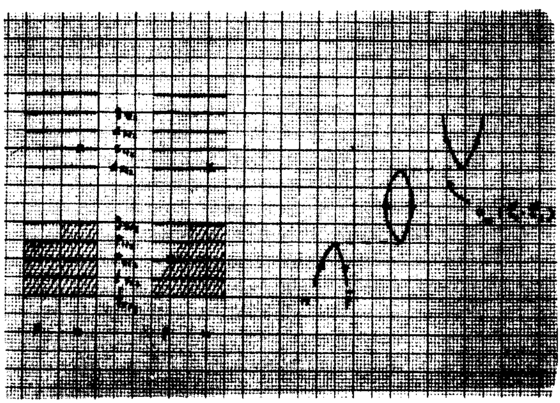

Figure 2 - Particle-hole states in Zr^{90}

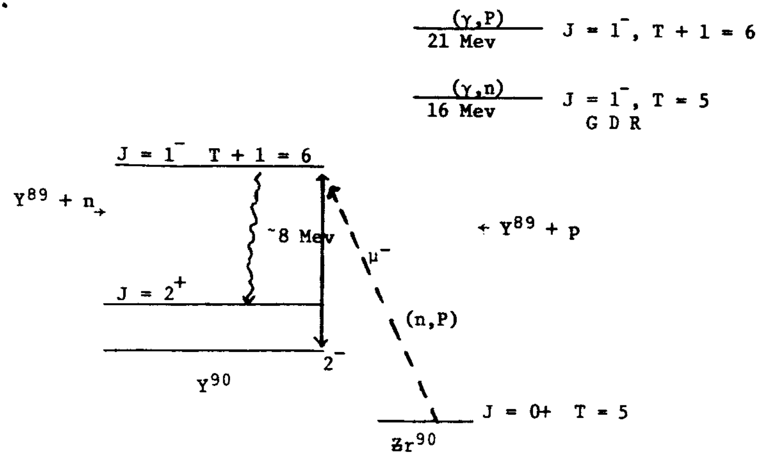

Figure 3 - Energy levels of Y^{90} and Zr^{90}

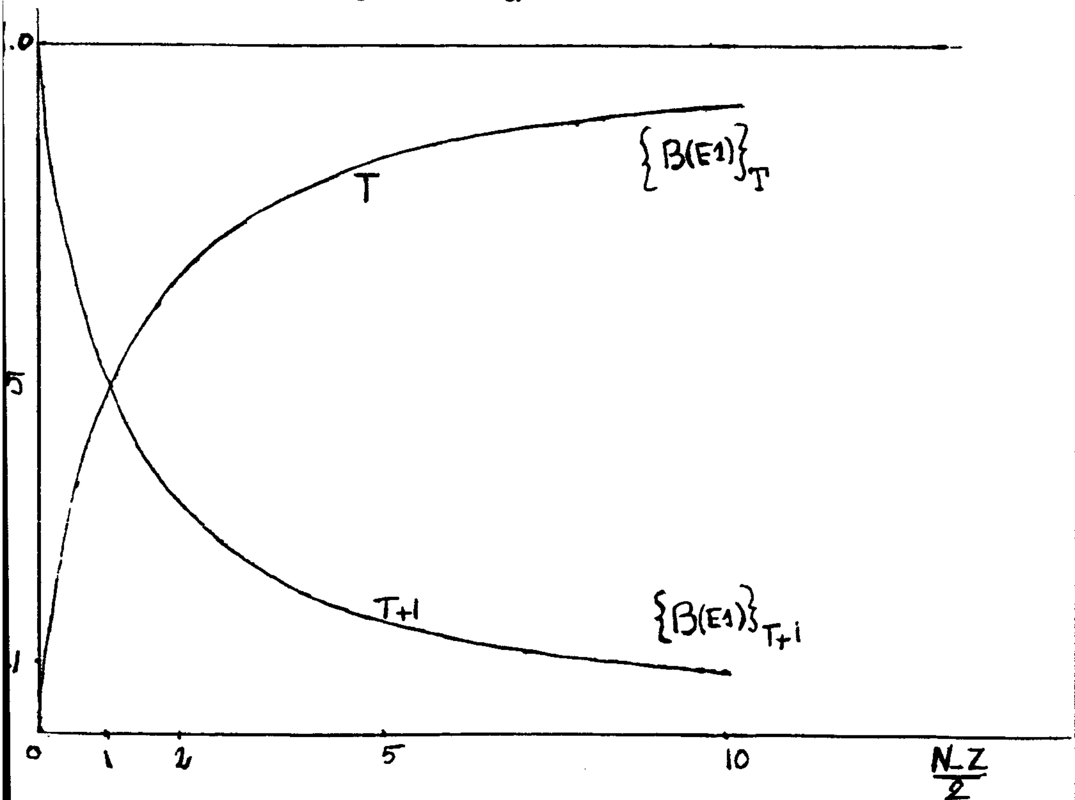

Figure 4 - Graph showing relative value of the reduced El transition rate in arbitrary units normalized to 1 as a function of the number of excess neutrons in a nucleus.

SEGEL: In the decay of the analogue state in the Yttrium 90, one that you showed us, the 1- to 2+, you said that was about a Weisskopf unit. For an electric dipole that would be very very strong. Is there any obvious shell model or physical reason why this state should decay so strongly by gamma ray?

FALLIEROS: The fact that this state is unbound by one Mev means that some s neutron can go very close to the 1- level. The question of the energy of this level could explain the higher transition 1^- to 2^+.

NUCLEAR STRUCTURE MEASUREMENTS WITH THE BROOKHAVEN COSMOTRON*

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About a year ago the Brookhaven group undertook a study of the Brookhaven Cosmotron to determine whether the energy resolution and energy stability of the machine were adequate for nuclear structure measurements. Figure 1 shows some of the characteristics of the Cosmotron and the results of the preliminary measurements. The internal beam intensity is around 5×10^{11} protons per pulse. We can extract 20 to 30% of that giving about 10^{11} external protons per pulse. The time average current with a 2.5 sec rep. rate is 8 nA and the beam spill length is 100 to 200 milliseconds. The energy range can be varied from 500 MeV to 3 BeV. In our preliminary experiments a year ago we determined the beam energy spread to be less than 3 MeV and the long-term beam stability to be better than 1.5 MeV. This latter measurement extended over an 18 hour period and included shutting the RF off, the magnetic field off, and starting up again. With these encouraging results we decided to build a large spectrometer system which

* Work supported by the U.S. Atomic Energy Commission.

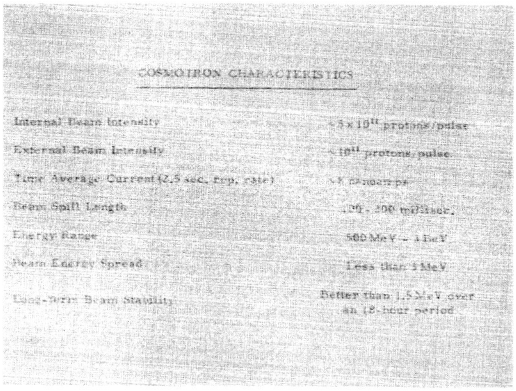
would allows us to do elastic and inelastic scattering and (p,2p) measurement at 1 BeV incident energy. In this project we were joined by groups from Rice University, Maryland, and Los Alamos.

Figure 2 shows the experimental setup. The beam exits from the Cosmotron through the external shims, passes through a quadrupole triplet, and is then bent 12 degrees to reduce the background created when the beam passed through the Cosmotron exit window. A series of three bending magnets, two on railroad tracks, is then used to change the angle at which the beam strikes the target. The second quadrupole triplet, also on tracks, is used to obtain the desired beam spot at the target. The distance from the shims to the target is of the order of 100 feet. We have a fixed spectrometer which is located at 20 degrees with respect to the zero degree beam line, and a moving spectrometer which can be rotated between 50 and 90 degrees with respect to the zero degree beam line. The magnet associated with this spectrometer is on railroad tracks so it is easily moved. The scattering angle into the fixed spectrometer can be varied from -5 degrees to 40 degrees. Consequently the scattering angle of the second spectrometer can be varied over the range 30 to 110 degrees. The angle between the two spectrometers is variable between 70 and 110 degrees. To get the energy resolution and the large solid angles required to do large momentum transfer measurements, we are using a magnetic spectrometer in conjunction with wire spark chamber hodoscopes as shown in Figure 3. S1 and S2 are our trigger scintillation counters. There are four horizontal hodoscopes before the magnets and four after the magnets. In our high resolution work the bending angle is mainly determined by planes P3, P4, P5, and P6. The other planes are used for redundency. That is, they are used to guarantee that the sparks in hodoscopes 3-6 are located at the point at which the particle passed. It is this redundency which is very important in reducing accidental counts and allows one to do very low cross section measurements.

Figure 4 is a picture of the hodoscopes as they are set up in the spectrometer. We have to use helium bags to reduce the multiple coulomb scattering which is the largest source of energy smearing in our system. Figure 5 shows a close-up of a hodoscope, 18 in. long by 6 in. high; the wires are 50 mils apart. Each wire is threaded through a magnetic core and after each event we read out all the cores that have flipped. This information is stored in a buffer memory. We can spark the hodoscopes about 100 times per beam pulse. Between beam pulses we dump the data from the buffer memory onto magnetic tape and simultaneously dump it into the Merlin computer. The computer analyzes the data and presents us with an on-line display so that we can determine whether or not the experiment is running properly.

Now just to show you that we really have started taking some data on this experiment the next few figures show some preliminary spectra. The momentum spectrum shown in Figure 6 was obtained by putting the 1 BeV beam from the Cosmotron right through the spectrometer. The points are .8 MeV apart and the full width at half maximum is 5 MeV. This width is almost entirely due to multiple coulomb scattering in the planes. We are nearing completion of a set of planes 1/10 as thick as the ones we used to take this data, and which will give a multiple coulomb scattering contribution to the energy resolution of less than 1 MeV. We are confident that it is possible to obtain a total resolution between 1 and 2 MeV for a single spectrometer. Figure 7 shows the scattering of 1 BeV protons from water at 9.3 degrees. You can see the ground state and the first excited state of oxygen. The rather large peak is p,p elastic scattering from the hydrogen in the water. The background rate, that is, the target-out to target-in ratio, is only a few percent and offers no problems. In Figure 8 we show the scattering of 1 BeV protons from carbon at 9.3 degrees. Again, you can see the separation of the elastic and first excited states. With our new planes we expect that this separation will improve substantially. The rather large bump is quasi-elastic scattering. To get some preliminary numbers we also ran caroon at 40 degrees which corresponds to a momentum transfer of a little over 1 BeV/c. At this angle we are measuring a cross section of a few microbarns. The background was still only a few percent. Even more important the accidental coincidence rate was less than a fraction of 1%. This is mainly due to the fact that the planes have such a high rejection ratio for accidental events.

I think you can see that this equipment can be quite useful for investigating nuclear structure.



COSMOTRON CHARACTERISTICS

Internal Beam Intensity	~5 x 10^{11} protons/pulse
External Beam Intensity	~10^{11} protons/pulse.
Time Average Current (2.5 sec. rep. rate)	~[illegible] nanoamps
Beam Spill Length	[illegible] - 200 millisec.
Energy Range	500 MeV - 3 GeV
Beam Energy Spread	Less than [illegible] MeV
Long-Term Beam Stability	Better than 1.5 MeV over an 18-hour period

Figure 1 - Characteristics of the Cosmotron and results of preliminary measurements.

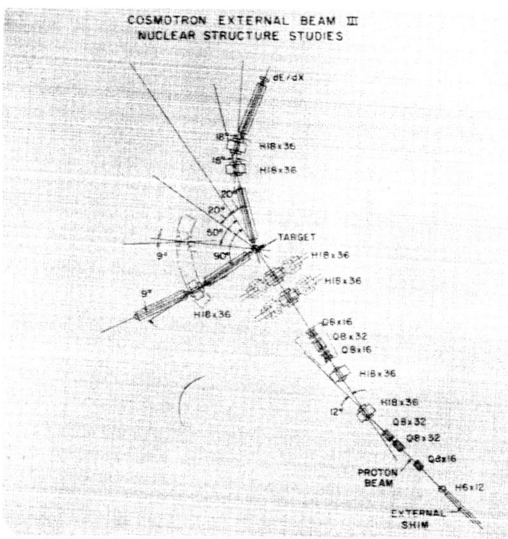


Figure 2 - Experimental setup - Cosmotron external beam III.

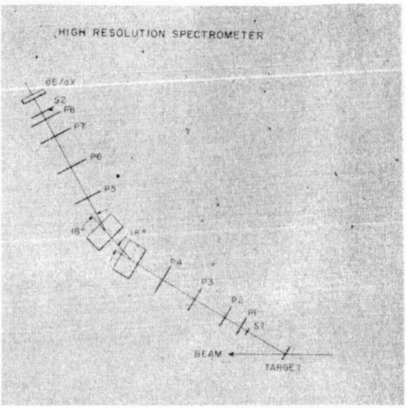


Figure 3 - High resolution spectrometer.

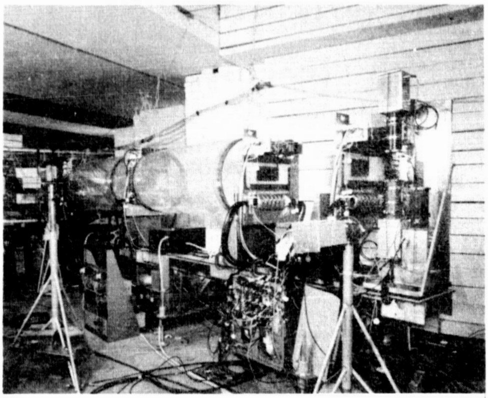


Figure 4 - Equipment view showing first four hodoscopes of the spectrometer.

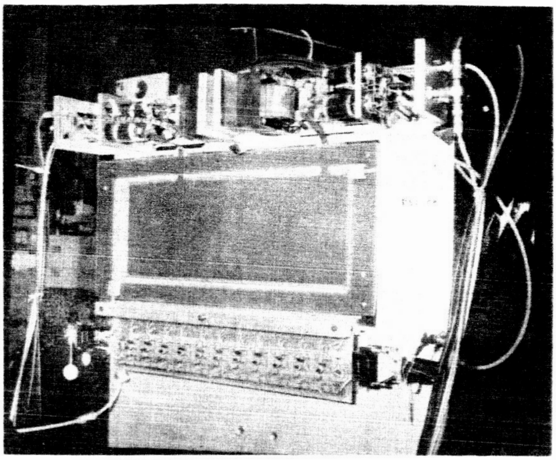


Figure 5 - Close-up of 18" x 6" hodoscope.

Momentum spectrum of the 1 Bev external proton beam from the Brookhaven Cosmotron.

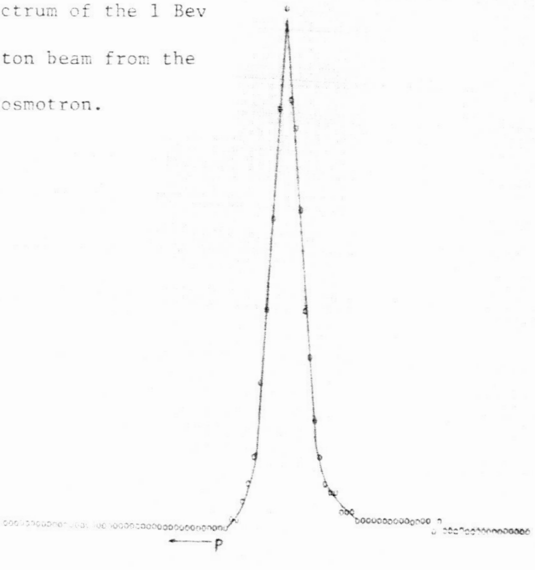


Figure 6

Scattering of 1 Bev Protons from H_2O at 9.3° (lab). Each data point is separated by 0.83 MeV. The left-hand group is elastic scattering from O^{16} the near-by group from the O^{16} states at ∿ 6 MeV. The strong, right-hand group is mostly elastic p-p scattering and some quasi-elastic scattering.

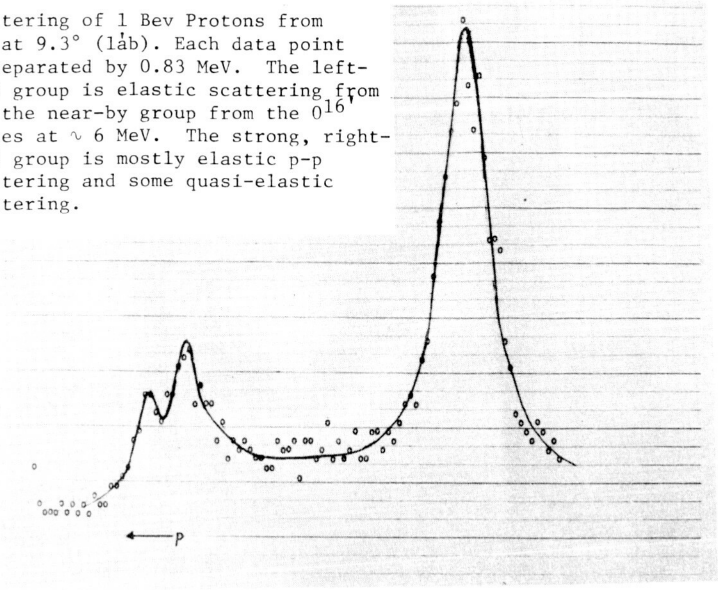


Figure 7

Scattering of 1 Bev protons from graphite at 9.3° (lab). The left-hand group is elastic scattering from C^{12}, the adjacent group is an inelastic group from C^{12} (4.43 MeV) while the broad right-hand group is quasi-elastic scattering.

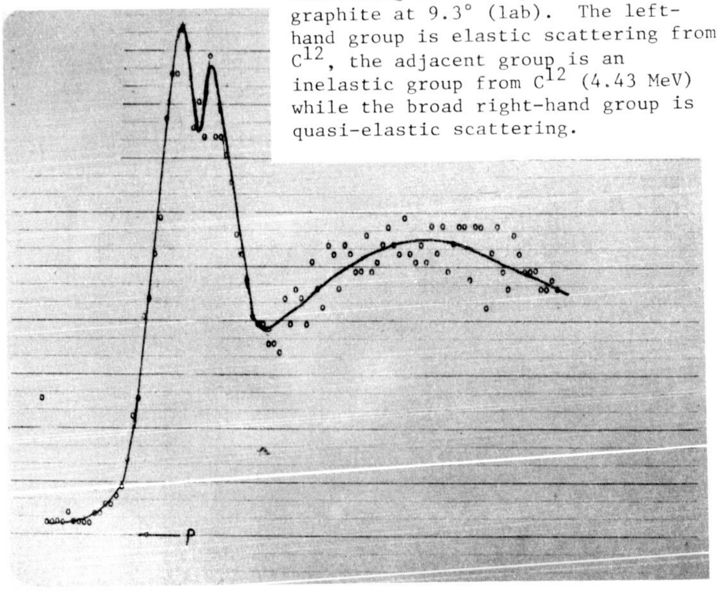


Figure 8

SUMMARY OF CONFERENCE

Chairman, V. L. Telegdi

N66 32784

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SUMMARY OF WILLIAMSBURG CONFERENCE ON INTERMEDIATE ENERGY PHYSICS

D.H. Wilkinson

University of Oxford

Before I begin I have two announcements to make on behalf of Her Majesty's government. The first is that we are terribly pleased to see how well you are getting on over here; the second is that I shall be happy to receive your taxes at the end of the session.

You may be wondering how Linc Wolfenstein and I are going to divide between us the job of summarizing the conference. So am I. I think the arrangement that I reached with him is that I talk about the machinery and he talks about the nuts and bolts: I talk about the nuclear aspects of what we have been doing for the last couple of days and he talks about the couplings - the elementary particle aspects.

In doing my job I'm certainly not going to try to be synoptic, either in the sense of going into all the different approaches to the subject that have been talked about, nor in the sense of mentioning everybody who has said anything. In fact I must be very selective and try to pick out the more novel things, recognizing that much of real value will have to go unmentioned. There just has been too much talked about to summarize in toto in a short time. Also, I don't intend, in any way, to referee or adjudicate between certain, shall I call them, alternative, accounts of the subject we've received - particularly on the first day. My personal view is that Telegdi's account was superior to that of Devons in the measure that he is my chairman this afternoon and Devons' account was superior in that it was funnier, though I think this was probably just due to Telegdi's better-developed sense of propriety. Nor shall I reveal in whose talk it was that, when the alarm clock went off, Torleif Ericson turned to me and said, "Ahh...g'morning."

Well, with that maybe I can approach my subject. I have been rather exercised to know whether to try to summarize this conference or perhaps rather another one that I had thought we might be going to have. By this I do mean something rather serious. In this conference, we've plunged right into the middle of things and had, as it were, an account of the state of the art, a topical conference, a discussion of the kinds of nuclear structure measurements that are being made by what are largely new methods. We haven't attempted to justify to ourselves in any detail why we should want to use these new methods. In the other conference, the one that we have not had, we would have looked rather more critically at the kinds of nuclear structure information that are going to come out of using elementary particle probes and the higher energy regions. To a large degree, the kind of work that we have heard about at this conference started simply because elementary particle beams were available from big accelerators which had begun to outlive their usefulness for elementary particle work. But now one is making new particle beams at existing accelerators and talking about making new big accelerators largely for nuclear structure studies. Now the mere fact that it can be done is certainly no reason for doing an experiment or for embarking on a new type of research. It's not even a good enough reason that it is very expensive. Personally, I don't feel that we've had enough emphasis at this meeting, or at any other for that matter, on the novelty of the nuclear structure information that will come from these new methods. In this field, the borderland between nuclear structure and elementary particle physics, it is most important to distinguish critically between the phenomenon and its interpretation. Too often, in my view, have the more purple proposals, although there are honorable exceptions, emphasized what can be done and failed to point out the formidable difficulties that interpose themselves between the data and their translation, in unambiguous terms, into information about the nucleus. Final state interactions, multiple and plural scattering, off the energy shell, momentum dependence, configuration mixing, intermediate coupling, higher order terms: these are the four-letter words of intermediate energy physics; perhaps it is modesty that prevents our hearing them more often. There are also some signs of reticence in pointing out what we know already. I may therefore, just from time to time, draw your sober attention to comparisons between the new approaches and old, sometimes very old, ones. Of course, alternative approaches are always valuable. If something is important it should always be done at least twice. If it is sufficiently important, it should be done by a number of totally different methods. All this having been said, I want to declare my hand: I am in favor of nuclear structure physics at high energies, I am in favor of the meson factories, I am in favor of kaons, anti-protons and neutrinos for the nucleus. But I also think that our chance of getting them is the greater if we recognize and admit that the way is rough.

My sermon over, this brings me at last to the conference proper.

Beginning at the beginning, with muonic x-rays, we saw, I think for the first time, some kind of confrontation between the information on the charge distribution that one gets from electron scattering and from the muonic x-rays. Until quite recently, certainly until the renaissance of muonic x-ray work through the GeLi-counter, muonic x-rays told us only $\langle r^2 \rangle$. They hadn't approached a second parameter; they hadn't approached the details of the nuclear charge distribution. But now they are doing that. The results are now sensitive to two parameters. Ravenhall, in his interesting talk, compared electron scattering with muonic x-rays. He did this in what I consider to be a slightly optimistic spirit, assuming that we understand both processes perfectly. In other words, he combined the results from electron scattering and muonic x-rays and showed that you can get a hint that the nuclear charge distribution may have a slightly longer tail than is represented by the familiar Fermi parameterization. I think this is a fine and provocative thing to do, but I personally would like to see us, as far as possible, keep the two approaches separate. Find the charge distribution from electron scattering on the one hand and from muonic x-rays on the other - and then put the two together at the end when one has learnt as much as one can about the parameterization from the two approaches independently, with all their attendant uncertainties. Ravenhall was not doing this: he was putting them together at the beginning and using the charge distribution just as a link between the electron scattering and the muonic x-rays. Incidentally, the fact that he could get such an interesting suggestion by combining quite old and very-much-improvable electron scattering data with quite new and very-much-improvable muonic x-ray data shows what we may have in store.

I think it might be useful to ask what sort of precision one must achieve in the muonic x-ray measurements if you want to make statements about two parameters and, more particularly, if you want to make statements about three parameters. Now electron scattering, particularly the recent Stanford work in calcium, is beginning to show that one can there meaningfully talk about a third parameter. Of course, we don't yet know how to use it. The way in which one uses a third parameter is, at the moment, very much a matter of personal choice, and thereby hangs a severe problem, but at least it looks as though one can begin to think in those terms. So in the muonic x-ray case also one must talk in the same terms of three rather than two parameters or one is not in business. This has not been mentioned at this meeting explicitly, although I thought it would be. Since it hasn't I will give some numbers that come from the Los Alamos meson factory proposal. Let's suppose that we'd like to find out about the charge distribution in the form: $(1 + \alpha r^2)/(\exp[r-R]/a + 1)$

The term αr^2 is a representation of one possible use of the third parameter- to make the nucleus hollow in the middle. The rest is the usual Fermi-type distribution. The question is: how accurately must one make the muonic x-ray measurements to get all three parameters to a usable accuracy? Take the reasonable ranges: a 0.5 to 1.5 fm; R 1.0 - 1.2 $A^{1/3}$ fm; α 0 - 1.25 $(A^{1/3}\ \mathrm{fm})^{-2}$. $\alpha = 1.25\ (A^{1/3}\ \mathrm{fm})^{-2}$ makes the central charge density in a heavy nucleus about half that of the edge, so it's not wholly unreasonable. Now if you talk about the high energy transition, 2p$\rightarrow$ 1s, then this complete range of parameters corresponds to about a 1 MeV change in the x-ray energy which, of course, is enormous. But if one fixes this energy exactly and then goes to the next transition, say 3d $\rightarrow$ 2p, the variation in its transition energy given by the full range of variation of the parameters is down to some tens of keV. If we have fixed these two transitions exactly the range of energy variation of a third transition corresponding to the complete range of variation of the parameters is something like 2 keV. This shows that if one wants to attempt to determine three parameters from muonic x-rays alone then you have to do measurements that are significantly better than a couple of kilovolts. GeLi-counters now have resolutions of around 5 keV or better and that will go down a little bit, presumably. So, with the kind of statistics that one would get out of a meson factory, one should achieve energy determinations of the order of 100 eV - being realistically optimistic. With that, one could indeed obtain the three parameters reasonably well. But there are two extremely important provisos: 1) Calibration with an accuracy of less than a keV is difficult. The N^{16} type of calibration that we've seen a lot of at this meeting is limited by the accuracy with which one does conventional nuclear structure energy level measurements. It's not going, I think, to improve rapidly to the 1/10th of a keV region at 6 MeV although it is not impossible. There's a big problem in utilizing the kind of accuracy of which the GeLi-counter is already capable - a keV or better. 2) The polarization question about which we have heard quite a lot - the second order shift which is associated with virtual transitions into excited states and back again to the ground state. We do not know how big this polarization effect is. It has been lumped into the parameterization of the charge distribution. It is obviously very difficult to determine it and to know you have determined it. Typically the calculations of polarization shifts for the low-lying states of heavy elements range from about 3 keV up to about 100 keV; the latter figure may be seen to be too high, because of the approximations that have been made but the former may well be too low. So any analyses of muonic x-ray energies that are really sensitive to a few keV - 5 keV let's say - may be disastrously disturbed by polarization effects. But how do we know? For certain investigations, polarization effects may be made to cancel out or nearly cancel out but in the dead reckoning kind of work they will not. Certainly one has got quite a long way to go in muonic x-ray studies; both technically and in understanding the finer details of the process. It may be that one does better by comparing directly with electron scattering as Ravenhall was doing, but I am uneasy about that because electron scattering has its problems too; I return to my suggestion that one should get as much independent information out of the two methods as possible before putting them together.

We heard quite a bit about the isotope shift and, so far as one sees at the moment, things are going quite nicely there, both in the light elements, calcium, and in the heavy ones. The conclusions that were being drawn from the muonic x-ray isotope shift tallied very well with what we knew from other data: electron scattering in the calcium case and the long-familiar optical isotope shift in the heavy elements. Let us remember that we have known about these effects for decades from atomic spectra, and as was remarked, perhaps one of the useful features of the muonic x-ray studies will be to normalize the isotope shifts that one gets from atomic spectra. The muonic shifts can be used to normalize because in the case of the muon one has just a hydrogenic atom with none of the terrible complications that attend the determination of the electronic wavefunctions in the atomic case. I'm told by our chairman, I know nothing about this, that there are very delicate and difficult problems in the optical isotope shift in lighter elements where the muon data will be able to help separate out the easily understood parts of the effect from those that give great difficulty.

Let us now go on to the quadrupole moment. One important aspect of the quadrupole effect in muonic x-rays was not mentioned, perhaps because it is so obvious, and that is its sensitivity to the sign of the quadrupole moment. It is extremely difficult to get experimental information about the sign of the nuclear quadrupole moment from conventional methods although the reorientation effect (as it used to be called) in Coulomb excitation provides one approach and it is a great relief (although, of course, we knew it all the time, now that it turns out to be right) to discover that the signs of the quadrupole moments in the heavy elements are consistent with our past beliefs and prejudices. There may be a future for muons in determining signs of quadrupole moments; there are some regions of the periodic table where one is still unsure both experimentally and theoretically of the sign of the quadrupole moment. However, the sensitivity from muon work drops off rather rapidly as one goes to the lighter elements where the ambiguities are mostly found. We might also note in passing that muons look as though they will be quite a useful tool for measuring smallish quadrupole moments in regions not too far from spherical symmetry; we saw at this meeting quadrupole splittings or broadenings due to rather small quadrupole moments.

I don't think it was emphasized at the meeting that the quadrupole splitting in heavy elements is very sensitive to the penetration of the muon wave function into the nucleus. The largest effect in the x-ray energy comes, of course, from the penetration in the 1s-orbit. The 2p-orbit, from which the quadrupole effects of the 2p $\rightarrow$ 1s transition come, also penetrates very significantly and the quadrupole splittings that one observes in the heavy elements are less by a factor of about 2 than those that one would get without penetration. So quadrupole splittings in 2p $\rightarrow$ 1s transitions do not yield quadrupole moments - rather, one is measuring the form factor of the quadrupole moment. One determines the quadrupole moment times the penetration factor and one must separate out these two effects. This can be done, from muonic x-rays alone, by going to the next stage, by looking at the 3d $\rightarrow$ 2p transitions and the 3d $\rightarrow$ 2p $\rightarrow$ 1s cascades which are exceedingly complicated. This has not yet been properly done - one has not yet unravelled in all detail the complicated quadrupole patterns associated with the 3d $\rightarrow$ 2p transitions. But if one can do it, then one will be able to separate out the penetration effect from the intrinsic quadrupole moment effect and so get a measure of both. The quadrupole moment will cancel out in the comparison between the 3d $\rightarrow$ 2p and 2p $\rightarrow$ 1s sets of transitions giving the penetration factor which can then be put back into either set to get the quadrupole moment. These considerations make it seem to me premature to try to discuss detailed models of the structure of the quadrupole moment form factor either static or dynamical. When one has a detailed model of a nucleus, particularly the way in which the surface thickness changes with angle, then one can compute the static form factors by dead reckoning. At the same time, in understanding the dynamical quadrupole effects, the involvement of excited nuclear states whose importance is due to the fact that the magnetic fine structure splitting between the $1p_{3/2}$ and $1p_{1/2}$ states is of comparable energy to that of the strongly-enhanced E2 first excited state transitions, there are very interesting model-dependent questions; particularly whether the off-diagonal E2 matrix elements are the same as the diagonal ones, in other words, whether the simple-minded Bohr-Mottelson account of the situation is applicable. If one does look at the complete picture of the L and K transitions, then one will be able to settle at the same time both the problem of the intrinsic quadrupole moment and also the comparison between the diagonal and the off-diagonal E2 matrix elements. So, as a complete outsider, my reaction to this situation is that one ought to take some very small number of cases and really do them extremely well.

It's very nice to have data on a lot of different nuclei, but my own purely personal feeling is that it would be more valuable over the next year or so to take very few cases and do them in very great detail.

Another important point that was raised several times at this meeting was the magnetic hyperfine splitting of the 1s-state: the problem of the distributed dipole moment. We saw that the 1s-state splitting, the two spin couplings between the muon and the nucleus, has indeed been detected. Since the 1s-state penetrates so deeply this, of course, is again a form factor matter--something which depends on the detailed spatial distribution of the magnetic moment. Again, we've had information on this for many years from phenomena such as the magnetic hyperfine structure anomaly. There are even review articles on it. So, don't let's pretend that we're finding something very novel here, yet at least. However, crude as its present information is, it is another approach to the problem and, furthermore, one of considerably greater generality than the others that are available. So we might hope to get, in the end, parameters that relate to the distribution of the magnetic moment running through a large part of the periodic table. This again is a model-sensitive matter; we already know the magnetic moment itself with essentially infinite accuracy but even a rather crude measurement of the form factor may be very valuable in chosing between models.

I should like to interject something here. As I said earlier on, there is no point in doing a measurement simply because it can be done. Sometimes one gets the impression that there is little other motivation. I think it's extremely important to recognize that there is no justification at all for this kind of work unless you are getting information that is model sensitive. The only direct quote that I'll make from this conference bears on this attitude: "People who are fussy about the kind of model we use might object..." We have to be fussy. If we are not going to get data that are model sensitive then we certainly ought not to get support for doing this kind of physics.

Before leaving muonic x-rays I should like to make the point which was made by one or two speakers, particularly Devons, that we are here not just finding out about nuclei as we normally know them; we are studying a new sort of object-a nucleus with a muon inside. And the fact that, for example, we are studying uranium with a charge of 91 instead of 92 may be quite interesting. The muon-nucleus coupling will essentially change the nuclear structure and the way in which that comes about is obviously a matter of great interest and importance. And that we can't do by other methods.

I'd like now to say a bit about muon capture. I don't want to spend very much time on this except to remark that we may have recognized quite an important clue as to new forms of collective motion.

The study of collective motion is something which has been extremely profitable for nuclear structure physics in the last decade. To find the simplest ways in which nucleons behave under various circumstances is clearly an important starting point for a more detailed model of the nucleus. Familiar among collective motions is the giant electric dipole vibration which we can visualize through the Goldhaber-Teller model, incomplete but invaluable as a sort of mnemonic. It can be described as protons vibrating collectively against neutrons without spin-flip. This T=1, 1^- vibration, has been pointed out by Walecka, Uberall and Foldy as being excited by the vector part of the coupling in the case of muon absorption. But then we have other possible similar collective vibrations such as the spin-isospin vibration in which the proton-neutron roles are interchanged and neutron-proton vibrates against neutron-proton. This again is T=1 with 0^-, 1^-, and 2^- states excited by the axial vector part of the muon coupling. The pure spin wave vibration is of no interest to us at the moment because it's of T=0 and so can't be excited by muon capture. The T=1, 0^-, 1^-, and 2^- states are difficult to identify in electromagnetic transitions although there are signs of the 1^- and 2^- components in inelastic electron scattering, increasing, as they should, with increasing momentum transfer. All these T=1 states should be excited by muon absorption. Indeed if one interprets quantitatively the absolute muon absorption rates, particularly in oxygen, then one gets agreement between theory and experiment only by raising the energy of the electric dipole, T=1 1^- vector vibration from that which you calculate without residual interactions up to the point experimentally observed in the photonuclear reaction and with it the energies of the T=1 0^-, 1^-, 2^- axial vector vibrations. We know that the axial vector contribution to the absorption must be quite significantly stronger than the vector contribution in the case of the Goldhaber-Teller model and it's almost the same for a more realistic model. The contribution from the axial vector part summed over all its components is about three times that from the vector part. So, the bulk of the absorption rate will come from the T=1, 0^-, 1^-, 2^- collective vibrations, and if we raise their calculated energy by the same factor that we know experimentally we must raise that of the T=1, 1^- vibration we get agreement between theory and experiment. So here we have, from the muon absorption process, a quite-significant clue about the existence of this type of unusual collective motion and also a rough hint as to where it is to be found. The detailed theoretical predictions depend very much on nuclear structure considerations and to some degree on couplings, particularly on the validity of the Universal Fermi Interaction. If you want to find out about the couplings themselves, you must look at transitions to particular states not at overall absorption rates. Then you tend to get completely bedeviled by the nuclear matrix elements. If you take transitions where the nuclear matrix elements are known you learn nothing about nuclear structure but you learn something about the couplings: not my side of the fence for this afternoon.

I'll go quickly on to pions and see what I've got here. Pion phenomenona are of several kinds and, again, x-rays and absorption are the two chief chapters. We saw a derivation of a pion optical model potential by Ericson. Whether this is exactly the right potential or not, I don't know and it is not important for what I want to say. What I learned chiefly from Ericson's talk was the enormously high standard of freshman physics in Sweden which apparently includes the Lorentz-Lorenz effect. I'm not being unkind in saying that, I didn't learn anything else only because I'd read all his preprints. The point that he is making, or the point I'd like to extract from his talk, is that in the shifts and widths of pionic energy levels we possibly do have an approach to the experimentally extremely difficult question of nucleon correlations inside the nucleus. As he pointed out, in the pion-nucleus interaction one should have two-nucleon processes in play as well as one nucleon processes and one would expect on rather general grounds to find there the analogue of the Lorentz-Lorenz effect, the non-linear dependence of the refractive index on pressure of a polarizable gas, an effect coming from the proximity of scattering centers. So, if one can detect this effect in the pion case and understand it, then it should give a quantitative measure of the degree of nucleon-nucleon correlation inside the nucleus which we would very much like to find out. The point about pionic x-rays is that, by measuring the width of the pionic states, one is determining an absolute time scale. There is no point in simply seeing pions being absorbed in nuclei with two fast nucleons coming out and saying that this proves that we have correlations. It doesn't tell us a thing about how strong those correlations are, what fraction they represent of the overall wavefunction. But through the x-rays one gets an absolute measure of the time, and this can then be directly related, in principle, to the nucleon-pion properties themselves and so can be turned into a measure of the absolute degree of correlation. That, I think, is more a hope at the moment than a real achievement but Ericson's work demonstrates the value of better experimental data and further theoretical study, particularly perhaps on the importance of final state as well as initial state correlations in determining the absorption rates.

An interesting point that Ericson made is that radiative pion capture as observed at Liverpool, may, by its analogy with muon capture, enable us to get some kind of handle effectively on the neutrino spectrum in muon capture. This might be valuable in discussing the details of the excitation of the collective states.

We do have some data which are consistent with Ericson's potential, reported by Crowe, namely the energy shifts of high pionic levels. Accurate pionic x-ray measurements over a wide range of elements and transitions, showed departures of the transition energies from those computed by taking account vacuum polarization alone which were rather nicely accounted by the Ericson potential. This, obviously, should be pursued. It is ;ely a matter of the tail of the real part of the potential. If it comes right, one will have more confidence in interpreting data, the level hs, which depend on the imaginary part of the potential which is itself fly a two-nucleon matter - the correlations. So it seems as though a inuing study of the energy shifts of the not-very-much-shifted high ic states will be valuable for testing our ideas about the pion potential. whole point here is that it's useless simply to parameterize the pion ntial. We can only get to a measure of correlations if we are using a ntial which is computed from pion-nucleon and pion-nucleon-nucleon . So, simply to parameterize a potential that accounts for the pion y energies will not get us anywhere in the study of correlations.

The absorption of pions may be very valuable in looking at certain cts of nuclear structure. We saw some very nice data both from Rochester from Liverpool on quantitative aspects of the nucleon-nucleon correlation owing stopped pion capture. In particular the Rochester data showed that n-n to n-p ratio, the ratio of neutron-neutron to neutron-proton pairs approximately 4 to 1 and as high as 6 to 1 in some cases, for example ;en. Since the neutron-proton initial state is a triplet and so has a er statistical weight, one naively expects a ratio of about 3 to 1. One n't be too hasty in the interpretation of data of this kind. The simplest-ed explanation, of course, is just that the triplet force at the nucleon-eon separations of about 0.5 fm involved in pion absorption, is stronger the singlet force. But, as Koltun pointed out, there is also rescattering e taken into account, the charge-exchange version of which contributes to capture in the triplet but not in the singlet state; this then boosts the n-n to n-p ratio. So one cannot interpret the data very simply and directly. This may be an example of a case where the elementary particle physics, so to speak, is a little bit too difficult at the moment to permit us to interpret the nuclear structure aspects, important though they obviously are.

This, as summaries usually are, has been far too short and far too selective. But if I may, in addition, give an overall impression it is that we are only just beginning. We are trying out our new tools but have not yet learnt much about the nucleus that we didn't know before. In that narrow sense we have learnt nothing from this conference. In the longer view we have learnt the potential power of many new approaches to the nucleus. Whenever there is a new way in there is something new to be found even though it may take time and a lot of hard work to find it. I am convinced as I said earlier, that these new lines which give us new interactions, new momentum transfers and that are sensitive to different aspects of nuclear structure from the traditional approaches should be pursued and pursued vigorously; we shall probably get further if we don't try to run before we are sure we know how to crawl.

ERICSON: I wanted to make a comment on your statement that in mu-mesic x-rays all quadrupole effects have been sorted out in the 2p-1s transition. They have actually also been sorted out in the 3d-2p transition, contrary to what you said. So, I just wanted to correct this misunderstanding.

WILKINSON: Well, I do apologize if that's true. My understanding was that there was no case in which all the possibly visible 3d-2p lines had been detected and their energies measured with sufficient precision to do the unscrambling job that I was talking about.

TELEGDI: In discussing the problem of analyzing mu-mesic x-rays winding up in an ultimate accuracy of the order of a faction of a kilovolt you suggested some problems, the best known being the ill-known or ill-computed nuclear polarization. There's one more effect which had been made clear to me by Dr. Hargrove which is present to this level of accuracy when one includes in higher states. That is that when you make very refined measurements in the higher states you have to allow for the shielding by atomic electrons. This is very hard to handle because you don't know quite how many are there at the time of the mesic transition.

SUMMARY OF CONFERENCE

L. Wolfenstein

Carnegie Institute of Technology

I want to summarize the various things that have been learned and can be learned about the interactions of elementary particles from experiments in this general energy range. We have been talking about below 1 BeV. I shall not try to cover all the things that have been discussed at this conference, but in order to give some wider perspective, I will discuss some things which have not been discussed at this conference.

I'll start with the weak interactions. One question concerning weak interactions of a very fundamental sort has been mentioned. It concerns the question of CPT invariance. Discussion was given on an experiment, of a very preliminary character, to compare the π^+ and π^- lifetime. It should be noted that there is rather strong evidence for CPT invariance in strong interactions given by the equality of the K and $\bar{K}$ masses to a high degree of accuracy of the order of a part in 10^{14}, but in weak interactions the evidence is much less. We have, however, rather stronger data on the μ^+, μ^- lifetime equality from experiments at Columbia a couple of years ago, which show a very close equality of those lifetimes.

Let me say then a little about mu decay, a subject not, I think, mentioned in this conference. Mu decay is in a sense the prototype of all weak interactions, in that it's a purely leptonic process. This means that it provides the simplest weak interaction to study without worrying about strong interaction effects, since processes like neutrino-electron scattering have not been done. So, it's of great interest to find out as much as one can about mu decay. The lifetime of the mu, which has been measured very accurately, gives us a value for the fundamental Fermi constant. Measurements of the ρ parameter for the decay spectrum, which has been measured recently by groups at Columbia and Chicago to an accuracy of the order of 1%, agree with the theoretical value of $\rho = 0.75$. I think it's interesting to pursue experimentally as much as one can about mu decay. The fact that it agrees very well with the theory we believe, the $\gamma_\mu(1 + \gamma_5)$ theory, should not keep us from searching further for possible deviations. One might give for historical perspective the fact that in the 1930's, the Dirac equation gave a very good understanding of the energy levels of the hydrogen atom and of the g factor of the electron. We now know that it is the deviations from the Dirac equation, the fact that the g factor is not 2 and the energy levels are not those of the Dirac equation, which are the real triumphs of quantum electrodynamics. Perhaps we may find small deviations in mu decay, which may give us a new understanding of weak interactions. Unfortunately, we have no reason to believe that this will occur at the level of one part in 100, or one part in a 1000, but the kind of information that we hope to find out is about the structure of the weak interaction -- questions related to such things as possible intermediate bosons or higher order effects, as well as any other kinds of structure.

Let me turn then to the semi-leptonic weak interactions. These are the weak interactions such as those responsible for the beta decay of the neutron or mu capture. Now, there are various questions we may ask about these. One question concerns the mu-electron universality for the axial vector current. Evidence comes from the ratio of

$\pi \to e$ decay to $\pi \to \mu$ decay, known quite well. A detailed analysis given by Walecka of the mu capture experiment for the rate of mu capture in C^{12}, measured by Siegel and collaborators, as compared to the B^{12} β-decay ft-value also gives evidence of universality for the axial vector interaction, claimed to be of comparable accuracy. A second question concerns the conserved vector current theory (CVC), and measurements over the last few years of the branching ratio, for $\pi^+ \to \pi^0 + e^+ + \nu$ (this rate is predicted to give a branching ratio of 10^{-8}), has given this ratio to an accuracy of the order of 10% as a confirmation of CVC. We also obtain a prediction of g_M, the weak magnetism coupling in mu-capture from CVC. Another, somewhat less clear, but apparently quite important hypothesis, sometimes called the partially conserved axial vector current hypothesis, gives us an understanding of the Goldberger-Treiman relation for the pi lifetime. At the same time, it gives us a prediction for the induced pseudo-scalar interaction in mu capture. This prediction is that the induced pseudo-scalar coupling is approximately 7.5 times the axial vector coupling. One other property of great interest is the so-called behavior of the weak interaction current under the G transformation. G is a transformation invariance of the strong interaction and if one assumes that the weak interaction current transforms like $\gamma_\mu (1+\gamma_5)$ then one makes a prediction about the transformation property of the weak currents. In particular, one argues then for the absence of so-called induced scalar, or induced tensor couplings. Now, we ask to what extent one can check these various assumptions from muon capture experiments. Since onehas a limited amount of data, one just has to say "Assume some of the hypotheses" and ask how well one has checked some of the others. As one example, Fig. 1 is a copy of a graph given in a recent publication by H. P. C. Rood from CERN in which he analyzes essentially the H and He^3 data. (In reading this curve, I believe one should take the right-hand dashed line for He^3 and move it a bit over to the right, both because the center of gravity and the error, I think, are slighly mis-estimated in the paper.) The cross shown, which is the universal Fermi interaction with all the assumptions I make, namely $g_S=0$ (no scalar), $g_T=0$ (no induced tensor), with a pseudoscalar of about 7.5, lies pretty much in the center of the experimental region, and therefore tells us that within the accuracy of the most useful experiments, H and He^3 capture, we have very good agreement with all these assumptions. On the other hand, it's clear that the limits are not so great. One would ask how much variation in the induced pseudoscalar, or how much tensor one could have, one sees that there is a fairly large variation. If you vary the induced pseudoscalar, for example, then you might come down perhaps to five and maybe up to 12 or something like that. I would say that it would be of great interest to get a more accurate figure in hydrogen. Hydrogen can now be calculated quite well. The chief uncertainties for calculating muon capture in hydrogen, I would say, are no longer the molecules, which seem to have been calculated quite well now. A number of people have calculated and all gotten the same results for the molecular wave function. So muon capture, even in liquids, would seem an interesting thing to pursue with a higher degree of accuracy. The greatest uncertainties most likely are due to the uncertainty in the ft-value of the neutron, which is only known to about 3%, and in radiative corrections which have not been calculated. So that, theoretically, if one had the ft-value of the neutron to 1%, one could interpret an experiment on muon capture rates, either in gases or in liquids, to an accuracy of about 2%. So it would be worthwhile to have a 2% number on the muon capture rate in hydrogen - a very fundamental number. When one comes down to that level of accuracy, of course, one would have to be concerned with such questions as what is the axial vector form factor. One doesn't know. It has tried to be measured by neutrino scattering experiments but these are not very accurate. Perhaps the most likely interpretation of such experiments would be as giving us a better value for the induced pseudoscalar coupling. In fact the theory does require that the induced pseudoscalar be very close to this number 7.5 g_A. If it were to be very different in capture in hydrogen, this would indicate that we really did not understand the Goldberger-Treiman relation. It would look like that was an accident. Of course, it has been pointed out that in complex nuclei it's less clear what the induced pseudoscalar interaction should be.

There are other problems in muon capture which have been alluded to. These, however, had to do with capture in complex nuclei. The problem of the capture rate in He^4 and others, on the whole, I think belong to Professor Wilkinson's part of the afternoon - though he did not mention them.

Now, I want to turn to the electromagnetic interaction. There are various questions to be raised. One which has been raised recently is the question of charge conjugation invariance of electromagnetic interactions. This has been raised, as you know, because of the discovery of CP violation in the K^0 decay and this has led people (particularly Professor Lee) to suggest that perhaps this can be explained as due to a parity-conserving, but C-violating electromagnetic interaction. One of the things suggested by this has been a search for the decay of the π^0 into 3 gamms rays. Unfortunately, the phase space considerations, in so far as you can make them, indicate that this rate ought to be of the order of 10^{-6} of the rate of π^0 going to 2 gamma rays or less. The phase space is slightly hard to calculate. Where you compare 2-body phase space to 3-body phase space you need a radius, and it is not clear what the radius of π^0 is; whether one should use the pion mass or some vector meson mass which, of course, makes a very large difference. Searches have been made for the decay of the π^0 to 3 gammas at CERN and at Dubna. These give a limit to this rate which is perhaps a little better than 10^{-5} of that going to 2 γ's, but that is not very significant.

A second question with respect to electromagnetic interactions is the general question of whether the muon and electron have the same electromagnetic interactions. A number of tests of this have been made, all of which give us the answer that they do have the same electromagnetic interactions. One of these, of course, is the classic experiment on the g factor or (g - 2) of the mu-meson. At higher energies, experiments have been done on the scattering of mu-mesons from protons, which agree with the electron scattering from protons. One might also say that these experiments concerning mu-mesic atoms, to the extent that they agree with electron scattering, provide a measure of the electromagnetic interaction being the same. Finally, of course, there are the very elegant measuremen s of the hyperfine structure of muonium which Professor Hughes reported.

Now let me turn to the electromagnetic interaction as a probe of the structure of particles. This was alluded to in two contributions to this conference. In the first place the discussion was given by Professor Hughes concerning the hyperfine structure anomaly in H. The way this atomic physics problem got into this conference was that Professor Hughes' experiment itself becomes as good a way as any, or a better way, for determining the fine structure constant. Considering the years when one wondered if one could find muonium at all, I think this is a great credit to Professor Hughes.

The result, when you take the value of α from muonium, and put it into the hyperfine calculation, is an anomaly of 45 parts per million in H. This is the same order as the effect expected from the two-photon contribution, the contribution that comes from the 2-photon exchange in the hyperfine structure where various things may happen to the intermediate nucleon. One is then exploring the structure of the proton in a rather dynamical way. All the theoretical attempts to understand this have failed very badly. Professor Hughes has made the very interesting suggestion that in the future it may be possible to do the hyperfine structure experiment on the atom made up of a proton and a muon, instead of a proton and electron. Such an experiment requires both intense muon beams and very powerful lasers, so that it is a futuristic experiment, but it points to some interesting possibilities for the future.

A second experiment related to the electromagnetic structure of particles was the discussion of the possible measurement of the electromagnetic form factor of the pi-meson by comparing the elastic scattering of π^+ and π^- mesons from the alpha particle. And, in particular, the idea of this is that if one could compare the π^+ and π^- scattering and look at the difference between these scatterings, that one might interpret that difference as being due to Coulomb interference. Then from the Coulomb interference, one would try to extract the Coulomb amplitude, and from the Coulomb amplitude the form factor. We would then see if the form factor that one obtains - the form factor of He^4 - was really the same form factor as you obtained from electron scattering or if it differed. If it differed you would say it was due to the finite size of the pi meson - to the form factor of the pi meson. A number of people are trying this experiment. It is not clear whether the experiment can be analyzed unambiguously. In the analysis one must take into account, of course, the fact that the π^+ and π^- purely nuclear phase shifts are changed because of the Coulomb interaction. The π^+ is repelled a little, the π^- attracted a little by the Coulomb interaction. So their strong interactions are different.and one has to take that effect into account, and be able to analyze it well enough in order to make sure that one can extract from the experiment, truly, the Coulomb amplitudes. It takes then a rather large amount of study, most likely studies at different energies, in order to make sure.

We want to turn now to the strong interaction; first from the point of view of symmetries. It is, of course, interesting in the strong interactions to check again time reversal invariance. The question has been raised again by the K^0 decay experiment, that time reversal invariance might not hold in the strong interactions and therefore there is great interest in trying to check this. People have thought about possibilities, and they are none of them very easy, if you want to check it to a reasonable degree of accuracy. As an example of one kind of check which has been done in the past, we have the equality of the asymmetry in proton-proton scattering with the polarization; that is, two types of experiment can be done: one with a polarized beam on an unpolarized target (where you measure the asymmetry); one where you take an unpolarized beam and measure the outgoing polarization. The equality has been observed, but not again to a very wonderful degree of accuracy.

The second invariance principle, which we did have considerable discussion about, was charge independence or charge symmetry. There are various types of evidence about this. From an experiment, not discussed here, but done quite some time ago, there have been studies of the (d d) reaction giving $He^4 + \pi^0$, which doesn't happen by charge symmetry, and has also not been observed to a fairly good degree. The cross section limit is quite good on that. Another experiment relevant to charge symmetry discussed is the experiment on the neutron-neutron scattering length, which is of course a low-energy phenomenon but comes in here because it's measured by pi-meson absorption on deuterium; by measuring the neutron spectrum rather than by measuring the gamma ray spectrum, one can do a rather good determination of the scattering length as discussed by Dr. Haddock. The result he gives for the neutron-neutron scattering length is 16.5 ± 1.3 Fermis, in good agreement with the expectation from the proton-proton data, where, if one extracts the Coulomb effects and gets a nuclear result, this result was quoted as being between 16.6 to 16.9, (in excellent agreement). It was pointed out by Dr. Signell that the analysis of the proton-proton data is not unambiguous, and that the quoted result depended upon the use of hard-core potential in extracting the Coulomb effects. As is well known, the neutron-proton scattering length is quite different from this. However that's a very sensitive question, as we know, because of the fact that we are very close to the virtual singlet S state of the deuteron, and it has been suggested that this can be understood (this rather large apparent violation of charge independence) by such things as the mass differences between the pi mesons and possibly mass differences between the vector mesons if that is a meaningful concept. So if the violation of charge independence seems rather large, it is not necessarily so significant. Then Professor Breit, in the discussion of the nucleon-nucleon phase shift analysis, making use of the one-pion exchange term to describe the long range part of the interaction, fitted the proton-proton data and the neutron-proton data independently. He gave these, I think, as his latest results: for g^2, (the pion-nucleon coupling extracted from the data) for the p-p data, 15.1 ± 0.4; for the n-p data, 13.9 ± 0.9. This agreement is perfectly good within statistics; however we were told, this morning, by Professor Rose, that we should not believe anybody's errors (that they all are too low) and that, therefore, the agreement is even better (if that's the way to say it). Furthermore, we were told by Professor Breit that he can explain this disagreement (between 13.9 and 15.1), which doesn't exist, by making coulomb corrections. So the situation is too good to be true.

I might make one comment about the number of 13.9, which comes from a very detailed analysis of much data by Professor Breit. It is remarkably similar to the result quoted by Ashmore, et al, who analyzed simply the differential cross section at large angles at one energy for n p scattering, and extrapolated to the pole, and got a value of 14.3 ± 1. So this has stood up remarkably well.

One other symmetry subject, which I think is of some interest, is the question of symmetry breaking. There is, as you know, a great deal of interest in elementary particle physics in the subject of symmetry breaking. This is, of course, because the unitary symmetry, the SU(3) symmetry, is very much broken and we can only understand unitary symmetry when we understand its breaking simultaneously. Now the SU(2) symmetry, the isotopic spin symmetry, is really a very good symmetry, and we can understand it perfectly well without understanding its breaking. Nevertheless, it we want to understand the general models of symmetry breaking, it's very useful to see if we can understand the symmetry breaking in the case of the SU(2) isotopic spin symmetry. One of the kinds of things that we might try to understand, or to study rather, is the symmetry breaking as it shows itself up in pion-nucleon scattering. A study has recently been made by Ollson, reported in PHYSICAL REVIEW LETTERS, who tried to deduce from pion-nucleon scattering data (I don't know how really reliable this analysis is, but it's a nice idea to try to do this) the mass difference between the N^{*++} and N^{*0} as observed from π^+ - proton and π^- - proton scattering and got a mass difference of -0.45 ± 0.85 MeV. Of course, the very small value of the mass difference incidentally is in itself an evidence of charge independence (i.e., the fact that the resonance occurs at the same place in π^- - proton and π^+ - proton scattering). But the mass diffe rence itself provides a challenge to the theorists who think they can calculate symmetry breaking. I won't try to compare it with a theoretical number, however.

Finally, I want to turn to subjects, not of symmetry, but of dynamics. That is, we want to ask about the things that cannot be explained by symmetry. We heard a great deal about phase shift analyses of pion-nucleon and nucleon-nucleon scattering. There is really considerable interest in the question of low-energy pion-nucleon scattering. One reason that there is interest in trying to get better values for low energy pion-nucleon scattering is a somewhat tangential reason (I just mention it as an aside), the fact that people now have very ambitious efforts to measure the parameters in the decay of the Λ and Σ hyperons in order to test time reversal invariance in non-leptonic decays. In particular, the so-called β parameter (if you know about this subject), is the one which measures time reversal violation; it measures essentially the relative phase of the S and p wave outgoing amplitudes. However the S and p wave amplitudes are not exactly in phase even if time reversal is good because of the final-state interactions, which are described in terms of the phase shifts for the pion-nucleon system. So, if one insists on doing this experiment very accurately (there are two groups: one at Brookhaven one at Cern, who are taking millions of pictures and so forth) it is very useful to have as accurate as possible scattering data at low energies, in the case for the Λ decay around about 40 MeV pion energy.

What was discussed here were the phase shift analyses of the pion-nucleon and nucleon-nucleon scattering. The thing which is most striking about these is that in the nucleon-nucleon case we see no resonances. In the pion-nucleon case, we can't help but find too many resonances. It seems a little unfair. The nucleon-nucleon case,

case, of course has a deuteron, it has the singlet S virtual state at low energies, but after that there seems to be no resonances. The typical phase shift shown by Breit looked like Figure 2. The phase shift as a function of energy goes up, gets very frightened, and comes down again instead of trying to find a resonance. On the other hand, in the pion-nucleon system, there are a large number of resonances. They are listed in Figure 3 for the isotopic spin 1/2 case, which is quite striking. A new resonance is seen at 1400 MeV in production experiments (if that is the true interpretation of the production experiments) done by Cocconi's group at CERN, and by the Brookhaven-Carnegie Tech collaboration, in inelastic high-energy proton-proton scattering. This is presumed to be related to the P_{11} phase shift, which resonates in some analysis. There is the $\frac{1}{2}^-$ with its claim to 2 resonances, rather horribly close together; the $3/2^+$ state that is rather badly abused and doesn't have any resonance; the $3/2^-$ state resonance at 1520. These are again only rough energies, not the same rough energies, perhaps as given by Dr. Roper, but equally rough. Finally, there are two resonances at 1680, the $5/2^+$ and the $5/2^-$. So in the $I = \frac{1}{2}$ state one may have six resonances below 1 BeV pion lab energy. In the $I = 3/2$ state there are not so many, just the old (3,3) and that little knee around 800 MeV which is interpreted as an s wave resonance perhaps. This large number of resonances provides a challenge for the people in elementary particle physics because they are bound to an ideology which demands that every resonance found here with zero strangeness and $I = \frac{1}{2}$ has to be part of a multiplet, either an octet, or possibly, though not very likely, an anti-decuplet. So it means that every one of these has to have at least six other partners, and in the case of these newly discovered resonances at 1400, 1520 and 1680 MeV, most of the partners are not known and that may be embarrassing, although since they have just been found in the N^* system perhaps they can be found later in the Y^* a nd the Ξ^* in elementary-particle physics. But certainly these discoveries at this lower energy provide an important challenge to the people in higher energy physics.

Now I want finally to make a general remark about the conference, and an explanation of name of this conference which is "Intermediate Energy Physics". This explanation is in part for the aid of the people who come from foreign countries. The thing is that in order to understand the name of this conference - one needs to know a little about physics here. One of the first things is that every paper that appears contains a title which is followed by either a * or a +. The * does not mean that the entire paper has been moved to California, and the + does not mean that the entire paper has been buried in Columbia. What it does mean, if you look at the bottom of the page, is that the entire paper has the support of some collaborators who only are given by initials and who have contributed money to the experiment, and that's why they are mentioned at the bottom. Now these people who provide the money at one stage had some difficulties, for technical reasons that I can't explain for lack of time (and lack of ability), in supporting this area of physics and so they decided to invent a new bureau - a sub-bureau, department, or whatever you call it - which they labelled I E P. Now the thing which is perhaps not so well known is that this stood for intermediate expense physics, but since you are not supposed to (in conference) discuss these things like money - that's not considered polite, and I apologize for mentioning the subject at all - why, the rather euphemistic expression that has been developed for conference proceedings is to call it intermediate energy physics. So that is the explanation of the title of this conference.

Now I only want, in concluding, to take my opportunity as the last speaker to thank Bob Siegel and the other people here for their effort in making this a very fine conference and for their warm hsopitality. I think that we all have found it a very interesting conference on certain aspects of nuclear physics and certain aspects of elementary particle physics.

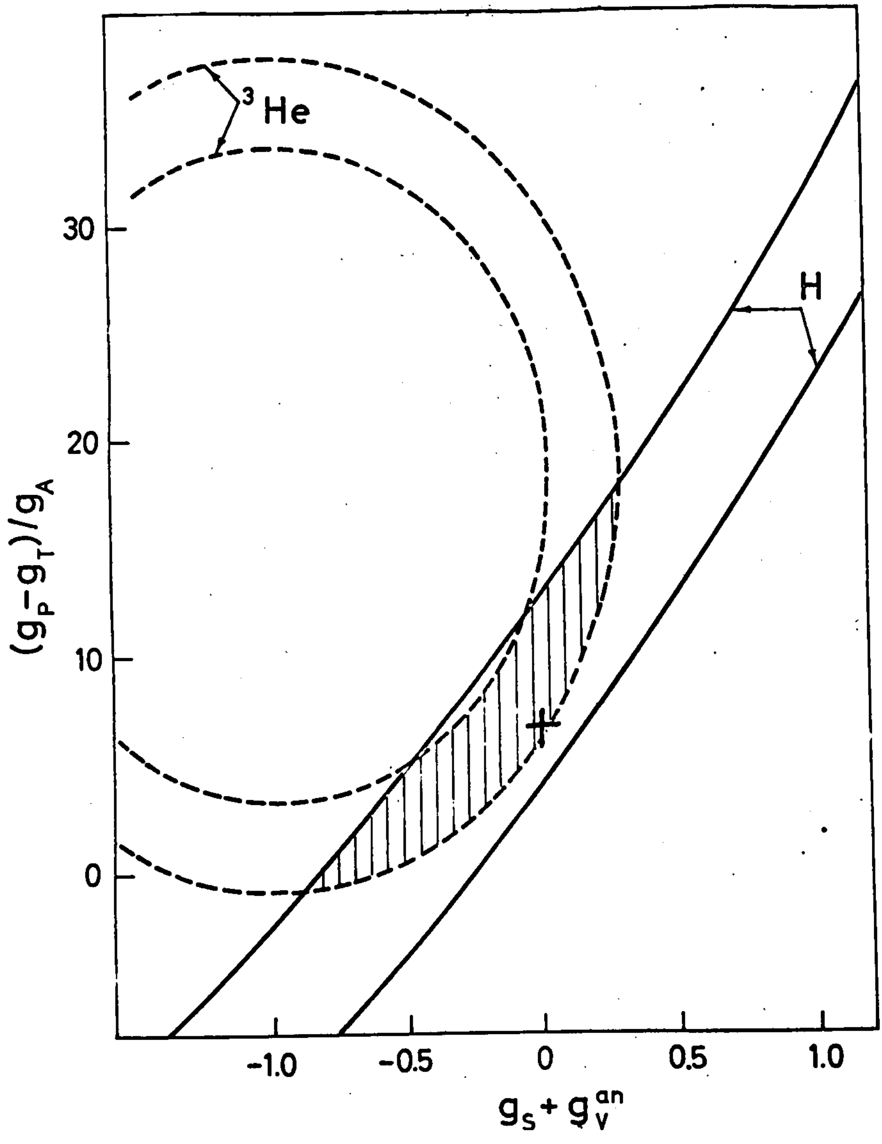

Figure 1. Coupling constants that fit experimental Muon Capture Rates in H and ^{3}He (assuming universal CVC values for g_A and g_M). The shadowed area indicates the region allowed by both experiments ($g_V^{an} = g_V - 0.97$).

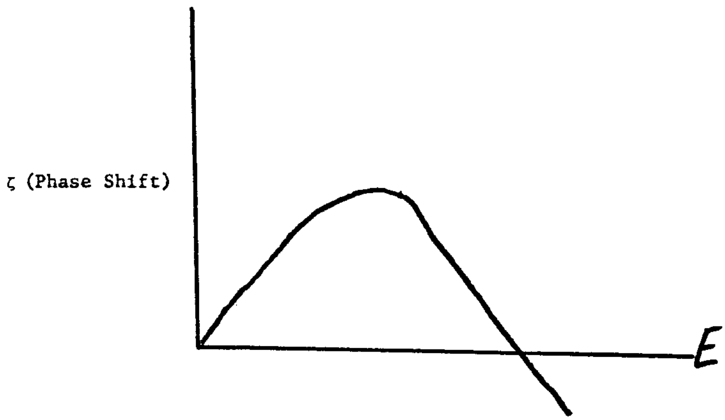

Figure 2. Typical Nucleon-Nucleon phase shift as a function of energy.

Figure 3. Pion-Nucleon Resonances

for I=1/2

J	P	E(Mev)
1/2	+	940, 1400
1/2	-	1520, 1680
3/2	+	None
3/2	-	1520
5/2	+	1680
5/2	-	1680

MORAVSCIK: I would like to mention one field that was not mentioned by either of the two speakers, and I think they simply didn't mention it because the conference didn't deal with it. However it belongs I think to the subject matter here, and this is photo-production processes, in particular, photo-production of pi mesons. Historically, this has been an important tool, in fact as important as pion-nucleon scattering, in getting information about pions in general and pion-nucleon interaction. I would also like to mention that there are a fairly large number of electron synchrotrons, some of them quite new in this sub BeV region - or almost sub BeV region - around the world. As probably some other people in I.E.P., they are maybe slightly demoralized in the sense that the first page of the New York Times usually goes to the events above 10 BeV. I think one of the things that this conference has done is to reassure people who work in this field that there is a considerable amount of very interesting work that is left to be done in the intermediate energy range, and I think this is also true for photo-production processes. For instance, just to mention one, there is much interesting information in using polarized gamma rays for pion production processes. Since there is no real representative of this breed at this conference, as far as I can remember, I just mention this as something that maybe should be there for the sake of completeness.

TELEGDI: There's an interesting aside with regard to the muon-electron universality discussed by Professor Wolfenstein and the general validity of quantum electro-dynamics. It must be mentioned that the only indication of the breakdown of quantum electro-dynamics nowadays are the well known pair experiments of Pipkin and associates at CEA. Corresponding muon pair production experiments either show no anomaly at all or an anomaly in the opposite sense, so I don't know whether this is an antindication of μ - e universality or quantum-electrodynamics, but it is amusing to note this particular discrepancy at this high momentum transfer process.

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