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FOREWORD

When the Organizing Committee of the 8th Rochester Conference on High Energy Physics started to plan the programme of this Conference, it was soon realized that the increasing development of the subject did not allow the usual scheme of the organization to be easily followed in a sufficiently short time. Therefore, it was suggested that a new scheme should be tried, based on the presentation of reports on the different parts of the subject by rapporteurs, whose duty it would be to collect the information from the original contributors and to present this information in the most compact form for discussion. It was hoped that a considerable part of the time could then be devoted to the discussion of the most interesting topics. However, it was also realized that it might well be an impossible task for the rapporteurs to give sufficient emphasis to all the original contributions, and even further that in the discussions this omission might not be fully repaired. Therefore, we thought it would be a good idea to publish in the Proceedings not only the reports and the discussions, but also some short original contributions, for which the following requirements were asked: firstly, that the contribution should not have been adequately presented in the reports and the discussion; secondly, that the contribution should not be published as such, or even as a part of a major paper, within one year after the Conference.

As a matter of fact, the number of contributions which satisfied both of these requirements, especially the second, proved to be small. These contributions appear in the Appendix of these Proceedings. An exception was also made for the pure theoretical sessions which could not be organized by the rapporteur method. Feynman has clearly explained the reasons (see Session 7, p. 216).

The Organizing Committee which is responsible for the publication was fully aware of the fact that it is very important to have the minimum delay between the end of the Conference and the publication of the Proceedings.

It was therefore decided to sacrifice uniformity of editing to the necessity for very quick publication, and it was also decided not even to produce a preliminary version of the Proceedings. The necessity for speed may also have resulted in typographical errors left uncorrected. For the same reasons the completeness of bibliographical references was rather sacrificed. We hope that our good intentions are clear and that our shortcomings, for which we apologize, will be excused.

Finally, I have much pleasure in thanking the CERN Scientific Information Service and all the scientific secretaries and undersecretaries whose help has been essential in the collection of notes of the discussions and in the editing of the Proceedings. Particularly I wish to thank the secretary of the Organizing Committee, Dr. Prentki, for his efficient and energetic collaboration, and also Dr. de Shalit for the method suggested for recording the discussions.

B. Ferretti
NUCLEON STRUCTURE - Experimental

W. K. H. PANOFSKY, Rapporteur
Stanford University, Stanford (Cal.)

This report deals with a subject which in certain respects is broader, and in others narrower, than the title indicates. It is broader in that I would like to include the experimental information on the limits of validity of quantum electrodynamics and some comments on the interactions of \( \mu \) mesons; it is narrower in that almost all questions involving strongly-interacting particles, which will be dealt with in other sessions of this Conference, have a direct influence on nucleon structure. A major reason for combining the discussion of the limits of electrodynamics and of nuclear structure is that in some, but not all, of the phenomena discussed here the two questions cannot be separated.

I. Limits of quantum electrodynamics

In order to discuss the quantitative meaning of experiments bearing on breakdown of quantum electrodynamics (QED), one needs a model of how such deviations might occur; anticipating the conclusion of this talk, we can say here that no direct experimental evidence on such a breakdown exists at this time. Discussion of electrodynamic limits involves the introduction of a minimum length or maximum momentum into the theory. In an extreme view, such a length can be a distance within which special relativity fails to hold. On a less radical basis, within the framework of special relativity, such a cut-off can be introduced as a limit on the invariant four-momentum transfer \( q^2 = p^2 - AE^2 \) which is characteristic of the process. In experiments dealing with photons and electrons this number is negligibly small for real particles. Hence deviations would manifest themselves in terms of cut-offs on virtual particles far off the energy shell which occur in the various diagrams contributing to the process. Such cut-offs can thus occur for either virtual photons or virtual electrons; there is no obvious reason why the two parameters should be equal. Following Drell\(^{1}\) we shall quote the end results of experiments in terms of a modified propagator for photons,

\[
\frac{1}{q^2} \rightarrow \frac{1}{q^2} + \frac{1}{q^2[1 - (\hbar A_e/q)^2]} \quad (1)
\]

for virtual photons of four-momentum \( q \); and

\[
-\frac{1}{p^2} \rightarrow -\frac{1}{p^2} + \frac{1}{p^2[1 - (\hbar A_e/p)^2]} \quad (2)
\]

for virtual electrons of four-momentum \( p (\gg m_e) \); \( A_e \) and \( A_e \) are then the cut-off parameters (in cm\(^{-1}\)) for the photon and electron propagators respectively. We are not trying here in any way to justify the specific form of Eqs. (1) and (2): they are used only to standardize the meaning of the "minimum distance" at which QED is valid experimentally.

A large quantity of the experimental information bearing on this subject is in published form; experimental details will be given here only if they are not readily available elsewhere.

A. Experiments defining the limits on the photon propagator cut-off

It is well known that the proton form factors as observed by electron scattering\(^{2}\) produce a 0.12 Mc/sec correction to the Lamb shift; this is within the present uncertainty of the measurement and its interpretation. Hence the Lamb shift cannot give us a limit on \( A_e \) that is tighter than that inferred directly from the e-p scattering experiments. We will discuss later the status of the e-p scattering experiments of Hofstadter and collaborators; at this point we will only recall that an r.m.s. radius of 0.8 \( \times 10^{-13} \) cm is a reasonable fit to the behaviour of the data at low momentum transfers. In dealing with e-p scattering we have no formal method for distinguishing nucleon structure and electrodynamics. On the other hand, it is exceedingly unlikely that a cancellation between electrodynamic and nucleon structure effects will occur. Since a finite size of r.m.s. radius \( \langle r \rangle \) reduces the scattering amplitudes by a factor \( 1 - \frac{1}{r} q^2 \langle r \rangle^2 \) for small \( q \), we conclude that on the basis of the e-p scattering experiments

\[
A^{-1} < \frac{1}{2} \times 10^{-13} \text{ cm}.
\]

The ambiguity between nucleon structure and electrodynamic effects can in principle be resolved by e-e scattering experiments. However, the invariant momentum transfer in an event where an electron of energy \( E \) strikes an electron of mass \( m \) at rest in the laboratory is given by
Fig. 1. Interpretation of electron-electron scattering experiments in terms of a photon propagator cut-off $A$. The two straight lines represent the potential cut-off parameters which could be determined if an $e-e$ scattering experiment were carried out at a specified accuracy ($1\%$ and $10\%$ are shown) at a particular laboratory energy. Shown also are (a) the highest energy at which $e-e$ scattering has been performed, (b) $e-e$ scattering experiments in progress, and (c) colliding-beam experiments proposed.

$$q = (2mE)^{1/2};$$  \hspace{1cm} (3)

this means that the center of mass motion makes the value of the momentum transfer much smaller than in experiments involving heavy particles. Fig. 1 shows the present status of $e-e$ scattering experiments. The highest energy of a completed experiment is 15.7 MeV (University of Illinois), much too low to be of interest here. Two experiments are in progress at Stanford University: one (Dally and Hofstadter) using magnetic analysis and counters (up to 1 GeV); and a second (Poirier, Bernstein and Pine) studying $e^{-}\bar{e}$ collisions in beryllium with a cloud chamber (200 MeV lab energy). I consider it extremely unlikely that either experiment will give information on the limits of electrodynamics beyond that already established by $e-p$ scattering; the most likely results will be measurements of radiative corrections.

Very exciting possibilities in pushing these limits further exist in colliding-electron-beam experiments. The idea is not new. Parameters of two projected experiments are shown in Fig. 1. MURA has constructed a model which permits circulation of electron beams of 50 MeV each, of presumably sufficient intensity to permit $e-e$ scattering experiments at a value of $q = 100$ MeV, equivalent to 10 GeV laboratory energy. A more ambitious experiment has been designed by O’Neill and collaborators; the basic layout is shown in Fig. 2. The 500 MeV linear accelerator beam is alternately deflected into two storage rings; the rings have RF accelerating systems to compensate the radiation loss. Detailed studies on beam dynamics, potential counting rates and background conditions have been carried out; the experiment looks feasible, and would push our knowledge covering the electrodynamic cut-off radius down by an order of magnitude.

Fig. 2. Layout of planned 500 MeV colliding-beam electron-electron scattering experiment. Note the double inflection system bending the beam into two storage rings.
**TABLE I**

<table>
<thead>
<tr>
<th>Experimenters</th>
<th>Values of $\frac{g_{\mu^+}}{2}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Assuming $\mu^+$ mass of Crowe, Cohen and DuMond $^{15}$</td>
<td>Inequality based on $\mu$-mesic absorption edge $^{19}$</td>
</tr>
<tr>
<td>Lundy et al. $^{15}$</td>
<td>$1.0015 \pm 0.0006$ &gt; $1.0011 \pm 0.0004$</td>
</tr>
<tr>
<td>Coffin et al. $^{16}$</td>
<td>$1.0026 \pm 0.0009$ &gt; $1.0021 \pm 0.0007$</td>
</tr>
</tbody>
</table>

The experiment of Lundy et al. $^{15}$ is an extension of the original meson parity experiment: it is a passive detection scheme (rather than the active induced-transition scheme of Coffin et al.) in which the information available from the muon precession rates in a large (3700 gauss) magnetic field is fully utilized. Positive muons are stopped in an absorber (Bromoform in the best run) in such a magnetic field; the detection probability of a decay positron at a given angle is then a rapidly varying sinusoidal function of time (near 50 Mc). If the detectors are gated at a frequency near the precession frequency, the counting rate will be a steep function of the frequency difference between the counter gate frequency and the precession frequency. In fact it is easy to show that the plot of counting rate vs gating frequency will exhibit a resonance-like character of full width at half-maximum $\Delta \omega = 2/T$, where $T$ is the muon mean life. This relation is equivalent to the width-defining factors in the "active" resonance method of Coffin et al. $^{16}$. Fig. 3 shows the gate programme and probability of positron decay in a given direction as a function of time. Fig. 4 shows a resonance curve in Bromoform. This new measurement gives a limiting value of the $\lambda_{\pi}$ parameter about four times the $e-p$ scattering amount; it is perhaps more profitable to interpret the measurement as a new determination of the muon mass, as is suggested by the authors.

**B. Experiments defining the limits on the electron propagator cut-off**

In discussing the $e-e$ scattering problem, we pointed out that the difficulty in reaching large momentum transfers rests with the c.m. motion as expressed by Eq. (3); on the other hand, the $e-p$ scattering experiments cannot distinguish nucleon structure and electrodynamic effects.

$$\begin{align*}
(k - p_+)^2 &= -2k \cdot p_+ \quad (k - p_-)^2 = -2k \cdot p_- \\
&= -2k_\beta E_1 \quad = -2k_\beta E_1 (1 - \beta \cos \theta_+) \\
\end{align*}$$

Fig. 3. The "stroboscopic coincidence" method of Telegdi. The diagram shows the disposition of the apparatus. $C_\mu$ is the muon counter, $C_e$ the electron counter and $M$ the moderator. The upper curve shows the time sequence of gates. The lower curve shows the rate of emission of decay electrons in a given direction, neglecting the decrease in amplitude.

Fig. 4. Coincidence rate as a function of the frequency ratio between precession rate and gate rate (Lundy et al.). [0° detector position, Bromoform, Gate Frequency $v_0 = 48.63$ Me/s]

Fig. 5. The two principal Feynman diagrams for the production of electron-positron pairs by photons in the Coulomb field of the proton. The diagrams are drawn for the positron emitted at large angles. In (a) the intermediate electron is far off the energy shell; in (b) the intermediate positron is almost real.
An experiment avoiding both these difficulties is in progress at Stanford by B. Richter\(^{19}\). The experiment consists of measuring the cross-section for large-angle positron-electron pair production in hydrogen by Bremsstrahlung X-rays. To understand why this experiment circumvents the difficulties enumerated above, consider the two lowest-order diagrams contributing to the process (Fig. 5). Both diagrams are drawn to lead to a positron emitted at a large angle. In diagram (a) the electron in the intermediate state is forced "off the energy shell" by an amount depending on the photon and positron energies; a large invariant momentum transfer can thus be involved. The second part of diagram (a) represents only the scattering amplitude of e–p scattering, which we assume known; the fact that the incident electron is virtual does not affect this conclusion, since the e–p scattering amplitude is a function of a single invariant momentum transfer as long as the collision is elastic. We assume here that the kinematic conditions are controlled experimentally so that meson emission cannot take place. These remarks also apply to diagram (b), except that the intermediate-state positron is almost real, and thus the invariant momentum transfer is very small.

Diagram (a) thus offers the possibility of examining the electromagnetic behaviour of a virtual electron of large four-momentum but at the same time using the proton as a "known" constraint to reduce c.m. motion. Hence at a given available laboratory energy a considerably larger sensitivity to determine a cut-off parameter — such as \(A_e\) in Eq. (2) — exists than in e–e scattering. On the other hand, by the use of the empirical e–p scattering cross-section the ambiguity in interpretation with nucleon structure is eliminated.

The actual sensitivity depends, of course, on the choice of experimental parameters. If only the positron is counted at large angles (as in the experimental work carried out so far), then diagram (b) will decrease the dependence of the measurements on the cut-off value. If both pair fragments are counted at large angles in coincidence, then both diagrams are equally affected by the cut-off and the sensitivity is increased. Table II indicates the situation.

The theory of this reaction has been considered in detail by Bjorken et al.\(^{20}\). In addition to the diagrams described above and their radiative corrections, other diagrams have been considered in which the incident photon is absorbed first on the proton (Compton diagram); it turns out that such diagrams cancel in the symmetrical arrangement (Method II); in Method I they are significant and set the upper limit of the usable incident energy, since the proton Compton effect calculations become uncertain at high virtual momenta.

Despite the contribution of the "insensitive" diagram, it is seen that Method I has considerably higher sensitivity to a potential cut-off than an electron-electron experiment at the same laboratory energy; Method II is very much superior.

The experimental situation stands as follows. A series of measurements has been completed in the experimental arrangement shown in Fig. 6. It should be noted that we are dealing with a cross-section in the range of \(10^{−35} \text{cm}^2 \text{MeV}^{-1} \text{sterad}^{-1}\). The electron beam produces thick-target Bremsstrahlung in a tantalum radiator of 0.1 radiation length and is then magnetically swept out. The X-rays strike a hydrogen target, and positrons at 90\(^°\) are counted in a double-focusing 180\(^°\) spectrometer (Fig. 7).

![Fig. 6. Disposition of equipment in the large-angle pair-production experiment of Richter.](image)

<table>
<thead>
<tr>
<th>Method</th>
<th>Bremsstrahlung limit</th>
<th>Positron (or electron) energy</th>
<th>(q^2)</th>
<th>Accuracy</th>
</tr>
</thead>
<tbody>
<tr>
<td>i) Single e(^+) counter at 90(^°) (present arrangement)</td>
<td>140 MeV</td>
<td>70 MeV</td>
<td>(115 MeV)(^2)</td>
<td>15%</td>
</tr>
<tr>
<td>ii) e(^+), e(^-) in coincidence; each at 30(^°)</td>
<td>500 MeV</td>
<td>249 MeV</td>
<td>(185 MeV)(^2)</td>
<td>10%</td>
</tr>
</tbody>
</table>
Two Cherenkov counters in coincidence constitute the detector.

Several experimental problems are of interest. The first is the question of absolute normalization. Since an absolute cross-section is needed here, it was decided to compare this cross-section with the yield of elastic electron scattering in hydrogen. This involves the problem of comparing the area of an elastic "line" with a continuum. This problem is complicated by the radiative straggling of the elastic peak. Analysis shows that dependence on the spectrometer characteristics can be eliminated by generating the elastic peak by varying the incident energy; results from a typical run are shown in Fig. 8.

The process to be observed competes with several reactions that must be eliminated by auxiliary runs. These are:

(i) elastic scattering of positrons produced in the target material ahead of the region seen by the spectrometer;
(ii) elastic scattering of positrons produced in the forward direction in the target hydrogen;
(iii) empty-target background. Table III shows a typical combination of runs which permit subtraction of such backgrounds.

### TABLE III

<table>
<thead>
<tr>
<th>Target</th>
<th>Target position</th>
<th>Radiator in addition to 0.1 R.L. ahead of magnet</th>
<th>Counts (6 \times 10^{13}) electrons</th>
<th>Corrected yield</th>
</tr>
</thead>
<tbody>
<tr>
<td>Full</td>
<td>Downstream</td>
<td>None</td>
<td>1.191 ± 0.117</td>
<td>1.39 ± 0.240</td>
</tr>
<tr>
<td>Full</td>
<td>Downstream</td>
<td>(\sim 0.02) R.L.</td>
<td>3.86 ± 0.311</td>
<td></td>
</tr>
<tr>
<td>Full</td>
<td>Upstream</td>
<td>None</td>
<td>2.14 ± 0.176</td>
<td></td>
</tr>
<tr>
<td>Full</td>
<td>Upstream</td>
<td>(\sim 0.02) R.L.</td>
<td>3.54 ± 0.422</td>
<td></td>
</tr>
<tr>
<td>Empty</td>
<td>Downstream</td>
<td>None</td>
<td>0.201 ± 0.058</td>
<td>0.227 ± 0.113</td>
</tr>
<tr>
<td>Empty</td>
<td>Downstream</td>
<td>(\sim 0.02) R.L.</td>
<td>0.202 ± 0.143</td>
<td></td>
</tr>
<tr>
<td>Empty</td>
<td>Upstream</td>
<td>None</td>
<td>0.135 ± 0.067</td>
<td></td>
</tr>
<tr>
<td>Empty</td>
<td>Upstream</td>
<td>(\sim 0.02) R.L.</td>
<td>0.101 ± 0.101</td>
<td></td>
</tr>
</tbody>
</table>

Yield from large-angle pairs 1.16 ± 0.26

---

*Fig. 7. Vertical cross-section through the large-angle pair-production equipment.*

*Fig. 8. Typical elastic electron scattering curve in liquid hydrogen in which the primary electron energy is varied, as used in the pair-production experiments.*
The reciprocal of the electron propagator cut-off parameter $\lambda_e$ vs. the ratio of cross-sections for large-angle pair-production, calculated with and without cut-off. The present experimental value is shown.

The result of runs carried out thus far, combined with the calculation of Bjorken et al., is

$$\frac{\text{Yield observed}}{\text{Yield point theory}} = 0.96 \pm 0.14.$$ 

In Fig. 9 the present experimental value is compared with the theoretical curve due to Bjorken et al. The lack of sensitivity in one direction is due to the 70% contribution of diagram (b) to the cross-section. The cut-off limit can be inferred from this figure.

The experiment is being continued in a new spectrometer which consists of a double magnet system: one magnet counteracts the dispersion of the other, giving an instrument more suitable to the measurement of continuous spectra (Fig. 10). Shielding up to a thickness of 6 ft of steel is employed.

It should be noted that, in principle, results similar to those reported here can also be obtained by a study of large-angle Bremsstrahlung if the photon is observed at a large angle to the incident electron; experimentally this problem does not look promising.

We see from the preceding discussion that at this time there exists no direct experimental evidence which limits the validity of QED; the range of observation extends down to 0.3 fermi for a photon propagator and down to about 0.6 fermi for the electron propagator, using the fairly arbitrary cut-off equations (1) and (2).

I will not report in detail on other experimental work in electrodynamics which has no direct relation to the high momentum limits of electrodynamics. Among such experiments are contributions on various electromagnetic phenomena such as Bremsstrahlung and pair production. I would, however, like to mention a contribution by Varfolomeev et al. The point in question is an effect first predicted by Landau and Pomeranchuk and followed up in papers by Migdal and others. The basic effect is as follows: at high energies Bremsstrahlung is directed forward into an angle of the order $\theta = mc^2/E$ due to the presence of denominators of the type $1 - \beta \cos \theta$, where $\beta c$ equals the electron velocity. These denominators basically describe the retardation effect due to the continued reinforcement of the photon field by the relativistic electron; the electron and photon stay in phase for a very large distance in the forward direction. Since this distance is so long, it has been predicted that multiple Coulomb scattering of the electron over this distance can destroy the coherence between electron and photon. Also the polarization of the intervening matter will attenuate the electric field. It is thus expected that the Bremsstrahlung cross-section at high energy will fall below the Bethe-Heitler value. Varfolomeev et al. have carried out Monte-Carlo calculations for a shower using both the Bethe-Heitler and the Migdal formulae as basic inputs. The calculations are compared with an integral particle count in showers of $10^{13}$ and $10^{12}$ eV in emulsion. In the $10^{13}$ eV shower the differences are not significant; in the $10^{12}$ eV shower the agreement with the Migdal curve appears to be significantly better. This "decoherence effect" and the "polarization effect" thus appear to be real. Evidence concerning these effects has also been observed by Mięsowicz et al.
II. $\mu$ meson interactions

It was my intention to include a report on high-momentum-transfer phenomena involving $\mu$ mesons; however, no new experimental material beyond the review by Fowler and Wolfendale has come to my attention. We are still faced with the problem of excess scattering for momentum transfers greater than 1 GeV/c over Coulomb scattering on extended nuclei; that either the theoretical or experimental alternatives to a truly anomalous interaction have been explored is not completely clear now. The situation regarding $\mu$ meson interactions underground is still less satisfying than that regarding scattering. I believe that the theoretical work on the virtual photon picture has not been complete enough to permit conclusions to be drawn. For example, the effect of finite nuclear size has been shown to have a major influence on the ratio of electron-induced to photon-induced processes. Similar influences are expected to affect the $\mu$ meson results.

III. Electromagnetic nucleon structure

I should like to devote the balance of this report to summarizing and bringing up to date the information on the electromagnetic nucleon structure obtained by various elastic and inelastic electron-scattering processes. My presentation is not intended to be a balanced review of the situation: almost all of the experimental material on high-energy electron scattering of Hofstadter and collaborators and most of the material on the electron-neutron interaction known to me has been published and will only be outlined here.

The electromagnetic interaction of a charged incident particle with a nucleon takes place with the nucleon charge-current (including the "normal" Dirac magnetic moment) and its anomalous magnetic moment. The structure of the nucleon manifests itself by multiplying each of these interaction amplitudes by functions of the invariant momentum transfer $q^2$. Invariance arguments indicate that not more than two such functions $F_1(q^2)$ and $F_2(q^2)$ are permissible. Such functions, the electric and magnetic form factors respectively, exist presumably to describe both the neutron and proton structures. In theoretical discussions it is often preferred to introduce a form factor that is an "isotopic scalar" $F^S$, i.e. the same for proton and neutron, and a form factor $F^V$ that is an isotopic vector, i.e. changes sign between proton and neutron. This means that in a purely formal sense effective values of charge $e$ and anomalous moment $\mu$,

$$e^S = e(F_{1p} + F_{1n}),$$

$$e^V = e(F_{1p} - F_{1n}),$$

$$\mu^S = \mu_p F_{2p} + \mu_n F_{2n},$$

$$\mu^V = \mu_p F_{2p} - \mu_n F_{2n},$$

describe the results, where the $\mu^*$'s are the anomalous parts of the static magnetic moment of the nucleons. If the momentum transfers are not too large, then the functions $F_1$ and $F_2$ can be visualized respectively as the Fourier transforms of the charge and magnetic moment distributions of the proton and neutron. In this sense, "models" of the nucleon charge and moment can be constructed and compared with the data. The Fourier transform of a distribution can be expanded as a power series in $q^2$; the leading terms are

$$F(q) = 1 - \frac{1}{6} q^2 \langle r \rangle^2 + \ldots ,$$

where $\langle r \rangle^2$ is the mean square radius of the distribution. However, such an expansion has little meaning in the range $q > 10^9$ cm$^{-1}$ ($\hbar = 1$) where most of the measurements discussed have been taken.

A. Electromagnetic structure of the proton

Our direct experimental information on electromagnetic proton structure rests on the work of Hofstadter and collaborators. Their latest report has been submitted to Rev. mod. Phys., and contains the description of and references to earlier work.

The apparatus is shown in Fig. 11. Liquid and solid (CH$_2$) targets are bombarded by energy-analysed electrons from the linear accelerator and the scattered electrons are angle- and energy-analysed by a 36° radius, double-focusing 180° spectrometer. A conical Cherenkov counter is used as detector.

The electric and magnetic form factors $F_1$ and $F_2$ enter into the angular distributions in different ways; hence these can be determined in principle by measurements of the elastic $e-p$ scattering cross-section over a complete family of primary electron energies and scattering angles. By comparing the yield ratios directly at different angles but at the same momentum transfer, $F_1/F_2$ can be determined directly. From such a measurement Bumiller and Hofstadter have obtained a value $F_1/F_2 = 1.12 \pm 0.20$ at momentum transfers of $q^2 = 9.33 \times 10^6$ cm$^{-2}$ and $1.16 \pm 0.20$ at $q^2 = 4.58 \times 10^9$ cm$^{-2}$. At large values of $q^2 F_1/F_2$ might or might not diverge further from unity.

The sensitivity of the measured cross-sections to $F_1$, relative to the sensitivity to $F_2$, is different at different angles. Differentiation of the usual Rosenbluth cross-section $\sigma_R$ yields numerically, if $F_1 \approx F_2 \approx F$

$$f_1 = \frac{1}{F_{1p} \sigma} \frac{\partial \sigma_R}{\partial F_1} = 2 + 0.123 q^2 \tan^2 \frac{\theta}{2},$$

$$f_2 = \frac{1}{F_{2p} \sigma} \frac{\partial \sigma_R}{\partial F_2} = 0.690 q^2 + 0.221 q^2 \tan^2 \frac{\theta}{2},$$

where $\sigma$ is the Mott cross-section and $q^2$ is measured in fermi$^{-2}$. Fig. 12 shows these relations graphically. It is seen that at small angles and low $q$ the sensitivity to the electric form factor is high, while at large backward
Fig. 11. Experimental layout of electron scattering experiments of Hofstadter and collaborators in the analysed electron beam of the linear accelerator.

Fig. 12. Relative sensitivity of electron scattering measurements to the magnetic form factor $F_{2p}$ over sensitivity to the electric form factor $F_{1p}$. Plotted is the ratio

$$
\frac{f_2}{f_1} = \frac{\partial \sigma / \partial F_{2p}}{\partial \sigma / \partial F_{1p}}
$$

against angle for various $q^2$. $\sigma$ is the scattering cross-section.

Fig. 13. Plot of the accumulated data (Bumiller and Hofstadter) on the proton form factors, assuming that $F_{1p} = F_{2p}$. The square of the form factor is plotted against the square of the invariant momentum transfer $q$. Plotted also is the computed value based on an exponential model of r.m.s. radius = 0.8 fermi.
angles the yields are more dependent on the magnetic form factor. Hence the data runs can be programmed to determine \( F_1 \) and \( F_2 \) fairly independently.

Fig. 13 shows the present status of the data on \( F_\rho^p \) plotted vs \( q^2 \), assuming that \( F_{1\rho} = F_{2\rho} \). The line drawn through the points is the curve

\[
F(q) = [1 - (a^2 q^2/12)]^{-2}
\]

which corresponds to an exponential charge model. The r.m.s. radius

\[ a = 0.8 \times 10^{-13} \text{ cm}. \]

As will be discussed later, it appears possible to obtain a measurement of the form factor \( e \), which is essentially \( F_{1\rho} \), from measurements of pion production by inelastic scattering of electrons in hydrogen near threshold.

Elastic scattering of electrons on the deuteron constitutes another method of studying the electric form factor of the proton. In the range of momentum transfers possible in the deuteron experiment, electric scattering predominates; in Born approximation the electric form factor of the deuteron, \( F_D \), is thus given by

\[
F_D^2 = F_1^2 (F_{1p} + F_{1n})^2,
\]

where \( F_0 \) is the deuteron form factor computed for point nucleons. Actually, there is a question which term on the right-hand side of (12) should be considered the unknown. McIntyre used the experimental data on \( F_D \) primarily as a tool to investigate the deuteron wave function, using Hofstadter and collaborators’ values for \( F_{1p} \) and taking \( F_{1n} = 0 \); Schiff \( ^{29} \), on the other hand, uses the experiments to analyse the data in terms of an upper limit on \( F_{1n} \). We can state here only that the elastic deuterium data are consistent with Hofstadter and collaborators’ values of \( F_{1p} \) and with \( F_{1n} = 0 \). We will return to this point later.

B. Electromagnetic structure of the neutron

Three experimental methods provide various types of information concerning the electromagnetic properties of the neutron:

1. Electron-neutron interaction at thermal neutron energies—gives \( F_{1\rho}(q) \) for small \( q \), i.e. a value of the r.m.s. electric radius.
2. Inelastic scattering of high-energy electrons on the deuteron—previously analysed in terms of \( F_{2\rho}(q) \).
3. Pion production by electrons near the photo-pion resonance—gives \( \mu^2(q) \); analysed in terms of \( F_{2\rho}(q) \).

The data on the low-energy electron-neutron interaction are as follows: Table IV shows the most recent values; no major changes from earlier values have occurred.

It is well known that the \( e - n \) interaction energy \( \delta V \), assumed extended over a sphere of radius \( r_0 = e^2/4mC^2 \) (other than the "Foldy term" which is due to the relativistic interaction between a moving charge and a point magnetic moment), is related to the r.m.s. charge radius by the relation

\[
\langle r \rangle^2 = r_0^2 (2\delta V/mC^2).
\]

The entries in Table IV are obtained from this.

2. The magnetic form factor of the neutron has been studied by inelastic scattering of electrons on the deuteron by Yearian and Hofstadter; their results are published in a series of papers \( ^{22} \), \( ^{27} \), \( ^{28} \). A typical scattering curve obtained is shown in Fig. 14. The figure shows an elastic peak in hydrogen (used to normalize the data) and the inelastic deuterium data. The inelastic deuterium scattering curve consists of

a) the quasi-elastic scattering from the neutron and proton broadened by the momentum distribution within the deuteron,

b) fast \( \pi^- \) mesons not completely eliminated by pulse-height selection,

c) inelastic electrons such as those leading to pion emission.

Yearian and Hofstadter use the theory of Jankus \( ^{29} \) with four-momentum transfer substituted for three-momentum

\[
\left( \frac{p_{f} + p_{i}}{2} \right)^2 = \left( \frac{p_{i}}{2} \right)^2 + (2\delta V/mC^2).
\]

The entries in Table IV are obtained from this.

3. The magnetic form factor of the neutron has been studied by inelastic scattering of electrons on the deuteron by Yearian and Hofstadter; their results are published in a series of papers \( ^{22} \), \( ^{27} \), \( ^{28} \). A typical scattering curve obtained is shown in Fig. 14. The figure shows an elastic peak in hydrogen (used to normalize the data) and the inelastic deuterium data. The inelastic deuterium scattering curve consists of

a) the quasi-elastic scattering from the neutron and proton broadened by the momentum distribution within the deuteron,

b) fast \( \pi^- \) mesons not completely eliminated by pulse-height selection,

c) inelastic electrons such as those leading to pion emission.

Yearian and Hofstadter use the theory of Jankus \( ^{29} \) with four-momentum transfer substituted for three-momentum.
transfer to complete the part (a) of these curves; at least approximately, the remaining contributions (b) and (c) agree with measured \( \pi^+ \) yields and theoretical calculation of inelastic electrons from pion production. The resultant curve for the electrodisintegration of the deuteron exhibits almost constant width under various experimental conditions.

In the approximation that the neutron and proton in the deuteron are free, we would have

\[
\sigma_D = \sigma_p + \sigma_n, \tag{14}
\]

where \( \sigma_D \) is the cross-section (obtained from the area of the inelastic scattering curve), and where \( \sigma_p \) and \( \sigma_n \) are the elastic proton and neutron cross-sections. In fact, because of various effects that will be discussed in the theoretical report at this session, Eq. (14) must be corrected by a small and somewhat uncertain correction \( \Delta \) to account for the specific effects of the deuteron. This means that

\[
\sigma_n = \sigma_p \left( \frac{\sigma_D}{\sigma_p} (1 + \Delta) - 1 \right). \tag{15}
\]

It has been pointed out by Drell that the uncertainties of the deuteron correction will be lessened (probably well below 10\%) if \( \sigma_D \) in Eq. (15) is not taken as the measured area of the deuteron experiment but is computed from the measured peak of the curve combined with the Jankus formula for computing the total cross-section. In the data the first method is called the "area method" and the second the "peak method" or "differential method".

Fig. 15 shows a set of the data evaluated under the assumption that \( F_{1n} = 0 \); \( F_{1n} \) is then computed from the peak method. Note that the existing data are for quite large values of \( q^2 \); specific models are almost impossible to discuss in terms of these data.

The question naturally arises: "Is the assumption \( F_{1n} = 0 \) valid at all?" And, if not: "What conclusions can be drawn concerning the relation between \( F_{1n} \) and \( F_{2n} \) if the assumption \( F_{1n} = 0 \) is dropped?" Finally: "What is known about \( F_{ln} \)?" We will proceed to analyse the data on the basis of the four form factors \( F_{1p} \), \( F_{2p} \), \( F_{1n} \), and \( F_{2n} \) respectively. To simplify the analysis we will take \( F_{1p} = F_{2p} \) and ignore the error introduced by this assumption. The equations then reduce to

\[
\left( \frac{\sigma_n}{\sigma_p} \right) \frac{F_{2p}}{F_{2n}} = f(q^2, E, Y), \tag{16}
\]

where \( E \) is the primary energy and \( Y = F_{1n}/F_{2n} \). \( Y \) was assumed to vanish in the data shown in Fig. 15. The function \( f(q^2, E, Y) \) is shown in Fig. 16 for \( E = 600 \) MeV. Some of the features of the curves are of interest: for low values of \( q^2 \) the function is a quadratic function of \( Y \) because of the contribution of "true" electric scattering. At large values of \( q^2 \) there is a strong linear dependence

Fig. 14. A typical experimental run on inelastic electron scattering on the deuteron (Yearian and Hofstadter). Shown are (a) the inelastic peak from deuterium, (b) contamination from \( \pi^+ \) mesons and inelastically scattered electrons leading to \( n^+ \) production, (c) the elastic peak in hydrogen used for cross-section normalization.

Fig. 15. The present data of Yearian and Hofstadter on the magnetic form factor of the neutron \( F_{2n} \), obtained by inelastic deuteron scattering. The data are reduced by the peak method: it is assumed that \( F_{1n} = 0 \) for all \( q \).
on $Y$ (which can assume both positive and negative values); this is due to interference between the Dirac moment and Pauli moment terms in the magnetic scattering. For each value of $q^2$ and $E$, the function $f[q^2, E, Y]$ reaches a minimum value as a function of $Y$; this is caused by the quadratic dependence of $\sigma_n$ on the electric form factor. From the existence of the minimum ($f_{\text{min}}$) of the function $f[q^2, E, Y]$ in Eq. (16), we can write

$$F_{2n}^2 \leq F_{2p}^2 \left( \frac{\sigma_n}{\sigma_p} \right)_{\text{max}} \left( \frac{1}{f_{\text{min}}} \right). \quad (17)$$

Hence, even without knowledge regarding $F_m$ we can derive an upper bound to the neutron magnetic form factor from the maximum values $\left( \frac{\sigma_n}{\sigma_p} \right)_{\text{max}}$ obtained from experiment. Table V gives the measurements of Yearian and Hofstadter at $E = 600$ MeV and the upper-limit magnetic form factor calculated from the data.

To obtain lower bounds on $F_{2n}$, more specific calculations are necessary, based on independent information on $F_{1n}$.

Consider the points of Yearian and Hofstadter taken at 600 MeV; each of these measurements defines a certain permissible area in the $F_{1n}-F_{2n}$ plane. In order to obtain a permissible range of values of $F_{2n}$, the range of the allowable value of the electric form factor must be defined.

The electron-neutron interaction result is not of help in this respect since it defines (in combination with the zero charge of the neutron) only the first two terms of a series expansion of $F_{1n}(q)$ in powers of $(qa)^2$ where $a$ is a length characteristic of nucleon dimensions; actually at the $q$ values of the measurements of interest, $(qa)^2$ becomes of the order of 10.

Schiff $^{2b}$ has placed a limit on $F_{1n}$ by constructing a model to the following requirements:

(a) the neutron is neutral;
(b) the neutron electric distribution has zero second moment;

TABLE V

<table>
<thead>
<tr>
<th>$\theta$</th>
<th>$q^2 \times 10^{-26}$ cm$^{-2}$</th>
<th>$F_{2p}^2$</th>
<th>$\sigma_n/\sigma_p$</th>
<th>$\left( \frac{\sigma_n}{\sigma_p} \right)_{\text{max}}$</th>
<th>$f_{\text{min}}$</th>
<th>$(F_{2n}^2)_{\text{max}}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>45$^\circ$</td>
<td>4.6</td>
<td>0.42</td>
<td>0.090$^{+0.122}_{-0.06}$</td>
<td>0.212</td>
<td>0.18</td>
<td>0.495</td>
</tr>
<tr>
<td>60$^\circ$</td>
<td>7.2</td>
<td>0.26</td>
<td>0.261$^{+0.070}_{-0.072}$</td>
<td>0.331</td>
<td>0.29</td>
<td>0.297</td>
</tr>
<tr>
<td>75$^\circ$</td>
<td>9.3</td>
<td>0.20</td>
<td>0.325$^{+0.098}_{-0.091}$</td>
<td>0.420</td>
<td>0.35</td>
<td>0.240</td>
</tr>
<tr>
<td>90$^\circ$</td>
<td>11.3</td>
<td>0.15</td>
<td>0.254$^{+0.257}_{-0.140}$</td>
<td>0.511</td>
<td>0.36</td>
<td>0.223</td>
</tr>
<tr>
<td>105$^\circ$</td>
<td>13.0</td>
<td>0.12</td>
<td>1.01$^{+0.49}_{-0.44}$</td>
<td>1.50</td>
<td>0.33</td>
<td>0.600</td>
</tr>
<tr>
<td>105$^\circ$</td>
<td>13.0</td>
<td>0.12</td>
<td>0.401$^{+0.191}_{-0.150}$</td>
<td>0.599</td>
<td>0.33</td>
<td>0.218</td>
</tr>
<tr>
<td>120$^\circ$</td>
<td>14.2</td>
<td>0.107</td>
<td>0.58$^{+0.386}_{-0.239}$</td>
<td>0.964</td>
<td>0.26</td>
<td>0.400</td>
</tr>
<tr>
<td>135$^\circ$</td>
<td>15.1</td>
<td>0.094</td>
<td>0.452$^{+0.202}_{-0.154}$</td>
<td>0.654</td>
<td>0.21</td>
<td>0.292</td>
</tr>
<tr>
<td>135$^\circ$</td>
<td>15.1</td>
<td>0.094</td>
<td>0.342$^{+0.064}_{-0.078}$</td>
<td>0.406</td>
<td>0.21</td>
<td>0.180</td>
</tr>
</tbody>
</table>
the electric distribution for large radial distance approaches that of the proton but with opposite amplitude, i.e. the electric form factor is an isotopic vector at small $q^2$;

the electric distribution must fit the data of McIntrye and Dhar. Schiff attempted to fit these conditions by a superposition of two radial exponential charge distributions of opposite sign plus a singular core to preserve neutrality.

In order to fit all the conditions, (c) could be met only at quite large distances and thus involves only a small fraction of the total charge. Schiff used the data of McIntrye and Dhar—condition (d)—by deriving the condition

$$1.0 > \left| \frac{F_{1n}}{F_{1p}} + 1 \right|^2 > 0.8, \quad \text{for} \quad q < 2.9 \text{ fermi}^{-1},$$

from the data using Eq. (12). At the present moment the limit appears to be set much less rigidly by the experiments, since:

(i) Calculations by Blankenbecler on a highly simplified deuteron model using bosons, but treating the problem relativistically, give a correction near 30% to Eq. (12) at $q = 2.9 \times \text{fermi}^{-1}$.

(ii) McIntrye's various deuteron models show a spread of about a factor of 2 at $q = 2.9 \text{ fermi}^{-1}$, although presumably only repulsive-core models need be considered.

(iii) There is some uncertainty in the value of $F_{1p}$. At the values of momentum transfer (up to 2.9 fermi$^{-1}$) used by McIntrye and Dhar and considered by Schiff the scattering is principally electric; hence the uncertainty in $F_{1p}$, as indicated by the errors shown in Fig. 13.

(iv) When the restriction imposed by Schiff, requiring an isotropic vector for the charge form factor, is combined with the experiment his model involves so little charge that it is physically uninteresting; therefore there remains no valid reason for fixing the relative signs of $F_{1n}$ and $F_{1p}$.

I tend therefore to place a somewhat wider margin on $F_{1n}/F_{1p}$ within the range of the observation of McIntrye et al. than is implied by Eq. (18).

A still more difficult problem is the continuation of a restriction on $F_{1n}$ beyond the range of observation. This is clearly impossible without a model. Any particular model such as the two-exponential model of Schiff, which was chosen to fit data in a certain range of $q$ with a minimum number of parameters, will probably produce excursions for larger values of $q$ which may be larger or smaller than the real amount. This situation is of course symptomatic of the fact that good information on the electric form factor of the neutron for large momentum transfers simply does not exist.

To illustrate the dependence of the data on $F_{1n}$, we will use the Schiff model. For purposes of analytical continuation of the limits imposed by the lower $q$ measurements, I will use (in Schiff's notation)

$$F_{1n} = - \frac{a^4}{(a^2 + q^2)^2} + \frac{\beta^4}{a^2} \left( \frac{1}{a^2 + q^2} + 1 - \frac{\beta^2}{a^2} \right),$$

where $a = 4.32 \text{ fermi}^{-1}$ corresponds to an exponential of r.m.s. radius of 0.8 fermi for the first term, and where

$$1.20a > \beta > 0.85a.$$

This choice permits an experimental spread of the factor $(F_{1n} + F_{1p})^2/F_{2n}^2$ in Eq. (18) from 0.70 to 1.35, corresponding to a variation of $\gamma = F_{1n}/F_{2n}$ of $\pm 0.16$ at $q^2 = 8.5 \times 10^{26} \text{ cm}^{-2}$.

Fig. 17. A typical function of $F_{1n}/F_{1p}$ which meets the conditions $\langle r_e \rangle^2 = 0, e_n = 0$ and the limits possibly set by the experiments of McIntrye and Dhar, calculated from the Schiff model with $\beta = 1.2a$ and $\beta = 0.85a$. Note that this is a totally arbitrary model.

Fig. 17 shows the “continued” limits on $Y$ under these assumptions. These limits are then applied to the measurements of Yearian and Hofstadter in terms of the permissible ranges of the magnetic form factor $F_{1n}$. The result is shown in Fig. 18, which contains:

(a) the values computed for $F_{2n}$ if we make the arbitrary assumption $F_{1n} = 0$ (shown as dots),

(b) values computed by the (also highly arbitrary) method of continuation discussed above, shown as arrows to indicate that other models would permit wider excursions,

(c) values computed from Eq. (17) which are upper limits to $F_{2n}$ (in the standard deviation sense).
Fig. 18. The values of $F_{2n}^2$ deduced from the data of Yearian and Hofstadter (area method), allowing for latitude in the value of $F_{1n}$. Shown are (a) values computed assuming $F_{1n} = 0$ (dots), (b) values computed using the arbitrary limits plotted in Fig. 17 (arrowheads indicate that these limits can be exceeded by arbitrary amounts), (c) values computed from Fig. 16 and Eq. (17) giving upper limits (in the standard-deviation sense) imposed on $F_{2n}^2$ for arbitrary $F_{1n}$ (indicated as “inverted ground” symbols).

From this figure, I conclude:

(i) The magnetic neutron moment is not concentrated at a point; specifically, it is unlikely that the radial extent of the neutron moment is smaller than about 70% of the extent of the proton moment.

(ii) No reliable upper limit on the neutron magnetic size can be set from these data.

I should like to state clearly that the solution

\[ F_{1n} = 0 \quad \text{for all } q, \]
\[ F_{2n} = F_{4p} = F_{6p} \quad \text{for all } q, \tag{21} \]

which has been extensively analysed by Hofstadter and collaborators, fits all the existing data. The object of the discussion here is to report on those conclusions regarding the form factors which can be drawn from the existing data without arbitrary simplifying assumptions.

Hofstadter informs me that improvement in the knowledge of $F_{2n}$ will result from:

(a) further measurements on the inelastic deuteron scattering, emphasizing the peak method;

(b) measurements of inelastic deuteron scattering at small angles to obtain evidence regarding $F_{1n}$ without need of using the elastic deuteron data; this will improve our knowledge of $F_{1n}$ at smaller $q$ values only, but even there the problem is difficult since the proton cross-section is much bigger than the neutron cross-section;

(c) extension of the elastic deuteron data to higher momentum transfers—this will be possible by virtue of the development of multiple channel detection equipment;

(d) coincidence measurements in inelastic deuteron scattering.

It should also be pointed out, however, that progress in the theoretical work on the deuteron will be necessary in order to narrow the choice of parameters considerably in the future.

(3) In addition to elastic and quasi-elastic scattering of electrons, another method yields quite direct information on electromagnetic nucleon structure: the study of the various cross-sections for pion production by inelastic electron scattering on protons.

Dispersion theory permits photo-pion processes to be expressed in terms of diagrams in which the photon interaction with electromagnetic properties of the physical nucleon is followed by the relevant meson-nucleon interaction. “Electro-pion” production as compared to “photo-pion” production can thus be analysed in terms of the “off-the-energy-shell” behaviour of the (photon)-
(physical nucleon) interaction, and thus gives the form factors of the nucleon property relevant to the process.

The only experiment carried out so far that can be analysed in terms of nucleon form factors is the electron-pion experiment of Panofsky and Alton [31]. In this experiment inelastically scattered electrons were observed with a low-resolution magnetic analyser; a liquid hydrogen target was bombarded with electrons up to 700 MeV energy. The initial and final energies were programmed together so that the pion (not observed directly) was emitted at the peak of the photo-pion resonance. As the primary and secondary detection energies are varied together, the momentum transfer \( q^2 = (\mathbf{p} - \mathbf{P})^2 - E^2 \) is varied, but the energy of the pion-nucleon system is not. The behaviour of the cross-section is shown in Fig. 19. The cross-sections are plotted as a function of two variables: \( E \), the energy of the \( \pi - N \) system, and \( q^2 \), the invariant momentum transfer. The data at \( q^2 \neq 0 \) are derived from the work reported here.

At resonance, about 75\% of the cross-section is contributed by the diagram shown in Fig. 20. In this diagram the absorption is magnetic dipole since the final state is a \( P \)-wave meson. The basic matrix element is proportional to

\[
(\mu_p - \mu_n) \sin \delta_{23} e^{i\delta_{23}},
\]

where the symbols have their usual meanings. However, \( \mu_p \) and \( \mu_n \) are functions only of \( q^2 \); hence the measured cross-section at constant energy \( E \) at resonance but variable momentum transfer \( q \), which corresponds to this diagram, measures \( \mu^p = \mu_p F_2^p - \mu_n F_2^p \).

There are of course many factors which cloud this simple picture. First, since the diagram of Fig. 20 does not represent all the possible channels, the percentage contribution and momentum dependence of other diagrams must be known. Despite the lack of detailed agreement between the dispersion relations and experimental photo-pion data, this situation introduces only small uncertainties. The reason is that the inelastic data (pion production by virtual photons) are interpreted relative to the experimental photo-pion production cross-sections by real photons. For instance, the dependence on the "minor" phase-shifts \( \delta_{34}, \delta_{13}, \) etc., which is not negligible in fitting photo-pion data, is not significant here. There is some admixture of longitudinal matrix elements (absent in photo-pion production); these are small, however, and are included in the theory.

The principal uncertainty in the dispersion-theoretical interpretation [32] stems from the lack of precise information on recoil terms at this time. Since the measurements are taken relative to the photo-pion production cross-section \( (q^2 = 0) \), only those recoil terms varying as \( q^2 \) are important. I will simply present here the data as compared to the calculation of Fubini et al.[32]; a more complete relativistic calculation is in progress.(*)

The measurements are complicated by the fact that electrons other than those from the processes under study—

\[
e + p \rightarrow p + e^+ + \pi^0,
\]

\[
e + p \rightarrow n + e^+ + \pi^+
\]

(*) Nambu (private communication to Drell) informs us that, as the present result of these calculations, a correction of \( \sim 25\% \) should be applied to the resonance cross-section at the largest values of \( q^2 \) used in the experiment (\( q^2 \sim 7 \text{ fermi}^2 \)). The sign of the correction would correct the measurements upward. Nambu estimates that the residual uncertainty beyond this correction is small.
Fig. 21. Comparison of theoretical (Fubini et al. 23) and experimental (Panofsky and Allton 31) data on electron-pion production. The curves shown are, from the top down: (a) point interaction: (b) to (h) $F_{1n} = 0$, $F_{2p}$ from an exponential model of r.m.s. radius = 0.8 fermi. Curves (b) to (h) are computed with variable neutron magnetic r.m.s. radii from 0 to 1.2 fermi in steps of 0.2 fermi (exponential model). Experimental points (squares) are computed by eliminating background by studying counting rates as a function of additional radiator. Experimental points (dots) represent an upper limit to the cross-section calculated by subtracting the theoretical large-angle Bremsstrahlung from the observed values. 

—also contribute to observed counts. Among the other processes leading to low-energy electrons are large-angle Bremsstrahlung, $\pi^0$ decays, Coulomb scattering in the hydrogen target of degraded primary beam electrons, etc. It can be shown that all these background processes can be eliminated by studying the yield of secondary electrons as a function of the thickness of radiating material in the primary beam.

The resultant data are shown in Fig. 21. The figure contains both the data corrected for the unwanted reaction by the “extra radiator” studies referred to and those computed by subtracting the largest of the background reactions (large-angle Bremsstrahlung) directly from the observed values.

The theoretical curves in Fig. 21 are computed using the values of $F_{2p}(q)$ from the work of Hofstadter and collaborators and setting $F_{1n} = 0$. The sensitivity to this assumption is slight, as is shown in Fig. 22: we show the theoretical curves for $F_{1n} = 0$ and $F_{1n} = 0$ for $\beta = 1.2a$, using the Schiff model [Eq. (19)]. No extrapolation is involved here since the range of values of $q^2$ is well within the range covered by McIntyre and Dhar 23).

Fig. 23 shows a composite plot of the values of $F_{2n}(q^2)$ vs $q^2$ for the data from pion production by electrons and from inelastic electron scattering in deuterium. $F_{1n} = 0$ is assumed; hence the various uncertainties concerning the deuterium measurements discussed previously should be taken into account in interpreting these data; on the other hand, the omission of the as yet only estimated recoil corrections to the electron-pion data should also be considered. Clearly the two methods of determining $F_{2n}$ are in agreement within these uncertainties and the experimental errors.

Attempts are being made to measure directly the value of $e^\gamma$, i.e. $F_{1n} - F_{2n}$, by studying electron-pion production near threshold. Fig. 24 shows the theoretical cross-sections for various values of electric form factors if the primary and secondary energies are programmed to correspond to $S$-wave $\pi^-$ electron-pion production (such as in the curve $E = 1100$ MeV corresponding to 23 MeV above $\pi^-$ threshold). Note that extreme care in defining the initial and final electron energies will be necessary due to the steepness of the excitation curves.

Conclusions. Let me try to summarize the present experimental conclusions without attempt at interpretation:
Session 1

(1) Quantum electrodynamics holds at least down to "distances" [defined by Eqs. (1) and (2)] of 0.3 fermi for photon propagators and about 0.8 fermi for electron propagators.

(2) The magnetic form factor of the proton is represented by a smoothly decreasing curve which can, for example, be reproduced by an exponential model of r.m.s. radius 0.8 fermi up to momentum transfers of \( q^2 = 20 \times 10^{26} \text{ cm}^{-2} \).

(3) The electric form factor of the proton equals the magnetic form factor to within \( \pm 20\% \) up to momentum transfers \( q^2 = 9 \times 10^{26} \text{ cm}^{-2} \).

(4) The radial magnetic extent of the neutron is at least 70\% that of the proton as observed at corresponding momentum transfers.

(5) The charge of the neutron is zero and the r.m.s. radius of the charge is less than \( 10^{-14} \) cm. The charge form factor of the neutron might easily become as large as 20\% of that of the proton at momentum transfers of \( 3 \times 10^{13} \text{ cm}^{-1} \) and is unknown above that value.

(6) The "structure effects" enumerated in (2)-(5) have not been separated experimentally from possible electrodynamic effects.

There is considerable hope that by the time of the next Annual Conference on High Energy Physics the latitude of conclusions such as these will have narrowed considerably.

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DISCUSSION — see p. 24.
NUCLEON STRUCTURE — Theoretical I

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Theoretical approximations and assumptions in obtaining nucleon form factors from the data

A) \( e^- (\text{electron}) + p (\text{proton}) \rightarrow e^- + p \) for proton structure

B) \( e^- + d (\text{deuteron}) \rightarrow e^- + p + n (\text{nucleon}) \) for neutron structure

C) \( e^- + d \rightarrow e^- + d \) for neutron structure

D) \( e^- + p \rightarrow e^- + \left( n + \pi^+ \right) + p + \pi^0 \) for neutron structure

The form factors are defined as follows by the electromagnetic vertex operator between real physical nucleon states:

for the proton

\[
G_p(q^2) \gamma_\mu + i G_{2p}(q^2) \sigma_\mu q^\nu,
\]

for the neutron

\[
G_N(q^2) \gamma_\mu + i G_{2N}(q^2) \sigma_\mu q^\nu.
\]

\( q^2 = -q_\mu q^\mu \) is the square of the four momentum transfer which will be given here in units of

\[10^{13} \text{ cm}^{-1} = (1 \text{ fermi})^{-1} \cong 200 \text{ MeV}.\]

We use units of \( \hbar = c = 1 \).

For theoretical analysis it is more convenient to consider the isotopic scalar and vector form factors defined by

\[
G_{1,2}^{P} = G_{1,2}^{S} + G_{1,2}^{V}; \quad G_{1,2}^{N} = G_{1,2}^{S} - G_{1,2}^{V}
\]

with

\[
G_i^S(0) = G_i^V(0) = \frac{1}{2} e;
\]

\[
G_i^V(0) = 1.85 \left( \frac{e}{2M} \right) = 1.85\mu_B = \frac{1}{2}(K_P + K_N)\mu_B;
\]

\[
G_i^S(0) = -0.06\mu_B = \frac{1}{2}(K_P - K_N)\mu_B.
\]

IA. Here there is no problem. Elastic scattering of electrons from protons gives \( G_1^P(q^2) \) and \( G_2^P(q^2) \) directly involving the exchange of two photons have been shown to be small \(^1\) in the range of present experiments, so that the measurements can be analysed using the Rosenbluth \(^3\) formula

\[
\frac{d\sigma}{d\Omega} = \frac{a^2}{4E_0^5} \cos^2 \frac{\Theta}{2} \left[ 1 + \frac{1}{E_0^2} \sin^2 \frac{\Theta}{2} \left( F_1^P(q^2) + \frac{-q_\mu q^\mu}{4M^2} \left( 2[F_1 + K F_2] \tan \frac{\Theta}{2} + K^2 F_2^2 \right) \right) \right],
\]

where

\[
G_1 = e F_1, \quad G_2 = \mu_B F_2,
\]

plus the Schwinger \(^4\) radiative correction. Assuming validity of the present quantum-electrodynamic field theory of the electron, one learns the invariant (normalized) functions \( F_1^P(q^2) \) and \( F_2^P(q^2) \) directly. Evidently measurements at different \( E_0 \) and \( \Theta \), but fixed \( q^2 \), provide separate information on \( F_1^P \) and \( F_2^P \). It is found \(^5\) that \( F_1^P / F_2^P \approx 1.0 \pm 0.2 \), up to momentum transfers \( \sim 3 f^{-1} \).

IB. Inelastic electron scattering from the deuteron,

\[
e^- + d \rightarrow e^- + n + p,
\]

leads to three final particles and thus to a continuous energy spectrum of scattered electrons at a given angle. For such an experiment to probe nucleon size there must be large momentum transfers. Since the deuteron form factor is very small for such large values of \( q \), in first approximation one can neglect the interference between scattering amplitudes from the neutron and the proton and express the cross-section as a sum of single particle cross-sections. A simple approximate sum rule gives the area under the inelastic curve—that is, the cross-section
There are corrections to this sum rule but we remark first that the contribution of the neutron in (3) is appreciable only for large scattering angles and high incident energies such that the magnetic scattering dominates in (2). This is because the neutron is neutral and, in addition, as first shown by Foldy \(^*\) \((**\), the second moment of its charge distribution is very small. In fact not until

\[ q = \sqrt{-q_{\perp} q^a} > 2.5 f^{-1}, \]

corresponding to scattering of 600 MeV incident electrons through 60° or 500 MeV electrons through 75°, does scattering from the neutron contribute as much as 30% as that from the proton. Hence under conditions for which (3) provides information on neutron structure, what is being measured is \((F_x N + K_N F_x N)^2\).

In principle, if not yet in practice \((**\), measurements at different \(E_0\) and \(\theta\) but the same \(q\) values give separate information on \(F_x N\) and \(F_y N\) as in the proton case.

In anticipation of more precise experimental numbers we inquire into corrections to approximate sum rule (3).

These have been discussed by Blankenbecler \(^*\) and may be summarized briefly as follows, in terms of a correction factor \(\Delta\):

Kinematic corrections contributing to \(\Delta\):

(a) Knowledge of the free particle cross-sections over a finite energy range is required by the width of the momentum distribution in the deuteron.

(b) Bound nucleons do not satisfy the free particle energy-momentum relation so that terms in the current operator which are proportional to \((E^2 - |p|^2 - m^2)\) may contribute.

Mesonic corrections contributing to \(\Delta\):

(a) Additional currents exist in the deuteron due to exchange of charged mesons.

(b) The nucleon electromagnetic vertex may be "warped" by meson exchange with the other nucleon.

(c) Effects of meson exchange between outgoing nucleons are not completely summarizable by a static interaction potential.

Blankenbecler has evaluated the kinematic corrections. For a physical picture of this effect consider two free nucleons moving back-to-back in arbitrary directions with a mean relative speed \(\langle v^2 \rangle = \frac{1}{M} \langle T \rangle.\) Evidently an electron scattering from them is scattering from a neutron or proton in a band of relative energies about the laboratory energy and there are corrections to Eq. (3) resulting from the curvature of the cross-section with energy (as well as angle, which must be properly transformed in relating relative and laboratory co-ordinates). Numerical values for this correction have been given by Blankenbecler who considered a Breit deuteron with instantaneous interaction potential. The current operator then reduces to the sum of free nucleon current operators and effects included are final state interactions, correct relativistic kinematics and phase-space factors. His results take the form

\[
\int \frac{d\sigma_D}{d\Omega} \frac{d\sigma_D}{d\Omega} = \left(1 + \frac{\langle T \rangle}{M} \right) \left(\frac{d\sigma_D}{d\Omega} + \frac{d\sigma_N}{d\Omega} \right),
\]

where both \(\delta\) and average ground state kinetic energy \(\langle T \rangle\) depend on the deuteron model. For 500 MeV incident electrons scattering through 135°, \(\Delta = +4.5\%\) for \(\langle T \rangle = 30\) MeV; it decreases for smaller scattering angles.

On dimensional grounds one expects kinematical correction (b) to be \(~\langle V^2 \rangle / M\) which may again contribute terms of the order of a few per cent. Without a knowledge of the bound current operator nothing more can be said.

One can discuss the mesonic corrections only in relation to their observed role in deuteron photodisintegration since there exists no successful theory of these effects from first principles. We do this as follows. At high energies, the cross-section for \(\gamma + d \rightarrow n + p\) is proportional to the Fourier transform of the deuteron wave function at the corresponding large momentum transfer. Both the simple one-body currents and the meson exchange currents contributing here are weighted by approximately the same two-body correlation function which falls off rapidly for large \(q\). Fig. 1 indicates the importance of meson exchange effects which give rise to a resonance when the two outgoing nucleons have a relative kinetic energy corresponding to resonant exchange of a meson. Taking into account centre of mass motion and folding with the deuteron form factor has the effect of displacing this resonance from a relative kinetic energy of 320 MeV to 250 MeV. On the other hand, electrodisintegration frees the energy from the momentum transfer by the virtual photon and can occur strictly as a one-nucleon process. At the peak of the inelastic electron spectrum the second nucleon is an idle spectator and the one-body currents contribute in proportion to the zero momentum amplitude of the deuteron

\(\text{(*) References to the original papers are contained in this very informative paper.}\)

\(\text{(**) Yearian and Hofstadter \(^*\) quote experimental errors of \(~20\%\) for the sum rule (3) so that \(\frac{d\sigma_N}{d\Omega} \) is not presently known to better than \(50\%)\) and it is not yet possible to provide a severe limitation on the ratio \((F_x N / F_x P)\). As a numerical example the observations at 600 MeV, 90° and at 500 MeV, 135°, corresponding to \(q = 3 M^{-1}\), can be fit by assuming \(F_x N = F_x P\) and with \(0 \gg F_x N \gg -0.7 F_x P\). A negative value for \(F_x N\) corresponds to the intuitive notion of negative charge extending beyond a positive core. We return to this question in part I C.\)
form factor, whereas the meson exchange effects are inhibited by a deuteron form factor at a momentum transfer of the order of the meson resonance. A rough calculation of this effect based on the observed photodisintegration resonance leads to a 10% increase in the total cross-section for 600 MeV electrons scattered through 135°. This increase is less important for scattering at lower energies and through smaller angles. It comes from the large energy loss collisions (**) corresponding to the left side of the curve in Fig. 2 and falls to a negligible value at the peak.

This result suggests that it is possible to avoid the meson corrections in (3) by turning to an analysis of the peak of the inelastic spectrum as a measure of neutron structure. The primary uncertainty here is the zero momentum amplitude of the deuteron wave function, the difference between a Hulthé and Rustgi (***) repulsive-core wave function being < 5%. A. Goldberg (***) has made an impulse approximation calculation of the inelastic spectrum and peak height, in particular, which takes into account the correct relativistic kinematics. As in the sum rule discussion, the peak analysis measures \( F_N^2 + K_N F_N^2 \) with an uncertainty of 50% with present experimental errors. Recent work of Marshak and collaborators on the important role played by final state potential interactions in photodisintegration contributes only small corrections in this analysis, since it is the low and not the high momentum components which are important here.

**Fig. 1.** Comparison between theories and experiment for deuteron photo-disintegration.

**Fig. 2.** Spectrum of inelastically scattered electrons from deuterium as a function of energy of the scattered electron for an incident electron energy of 600 MeV and a scattering angle of 135°.

I C. Elastic electron-deuteron scattering measures the isotopic scalar part of the charge form factor \( F_E^2 = (F_N^2 + F_N^2) \), since the nucleons scatter coherently in an elastic process. Also the isoscalar part of the anomalous moments is very small, Eq. (1), and contributes negligibly here. McIntyre and Dhar (***) have made measurements of the elastic cross-section up to \( q = 2.8 f^{-1} \) and therefore have probed beyond the second moment \( q^2 \) of \( F_N^2 \).

Schiff (**) has shown that any deviation of the neutron from a point with \( F_N^2(q^2) = 0 \) appreciably depresses the cross-section in the range \( 2 < q < 3 f^{-1} \). If this deviation is in accord with our intuitive notions, the cross-section reduces by 30% at \( q = 3 f^{-1} \), and that \(-0.10e\) reduces it by more than 60% and below the stated experimental errors.

However theoretical attempts to make a quantitative determination of the neutron's charge distribution on the basis of this process have run into difficulties of the relativistic two-nucleon problem. Blankenbecler (**) has studied relativistic corrections to the usual description of a deuteron as two statically bound Pauli nucleons by considering a simplified but calculationally feasible model of two bosons, one of which is charged, which satisfy a Bethe-Salpeter equation. For a separable potential model with correct scattering length and effective range (**) he found a 25-30% reduction in the cross-section at \( q = 3 f^{-1} \) below its value calculated using a Schrödinger equation. This reduction appears to be due in part to relativistic corrections to the current operator and in part to Lorentz contraction of the recoiling deuteron's wave function as seen in the laboratory system, which has the effect of reducing wave function overlap in the scattering matrix.

(*) The "resonance" between the outgoing nucleons occurs \( \sim 100 \) MeV below the peak and with a half width \( \sim 70 \) MeV.

(**) This corresponds to a potential which is repulsive and \( a \frac{1}{r^2} \) for \( r < 0.38 \) Fermi.

(***) The form factor is, of course, constructed to have zero or very small second moment.
element\(^{10}\)(\(^*\)). Perhaps this result may be best stated in a way which relates it to the sensitivity of the cross-section to the nuclear forces. A relativistic calculation whose static limit corresponds to a Hulthén deuteron gives a scattering curve close to that calculated for a non-relativistic deuteron with hard-core interaction. Such effects along with contributions from the D-state in a physical deuteron play an important role because the cross-section is small and depends on the high Fourier components of the deuteron distribution.

Finally we remark that elastic scattering at small \(q\)-values measures the proton charge radius but only in the combination \(<r_{2D}^2>+<r_{2P}^2>\), and so is not a sensitive way of determining \(<r_{2P}^2>\).

I D. The process

\[e + p \rightarrow e' + \left\{p + \pi^0 \atop n + \pi^+\right\}\]

probes the electromagnetic structure of the neutron and is free of the complications of the two-nucleon problem. This was first realized by Fubini, Nambu, and Wataghin\(^{15}\) who have given a dispersion theory analysis of this cross-section, and may be understood as follows. We write the matrix element for the process in Fig. 3 as

\[T_{\pi} = e A_{\mu}(q) \langle P_2\mid j^\mu(q)\mid P_1 \rangle\]

where \(\langle P_2\mid j^\mu(q)\mid P_1 \rangle\) is the photo-meson production matrix element for "photon" momentum \(q_\mu q^\mu < 0\).

Fig. 3. The matrix element for electron-production of mesons.

\[T_{\pi} = e A_{\mu}(q) \langle P_2\mid j^\mu(q)\mid P_1 \rangle\]

The theoretical problem is in principle, if not in fact, clear and well defined.

Experimentally\(^{10}\) one measures \(F_{1,2}(q^2)\) by programming runs for fixed relative pion-nucleon momentum, riding down the ledge of Fig. 5 near threshold to measure s-wave \(\pi^+\) production via the Kroll-Ruderman term, and hence\(^{10}\)

\[(F_1^p - F_1^N)^2 = (F_2^p)^2,\]

or riding down the peak to determine primarily the magnetic form factors. We note that

\[\langle P_2\mid j^\mu(q)\mid P_1 \rangle\]

is also a meson electromagnetic form factor in the meson current term but we set it to unity since this term is of secondary importance in the present Panofsky\(^{16}\) experiments. The theoretical problem is in principle, if not in fact, clear and well defined.

Fig. 4. The inhomogeneous terms in dispersion relations for electron production of mesons.

Fig. 5. Cross-section for electron production of mesons as a function of momentum transfer \(q_\mu q^\mu\) and of the relative pion-nucleon energy \(E\). The shaded curve at \(q_\mu q^\mu = 0\) is theoretically computed for photo-meson production. The curve and experimental points are taken from Panofsky's report (p. 16).
it is the isovector part of the moment form factor, \( F_1^{(\pi)} - (K^0 - K^0) F_3^{(\pi)} \), which dominates at resonance in contrast with the inelastic deuteron cross-section in which the neutron and proton contribute incoherently.

The theoretical difficulties centre about the problem of obtaining accurate solutions to the dispersion relations for the conditions of large momentum transfer \( q^2 \) and low energy of the final pion-nucleon system in its centre of mass, so that it is dominated by the 3,3-resonance. The large momentum transfer makes the static approximation, which was the reference point for the photo-meson analysis \( 17 \), a much poorer approximation here. Fubini, Nambu and Wataghin have formulated the dispersion relations covariantly taking only the 3,3-resonance into account in the absorptive amplitudes. Work on their solutions \( 18 \) is now in progress. The solutions which exist at present calculate the imaginary parts of the \( e-\pi \) amplitudes in the static approximation. The imaginary parts are then inserted into the relativistic dispersion relations to obtain real parts, i.e. only the resonant amplitudes in a multipole expansion have imaginary parts and these imaginary parts generate real amplitudes for all multipoles. The recoil effects are important because, for \( q \sim 3f^{-1} \), there is a relative velocity of \( v/c \sim 1/2 \) between

the natural system of dispersion relations (\( P_1 + P_2 = 0 \)) and the centre of mass system (\( P_1 = q = 0 \)) in which the phase of the amplitudes for each \( (J, L) \) and isospin \( I \) is given by \( e^{i\delta_{HL}} \) with \( \delta_{HL} \), the corresponding pion-nucleon scattering phase-shift \( 19 \). The recoil effects are found to play a more important role in generating new multipoles than in modifying the resonant ones.

It is difficult to define the quantitative accuracy of this present treatment but in view of the fact that the photo-production analysis is in error \( \sim 20\% \) at the \( \pi^+ \) maximum, and in view of other difficulties and uncertainties involving the crossing terms and the role of longitudinal quanta, it is impossible to claim better than \( \sim 30\% \) \( 19 \) \( (*) \).

In resumé to Part I, both theory and experiment have a long way to go before anything definitive can be said about the neutron beyond the fact that its moment is not a point but is similar to the protons, and that its charge has zero \( (< 0.1f) \) root mean square radius. The proton is in good shape, and in particular so is the isovector part of the moment form factor \( F_1^{(\pi)} \) for \( q < 2.8f^{-1} \) since the isoscalar part of the anomalous moment is very small,

\[ G_s^S(0)/G_{t}^V(0) \simeq 3\% \text{ and } F_1^V \text{ is still } \gtrsim \frac{1}{2}. \]

LIST OF REFERENCES — see p. 33.

DISCUSSION—Panofsky and Drell I.

**Hofstadter:** I think that the differences between Panofsky’s analysis of \( F_{2n} \) and ours are statistical and, I feel, trivial in comparison with the main results at the present moment. In fact, the “inverted grounds” that he shows in Fig. 18 move up as if an earthquake hit them when the experimental points change. A good deal of this has been said ten times over in different languages and I think it is mainly a question here of waiting for better data. One should not go over-board saying that \( F_{1n} \) is zero or not zero—it is better to wait. Experiments are in progress. We have results already, but unfortunately I have not been able to reduce them in time for this meeting. The differential method was not discussed in connection with \( F_{1n} \), and I feel that this is the better method in that case. In that respect we are also making a rather co-ordinated attack on many facets of the deuteron problem, such as the coincidence experiment, and I think that these will really give quite a lot of information. These different pieces of information will have to be consistent in order to be accepted. Moreover, McIntyre has new data which I believe support Schiff’s analysis quite well, and I think this is an indication that \( F_{1n} \) must be close to zero. Finally, there is complete consistency between our differential method and our area method, and this suggests to me that the meson-exchange effects that Drell spoke about are rather small. But perhaps I am moving in circles here where theoreticians would fear to tread.

**Drell:** Just one more comment on the meson-exchange effects. We estimated them to be of the order of 10% in our discussion at the board. They are certainly small compared with uncertainties in the neutron cross-section. The deuteron cross-section has an error of 20%. More than half of this cross-section is due to the proton with its own 20% uncertainty. So I think that the meson-exchange effects are within the error, and therefore I am in agreement with you when you say that your curve shows that they are small.

**Hofstadter:** I do not agree with the experimental errors of 20%.

**Drell:** I was quoting your paper.

**Hofstadter:** You are not. You are quoting \( F_1/F_2 \) and not \( F_1^{(**)} \).

**Bernardini:** From the plot of \( F_{2n} \) I had the impression that it decreases more strongly with momentum transfer than \( F_{2n} \) and that both fall much below 1. Is this correct?

\( (*) \) Lindner and Gartenhaus \( 19 \) have been analysing the accuracy of this problem.

\( (**) \) Editor's Note: Drell had in mind the 20% error of the deuteron cross-section. Hofstadter was referring to the error in the measurement of the proton cross-section which is now known to an accuracy of 10%.
It is correct that both decrease at large momentum transfer.

Following the literature so far, I essentially had this impression, that $F_{1p}$ is almost equal to $F_{2p}$ and practically independent of the value of the momentum transfer. $F_{1n}$, which is essentially determined by the second moment of the charge distribution, is practically zero, but $F_{2n}$, which is correlated, let us say, with the outside cloud of the neutron, was more or less in agreement with what you get from the proton. In other words, $F_{2n}$ is very close to $F_{1p}$. I would like to know if the situation has now changed.

As far as I understand the value of $F_{1p}$ and $F_{2p}$ is far less than one at large momentum transfers. They are equal to one another within an uncertainty of about 20%. As I understand it, $F_{1n}$ is very small, practically zero, at small transfers, but, as Panofsky pointed out, at large momentum transfers it may be as large as 30% of $F_{2n}$. There is no indication of its exact value at present.

And it remains, that $F_{2n}$ is almost equal to $F_{2p}$. This is the only thing I would like to have clear. Is it true?

As far as I understand it, it is.

I think that $F_{2n}$ is close to $F_{1p}$ at large momentum transfers but it could be much smaller. I would like to say two further things. One is, that the remark made by Bernardini, that $F_{1n}$ measures the root-mean square radius is, of course, only true at very small $q^2$, and there we know it is zero. The other thing we would like to emphasize is the large degree of ignorance about $F_{1n}$, particularly at large momentum transfers, say above $q^2$ of about 7 or $8 \times 10^{-4}$ cm$^2$. Coming back to the ratio $(F_{2n}/F_{1n})^2$, one can definitely conclude that its upper limit is fairly close to unity, i.e. the magnetic structure of the neutron cannot be much smaller than that of the proton. On the other hand, a lower limit of the ratio cannot be deduced directly from the data presented, but there are limitations from other fields of nuclear physics. At very large momentum transfers, which essentially measure the fluctuation at very short range, there might be large deviations of the ratio from unity.

That the data can be expressed in terms of two special functions of $q^2$ only, namely $F_1$ and $F_2$, is an assumption that holds if only a vector-particle is exchanged between the electron and the proton. This is very likely the case, but it is not necessarily true. For instance, at high momentum there were contributions to the interaction analogous to gravitation, then there would be terms other than $F_1$ and $F_2$, depending on energy in other ways. Again, if the electron and the proton could combine to form a virtual boson in some way, which would then re-disintegrate into the electron and the proton, then there would be different functions of $q^2$ and the energy of the electron. So it would be ultimately of some interest to verify whether the distributions in angle and in energy are completely expressible in terms of just the two functions $F_1$ and $F_2$ of $q^2$.

Experimentally the main difficulty, of course, right now is that we have too many variables rather than too few.

In regard to the analysis of the inelastic electron-deuteron scattering data, there has been a calculation by Jankus which takes into account the final state interactions of the proton and neutron and which was the basis of the comparison with the experiment of Hofstadter and collaborators. I want now to remark only that a more recent calculation by A. Goldberg at Stanford shows that the final state interaction is not important; this calculation was carried through entirely by means of the impulse approximation, and practically the same form of the inelastic electron energy distribution was obtained. This makes it very much easier to compare different models of the deuteron with the experimental results.

Is it really so clear in connection with the $\mu$-meson $g$-value that one should argue from the $\Delta$ that one gets from the electron scattering? There may be another fundamental length coming in here since the $\mu$ meson is different from the electron. It may have a structure yielding deviations which are not quite so small. Perhaps the experiment is worth doing, even if one does not get the accuracy indicated by Panofsky.

I am very grateful to Marshak that he has brought up this point. I would not have had the courage to do so myself. The fact is that everyone has been thinking about what I would call the $(g-2)$ experiment. The present experiments are all limited by the ignorance about the mass, and they are best interpreted as measurements of the mass. In experiments of the $(g-2)$ type, a particle is made to circulate in an orbit. Thanks to the anomaly the spin turns through 90° after 250 revolutions. Although various laboratories have been thinking of this experiment, my present impression is that with the exception of Nikitin and perhaps people in Rochester, there is nobody very active on it. The reason why one does not undertake this experiment with as much vigour as one should is the very common impression that the classical term $\alpha/2\pi$ is well accepted and that the real interest of the experiment is to go beyond to the next order, i.e. the $\alpha^2$ term. Now I understand that some theorists feel that maybe even this first order correction might be affected for the $\mu$ meson by as much as 10%. In this case the experiment would be very rewarding. Even if you could make the meson turn around 250 times in an orbit, which is hard enough, you could perhaps not measure these 250 turns to one part in a thousand, but you certainly could measure them to the accuracy required to detect a 10% effect. It would be interesting to know what the theorists think about the deviations other than those due to the $\alpha^2$ term, which is known to be different for electrons and muons.
Tamm: Of course for the $\mu$ mesons we can expect quite new effects, because somehow the large mass of the $\mu$ meson has to be explained, and they may be coming in far earlier than in the case of electrons.

Takeda: I would like to ask a question of Panofsky about the following thing. They measured $F$ as a function of $q^2$, but only for positive $q^2$. There is a possibility, I think, that we can measure $F$ functions for negative $q^2$ values, if we do an experiment on $\mu$ pair production, or something like that.

Panofsky: I do not understand why $\mu$ pair production would measure electrodynamics for the opposite sign of $q^2$.

Takeda: In the elastic scattering of electrons by protons you only measure the $F$ for positive $q^2$ values. If we write the diagram for $\mu$ pair production we have $\mu$ pairs produced by virtual $\gamma$-rays. For fairly high energy $\mu$ pairs you have a possibility that the $q^2$ is negative instead of positive. Of course, there are some other processes which compete with this process and where we find $q^2$ positive. Therefore it is very difficult to get information for the function $F$ where the $q^2$ is negative. Still, I think there is a certain chance of seeing this effect. This possibility was pointed out to me by Suura of Hiroshima University.

Panofsky: I would like to make an experimental comment first. The $\mu$ pair production, which was completed at our laboratory two years ago, demonstrated that the process exists. However, at least within the limits of the experimental possibilities we know about, the experiment in hydrogen would be extremely difficult at present. On the other hand, I am still confused about the theoretical point. Maybe the questioner would like to draw a diagram on the board.

Drell: May I draw a picture and make a remark?

A is one of the two pair production diagrams which Panofsky drew, either for $\mu$ or electron pairs.

In B a photon is absorbed by the nucleon which then emits a virtual photon which converts to a pair. At such a vertex (V) the sign of $q^2$ is negative, and I believe this is what Takeda was referring to. If one could do a coincidence experiment and spread both particles of the pair at large angles, then there would be a large time-like momentum transfer here. Of course, the four-momentum transfer is zero at $W$ since the photon is real. This is a small correction, except at extremely high energies, to the regular Bethe-Heitler type terms and I imagine it is surely a question of experimental feasibility. In principle, I think I understand the remark, however.

Feynman: I think I see a difficulty. In all cases in which we purport to obtain a negative $q^2$ for a photon coupled to a proton there are two interactions with the proton. If there is only one interaction with the proton there has to be a positive $q^2$, since the proton is in a real energy state both before and after. If there are two interactions with the proton it is not, in my opinion, legitimate to use the form factor twice. It is evident from the complexity of the thing that in case of the interaction of two photons with the proton one cannot simply use the product of the form factors for each interaction. Therefore, I think that, at least so far as we have seen, there is no way of obtaining directly the form factor for the interaction of a photon of negative $q^2$.

Budini: We think that another possibility would be to measure the Bremsstrahlung at large angles, that is taking the photon and possibly also the scattered electron at a large angle. We think this could have some advantage, or be just an alternative experiment to that on pair production. There is a peculiar thing in this case: if one uses a form factor for virtual electrons, as the one chosen by Drell, it becomes bigger than unity at large angles.

Panofsky: We did consider this experiment and it is more difficult than looking at the large angle pairs. However, I do not quite understand the remark without somebody drawing a picture.

Budini: Consider the two diagrams.

In case A

$$ (p' + k)^2 = 2p'k (1 - \cos \theta), $$

while in B

$$ (p_0 - k)^2 = -2p_0 k (1 - \cos \theta), $$

which is now negative. In process B the form factor considered by Drell would be greater than unity and the cross-section would increase instead of decreasing.
NUCLEON STRUCTURE — Theoretical II

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Theoretical attempts to interpret form factors

Prior to 1955 and the original work of Hofstadter, Chambers, and McAllister [21, 22], theory was presented with the problem of explaining the magnetic moments and the electron-neutron result [3] that the root mean square charge radius

\[ \langle r_{1N}^2 \rangle = 6 \frac{d}{d(q^2)} F_1^N(q^2) \bigg|_{q^2 = 0} < (0.1 f)^2. \]

In this it has not succeeded. Now it is presented with the richer body of information on the variation of \( F_1 \) and \( F_2 \) over a considerable range of values of \( q^2 \).

Let us find what appears to be the simplest challenge for theory in the available data on \( F_1 \) and \( F_2 \) and analyse to what extent theory succeeds in meeting it. I shall be primarily concerned with recent dispersion theoretic attacks on the nucleon structure problem [20, 23] which make use of a spectral representation for the form factors \( F_i, V \) and \( F_S, V \).

The point I shall develop is the following: the value and structure of the isovector part of the magnetic moment, \( G_2(q^2) \), appear to present the most modest challenge to the theory and the theory does poorly in attempting to explain it.

First recall the status of previous attempts.

II A. Relativistic perturbation theory failed badly—its most flagrant disagreement being the prediction of a large isotopic scalar moment, \( G_2^S(0) = -1.7 \mu_B \), for \( f^S = 0.080 \). This difficulty comes from a large isoscalar contribution by the nucleon current, i.e. from the second of the two perturbation diagrams, Fig. 6(b). We note that the meson current contributes only to the isovector part, its second order contribution, Fig. 6(a), being \(+ 1.6 \mu_B \). This result is increased by less than 10% when fourth order contributions are included [21, 24].

II B. The static theory in the original Chew cut-off version and subsequently in the more satisfying form of Chew and Low appears to do quite well in providing a qualitative basis for understanding both the size and structure of \( G_2^V(q^2) \), but is ambiguous in its treatment of the Dirac moment and of the electron-neutron interaction. It takes into account the meson current, Fig. 6(a), with the core cut-off replacing recoil of the nucleon, and goes beyond perturbation theory by taking into account meson rescattering in the resonant 3,3-state, corresponding to Fig. 6(c). The current is then entirely an isovector. Miyazawa and Fubini [25] have obtained reasonable values in this way for the moment

\[ G_2^V(0) = \begin{cases} 1.36 + 0.36 = 1.72 & \text{for } K = 5 m, \\ 1.80 + 0.50 = 2.30 & \text{for } K = 6 m. \end{cases} \]

where the first numbers are the perturbation values and the second ones are the rescattering contributions for the two indicated choices of a cut-off for the momentum integrals. We remark that the perturbation values depend linearly on the cut-off. The mean square radius of the moment distribution depends on derivatives of the cut-off function; a reasonable value of 0.56 for a gaussian cut-off with \( K = 5 m \) emerges [26] for a gaussian cut-off with \( K = 5 m \). In view of their strong cut-off sensitivity, the significance of this agreement is debatable. The charge distribution is even more sensitive to the cut-off, the total charge in the meson cloud depending quadratically on the cut-off. Salzman [27] and Zacharia- sen [28] have shown that \( \sim 0.5 \) is in the meson charge cloud with a mean square radius of \( \sim 0.42 f^2 \), for a cut-off of \( \sim 5 m \), and that resonant rescattering corrections are \(-20\% \). This result indicates that the nucleon core, which one would like to forget in the analysis of the isovector moment, must be spread out to a very large distance in order to cancel the neutron’s r.m.s. charge radius. In

(*) On the other hand the fourth order contribution from the nucleon current greatly alters the second order results of \( -1.7 \mu_B \) and \(+ 0.57 \mu_B \) to \( \sim 0 \) and \(+ 4.4 \mu_B \) for the isoscalar and isovector parts, respectively.

Fig. 6. Graphs representing the electromagnetic structure of the nucleon in the static model: a) meson current, b) nucleon current, c) meson nucleon rescattering correction to the meson current.
fact the distinction between core and meson cloud becomes unclear. I would like to make here two pessimistic remarks about the validity of the results of the static model as applied to the nucleon structure problem:

1) Suura has shown that recoil corrections to the static meson charge cloud distribution reduce its radius by a factor of two.

2) The momentum distribution of intermediate mesons in the calculation of $G_{2v}^2(q^2)$ is indicated in Fig. 7, as a function of the mass of the intermediate state $\sigma^2 = 4(k_n^2 + m_n^2)$. These distribution functions are weighted by $1/\sigma^2$ for the value of the moment, by $(1/\sigma^2)^2$ for the r.m.s. size, and so forth. Comparison of the curves calculated with relativistic perturbation theory, and with the static model, with and without resonant rescattering, shows that recoil effects are very important, and in fact surprisingly large. It should be recalled, perhaps, that previous successes of the static model have applied to processes dominated by a resonance which is not the case here.

II C. We turn then to a relativistic dispersion analysis of the vertex function and attempt to avoid both the critical cut-off dependence of the static theory and the weak coupling approximation of perturbation theory. Our discussion is based on the following spectral representations

$$G_{1,v}^r(q^2) = \frac{e}{2} \int_{4m_n^2}^{\infty} \frac{d\sigma^2}{\sigma^2 (\sigma^2 - q^2 - i\epsilon)} \text{Im } G_{1,v}^{r,\sigma}(\sigma^2)$$

$$G_{2,v}^r(q^2) = \int_{4m_n^2}^{\infty} \frac{d\sigma^2}{\sigma^2 - q^2 - i\epsilon} \text{Im } G_{2,v}^{r,\sigma}(\sigma^2)$$

We do not discuss here the possibility of making a rigorous proof of these representations. They have been derived on the basis of perturbation theory and, in particular, Nambu has shown that a spectral representation is valid to all finite orders of perturbation theory. Since the nucleon does not dissociate into a system of lighter mass, there are no difficulties in the perturbation derivation of these relations. Three comments are of interest here concerning (4):

1) A subtracted form is used for $G_1$ because we ask of the theory only that it account for the anomalous moments and not the charge $e$. In this we follow perturbation theory which indicates $G_1^r(\sigma^2) \approx \frac{m^2}{\sigma^2} \ln \sigma^2/m^2 \rightarrow 0$ as $\sigma^2 \rightarrow \infty$ whereas $G_1(\sigma^2) \approx \ln \sigma^2/m^2$. By adopting this form we do not rule out the possibility that $G_1$ vanishes as $\sigma^2 \rightarrow \infty$, we just take a less controversial approach. We return to this debatable point at the end.

2) The spectral weight functions, or absorptive amplitudes, $\text{Im } G^{r,\sigma}(q^2)$, are related to the matrix elements for processes in which the photon produces particles of total mass $\sigma$, which particles are annihilated forming a nucleon-antinucleon pair. They are expressed as a sum over all possible states which can be produced by a photon, the threshold for each state being the rest mass of the particles produced. This sum over states replaces the perturbation expansion and has several interesting regularities which may be exploited. As indicated in Fig. 8, charge independence of the meson-nucleon interaction and invariance of the theory under charge conjugation imply that states with an even number of pions contribute only to $G^V$ whereas states with an odd number of pions contribute only to $G^S$. The threshold for contributions to $G^V$ is thus $4m_n^2$ and to $G^S$, $9m_n^2$, and one might naturally anticipate the two-meson state to be important for the low $q$ and large distance behaviour of $G^V$. The nucleon-antinucleon state contributes to both $G^V$ and $G^S$, its threshold being $4M_P^2$. The $\pi \pi$ state also contributes to both $G^V$ and $G^S$, its threshold being $4M_P^2$.

$$\text{Im } F^{r,\sigma}(q^2) = a_\pi^{r,\sigma} + a_\pi^{r,\sigma}$$

Fig. 7. The spectral weight function of the magnetic moment in the static model and in the relativistic theory.

Fig. 8. States contributing to the spectral functions.
3) One can calculate \( G(q^2) \) from the spectral representations and for \( q^2 < 0 \), corresponding to a scattering experiment, compare directly the theoretical and experimental values. Except in the case of the electron-neutron interaction, experiment determines \( G(q^2) \) over a large range of \( q^2 \), but must necessarily rely upon model dependent extrapolations to determine the r.m.s. radius — i.e. the slope of \( G(q^2) \) at \( q^2 = 0 \). It is neither desirable nor necessary to compare theoretically calculated and experimentally extrapolated radius values when the form factors can be directly compared and should be reliable up to the meson resonance at \( q^2 \sim 8f^2 \).

With these three points in mind we study first the isovector moment form factor \( G_z^V \). Our earlier discussion has provided a number of reasons why \( G_z^V \) should be the first object of study:

1) \( G_z^V \) appeared to be in the best quantitative shape in the earlier perturbation calculations with the meson current and in the static theory considerations.

2) It is in the best quantitative shape experimentally.

3) \( G_z^V \) is large in value and spatial extension, which leads one to believe that there are no delicate cancellations and that contributions to it are dominated by low momentum states, such as the two-meson state, which can be calculated. The isoscalar moment requires study of the three-pion-state which is very difficult to compute and is experimentally of unknown structure since it is weighted so lightly \( (G_0^S(0)/G_z^V(0) = 0.06/1.85) \).

4) As formulated here the moment structure is a richer problem than the charge structure because we require that theory account for both its magnitude and extension, whereas the value of the charge \( e \) is assumed and the problem of calculating it is legislated away. We thus ask of the theory both that it put the correct area under the dispersion integral and that it weight the area correctly.

Following Federbush, Goldberger, and Treiman \( ^{30} \), we attack the evaluation of the weight function \( \text{Im} \ G_z^V(q^2) \) with successively increasing degrees of elaborateness and complexity.

At first we limit ourselves to contributions from the two-pion intermediate state for which we can write schematically

\[
\text{Im}[G_z^V(q^2)]_{\text{ext}} = \text{Re}[e^{-i\arg} \epsilon F_2(q^2) \langle \pi \pi | N \bar{N} \rangle]. 
\]

(5)

**Step I:** The pion form factor is set equal to unity and the amplitude for \( \pi + \pi \rightarrow N + \bar{N} \) is set equal to the Born approximation. This then is just relativistic perturbation theory for the meson current only and gives a moment of 1.6 \( \mu_B \) but a weight function much too concentrated at high momenta. This is usually stated by quoting a calculated value of \( \langle r^2 \rangle_y^V = 0.24 f^2 \) as compared with the experimental value of 0.64 \( f^2 \). The form of \( F_2(q^2) \) is given in Fig. 9 and evidently there is too large a contribution to \( \text{Im} \ G_z^V(q^2) \) from large values of \( q^2 \).

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\( \star \) In the following the abbreviation FGT is used.

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**Step II:** FGT have pointed out that it is possible to put an upper bound on the contributions to the absorptive amplitude from high momenta by appealing to physical unitarity limitations. The physical threshold for the process pion plus pion annihilating to form a nucleon-antinucleon pair is \( \sigma^2 = 4M^2 \), or an energy of 2\( M \) in the centre of mass system. Whereas there is no upper bound which can be constructed for \( \langle \sigma | N \bar{N} \rangle \) in the nonphysical region \( 4m^2 \leq \sigma^2 < 4M^2 \), this amplitude can be limited by unitarity in the physical region. This is because only two reaction channels contribute and the amplitudes for them achieve their maximum value when \( |\sin \delta \epsilon^{18}| \rightarrow 1 \). That there are only two channels may be understood as follows. In the centre of mass system only the two-pion state with \( l = 1 \) connects to the photon and so the \( N \bar{N} \) pair can be produced only in \( ^3S_1 \) and \( ^3D_1 \) states with unit isospin. In this way, again setting \( F_3 = 1 \), FGT find that the maximum possible contribution to \( \mu \epsilon^V \) from \( \sigma^2 > 4M^2 \) is 0.2 \( \mu_B \).

What does this tell in the light of Step I: breaking up the integrand in the relativistic perturbation calculation, one finds that a contribution of 0.73\( \mu_B \) was obtained there from the region \( \sigma^2 > 4M^2 \). Evidently it was very bad to use the perturbation amplitude above 4\( M^2 \) since, at most, a small contribution can come from that region. So, guided by this unitarity argument, we can choose as Step II
to cut off the weight function $\text{Im} \, G^V_2(\sigma^2)$ above $4M^2$. We obtain thereby $\mu_B^2 = 0.87 \mu_B$, or one-half of the observed moment, and evidently, a larger structure, with $\langle r^2 \rangle_2^V$ increasing to 0.44 $f^2$ or 70% of the observed value. However, as is seen from curve b of Fig. 9, the structure is still too concentrated and even with this cut-off too large a fraction of $\text{Im} \, G^V_2(\sigma^2)$ is coming from high values of $\sigma^2$. In fact one can run the cut-off down to $2M$ before achieving a reasonable structure (Fig. 9, curve d) but the moment value is then only 0.4 $\mu_B$. The problem shapes up here as one of increasing the contribution to the spectral function from low momenta.

Since we have seen that perturbation theory is very bad above $4M^2$, perhaps rescattering corrections to the Born approximation for $\langle \pi \pi | NN \rangle$ are important also below $4M^2$. Study of these is:

**Step III:** The problem here is how to continue the pion-nucleon amplitude into the non-physical region. The $\sigma^2$ dependence of the perturbation amplitude is explicit and its continuation presents no problem.

However to go beyond a perturbation treatment of the pion-nucleon interaction, one subjects $\langle \pi \pi | NN \rangle$ to a dispersion analysis, entirely in the non-physical region for pion-nucleon scattering. The rescattering terms, i.e. the corrections to the Born approximation result, are extended into the non-physical region by means of the Legendre polynomials which are used to express their angular dependence. Taking only rescattering in the resonant 3,3-state into account as in the static theory and for simplicity approximating the 3,3-resonance to a sharp peak, one obtains a large increase in the weight function just below the cut-off at $4M^2$. This serves to add $\frac{1}{2} \mu_B$ to the moment but to reduce $\langle r^2 \rangle_2^V$ to one-half of its experimental value. The reliability of this result as a guide to the rescattering corrections is doubtful, as FGT themselves emphasize. This procedure more than doubles the moment contribution from $\sigma^2 < 4M^2$ with the large part of the enhancement coming from high momenta so that $\langle r^2 \rangle_2^V$ is reduced. Above the threshold $4M^2$ the unitary argument leads to a large reduction in the weight function. Also the validity of the Legendre expansion for such large values of $\sigma^2$ is open to question, since it diverges for $\sigma^2 \rightarrow \infty$. Finally, both the relativistic perturbation theory and the static theory give much smaller rescattering contributions to the meson current contribution. The fourth order contribution in relativistic perturbation theory as calculated by Nakabayasi, Sato, and Akiba \cite{23} increased the perturbation result by only 9%. Miyazawa \cite{24} calculated a rescattering increase of 30% in the static theory. By making a $1/M$ expansion of the weight function in their dispersion analysis of the rescattering correction, Chew, Karplus, Gasiorowicz, and Zachariasen \cite{25} reproduced the static theory result with a 17% resonant rescattering correction. Perhaps it is reasonable to expect the Born approximation to $\langle \pi \pi | NN \rangle$ to be a fair approximation in this calculation because the large nucleon-antinucleon pair term, i.e. the $\phi^3$ term of the Dyson-Foldy-Tani transformed Hamiltonian plays no role in the calculation of $G^V_2$ and also one is in the non-physical region far from the resonance.

In résumé of our rescattering discussion, the large moment increase is not above mathematical doubts which FGT have indeed emphasized themselves. Moreover, it is not satisfying physically because the main contribution comes from high momenta and therefore gives too tight a structure, reducing $\langle r^2 \rangle_2^V$ to 0.32 $f^2$, or one-half the observed value, from the result of 0.44 $f^2$ in Step II.

**Step IV:** We here analyse contributions to the spectral weight function from additional intermediate states as considered by FGT. Since we are concerned with the isovector form factor, there is no contribution from the three-pion state and we must turn to $4\pi, K\bar{K}, N\bar{N}$, etc., states. Nothing can be said about the four-pion contribution, and little more about the $K\bar{K}$ state except that in perturbation theory it appears to be unimportant and, in fact, the $K$ meson current contributes zero to $G^V_2$ if one adopts Schwinger’s symmetric form of coupling $K$ mesons to $\Lambda$‘s and $\Sigma$‘s, viz. $g^4_{K\Lambda} = g^4_{\Sigma\bar{K}}$.

Turning to the $N\bar{N}$ state we have schematically,

$$\text{Im} \left( G^V_2(\sigma^2) \right)_{N\bar{N}} = \text{Re} \left( G^V_2(\sigma^2) \right) \left( N\bar{N} | \bar{N}N \right)_{12} + G^V_2(\sigma^2) \left( N\bar{N} | N\bar{N} \right)_{22}. \quad (6)$$

Here the scattering amplitude $\langle N\bar{N} | N\bar{N} \rangle$ is in the physical region, since the threshold is $(2M)^2$, and can be limited over the entire range of integration by unitarity arguments. As in the earlier discussion a $N\bar{N}$ pair must be in the $S_{12}$- or $3D_{12}$-state to connect with a photon. The difference in kinematical factors between this and the two-pion state discussed in Step II prevents one from putting an upper bound on the contribution to the isovector moment on the basis of unitarity arguments alone as was done there. One can, however, put such a bound on the radius upon setting $G^V_2(\sigma^2) = e/2 = G^V_4(0)$ and $G^V_4(\sigma^2) = 1.85 \mu_B = G^V_4(0)$ in Eq. (6), obtaining \cite{25} from Eqs. (4) and (6)

$$\langle r^2 | N\bar{N} \rangle^2 < \frac{1}{\pi M^2} = 0.013 f^2.$$

(*) This increase in mean square radius by a factor of two is primarily the result of the fact that the value of the moment is cut in half by removal of the high momentum contributions. Thus

$$\langle r^2 \rangle_2^V = \frac{6}{G^V_2(0)} \left[ \frac{d}{d\sigma^2} G^V_2(q^2) \right]_{q^2 = 0}$$

and the numerator is relatively insensitive to the cut-off whereas the calculated moment is sensitive to high momenta.

(**) This assumes also that $(G^V_2(0)_{2\pi} + (G^V_4(0))_{N\bar{N}} \gg \mu_B$ and that there is no strong cancellation between the moment contributions from these states.
Therefore, whereas it is impossible to limit the moment contribution of the $N\bar{N}$-state, the radius contribution corresponding to it is very small and a large contribution to the moment from the $N\bar{N}$-state will make the problem of its spatial extension more acute. We mention here that in perturbation theory, the contribution of the $N\bar{N}$-state to the iso-vector moment is $+0.57\,\mu_B$ but that the $N\bar{N}$ scattering amplitude in the Born approximation leads to a cross-section larger than the observed $\approx 50$ millibarns. Also the unitarity argument is a gross overestimate of $\langle N\bar{N}|NN\rangle$ at high energies since it ignores the annihilation channels. The rescattering corrections to the physical amplitude of $N\bar{N}$ scattering are evidently important and appreciably decrease the weight function.

It is at this point that one sees most clearly the difference between the dispersion theoretical and the perturbation approaches. By appealing to a high threshold of two nucleon masses and by relating the spectral amplitude to the physical process of nucleon-antinucleon scattering, the dispersion approach suggests a strong depression in the contribution from the nucleon currents relative to their prominent role in perturbation calculations. An analysis of even higher mass states such as $N\bar{N}\pi$ has not yet been made.

**Step V:** We return to the two-pion state and consider the influence of meson structure $F_\pi(\sigma^2)$ in the weight function, Eq. (5). In the absence of any knowledge of meson electromagnetic structure one can only make theoretical guesses and then check how sensitive the predicted results are to these guesses. FGT have calculated $F_\pi$ using a dispersion relation, keeping only the two-pion intermediate state (Fig. 10).

![Image of two-pion interaction](image1)

**Fig. 10.** Representation of the pion form factor due to pion-pion interaction.

The weighting function

$$\text{Re} \left[ F_\pi(\sigma^2) \langle \pi\pi | \pi\pi \rangle \right]$$

contains the $\pi-\pi$ scattering amplitude in the physical region, $\sigma^2 > 4m_\pi^2$. The resulting integral equation for $F_\pi(\sigma^2)$ can be solved with various assumptions on this experimentally unknown physical amplitude. FGT make a scattering length approximation for the $\pi-\pi$ scattering, choosing $\delta = k^2a^2$. This corresponds to $p$-wave scattering, with a scattering length which is expected to be $\sim 1/M - 1/2M$, corresponding to the "range" of an intermediate nucleon pair state. In fact, with the choice $a = 1/M$, the pion form factor has little effect on the previous results (*) since the $\text{Re} F_\pi(\sigma^2)$ stays close to unity over the range of the dispersion integral, Fig. 11, and $\text{Im} F_\pi(\sigma^2)$ stays small. The $\text{Re} F_\pi(\sigma^2)$ is of primary concern here since it multiplies the real Born approximation amplitude in the weight function, Eq. (5). For a somewhat larger choice $a = 2/M$, the phase-shift $\delta \rightarrow \pi/2$ over a large part of the range of the dispersion integral, $\sigma^2/4m_\pi^2 > 10$, corresponding to a $\pi-\pi$ scattering resonance which enhances the absorptive amplitude for $\gamma \rightarrow 2\pi$ at the expense of $\text{Re} F_\pi(\sigma^2)$ as shown in Fig. 11. This is an interesting result by FGT because it suggests that pion structure might lead to an enhancement of low momentum contributions to the weight function which is needed if a large spatial extension of the moment is to be achieved. The present choice however depresses the magnitude of the moment further so that although it leads to a mean square radius of $0.56 f^2$ when used with the perturbation weight function of Step II, the resulting moment is only $0.77 \mu_B$, $\langle q^2 \rangle^{(2)}$ in this approximation is shown in Fig. 9, curve c and is still seen to weight the high momentum states somewhat too heavily.

![Image of pion form factor](image2)

**Fig. 11.** The form factor of the pion for different choices of the pion-pion phase shift.

It is of great interest to give further study to the "size" of the pion and its role in nucleon structure. The contribution to $F_\pi$ of a $N\bar{N}$ intermediate state can be neglected only if the pion emerges with a large mean square radius $2a^2$ (*), and for momenta where $F_\pi(q^2)$ has not yet fallen to small values as in Fig. 11. Also $\pi-\pi$ scattering models with resonances but no bound states, so that

(*) Incorporating this in the perturbation calculation of Step II with $4m_\pi^2 < \sigma^2 < 4M^2$, one increases the moment to $0.9\mu_B$ and decreases the mean square radius by $\sim 10\%$ to $0.40 f^2$.

(**) For scattering length $a = 2/m_\pi$, $\langle r^2 \rangle_\pi = 0.16 f^2$, whereas for $a = 1/m_\pi$, $\langle r^2 \rangle_\pi = 0.054 f^2$, which is less than unity bound on the $N\bar{N}$ contribution.

Nucleon structure
\[ \delta \text{ goes through } \pi/2 \text{ at least twice, will lead to different forms of } F_F \text{ for which } \text{Re} F_F(\mathbf{q}^2) \text{ approaches a finite constant for large } \mathbf{q}^2, \text{ but oscillates through zero in the resonance region.} \]

We can certainly not rule out the possibility of a pion-pion interaction, or equivalently of pion size, which will help both the magnitude and spatial extension of the nucleon moment. At this point, however, the theory is still in difficulty attempting to make both \( \mu_1^V \) and \( F_F(\mathbf{q}^2) \) compatible with observation. Even if the very large rescattering corrections of FGT are included together with the meson form factor, using the scattering length \( a = 2/m \), the root mean square radius is significantly too small, 0.46 \( f_0^2 \), while the moment value is reasonable, 1.39 \( f_0^2 \).

The question to be answered then is whether or not this is a fundamental difficulty of the theory or just a calculational one, and if it is a calculational one, is the approach with mass spectral representations adequate to the task of a full quantitative study of higher states, of rescattering corrections, and of pion structure.

Turning to the situation with regard to the charge structure the results are even less satisfactory. By using the subtracted form of the representations (4) we ask of the theory only that it account for the structure of \( F_F(\mathbf{q}^2) \), and this it fails to do by a wide margin, predicting \( \langle r^2 \rangle_1^V \approx 0.4 f_0^2 \) by steps II or V and \( \langle r^2 \rangle_1^V \approx 0.14 f_0^2 \) when the large rescattering corrections of step III are included.

For the isoscalar form factors we must look to the three-pion state, the evaluation of which requires knowledge of the meson production amplitude in the non-physical region. To explain the small neutron charge size, we must require of it that it cancel the isovector form factor for low \( q \)-values. If one appeals to a simple physical model of the charge distribution which attributes to it an \( \langle r^2 \rangle_1^V \approx 0.64 f_0^2 \), then one is led to the problem of requiring the three-pion state to play a prominent role in charge but negligible role in moment structure. Unitarity arguments again limit the \( N\bar{N} \) state to a negligible radius contribution.

Perhaps we should be more modest in our demands on the theory, and since the moment \( \mu_2^V \) was observed in steps I and II to weight the high momenta heavily, give up at this stage the hope of calculating it. Turning then to a subtracted dispersion relation and introducing \( \mu_2^V = 1.85 \mu_B \) as a parameter in the theory we find mean Square radii differing in each case from the values of our earlier discussion by the ratio of the calculated to the observed moment. Since in each case \( (\mu_2^V)_{\text{calc}}/1.85\mu_B < 1 \), the radius disagreement becomes more acute.

Finally we remark on the possibility of calculating the charge \( e \) by using the non-subtracted form of dispersion relation in Eq. (1) for the charge form factor as well as the moment form factor. Chew \(^{30}\) has raised this question and calculated \( G_Y^V(0) = 0.64 e \) using the non-subtracted form of dispersion relations with the spectral weighting function of relativistic perturbation theory, with cut-off at \( \mathbf{q}^2 = 4M^2 \) (this is step II). On the other hand the rescattering calculation of FGT (step III) leads to \( G_Y^V(0) \approx 0 \) showing the extreme sensitivity of this calculation to the high momentum region and, consequently, also to the higher states which are neglected.

However, in principle, a very interesting question can be raised here. What are the consequences of the assumption that all physical amplitudes satisfy dispersion relations which require no subtraction in their construction. This “no subtraction” philosophy leads to an infinite set of coupled homogeneous integral equations for these amplitudes, which may or may not have solutions for any, none, or some values of the coupling constants.

With the motivation of exploring this point of view, Zachariasen \(^{33}\) and I have studied the quantum electrodynamics (QED) vertex. We selected the electromagnetic form factors of an electron because there is only one coupling in pure QED; at low energies perturbation theory leads to experimentally valid solutions; and a simple type of dispersion relation is obtained since there are only three external lines at a vertex.

In evaluating the absorptive amplitude we keep only the lowest order state in \( e^2 - a \), i.e. the electron-positron pair state which leads to two coupled homogeneous integral equations which we write symbolically as

\[ G_{1,2}(q^2) = \int \frac{\text{Re} \{ G_{1,2}(q^2) \langle e\bar{e} \rangle \langle e\bar{e} \rangle \}}{q^2 - q^2 - ie} \, d\sigma^2. \]  

Since these equations are homogeneous in \( G_{1,2}(q^2) \) the ratio \( G_1(0)/G_2(0) \) and the structure of the form factors are determined. These are equations of the type discussed recently by Omnès \(^{30}\) and have a solution in agreement with perturbation theory for small \( q^2 \) as follows:

\[ G_1(0) = \frac{\alpha}{2\pi}; \quad \langle r^2 \rangle_2 = \left( \frac{1}{m_e} \right)^2, \]

\[ G_2(q^2) \propto \left( \frac{m_e^2}{q^2} \right) \text{ln } \frac{q^2}{m_e^2} \text{ for } q^2 \to \infty, \]

\[ \langle r^2 \rangle_1 = -\left( \frac{1}{m_e} \right)^2 \frac{\alpha}{\pi} \left( \frac{33}{20} - 2C \right) \]

\[ G_1(q^2) \propto \left( \frac{q^2}{m_e^2} \right)^{\frac{13}{12} - C} \text{ for } q^2 \to \infty, \]

where \( C \approx 0.577 \) and by definition, \( G_i(0) = e \). The asymptotic behaviour of the charge form factor is different from the perturbation result of \( \text{ln } q^2/m_e^2 \), but violates the “no subtraction” philosophy. The negative value of the mean square charge radius is analogous to the result for boson propagators.

The conservative conclusion to be inferred from this result is that neglect of all but the \( e\bar{e} \) state is inadequate. In fact, it is an indefensible approximation because all
values of $q^2$ contribute in (7) whereas $ee$ is the leading order state only for processes with $\ln q^2/m^2 < 137$. It is for values $q^2/m^2 \sim e^{137}$, corresponding to energies above which Landau argues that QED processes must be damped out, that $G_1(q^2)$ begins to deviate from unity. It is hoped that further work along these lines will shed light on the question of whether the “no subtraction” philosophy and the condition $G_1(q^2) \to 0$ as $q^2 \to \infty$, lead to equations which are identities in $e$, or whether these requirements lead to equations $e = f(e)$ with solutions for particular $e$ values only.

Of more immediate experimental concern, we look forward to experimental information on electron-electron scattering and on large angle pair production to test QED at small distances where “small” means $\sim$ nucleon Compton wavelength.

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5. Bumiller, F. and Hofstadter, R. (private communication)
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18. Nambu, Y. (private communication)
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DISCUSSION

Gell-Mann: I understand from this talk that the theorists concerned with dispersion theory of $G_{1,2}$ can get out of a very deep hole only if they suppose that the pion-pion system has a sort of resonance in the $I = 1, J = 1$ state, so that the form factor of the pion can correspond to a very spread-out pion. If so it may be possible, using the observed magnetic moment and the observed structure of magnetic moment, to find very severe restrictions on what must be properties of this $I = 1, J = 1$ two pion state, and one should formulate these, the energy and the lifetime of this state, and ask people to look for it in production experiments of two pions.

Källén: I would like to point out that maybe there is another possibility. I think that Goldberger will agree with me when I say that the theoretical derivation of the particular dispersion relations on which the whole analysis was based is not quite in order. It is even worse because we know that, with the aid of the conventional assumptions in theories of this kind, we cannot prove the formula and, from counter-examples, we know that we cannot get a dispersion relation of this form. It appears to me to be particularly interesting that the region where the formula is not going to be exactly of this form, but where there are going to be extra contributions, is just the region around
the threshold. Therefore there are definitely extra terms in
the theory coming from energies which will not give a very
small radius and which may change the magnetic moment
very much.

Goldberger: I think that the thing that Källén is refer­
ing to is the fact that the rigorous derivation of the repre­
sentations from the general principles of quantum field
theory had not yet been presented. There may be additional
singularities in the neighbourhood of the beginning of the
branch cut indicated in the representations used by Drell.
There is some slight indication from the general theory
that there may be such terms. I do not believe that there
is any assurance that there will necessarily be modifications
of the formula as they stand.

Tamm: May I perhaps add that as far as I understand
even if you take for granted that this spectral representation
can be proved, still when you use it, it is quite a different
thing from using dispersion relations for scattering which
connect measurable things: cross-section and forward
amplitudes and so on. You have here, in fact, a rigorous
connection between unknowns, one unknown thing in the
left and infinite series of unknown things on the right, and
you have first of all to cut off this infinite series and then
in calculating each term you make approximations, some­
times amounting just to perturbation theory. So there are
so many approximations involved here that perhaps one
has not to take it too hardly that the theory at present is in
such an unsatisfactory state as you just heard.

Peierls: I wish to ask a very simple-minded question:
There is one very simple feature of the results which seems
to me incompatible with our usual pictures. If we just
look at the \( F_1 \) form factor we would have expected on the
old picture of the nucleon that we should see the charge
due to the proton itself, for whatever time of its life it is
a proton, plus the meson cloud. The first part forms a
core which we should guess to be perhaps of the radius of
the nucleon Compton wavelength. Now the very fact that
we have here, in fact, a rigorous connection between
unknowns, one unknown thing in the left and infinite series
of unknown things on the right, and you have first of all to
cut off this infinite series and then in calculating each
term you make approximations, sometimes amounting just
to perturbation theory. So there are so many approximations
involved here that perhaps one has not to take it too
hardly that the theory at present is in such an unsatisfactory
state as you just heard.

other of these two things must be happening or something
even more drastic which we cannot yet guess.

Tamm: May I make a second remark? I do not think
one can be certain at present that it is necessary to
assume new fields or a breakdown of electrodynamics to
interpret the data on the charge distribution in nucleons.
These data indicate that the scalar part of \( F_1 \) is large and
comparable with its vector part. The main contribution
to \( F_2 \) comes presumably from the three pion diagram.
Nobody has calculated this up to now and it may be really
large. Now, just to point out the connection with the
virtual particles picture of the nucleon, mentioned by
Peierls, the three pion term of the spectral representation
involves a \( (\gamma \rightarrow 3\pi) \) vertex. Now any mechanism of
the production of three pions by \( \gamma \)-ray involves a nucleon loop.
Thus the importance of the three pion terms in the spectral
representation has a correspondence to the fact, pointed
out by me some time ago, that nucleon-antinucleon pairs
must play an important role in the virtual particle picture
of the nucleon.

Miyazawa: I would like to point out that contributions
to scalar part from baryon pairs vanishes if global sym­
metry is correct. In this case the nucleon closed loop is
cancelled by the closed loop with cascade particles.

Schiff: I would like to ask Tamm if he could make a
few remarks about his earlier published qualitative sug­
gestion in regard to the spreading out of the nucleon core
by the nucleon-antinucleon pairs which are strongly coupled
to the meson cloud.

Tamm: It is an experimental fact, that the nuclear core
has a large radius, comparable with the radius of the mes­
onic cloud. There is nothing paradoxical in this fact if the
interaction of pions with nucleons is strong enough to
make probable enough the dissociation of pions into pairs,
which subsequently can annihilate crosswise.

Objections to this picture are based on arguments which
use the notion of centre of mass of the nucleon and which
ascribe to virtual particles the masses of the corres­
ponding free particles. Such arguments may be quite misleading,
e.g. when a pion with a moderate momentum dissociates
into a pair, the mass of this sub-system would seem to
increase from \( \mu_\pi \) to at least \( 2M \), and the centre of mass of
the whole nucleon would make a corresponding jump.

Drell: Following Gell-Mann’s remark I would like to
ask the experimentalists if they think that there is a way
of giving us some experimental information on the electro­
magnetic structure of the pion. \( \pi \rightarrow e \) scattering has the centre
of mass problem as \( e \rightarrow e \) scattering, which Panofsky discussed
earlier. I do not know whether \( \pi \)-mesic hydrogen atom is
feasible to give a mean square radius for the \( \pi \) meson or
not, but it looks like the \( \pi \) meson electromagnetic structure
will play an important part in resolving what appears to be
a difficult question here and I think I would like to know
whether anyone has a way of measuring it.
**Panofsky:** I think that the difficulty in \( \pi \)-mesic atom in hydrogen is that strong interactions would swamp everything of interest. But otherwise I have nothing to say.

**Yennie:** I would like to add something to Tamm’s remarks. Namely, he suggests that the physical nucleon has a large nucleonic extension, that the mesons create in nucleon pairs, and so on. If this is true, it should have some consequences in other grounds, for instance, in nucleon-antinucleon annihilation. Namely, as soon as nucleon and antinucleon come within a distance of about 1.6 fermi the nucleonic and antinucleonic structure should begin to overlap, so that two particles can begin to annihilate. This should give a rather extended absorptive region in the nucleon-antinucleon interaction.

**Tamm:** There seems to be no discrepancy between this possibility and experiment.

**Peierls:** I think there is a simple difficulty with the point of view that Tamm suggested; if one argues on the basis of ordinary perturbation theory, then surely we know that any virtual particle which needs the nucleon mass or more for its creation, cannot separate by more than the nucleon Compton wavelength. One could invoke such processes to cancel out the ordinary nucleon core, which we expect, But that seems to me a very artificial point of view.

**Panofsky:** I am wondering whether the assumption underlining the discussion, that the experiment indicates that there is no central core, is correct. I believe that Hofstadter has a comment on this. I believe that Hofstadter has succeeded rather well in fitting his measurements with the model of Clementel-Villi, which does have a central core. So I am not at all sure that this assumption is experimentally correct.

**Hofstadter:** That is true. Essentially this means postulating two cancelling cores and I do not know whether this is more interesting than a single core.

**Fubini:** What I would like to point out is that maybe the comparison between theory and experiment made on the ground of magnetic moment and of the mean square radius is probably not the best comparison we can make. I think that the situation now is the following: we do not know anything at all about the creation of many pions by a photon. So what we know about the spectral function is that it vanishes between zero and 2 meson masses and that its estimate is probably reliable between 2 and 4 pion masses. Above 4 pion masses we know very little. So I feel that a good comparison should be made in order to emphasize the region we know better. Fortunately experimental physicists have given us not only the magnetic moment and the radius but also they have given other information. In order to be more definite, I shall try to use for the comparison a twice subtracted dispersion relation:

\[
G(q^2) = \mu_n + \mu_q q^2 + \mu_A(q^2) q^4
\]

\[
\mu_A(q^2) = \frac{1}{\pi} \int \frac{g(\omega^2) d\omega}{(q^2 + \omega^2)^{3/2}}
\]

The first term comes mainly from momenta higher than 2\( M \). There one knows really very little and so the best thing is to take it from experiment. The second term comes mainly from momenta between 4\( M \) and 2\( M \) and also in this term it is very premature to make a comparison. Now in the third term, one sees that the most important contribution comes from the better known region. There is about 20% coming from the higher region and 15% of rescattering correction. Now if one calculates this term and tries to fit the Hofstadter experiment with the unique parameter \( \mu_n \), one does not succeed in doing that. The factor \( \mu_A(q^2) \) which one gets from low energy meson theory is indeed too small by a factor 2 and I think the only way of getting it larger is to make the pion radius much larger than 0.4 fermi. So I feel that a comparison on this point between theory and experiment would be extremely useful to see both the consistency of the theory and the question of the size of the pion.

**Sachs:** Concerning Tamm’s suggestion, I would like to remark that, although I do not quite understand how he gets the nucleon-antinucleon pair out to such a large distance, I believe there is an effect that might be very important even if the nucleon-antinucleon pair occurs very close to the core. The electromagnetic effects depend very much on correlations between nucleon and antinucleon, so if there are attractive forces between them one can get quite large electromagnetic effects which have about the same effect as a nucleon-antinucleon pair at a large distance.

**Lomon:** In some recent phenomenological work on the nucleon-nucleon interaction there has been an indication that there also may be a region of very strong interaction up to 0.8 fermi in this case.

An extension of the boundary condition model originally used by Fechbach and myself, and also by Bright, Boricius and others, has been made by adding potential tails outside of the boundary region. The potential tails that have been used have been both tensor and central tails. It turns out that when one adds these tails one obtains an energy independent boundary radius as well as an energy independent logarithmic derivative, and this is obtained with tails which are very similar to those given by the perturbation theory. The boundary condition radius turns out to be \( \sim 0.8 \) fermi, between say 0.75 and 0.85 fermi in the even states, indicating a strong interaction out to this distance.
SESSION 2
Monday, 30th June, 1958

The nucleon and its interaction with pions, photons, nucleons and antinucleons

Chairman  S. Ya. Nikitin

EXPERIMENTAL I
Rapporteur  G. Puppi
Secretaries  A. W. Merrison
            C. Whitehead

EXPERIMENTAL II
Rapporteur  O. Piccioni
Secretaries  G. von Dardel
            F. Farley
            R. Mermod
I must apologize for not being able to include in my report all the contributions I have received. I include only the contributions which fit some particular topics I am going to discuss.

1. \( \pi^- p \) scattering

I shall just discuss some new results on \( \pi^- p \) scattering starting with the low energy range. First, come the elastic scattering experiments for both \( \pi^+ \) and \( \pi^- \) performed at Rochester, namely

- at 24.8 MeV by D. Miller and J. Ring (3 points for \( \pi^+ \) cross-section)
- at 30 MeV by S. W. Barnes, H. Winick, G. Giacomelli, K. Miyake (4 points for \( \pi^+ \), 4 points for \( \pi^- \))
- at 31.5 MeV by B. Johnson and M. Camac (3 points for \( \pi^+ \))
- at 41.5 MeV by S. W. Barnes, B. Rose, G. Giacomelli, J. Ring, K. Miyake (6 points for \( \pi^+ \), 5 points for \( \pi^- \)).

The 41.5 MeV experiment seems to be the most adequate for analysis. Assuming charge independence, the differential scattering cross-section can be used to obtain six phase-shifts for the two isotopic spin states:

\[
T = \frac{3}{2}^+; \quad \frac{\alpha_3}{\eta} = -0.101 \pm 0.015, \quad \frac{\alpha_{13}}{\eta^3} = +0.234 \pm 0.019, \quad \frac{\alpha_{11}}{\eta^3} = -0.048 \pm 0.0068
\]

\[
T = \frac{1}{2}^+; \quad \frac{\alpha_1}{\eta} = +0.167 \pm 0.023, \quad \frac{\alpha_{13}}{\eta^3} = -0.055 \pm 0.062, \quad \frac{\alpha_{11}}{\eta^3} = -0.016 \pm 0.11.
\]

They fit the charge-exchange data of Tinlot and Roberts and of Spry.

It should be noted that the \( s \) phase-shifts agree very well with the Orear prescriptions, and that the phase-shifts for \( T = \frac{3}{2}^+ \) state agree also very well with the Columbia 58 MeV \( \pi^+ \) experiment based on 9 points (6 old ones by the Steinberger group and 3 recent ones by Winick).

Additional information about this low energy behaviour for the \( T = \frac{1}{2}^+ \) state comes from a \( \pi^- p \) experiment at 37 MeV, made at Columbia by Wooten.

Combining the 3 \( \pi^+ \) points at 24.8 MeV, the 3 \( \pi^+ \) points at the 31.5 MeV, the 6 \( \pi^+ \) and 5 \( \pi^- \) at 41.5 MeV, the 9 \( \pi^+ \) at 58 MeV and the 3 \( \pi^- \) at 65 MeV, the following solution has been obtained:

\[
T = \frac{3}{2}^+; \quad \frac{\alpha_3}{\eta} = -0.110 \pm 0.004; \quad \frac{\alpha_{13}}{\eta^3} = 0.088 (\pm 0.005)
\]

\[
T = \frac{1}{2}^+; \quad \frac{\alpha_1}{\eta} = 0.167 \pm 0.013; \quad \frac{\alpha_{13}}{\eta^3} = -0.006 \pm 0.022; \quad \frac{\alpha_{11}}{\eta^3} = -0.038 \pm 0.038.
\]

This solution checks very well with the older measurements of \( \pi^+ \) total cross-sections made in this energy region. Furthermore, it was found that where total \( \pi^+ \) cross-sections were calculated for elevated energies, such as 150 and 170 MeV, there was excellent agreement with the measurements of Ashkin. The same excellent agreement was found again between the \( \pi^+ \) angular distributions calculated for 150 MeV and 170 MeV and those measured by Ashkin at these energies.

This resulted in the conclusions that not only were the above scattering coefficients correct but also that the energy dependencies assumed (Chew - Low for \( \alpha_{33} \), \( \eta^3 \) for \( \alpha_{13} \), and \( \eta \) for \( \alpha_1 \)) were correct within the accuracy of the data over the above energy range.

(*) Using a Chew - Low dependence for \( \alpha_{33} \) and assuming \( \omega_0 = 2.17 \).
These $T = \frac{3}{2}$ scattering coefficients together with their energy dependencies were used next in a programme to determine the values of the $T = \frac{1}{2}$ coefficients from $\pi^-$ differential curves which had been measured at various energies. In particular, least squares solutions were found at each energy for the $\pi^-$ distributions measured at 31.5 and 41.5 MeV at Rochester, the distribution measured at 98 MeV by Holt at Liverpool and the two $\pi^-$ distributions of Ashkin made at 150 and 170 MeV.

The values found are given in Table I and show definitely that $\alpha_3$ does not vary with $\eta$, that $\alpha_{13}$ is small up to 100 MeV and then rises rapidly and that $\alpha_{13}$ is always small.

### Table I

<table>
<thead>
<tr>
<th>MeV</th>
<th>$\alpha_3^{\pi^-}$</th>
<th>$\alpha_{13}^{\pi^-}$</th>
<th>$\alpha_{13}^{\pi^+}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>31.5</td>
<td>+ 0.112 ± 0.019</td>
<td>- 0.017 ± 0.030</td>
<td>- 0.007 ± 0.012</td>
</tr>
<tr>
<td>41.5</td>
<td>+ 0.120 ± 0.015</td>
<td>- 0.006 ± 0.040</td>
<td>- 0.017 ± 0.021</td>
</tr>
<tr>
<td>98</td>
<td>+ 0.133 ± 0.003</td>
<td>- 0.019 ± 0.003</td>
<td>- 0.004 ± 0.002</td>
</tr>
<tr>
<td>150</td>
<td>+ 0.153 ± 0.013</td>
<td>- 0.016 ± 0.002</td>
<td>+ 0.027 ± 0.001</td>
</tr>
<tr>
<td>170</td>
<td>+ 0.158 ± 0.020</td>
<td>- 0.009 ± 0.030</td>
<td>+ 0.052 ± 0.014</td>
</tr>
</tbody>
</table>

The inclusion of $\pi^-$ data at 31.5 MeV as well as the analysis of $\pi^-$ data alone is still compatible with the solution shown above, but the agreement is not perfect. The following conclusions can be drawn from the Rochester data.

a) They give a very good determination of $\alpha_3$ from positive energy data, which is in excellent agreement with the zero energy data, charge-exchange and previous scattering data.

b) The $\alpha_{13}$ phase-shift is definitely found to be negative and its value is determined with a 10% accuracy. Assuming a Chew-Low dependence for $\alpha_{13}$ (with $\omega_{13} \sim \alpha_{13}$), one has $f^2 = - \frac{1}{3} \alpha_{13} \cdot \left(1 + \beta \omega_{13}\right)$.

If $\beta = 0$ one has $f^2 = 0.085 \pm 0.002$; if $\beta = \frac{1}{2.17}$ as from the analysis of $\alpha_{13}$, one has $f^2 = 0.14 \pm 0.02$.

c) The agreement up to 150 MeV for the $\pi^+$ phase-shift analysis allows us to consider the behaviour of the $T = \frac{3}{2}$ state as definitely fixed in this energy range, and with this also the total $\pi^+$ cross-sections.

d) Within the accuracy of the experiment in this low energy range, $\alpha_{13}$ looks negative and of the same size as $\alpha_{13}$, but the error is still large.

Next comes the 98 MeV experiment performed at Liverpool by Edwards, Frank and Holt, with the aim of measuring the $\pi^- + p$ elastic differential cross-section and the total cross-section.

The results of this experiment can be summarized in two numbers:

\[ \sigma_{\text{tot}} = 21.1 \pm 0.6 \text{ mb} \]

\[ D_\pi = 0.200 \pm 0.0055 \frac{\hbar}{\mu C} \]

The first is important, together with the Rochester results, in fixing the behaviour of the total cross-section of $\pi^- + p$ at low energy. The second one is important for the fit to the dispersion relations which we are going to discuss in a moment.

Particular care has been taken in this experiment in the monitoring of the intense beam by means of an ionization chamber. Also a water Cherenkov counter, with pulse height analysis, has been used to determine the electron and muon contamination of the beam and also to discriminate against electrons coming from charge-exchange scattering. There is not yet a phase-shift analysis available from Liverpool at this energy, but it should be noted that neither the extrapolation of the Rochester phase-shifts, nor the Anderson phase-shifts, give a good fit of the Liverpool differential cross-section. Something in between seems to be better, but the matter requires further investigation.

New information on the $\pi^- + p$ scattering comes from Russian work. Vasilevsky and Vishnyakov measured the...
elastic scattering of $\pi^-$ with an energy of 300 MeV on hydrogen by means of a hodoscopic system shown in Fig. 1. The angular distribution is analysed in $s$- and $p$-waves. Both the total cross-section and the real part of the forward scattering amplitude are in agreement with the 307 MeV Moscow experiment with counters. Korenchenko and Zinov present extended results of both $\pi^- + p$ elastic scattering and charge-exchange at 240, 270, 307 and 330 MeV. The 307 MeV and 330 MeV measurements are improved versions of an experiment already known. The 240 MeV and 270 MeV experiments are new (see Figs. 2 and 3) and the analysis is in progress.

An interesting contribution on the validity of the charge independence in the scattering has been presented by Salzman and by Stanghellini. Salzman uses for this purpose the known triangular inequality

$$I = -\sqrt{\sigma_+} + \sqrt{\sigma_-} + \sqrt{2\sigma_0} \geq 0$$

which must be fulfilled at each angle. The data he analyses are the Ashkin 150, 170 and 220 MeV measurements. The quantity $I$ is shown in Fig. 4. The result is not as good as one would like, especially at 170 MeV. Because of the overwhelming importance of the $T = \frac{3}{2}$ state for the energies investigated for which we expect $I = 0$, I would say that this test is not conclusive, taking into account both the errors and other uncertainties, such as the energy of the beams.

For the same purpose Stanghellini uses another relation, namely

$$2\sigma_0(0) = \left[ \text{Re } T_+(0) - \text{Re } T_-(0) \right]^2 + \frac{\gamma}{4\pi} (\sigma_+ - \sigma_-)^2$$

**Fig. 2.** Differential cross-sections for elastic and charge-exchange scattering of 240 MeV negative mesons by hydrogen.

**Fig. 3.** Differential cross-sections for elastic and charge-exchange scattering of 270 MeV negative mesons by hydrogen.

**Fig. 4.** $I$ as a function of angle for 150, 170 and 220 MeV pions.
where the knowledge of the real part can be reduced, by means of the optical theorem, to the knowledge of the $a_2(0)$ and the total cross-section. The two sides of the equality are compared at 5 energies from 150 and 307 MeV, with the result that they are always equal inside the errors and that at each energy the accuracy is of the order of 15%. Another computation is devoted to the $T = \frac{1}{2}$ state with the disappointing conclusion that in the energy region between 150 and 240 MeV the errors are too large to show these cross-sections. On the one hand, this justifies Salzman's results for $I \sim 0$, and, on the other, confirms that the knowledge we have of the $T = \frac{1}{2}$ state is, with the exception of the low energy data, non-existent.

Polarization effects in $\pi - p$ scattering

Ashkin, Blaser, Burger, Kunze and Romanowski present more extended results about the polarization of recoil protons in $\pi^- + p$ scattering at $(223 \pm 10)$ MeV.

The polarization has been measured at two angles for proton recoil $(15^\circ \pm 1.3^\circ)$ and $(30.7^\circ \pm 1.7^\circ)$ with the help of a cloud chamber, and on the whole something like 1500 events have been observed. The results are compared (Fig. 5) with four different sets of phase-shifts, two of Fermi and two of Yang type with different sign of $\alpha_1$. The comparison seems to favour the accepted Fermi type of phase with $\alpha_1 > 0$, and discriminates against Fermi type with $\alpha_1$ negative and the Yang type with $\alpha_1$ positive. The statistical accuracy does not rule out the Yang set with negative $\alpha_1$, but there are many other good reasons for rejecting this set. Thus, this experiment, together with the information from dispersion relations, seems to fix the sign of $\alpha_1$ at 223 MeV.

Grigoriev and Mitin present an experiment on the polarization of recoil protons from $\pi^+ + p$ scattering at 307 MeV. The recoil protons coming from the pion scattering at $140^\circ$ c.m. were sent into a stack of photographic plates, and the left-right asymmetry observed. The value obtained is $P = -(0.2 \pm 0.2)$ on the basis of 200 events.

This result is compared with three different sets of phase-shifts, one set using only $s$- and $p$-waves and the other two including $d$-waves (Fig. 6). The result of the comparison allows the exclusion of one set on the basis of the probable sign of the effect, and both the usual solutions with $\alpha_3$ negative can be accepted.

From the new information on the scattering processes, it is possible to draw some conclusions on its significance for pion physics.

1) The existing analysis for the $T = \frac{3}{2}$ state is confirmed.
2) The knowledge of the $\pi^-$ cross-section up to 100 MeV has been improved.
3) The values of the $s$ phase-shifts in the region around 40 MeV has been found to be in agreement with the Orear values. In this connection I would like to give the following best values for the $s$-wave scattering lengths:

$$a_3 = -0.110 \pm 0.004$$

as calculated at Rochester and

$$a_1 = 0.173 \pm 0.011,$$

(See Figs. 7 and 8.)

which is the weighted average of the following data (these exclude the zero-energy data):
The nucleon and its interaction

Fig. 7. $\alpha_3$ versus momentum.

$$\alpha_1 - \alpha_3 = 0.270 \pm 0.030 \quad \text{from Spry}$$
$$2\alpha_1 + \alpha_3 = 0.235 \pm 0.030 \quad \text{best value from scattering of } \pi^- \text{ at low energy}$$

$$\alpha_1 = 0.187 \pm 0.032 \quad \text{Rochester at 31.5 MeV}$$
$$\alpha_1 = 0.174 \pm 0.022 \quad \text{Rochester at 41.5 MeV.}$$

The value of $\alpha_1$ is obtained assuming charge independence.

4) Independent analysis at different energies, assuming the phase-shifts for $I$-spin $3/2$ as known, shows (Fig. 9) that there is some indication of a non-linear behaviour of $\alpha_1$.

5) Charge independence seems to be valid inside a 15% accuracy.

2. Dispersion relations applied to $\pi-p$ scattering

Many different approaches have been followed to analyse the possible inconsistency of the dispersion relations in $\pi-p$ scattering.

a) Analysis, as in the Puppi-Stanghellini paper, with the addition of new data. There is nothing new for the $\pi^+ + p$ data but the analysis for $\pi^- + p$ is now in a better shape than two years ago due to the use of the lower $\pi^-$ cross-sections measured at Rochester and Liverpool at low energy. In this way the discrepancy is partly reduced.

Salzman has recalculated the curves with Anderson phase-shifts. Even if one is not particularly happy about the behaviour of some of the Anderson phase-shifts, this is still an excellent way for an analytic calculation.

On the other hand, Bertocchi has recalculated the curves using the new information on the $\pi^-$ cross-section obtained at Rochester and Liverpool and the agreement between the two is good, as shown in Fig. 10.
This is calculated with \( f^s = 0.08 \) and all the curves here are still plotted with \( s \) and \( p \) analysis only. If you believe the \( s \) and \( p \) analysis should be maintained the situation is now better, but it is still a little uncomfortable. If, however, we go to \( d \)-wave the errors become quite large.

As regards the quantity \( \Re f(0) = D_- \) we have two new values; namely

\[
\begin{align*}
41.5 \text{ MeV Rochester} & \quad D_- = (0.104 \pm 0.014) r_s \\
98 \text{ MeV Liverpool} & \quad D_- = (0.200 \pm 0.0055) r_s.
\end{align*}
\]

The situation is now much better than that in the Puppi-Stanghellini paper, but there is still some discrepancy with \( f^s = 0.08 \), and a careful analysis of the errors is needed.

b) An extensive analysis of the errors of the different terms in the dispersion relations has been made by Hamilton. He uses Salzman’s results and calculates the effect on the dispersion relations of the uncertainties on the total cross-sections and on the scattering lengths. These errors are not sufficient to remove the discrepancy which still exists if the analysis is restricted up to 200 MeV to \( s \) and \( p \)-waves only. But an \( spd \)-wave analysis of the angular distributions to determine the forward scattering amplitude shows the insufficiency of the Moscow data to discriminate among the different \( D_- \) curves above resonance. The \( spd \) analysis also increases the error in the 150 MeV point, slightly increasing its absolute value. Hamilton’s opinion is now that the bulk of the discrepancy has disappeared. On this basis Noyes finds that the biggest errors come from integrals over total cross-section around resonance. He tries to represent the total cross-sections with a one level formula and uses two curves which in some way take into account the experimental uncertainties. Errors are evaluated from the difference of the integrals calculated using these two curves. They are rather large and he concludes that one cannot speak of a discrepancy between experiment and theory.

Another attempt by Lomon and Zaid is based on a modification of experimental data for the \( \pi^- \) cross-sections outside the best fit. In this way they emphasize the slope of these cross-sections in the intermediate energy region and increase the cross-section at the resonance. The behaviour does not satisfy charge independence because the \( \pi^- \) cross-section becomes less than \( \frac{1}{2} \) of the \( \pi^+ \) cross-section. But it is interesting to see that such a change on the \( \pi^- \) cross-sections improves the situation in two ways: it raises the value of the dispersion integrals and also decreases \( D_- \).

c) Subtraction of the singularity at energies different from \( \omega = 1 \) to avoid possible errors in the extrapolation to \( \omega = 1 \) of the scattering lengths determined at higher energies. Bertocchi makes this subtraction at the 41.5 MeV Rochester point, and Chiu at the 150 MeV Ashkin point. There are some little improvements, but there is essential agreement with the work of Salzman. Unfortunately these analyses depend critically on a single experiment and are thus less reliable than those based on the scattering lengths, which are deduced from a series of measurements.

d) Geffen and Lapidus compare the experimental results with the dispersion relations for \( \frac{\partial D(K)}{\partial K^2} \) (which have been calculated with the same hypotheses as Goldeberger). This quantity is essentially dependent upon the scattering lengths of the non-linear phases.

\[
\frac{\partial D_j}{\partial K^2} = b_j + 2a_{1j} + a_1 + \text{small terms}
\]

\[
a_i^S = a_i \eta + b_i \eta^S
\]

\[
a_i^P = a_i \eta^P
\]

\((i = I\text{-spin index})\)

They use Anderson’s phases to calculate \( \frac{\partial D_j}{\partial K^2} \) and they find good agreement with the experiments for the \( T = \frac{3}{2} \) state with a coupling constant \( f^s = 0.104 \pm 0.014 \). This value of \( f^s \) is in good agreement with all previous determinations. For the \( T = \frac{1}{2} \) state they could not find good agreement with the experiments, with a coupling constant equal to the one for the \( T = \frac{3}{2} \) state. Experimentally

\[
b_1 + 2a_{13} + a_{11} \simeq -0.03
\]

while from dispersion relations

\[
b_1 + 2a_{13} + a_{11} \simeq -0.166 \pm 0.014 \quad \text{with} \quad f^s = 0.08.
\]

The non-linear phases of the \( T = \frac{1}{2} \) state are missing as Puppi and Stanghellini have already noted. If the small \( p \)-waves existed with Chew’s values \( a_{11} \simeq 4a_{13} \simeq 4a_{33} \), and if one uses the experimental values for \( a_{33} \), one calculates a 6-7 mb contribution to the \( \pi^- \) total cross-section. The conclusions are the same as those from the analysis of \( D_- \) : the experimental information on \( \pi^- \) is scarce.

e) Modification of Goldeberger’s relations by a different behaviour of the total cross-section at high energy.

If there is inconsistency between experimental results and dispersion relations, we could modify the hypotheses which are needed to write Goldeberger’s relations. The hypothesis which seems less solid is the one about the behaviour of the total cross-section at \( \infty \). A further subtraction brings in a new parameter which can also be deduced experimentally. But since there is as yet no limitation to the behaviour at \( \infty \), the subtraction procedure could be applied again and the comparison between theory and experiment would become meaningless.

Geffen and Hamilton use a second denominator and from different considerations again find the impossibility of getting a significant improvement.

Geffen shows that in the relations with two subtractions, the new parameters represented by \( \frac{\partial D_j}{\partial K^2} \) cannot be changed substantially from those of Goldeberger (with a subtraction only). Since the derivative which appears in the relations with two subtractions is to be equal to that calculated with one subtraction in order to fit the \( \pi^+ \) data, the two relations are equivalent. So the conclusion is,
that \( n = 2 \) does not remove possible discrepancies. Hamilton says that increasing the number of subtractions to eliminate inconsistencies requires at least

\[
\text{Re}(T) \sim \omega^2
\]
at high energies. This behaviour is in contradiction with the scattering from a centre of finite radius.

In conclusion two positions are possible:

1) Assuming that the dispersion relations are correct in their usual form, and thus that a unique coupling constant exists, the data from pions of both signs allow a determination of a coupling constant around 0.08 with a 10% error. Thus there is no conflict between the global experimental information and the dispersion relations; or one can say that the general behaviour of the data follows that predicted by the dispersion relations. This can be seen in Fig. 11, where a combination of the \( \pi^+ \) and \( \pi^- \) data have been plotted so that the term in \( f_2 \) disappears. This is equivalent to the assumption that they are equal. The fit is quite good, and also gives some confidence that the \( sp \) analysis above the resonance is not too far from the truth.

2) If we want to separate the information of the positive mesons from the negative ones, we get a series of good determinations of \( f_2^\pm \) for positive mesons (with a bary-center \( \sim 0.09 \pm 5 \% \)), while there are not sufficient data for the negative ones. Thus the problem is still open: data above the resonance are not too useful because of the ambiguity of the analysis and the magnitude of the error, while below resonance only the data at 41.5, 98 and 150 MeV really discriminate between the various curves (and the centre of gravity of \( f_2^\pm \) is still less than 0.08).

As many people suggest, the solution cannot come from further manipulation of the algebra, but only from more precise experimental information. In particular it seems important to check the total cross-section data for \( \pi^\pm \) around the resonance and at least make a good experiment.
on $\pi^-$ elastic scattering around 135 MeV. The accuracy required both for total cross-sections and for $D_-$ would be of a few per cent.

3. Photoproduction

The first point I would like to discuss concerning this matter is the experimental evidence for the so-called retardation term in the photoproduction process $\gamma + p \to N + \pi^+$. As you will remember, this term is due to the direct interaction of the photon with the meson cloud. Its presence, already established at Illinois and by the Berkeley group, has now been extensively investigated at Stanford by Lazarus and Panofsky. Angular distributions have been obtained in the interval 5° to 20° lab. system for photon energies around 220, 300, 350 and 390 MeV by fixing the momentum of the produced pion. Fig. 12 shows the apparatus. At each energy, the theoretical curves (Fig. 13) with the retardation term fit the experimental points. The results of this experiment, as well as of the others at Cal. Tech., Illinois and Berkeley, have been used for a comparison of the energy dependence of the differential cross-section at 15° c.m. with the dispersion theory (Fig. 14). All cross-sections have been normalized to the Cal. Tech. magnet data.

A comparison has been made with two different types of phase-shift for the scattering, the first one using the combination of $a_{31} = a_{11}$ suggested by Chew, and the second one putting $a_{31} = a_{13} = a_{11} = 0$. Neither choice was a very good one and, in any case, the prediction of the theory is very sensitive to the choice of the small phase-shifts.
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The same conclusion has been arrived at by Uretsky, Kenney, Knapp and Perez-Mendez at Berkeley, who present differential cross-sections at 260 and 290 MeV. The 260 MeV experiment is a more extended work of an already published result and includes a new point at small angles and five more points at large angles. The other is a differential cross-section at 290 MeV in the interval 20° - 160°. The differential cross-sections are compared at both energies with the predictions of the dispersion relation with three different choices of small phase-shifts (Figs. 15 and 16). The general behaviour of the cross-sections is reproduced but the fit is very poor, especially at 290 MeV. So again the results seem to be very sensitive to the small phase-shifts.

In conclusion, it can be said that the problem of a good fit to photoproduction on the basis of dispersion relations, and consequently a refined check of the theory, depends very much on our knowledge of the phase-shifts in the scattering. Because this knowledge, especially concerning the $T = \frac{1}{2}$ state, is very poor, the problem remains open and awaits better information on the scattering processes.

The second point I would like to discuss is the situation of the pion-photoproduction in deuterium near threshold. This problem is interesting in two ways: firstly, because from it we can deduce the photoproduction from neutrons and check through the $n^{-}/\pi^{-}$ ratio the prediction of the theory; in the second place, because we need this ratio in order to establish the consistency between scattering and photoproduction through the Panofsky ratio. As you know, the theory makes quite a good prediction for the ratio

$$R = \frac{\gamma + n \rightarrow p + \pi^{-}}{\gamma + p \rightarrow n + \pi^{+}}$$

of photoproduction on free nucleons with a value around 1.3, and the experiments near threshold made at Illinois when extrapolated to threshold give 1.87 for the ratio in deuterium.

People always thought that, because when deuterium is used there are strong interactions between the particles in the final state, the result needs to be corrected; but no definite calculations have been made until recently. The problem is difficult because one must take into account nuclear as well as Coulomb interactions between the three particles.

The most complete calculation up to now seems to be that of Baldin who analyses the process

$$\gamma + D \rightarrow p + p + \pi^{-}$$

taking into account Coulomb interactions and nuclear interaction between the two protons. The result at which he arrives is that the high value of the $\pi^-/\pi^+$ ratio in deuterium is consistent, after correction, with a value as low as 1.4 for the free nucleons.

Baldin's calculations, based on an impulse approximation, are suitable also for some prediction about the correlations among the three particles in the final state and an experimental test of these can give us some confidence about the approximations used. Such a check is possible on the basis of an experiment performed at Moscow by Adamovich, Kuzmicheva, Larionova and Khazlamov.

Fig. 16. Differential cross-section against pion angle in centre of mass for 290 MeV photon energy.

Fig. 17. Cross-section as a function of photon energy.
They have investigated just the reaction \( \gamma + d \rightarrow p + p + \pi^- \) from threshold up to 190 MeV photon energy in photographic emulsions loaded with deuterium. The main point in favour of this experiment, based on 720 events, is that it allows a complete determination of the kinematics of the process.

In Fig. 17, the cross-section as a function of the photon energy is given. Then the relative importance of the configurations in which the two protons come out with small relative momentum is compared with the theory showing the importance of the Coulomb interaction between the two protons (Fig. 18).

![Fig. 18. \( \sigma_{25}/\sigma \) as a function of photon energy.](image)

Fig. 18. \( \sigma_{25}/\sigma \) as a function of photon energy.

Further, the quantity \( \int \frac{d\sigma}{dT} dE_\gamma \) is plotted against \( T \), (Fig. 19), the relative kinetic energy of the two protons, and finally (Fig. 20) the quantity \( \int \frac{d\sigma}{dx} dx \) where \( x = \frac{h\mu c}{E} \) is plotted against \( \epsilon \), a parameter which indirectly characterises the angle between the proton directions. Again the agreement is good. Some deviation for large values of \( \mathbf{p} \) (the half difference between the impulses of the two protons) are probably due to neglecting the interactions with the pion, at least the Coulomb interaction, but for not very large \( \mathbf{p} \) the approximation seems to be adequate. Assuming that this is the case, it is possible to obtain the value for the square of the matrix elements \( |K_n|^2 \), using the information in the configurations in which the theory is satisfied. In order to be outside the influence of the Coulomb interaction between pion and protons, only those events which satisfy the condition \( 5 \text{ MeV} < E_\pi < 30 \text{ MeV}, \mathbf{p} < 0.7 \) have been used. Fig. 21 shows the results of the computation. A best fit with a constant gives

\[
|K_n|^2 = (0.785 \pm 0.072) \times 10^{-67} \text{ cm}^2
\]

and the comparison with the positive pions obtained in other work gives

\[
(\pi^-/\pi^+) \text{ free nucleons} = 1.34 \pm 0.14
\]

in very good agreement with theoretical predictions. The finding of this experiment is not in contradiction with the previous one. The point of the \( \pi^-/\pi^+ \) ratio going down to threshold is now interpreted as due to the large effect of the final state interactions (mainly Coulomb interactions).
4. Comparison between photoproduction and scattering

As it is well known, it is possible to connect via detailed balance (which we still trust) the cross-section for photoproduction and the cross-section for radiative capture at a definitive energy in the c.m. Because the radiative capture is a process competing with scattering, there is the possibility of connecting scattering and photoproduction.

A proposed relation is:

\[ \sigma (\pi^- + p \to n + \pi^0) = 2 \left( \frac{P_{\pi^-}}{P_{\pi^-}} \right)^2 \times P \times \sigma (\gamma + p \to n + \pi^+) \]

connecting charge-exchange scattering of \( \pi^- \) with \( \pi^+ \) photoproduction on protons, and where

\[ P = \frac{\sigma (\pi^- + p \to n + \pi^0)}{\sigma (\gamma + p \to n + \gamma)} \]

\[ R = \frac{\sigma (\gamma + n \to p + \pi^-)}{\sigma (\gamma + p \to n + \pi^+)} \]

Except for detailed balance everything else is algebra. Now we have to introduce measurable quantities.

\( P \), the Panofsky ratio, is a ratio between transition probabilities at zero energy for the hydrogen mesic atom; the other quantities can be determined only for positive energy and they need to be extrapolated to zero energy. A further complication, which we already discussed, comes in because \( R \) is not known since photoproduction from free neutrons is today impossible, and so it must be estimated from deuteron data.

Using

\[ R = 1.34 \pm 0.14 \]

we get a Panofsky ratio calculated from positive energy data which is

\[ P = 2.5 \pm 0.4 \]

The same value has been obtained by Adamovich and co-workers directly from the measured cross-section for photoproduction of \( \pi^- \) and the value for \( (\alpha_3 - \alpha_1) \), and seems not to be in agreement with the mean weighted value of all the experiments, which is

\[ P = 1.67 \pm 0.07 \]

by roughly two standard deviations.

I am now going to present three points of view which represent three papers given at this meeting, namely:

1) The point of view of Moravcsik, who analysed both the uncertainties in the extrapolation of the experimental data to zero energy, and the theoretical background of the problem. In examining the extrapolation of the experimental data to zero energy, and the theoretical background of the problem, he reached the conclusion that the various uncertainties, have sufficiently large errors to indicate that the discrepancy is not yet significant, and more information is needed both on the theoretical side and on the experimental side.

2) The point of view of Baldin, who, assuming that the discrepancy is real, looks for a physical justification of it. Baldin’s idea is that in a Panofsky ratio determination we do not measure the usual \( \pi^0 \) but essentially a different particle, namely a \( \pi^0_0 \), which can be defined as a pseudo-scalar meson with I-spin equal to zero.

The mass of the neutral meson which has been observed from the Panofsky reaction is then the mass of \( \pi^0_0 \), and the mass of the \( \pi^0 \) (which forms with \( \pi^+ \) and \( \pi^- \) the vector state of I-spin = 1) is closer to the mass of the charged ones. So the mass difference between charged and neutral pion of I-spin 1 can be reduced and this is probably a feature that some people like. It is clear that the hypothesis must be tested against the bulk of experimental information on charge-exchange scattering and in any case must be submitted to experimental test. The most clear-cut experiment seems to be

\[ d + d \to He^4 + \pi^0 \]

assuming that I-spin conservation is true.

3) Lastly comes the point of view of Cini, Gatto, Goldwasser and Ruderman. The authors of this contribution propose a solution of the discrepancy inside the frame of existing particles (which are already too many!). They start from the following set of input data:

i) \( P = 1.5 \pm 0.1 \)

ii) \( R = \frac{\sigma (\gamma + n \to p + \pi^-)}{\sigma (\gamma + p \to n + \pi^+)} = 1.3 \)
The value of \( P \) is the best value of the Liverpool experiments. The ratio of photoproduction of charged pions on free neutrons and protons is the value predicted by the theory and is not inconsistent with the value we discussed before. The experimental value of the cross-section at 90° for photoproduction at \( E_\gamma = 170 \text{ MeV} \) is taken as a basis for the extrapolation at threshold.

This extrapolation differs from the previous one in that it takes into account explicitly the contribution of the retardation term

\[
2 \frac{\gamma (K - q) q \cdot e}{(K - q)^2 + \mu^2}
\]

which vanishes just at the threshold but is not negligible at the energy where experiments are performed. In this way they calculate a value at threshold for \( \gamma + p \rightarrow n + n^+ \) which is about 15\% higher than the conventional extrapolated value. (By the way, the coupling constant \( f^2 \) also benefits from this extrapolation and now comes out around 0.073.) The value of \( \left( \frac{a_3 - a_1}{\eta} \right) \) at \( \omega = 1.2 \) is taken as a basis for a new extrapolation to \( \omega = 1 \) in a non-linear form, which seems to be imposed by the theory, which predicts that \( \left( \frac{a_3 - a_1}{\eta} \right) \) must be (from crossing symmetry) an odd function of \( \omega \). That this must be the case can be inferred from the well-known results of \( \gamma_3 \) theory which predicts \( a_3 = a_1 \) for \( \omega \rightarrow 0 \). The precise form of the behaviour of the s-waves is not known but any reasonable \( \omega \)-dependence gives a value, for \( \left( \frac{a_3 - a_1}{\eta} \right) \) at \( \omega = 1 \), sensibly smaller than 0.27. The behaviour they like is of the form:

\[
\left( \frac{a_3 - a_1}{\eta} \right) = \left[ a \frac{\omega}{\mu} + b \left( \frac{\omega}{\mu} \right)^3 \right] \frac{1}{1 + \omega/M}
\]

which is a type of effective range approximation suggested by dispersion relationships.

This behaviour reduces \( \left( \frac{a_3 - a_1}{\eta} \right) \) to 0.24. The effects of the two new corrections are additive in the prediction of the Panofsky ratio which turns out to be \( P = 1.43 \) in good agreement with Cassels’s recommendation. The authors also make some comments about the existence of a neutral meson of I-spin zero.

Looking at the cross-section for charge-exchange scattering at low energy there seems to be no room for a \( \pi_\eta^0 \) produced 100\% in the Panofsky experiment and a \( \pi_\eta^0 \) produced with the cross-section calculated from the charged pion elastic scattering data. On the other hand, the proposed small contribution to the charge-exchange scattering of the \( \pi_\eta^0 \), following the suggestion \( a_1 \approx a_3 \), seems to be in violent conflict with dispersion relations, due to the fact that with this assumption the \( D_\eta \) changes sign. Any intermediate solution which contemplates the production of both kinds of neutral pions, in the Panofsky experiment, and which at the same time agrees with the positive energy data, seems to be unlikely when we look at the \( \gamma \)-ray spectrum measured by Panofsky, and probably also with the angular correlation experiment of Steinberger et al.

Bernardini makes the following comments in connection with the extrapolation at threshold. From the 170 MeV point of the cross-section at 90° there is a remark by Bernardini which can be followed better in Fig. 22. It shows the experimental situation for the square of the matrix element at 90° as a function of the energy. The curve is as we expect on the basis of the dispersion relation for a coupling constant \( f^2 = 0.08 \).

The extrapolation made by Cini, Gatto, Goldwasser and Ruderman is equivalent to neglecting all the information except the 170 MeV point and to shift down the curve until it passes through this point: this procedure is equivalent to bringing down the coupling constant to 0.073. This was already recommended by Beneventano et al.\(^1\) a few years ago.
This procedure is quite gratifying in that it helps in adjusting the discrepancy at the threshold, but is unpleasant because the fit up to 200 MeV becomes worse, and we believe that this interval has to be fitted because it is still insensitive to the peculiar behaviour of the small phase-shifts.

Barbaro, Carlson-Lee and Goldwasser have recently measured two points for $\pi^+\text{ photoproduction}$ at 220 MeV and 160 MeV with plates.

The 220 MeV point has a value in agreement with the old measurement (20% higher than the dispersion relation predicts). The 160 MeV point has a value of $$d_0 = (18.9 \pm 1.2) \times 10^{-13}$$ (the dispersion relation predicts between 18 and 19).

Bernardini also mentions that the Baldin correction which brings the $\pi^+/\pi^0$ ratio at threshold down to the expected theoretical value, is inadequate to resolve the discrepancy between the dispersion relation and experimental determinations for the $\pi^-/\pi^+$ ratio in the interval 190-230 MeV. (Fig. 23.)

The success of the Baldin procedure is again due to the fact that the extrapolation has been made from the only region where the dispersion relations, with or without correction, fit the experimental ratio.

In conclusion the problem still remains open. It is not too serious a problem because of the large uncertainties present in low energy pion physics, but if for a moment we accept it, the only proposal which seems to help is the one connected with the non-linear behaviour of

$$\frac{a_0 - a_1}{\eta}.$$
The angular distribution in the c.m. is
\[ (0.220 \pm 0.022) + \cos^2 \theta \]
(see Fig. 27) and the total cross-section for the reaction is \( \sigma_\gamma = (1.5 \pm 0.3) \times 10^{-27} \text{ cm}^2 \). The comparison of this result with the work of Cohn, Neganov and Parfenov on the total cross-section and angular distribution of the reaction \( p + p \rightarrow \pi^0 + d \) shows that charge independence is satisfied.

c) Very interesting results on the reaction \( \pi^+ + d \rightarrow p + p \) are presented by Neganov and Parfenov. They have measured the total cross-sections and angular distribution of the reaction \( p + p \rightarrow \pi^0 + d \) at five energies ranging from 174 to 307 MeV. The data show a sharp decrease in the total cross-section. This behaviour, together with the previous knowledge at lower energies, indicates the existence of a resonance with a peak around \( \eta = 1.55 \) (see Fig. 28).

The peak of the resonance occurs at a momentum in the c.m. of the \( \pi-d \) system which is exactly the same as for the \( \pi-p \) scattering, suggesting a close correlation between the two phenomena.
Fig. 28. Cross-section for \( p + p \rightarrow \pi^+ + d \) versus pion momentum.

\( d \) The other inelastic process \( p + p \rightarrow p + p + \pi^0 \) has been investigated at the same energy by Dunaytsev and Prokoshkin. They find

\[
\sigma_{pp} + \pi^0 = (3.20 \pm 0.17) \times 10^{-22} \text{ cm}^2
\]

and thus the inelastic cross-section at the energy amounts to \( (17.6 \pm 1.2) \text{ mb} \).

Independent measurement by Dzhelepov et al. and by Bogachen gives directly

\[
\sigma_{\text{tot}} = (41.6 \pm 0.6) \times 10^{-22} \text{ cm}^2
\]

\[
\sigma_{\text{el}} = (24.7 \pm 0.4) \times 10^{-22} \text{ cm}^2
\]

\[
\sigma_{\text{in}} = (16.9 \pm 0.7) \times 10^{-22} \text{ cm}^2
\]

The anisotropic part which is clearly present at 665 MeV for \( pp \rightarrow \pi^0 \) disappears at 480 MeV.

The behaviour of the reaction \( pn \rightarrow \pi^0 \) is also determined in the same experiment, which shows a strong anisotropic term both at 665 and 480 MeV. From the comparison between \( pp \rightarrow \pi^0 \) and \( pn \rightarrow \pi^0 \) it is possible to calculate the cross-section for \( \pi^0 \) production in the \( T = 1 \) and \( T = 0 \) states which at 660 MeV are respectively

\[
\sigma_{\text{tot}} = (17.1 \pm 0.6) \text{ mb}
\]

\[
\sigma_{\text{el}} = (3 \pm 2) \text{ mb}
\]

\( e \) At 586 ± 15.1 MeV the production of \( \pi^+ \) in \( n-p \) collision has been investigated by Kazalinov and Simonov. They found:

\[
\frac{\sigma (np \rightarrow \pi^+) + \sigma (np \rightarrow \pi^-)}{2} = (2.0 \pm 0.5) \text{ mb}
\]

and an angular distribution which was substantially isotropic. From the comparison between \( pp \) and \( np \) collisions which lead to charged and neutral pion production they come to the conclusion that

\[
\frac{\sigma_{\text{el}}}{\sigma_{\text{in}}} = 0.9 \pm 0.4.
\]

\( f \) The last paper I would like to present is that of Meshkovsky, Shalamov and Shebanov. They measured at 3 angles (19°30′, 38° and 56° in the laboratory system) the energy spectra of pions produced in \( pp \rightarrow pn \pi^+ \) collisions at 660 MeV. The spectra are strongly angle-dependent and the complete interpretation is premature, so let us confine our attention to the angular distribution of the processes which on the basis of this experiment as well as the previous ones can be written in the form

\[
\left( \frac{d\sigma}{d\Omega} \right)_{pp \rightarrow \pi^+} = (0.97 \pm 0.06) + (0.50 \pm 0.21) \cos^2 \theta
\]

and the total cross-section

\[
\sigma_{pp \rightarrow \pi^+} = (14.4 \pm 1.2) \times 10^{-22} \text{ cm}^2.
\]

From the previous work of Meshcheryakov and Neganov the main features of the process \( p + p \rightarrow d + \pi^+ \) is known, so by subtraction it is possible to obtain some information about the process \( p + p \rightarrow p + n + \pi^+ \). The result is the following:

<table>
<thead>
<tr>
<th>( d\sigma/d\Omega )</th>
<th>( \sigma )</th>
</tr>
</thead>
<tbody>
<tr>
<td>( p + p \rightarrow d + \pi^+ )</td>
<td>((0.100 \pm 0.014) + (0.435 \pm 0.250) \cos^2 \theta )</td>
</tr>
<tr>
<td>( p + p \rightarrow p + n + \pi^+ )</td>
<td>((0.870 \pm 0.060) + (0.070 \pm 0.210) \cos^2 \theta )</td>
</tr>
<tr>
<td>( p + p \rightarrow \pi^+ )</td>
<td>((0.97 \pm 0.06) + (0.50 \pm 0.21) \cos^2 \theta )</td>
</tr>
</tbody>
</table>
The anisotropic part ($\cos^2 \theta$) seems to be present only in the $pp \rightarrow d\pi^+$ reaction while the other process looks quite isotropic.

6. Present state of the analysis in phases p-p

Although during these last years, great progress has been made in the analysis in phases on the basis of the experimental data, and in the elaboration of models, the situation still presents many uncertainties. This is due partly to the different criteria of analysis followed by the various authors (i.e. the number of partial waves used in the analysis) and partly to the inadequate information concerning low and mean energies. This, better than by words, is demonstrated by the table in Fig. 30.

The present state of the analysis in phases presents a general difficulty due to the fact that, for each energy or for each group of energies, various solutions with comparable reliability are existing.

In order to give the results of the analysis in the simplest way, the following criteria have been followed:

1. Analyses extending at least until $D$-waves have been taken into account.

2. On each diagram has been plotted the behaviour at low and intermediate energies of the solutions foreseen by the Marshak - Signell model (for the low energies also Gammel Thaler model gives similar results). (Figs. 31 and 32.)

If we consider the sign of the phases of the Marshak - Signell model it is clear that the choice of the phases can be only one, and their behaviour remains approximately fixed, although within large limits not depending on experimental uncertainties but on different solutions.

The present solution could be improved by smaller experimental errors, although an improvement made by further complementary experiments appears to be more probable. A difficulty for the general solution proposed is evident and consists of the fact (here pointed out by Clementel and contained also in the analysis of Oehme)
Fig. 32. $^3P_0$, $^3P_1$, $^3P_2$, $^1D_2$ phase-shifts versus energy.

Fig. 33. Apparatus for measurement of $C_{nn}$ in proton-proton scattering.
and Feldman) that the relation exists between the signs of the phases $S$ and $D$ which at low energies should be of opposite sign. This is a point to be further investigated.

Let us examine the new contributions and their significance:

a) Measurement of the correlation coefficient $C_{nn}$ at $90^\circ$ c.m. at 382 MeV (Ashmore, Diddens, Huxtable, Skarsvag at Liverpool). This is the first measurement of the coefficient $C_{nn}$ which, as is well known, is tied to the polarization $P_1 P_2$ in the double scattering by the relation

$$C = \frac{P_1 P_2}{\epsilon}$$

where $\epsilon$ is the geometrical parameter obtainable from

$$\epsilon = \frac{(RR + LL) - (LR + RL)}{(RR + LL) + (LR + RL)}.$$

Fig. 33 shows the experimental arrangement and the results are

$$C_{nn} = +0.416 \pm 0.084.$$

Since

$$C_{nn} = \frac{\sigma_2 - \sigma_1}{\sigma_2 + \sigma_1}$$

at $90^\circ$ c.m., the results show that at these energies

$$\frac{\sigma_2}{\sigma_2 + \sigma_1} = (70.8 \pm 4.2)\% \quad \frac{\sigma_1}{\sigma_2 + \sigma_1} = (29.2 \pm 4.2)\%$$

and consequently show the amount of the forces depending on the spin.

Unfortunately, these results are not yet decisive for the choice of the various sets of phases proposed at high energies, because a complete analysis has been made only at 310 MeV, and therefore the comparison between the $C_{nn}$ calculated at 310 MeV according to this analysis and the $C_{nn}$ measured at 382 is not possible, because its behaviour with the energy is not known.

Fig. 34 presents the situation showing that the measured value is neither in agreement with the Gammel-Thaler model nor perhaps with the Marshak-Signell one.

On the other hand, at 380 MeV there is not enough information to make a complete analysis.


This is an experiment of triple scattering where a beam of protons with a polarization $P_1 = 46\%$ suffers a scattering in $H$ and a further scattering in $C$.

The depolarization factor is connected with the polarization $P_1$ of the incident beam and with the analysing power $P_3$ of the carbon $D P_1 P_3 = f$ where $f$ is the geometrical quantity

$$\frac{LL - LR + RL - RR}{LL + LR + RR + RL}.$$

The measurements have been made for various angles in c.m. in the scattering with $H$. Knowing $P_1$ and $P_3$, it is possible to measure $D$. The measured values are compared (Fig. 35) to those calculable from the Marshak-Signell and Gammel-Thaler models and clearly indicate a good agreement with the first model and a sharp disagreement with the second one. This experiment gives strength to the general belief of the choice of the phase-signs according to the Marshak-Signell model.
c) Cross section at 147 MeV unpolarized. Polarization for 45° c.m. from 46 to 147 MeV. Polarization as a function of the angle at 66, 95, 118, 147 MeV. (Cormack, Palmieri, Ramsey, Wilson.)

The results of this experiment can be summarized as follows:

1) The unpolarized cross-section at 147 and 95 MeV seems to be slightly different from the cross-section measured by Taylor but only in the interference region.

2) For the polarized cross-section \( P \frac{d\sigma}{d\theta} \) there is evidence of a \( \cos^2 \theta \) term down to 95 MeV depending on how far we go into the Coulomb interference region; the previous results showed this term only at 142 MeV and above.

3) No indication was found of \( \cos^4 \theta \) in this energy range.

4) The polarization has been followed at 140 MeV down to very small angles and evidence has been found of the contribution from the Coulomb interference.

5) Accurate measurements of the polarization at 45° c.m. have been made from 46 to 147 MeV, and reasonable agreement with the Gammel Thaler model found, and also the Marshak - Signell theory (Fig. 36).

In conclusion, from the polarization experiment it seems that at 95 MeV there is some contribution of F-waves, shown by the \( \cos^2 \theta \) term.

By the same group, plus Postma, experiments have also been done on the polarization in elastic \( p-d \) (Fig. 37) and \( p-a \) (Fig. 38) scattering. Thaler and McManus at MIT have used the Gammel and Thaler potential plus impulse approximation to add nucleon-nucleon scattering. The agreement is excellent even at angles so large that "off the energy shell" matrix elements are important.

d) Kumekin, Mescheryakov, Nuruschev and Stoletov measured \( D \), the depolarization parameter, at 90° and 635 MeV in a triple scattering experiment. They found \( D_{90°} = +0.87 \pm 0.21 \). This value indicates that at this energy the spin-orbit interaction is larger than the tensor interaction in \( p-p \) scattering.

e) Polarization in \( n-p \) scattering.

Fig. 39 shows some new measurements by Whitehead, Tornabene and Stafford at Harwell of the polarization in \( n-p \) scattering at a neutron energy of 77 ± 2 MeV. The
results cannot be analysed in detail because no value for the unpolarized cross-section is available, but the solid curve is obtained using the Marshak-Signell potential and is a fair fit.

By comparison with the previous measurements at 95 MeV one can say:

1. There is a little or no decrease in the peak value for the polarization, whereas Signell and Marshak predict a decrease from 50\% at 95 MeV to 41\% at 77 MeV.

2. The polarization at large scattering angles is appreciably smaller than predicted.

Golovin, Dzhelepov, Katyschev, Konin, Medved', Nadezhdn, Satarov have investigated polarization in p-n collisions and n-p elastic scattering at an energy around 600 MeV.

The results are shown in Fig. 40 where the behaviour of the polarization at 315 and 95 MeV are also plotted for comparison.

It is clear that there is a major change of polarization up to 315 MeV and a smaller variation from 315 to 600 MeV. The analysis of contributions from $T = 0$ and $T = 1$ states of the polarized cross-section shows that the $T = 1$ term rises with energy and the $T = 0$ term decreases.
The relatively big value of the $T = 0$ term at 635 MeV means that non-central interactions are of importance in the $T = 0$ state. Other results of this work come from the polarized cross-section at 90° (see Fig. 41). A comparison of the magnitude of these cross-sections at different energies shows that the phase-shifts for the $1\frac{1}{2}$ state change sign also in this region. This suggests that both phase-shifts have a similar energy dependence in the range 100-300 MeV.

The last results are the measurements of small angle scattering in $n$-$p$ collision. The geometry is shown in Fig. 42.

The cross-section at 5° c.m. is $(10 \pm 2)$ mb/steradian at energy 600 MeV and this rise, together with the big value of the polarization in $n$-$p$ scattering, shows that a simple optical model is not adequate. A full account of this work and some considerations on the determination of the amplitudes for $n$-$p$ and $p$-$p$ scattering are published in these proceedings.

I should like to express my thanks for the generous help I have received in preparing this report from Giacomelli, Stanghellini and Tomasini.

LIST OF REFERENCES


DISCUSSION

Feld: I want to discuss an effect which is seen already in some old data. It seemed to me rather interesting to point out in these days where one is doing such very accurate work in classical high energy physics (that is, pion physics), that there still remain some nice, simple effects which are not yet measured as well as they should be.

This effect was pointed out by Fermi some time ago. It concerns the reaction

$$\gamma + p \rightarrow p + \pi^0.$$  

You will recall that in the production of $\pi^0$'s the s-wave production is quite small as compared to the production of $\pi^+$ from protons, where the s-wave production is large. The reason is that the dipole moment in the final state, i.e. proton and $\pi^0$, is relatively small as compared to the dipole moment of a neutron and a $\pi^+$, because of the smallness of the pion mass. $s$-waves arise from electric dipole production.

To show how this effect arises we consider the reaction

$$\gamma + N \rightarrow N + \pi^0.$$  

We would expect this to show, at least near threshold, essentially zero s-wave production. (We might expect a small s-wave electric dipole production in the $\gamma + p$ reaction.) One way of seeing the effect is to observe that the final state is a mixture of isotopic spin $1/2$ and $3/2$, and the amplitudes for the two states essentially cancel in this reaction, as shown in Fig. 1.

As Fermi pointed out, when we start to go slightly above threshold, not only do the amplitudes squared increase with the momentum of the outgoing pion, but also a phase factor comes in, i.e. the amplitude for the $s$-wave production in the state can be written as

$$a_{tot} = ae^{\phi}a_t,$$

where the $a_t$ is the phase-shift for the $s$-wave pion scattering at the corresponding energy. The $a$'s for $T = 1/2$ pion scattering and for $T = 3/2$ pion scattering are almost equal in magnitude and opposite in sign. So, as one goes up in energy, instead of exact cancelling, one starts to get a small imaginary contribution to the total s-wave production for $\pi$'s. For neutrons this would look like Fig. 2.

For protons, however, the two amplitudes never exactly cancel, but one starts with a small real part, and going up in energy, one gets both a real and an imaginary part to the total amplitude for s-wave photoproduction.

![Fig. 2](image_url)

![Fig. 3](image_url)
Now one would observe this effect as an asymmetry in the $\pi^0$ production. That is, one has an angular distribution

$$A + B \cos \theta + C \cos^2 \theta,$$

in which the $B$ term arises from an interference between the $s$-wave and the normal $p \rightarrow \pi^0$ production of pions, which is the dominant effect. Unfortunately, the experiments are not quite good enough to show the effect very well. The results are shown in Fig. 4. (The crosses are from the work not quite good enough to show the effect very well. The circles are from McDonald, Peterson and Corson\textsuperscript{3}; also see Lindquist and Marklund\textsuperscript{4}. The solid curve is the theoretical prediction; the broken curve neglects retardation effects.)

![Fig. 4. $B/A$ versus photon energy.](image)

The indication is that the curve goes through zero between about 260 and 290 MeV, and this is also a point of the effect that is not unexpected. If the $s$-wave amplitude $\zeta$ has a real and imaginary part one can write $B$ as

$$B \propto (\zeta_r \cos \alpha_{33} + \zeta_i \sin \alpha_{33}),$$

where $\zeta_r$ and $\zeta_i$ are the real and imaginary parts of the $s$-wave amplitude for $\gamma + p \rightarrow p + \pi^0$, respectively. The two terms cancel for an $\alpha_{33}$ of about $45^\circ$, which is well below the resonance.

Now, these effects would be very interesting to observe, but the experiments are really not good enough to decide whether the predictions are true in detail. Accurate measurements will yield useful information about the low energy behaviour of the scattering $s$-wave phase-shifts and about the amplitudes for $s$-wave photoproduction near threshold.

Panofsky: I have two questions. In the paper of Cini and others on the extrapolation of $\alpha_3 - \alpha_1$ to $\omega = 1$, it would be implied that the phase-shift does not go linearly with $\eta$ and therefore there would have to be a long-range potential of some kind. From the more elementary point of view, of course, one would expect $\alpha_3$ and $\alpha_1$ to be linear for very small values of the momentum. I am wondering whether Cini and others wondered as to what kind of potential would be implied by the non-linear extrapolation to zero.

Chairman: Perhaps the authors would like to answer this question.

Ruderman: We have not looked for any specific models that would give such a dependence, for it is our point of view that any model which does not violate crossing symmetry must give $(\alpha_3 - \alpha_1)/k$ as an odd function of $\omega$. A static potential is not such a model. Rather, the potential must be velocity dependent. Although it may have a short range, a static potential which gives the same phase-shifts over a limited momentum range near threshold may have a long range. Of course the fact that it is an odd function of $\omega$ does not mean that $(\alpha_3 - \alpha_1)/k$ is not essentially constant for sufficiently small $k$. Our point is that the departures are not of order $k^2/M^2$ but rather of order $k^2/\mu^2$. One can actually make models even of zero range which give the $\omega$-dependence. For example, $\pi \cdot \pi \times \varphi \varphi (r)$ (one of the terms that come from the Foldy-Dyson transformation) gives such a behaviour even if $\varphi (r) \sim \delta (r)$.

Panofsky: My second question is addressed to Feld. My understanding was that the $B$ coefficient for the reaction $\gamma + p \rightarrow p + \pi^0$ has only just barely been determined. Now my understanding is that what you are plotting here is the $B$ coefficient for the neutron. Is that correct?

Feld: No, I am sorry. I have plotted the $B$ coefficient for the proton. There are essentially two independent determinations: at the low energy there are some early and rather crude measurements by Goldscheidt-Clermont, Osborne and Scott which give an indication of a negative $B/A$ with rather large errors; and at the higher energies there is the combined work of Corson and the Cal. Tech. group which give $B/A$ coefficients at energies from about 260 MeV to 400 MeV. As I have said these results do not definitely prove anything as yet, but they are all that exist. I just wanted to point out that when we get other and more accurate measurements they will give information which will be quite interesting.

Bernardini: I would like to make a few remarks. First, I am not clear on this point: from what Puppi said, the value of the interaction constant that comes out from the analysis of the photoproduction of $\pi^0$ only, is $\sim 0.09$. And I know that making use of the effective range approximation, in other words of the Chew-Low plot, including the results of Barnes and co-workers, is correct; but according to a pre-print of Edwards and Noyes, they find by considering the errors involved in the use of the dispersion relations (and Zavattini revised this analysis just a few days ago) that the $\pi^+$ and $\pi^-$ scattering data, the photo-meson production (now corroborated by the nice experiment recently performed by Goldwasser) and the evaluation of the deuteron quadrupole moment, are all
consistent within the limits of error with a slightly lower interaction constant, namely 0.075 with an error of only about 5%, hence my first question to Puppi: what is your opinion of the analysis of Edwards and Noyes? In other words is it still true that all these data on negative pions give a lower value than the value from positive pions? Secondly, about the $\pi^-/\pi^+$ ratio and the measurements done by the Russian authors, I want to emphasize that this cannot be considered as a direct measurement of the $\pi^-/\pi^+$ ratio, because, since the two neutrons are not observable in the emulsion, this gives only the behaviour of the cross-section for $\pi^-$ and not for $\pi^+$. The cross-section for the $\pi^+$ was taken, I believe, from the Illinois data. If so, the results of two different experiments are being compared, and it would, for instance, be essential to compare the intensity calibration of the machines. We have known in the past that such calibrations are very often off by more than 10% and this will of course be a serious trouble. I want to add, if we take the last measurement of Goldwasser on $\pi^+$ and combine it with the Russian data, the $\pi^-/\pi^+$ ratio instead of being 1.3 comes out to be about 1. In other words, it goes below the theoretical value. Hence we may say that the $\pi^-/\pi^+$ ratio at threshold has not yet been properly measured. Thirdly, I was called this morning by Hanson in Illinois. He asked me to mention that an experiment has been done in Illinois about the existence of the $\pi_0^\pi$. The experiment, as I understand it, consists essentially of a kinematical analysis of the photoproduction of neutral pions in helium and hydrogen. The experiment is a search for the threshold of the pair of $\gamma$-rays using two Cherenkov counters at 180°. Taking into account the different mass of helium and hydrogen there would have had to be some evidence of the $\pi_0^\pi$ if the mass of this particle is 4 MeV lower than the ordinary $\pi^\pi$, as we believe. But according to Hanson, Stoppani and Yamagata, the result of the experiment is negative; there is no evidence for the existence of the $\pi_0^\pi$. My fourth remark is connected with Feld’s observation. I would like to mention that if I remember correctly, about one year ago Koester in Illinois analysed very carefully this point on the basis of the Watson argument. He has found virtual agreement with the prediction within the limits of our knowledge about the phase-shifts.

Puppi: To answer the first question, I still believe that the coupling constant for the positive pions is around 0.09, and I have no reason to change this view because the new information has confirmed what we already knew. Concerning the paper of Noyes, I would like to hand the question to Noyes, but I would like to point out that there is a discrepancy in the calculation of $f^2$ (neglecting for the moment the error) between Salzman and the Bologna group on the one hand and Noyes on the other. Salzman and we calculate 0.07 for the Liverpool point, but Noyes calculates 0.085. I would like to add that the Noyes paper shows in a very nice manner how one can modify the errors with an analytical function, but it is not actually a fit to the data. With regard to the second question I agree with Bernardini.

Cini: Concerning the first question I would like to mention that I think that if one changes the extrapolation to zero kinetic energy of the real part of the forward scattering amplitude one can easily get a change in the coupling constant of about 10% and this change is just in the right direction. So I would expect that if instead of the Orear value for $D^+$ and $D^-$ at zero kinetic energy one uses the slightly lower value proposed by ourselves on the basis of our extrapolation, the value of the coupling constant can easily become lower, say 0.08.

Segrè: I would like to make two remarks. One is concerning the $d$ phase-shifts which Puppi mentioned. We have some data by Foote, Steiner and others at Berkeley, which I did not report because the experiments are very incomplete, on polarization of recoil protons from $\pi^+$ on protons at 330 MeV laboratory energy. These polarization data are quite incomplete but it is already clear that it is impossible to make an analysis without $d$ phase-shifts. I want to add another remark against the poor $\pi^0$ in the isotopic singlet state. It is not a very strong argument against it, but I think it might help to kill it, namely, if we take the bulk of the antiproton-proton annihilation data and we assume charge independence, the ratio between $\pi^+$ and $\pi^-$ and $\pi^0$ that come out do not leave much room for the existence of a new kind of $\pi^0$.

Goldwasser: I should like to comment on Bernardini’s second comment. I think he said that the Russian $\pi^-$ cross-section when taken together with my point at 160 MeV gives a $\pi^-$ to $\pi^+$ ratio of about 1. I would guess that the Russians used the Illinois $\pi^+$ cross-section from deuterium in making their calculation. In using the cross-section for $\pi^+$ from hydrogen you have to make some more complicated calculations. I should like also to make a second comment on the Feld point. I believe that there are two sources for an $s$-wave in the $\pi^+$ cross-section: one is the originally real $s$-wave amplitude originating from the proton recoil; the other is a secondarily scattered, originally imaginary $s$-wave amplitude originating from the $\pi^+$ $s$-wave which then scatters into a $\pi^0$. This second $s$-wave has an $\eta$ momentum dependence and overtakes in importance the recoil $s$-wave and I believe the curve that Feld drew (Fig. 4) that crosses over and becomes positive does so at $\alpha_{\eta\eta} = 45°$ only coincidentally, because the imaginary $s$-wave amplitude overtakes the real amplitude at that point. There is an experiment now in progress at Illinois by Koester, Modessit and myself which is attempting to measure this $B/A$ ratio. As far as I know the values are consistent with the theory.
**Marshak:** I wanted to ask about the asymmetry measured with polarized protons on protons giving the $\pi^+ + d$ where it was stated that a $d$-wave is required. This is quite interesting; at the lower energies in the 400 MeV region, the $s$ and $p$ interference was enough to explain the asymmetry. Could the experimentalists say something about the amount of $d$-wave required at 600 MeV?

**Chairman:** Dzhelepov, perhaps you will say something about these experiments.

**V.P. Dzhelepov:** Akimov, Savchenko and Soroko have studied the angular dependence of the asymmetry of emission $\pi^+$ mesons in the $p + p \rightarrow d + \pi^+$ reaction with polarized protons ($P = 0.44$) at the following energies: 536, 616 and 654 MeV. They have found a very strong angular dependence of asymmetry which could be seen on the slides shown by Puppi. It gives direct proof of the presence of a $d$-wave component in meson production, i.e. if $A(\theta) = constant$, there is no $d$-wave present. The analysis of these experimental data shows, however, that the amplitude of the $d$-wave is relatively small up to 654 MeV. It is also found that the $X_{D2} \rightarrow \gamma (S)p$ resonant transition gives the main contribution.

The data obtained in the Laboratory on Nuclear Problems (Dubna) on the production of charged and neutral mesons are in good agreement with Mandelstam's phenomenological theory.

**Feld:** With respect to the comments of both Bernardini and Goldwasser, I am sorry I did not mention the results of Koester at quite low energies. These show a negative asymmetry, consistent with the point of view I presented, which was also used by Koester to explain his results. But I think the rather interesting evidence which does exist from the Cornell and Cal. Tech. groups (with neither of which I am involved experimentally) indicates that in the region between 250 and 300 MeV the asymmetry coefficient $B/A$ is essentially zero, while between 320 and 400 MeV the coefficient $B/A$ becomes positive and rises to a value of about $+0.2$. These are published experimental results.

With respect to the other comments, I do not quite understand the remark about the charge exchange scattering, because it is my impression that the computation in which the phase-shifts of the different isotopic spin states are taken into account, in the way that I outlined, takes precisely that effect into account in a straightforward way. However, I think this is something that we will have to discuss privately.

**Noyes:** I have one comment on the nucleon-nucleon scattering phase-shifts. Puppi mentioned that there is evidence of $f$-wave at 95 MeV in $p-p$ scattering; one can say this is true even at 40 MeV. The high precision angular distribution at Minnesota combined with the polarization result at Harvard of a polarization of only 1% are incompatible with only $s$, $p$ and $d$-waves being present. One can say this unambiguously so that any phase-shift analyses at 40 MeV and above must include at least coupling to the $f$-state if they are to be realistic.

**Marshak:** There is just one point I would like to emphasize, in connection with the $D$-function. The experiments at 150 MeV give a negative value (according to Harwell) which becomes positive at 300 MeV and at about 600 MeV. The two theories which take into account the spin-orbit force differ in the prediction of the sign of the $D$-function at 150 MeV even though they agree quite well on the $s$ and the polarization. It will, therefore, be very interesting to fill in the dependence of the $D$-function on the energy and understand how a change in sign of this particular function takes place.

**Ceccarelli:** Returning to the $\pi^0$ I would just like to remark that its existence would allow a $\Lambda T = \frac{1}{2}$ channel for $K^+$ decay. This may perhaps introduce some difficulty.
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Fig. 5. s-state phase-shifts and it is seen that the \( S_{1/2} \) phase-shift seems to flatten out beyond 100 MeV; except that at 220 MeV an ambiguity appears associated with a positive or negative sign for the \( \delta_{13} \) phase-shift. Now the positive sign for the \( \delta_{13} \) phase-shift, that is the sign that differs from \( \delta_{21} \), gives the smooth curve that bends down and the negative sign gives a \( S \)-state phase-shift which jumps up to the point seen near the top of the diagram. Fig. 6 shows the behaviour of the \( P \)-state phase-shift, the top diagram showing the set which we prefer because it does not change sign and behaves in a smooth manner. However, we have two other reasons against the other set in which \( \delta_{13} \) becomes negative at large energies. Fig. 7 shows details of the phase-shift analysis at these points with preliminary errors on the phase-shift. We have so far only used the Ashkin data in the calculation but the rest will soon be included.

One corroborating piece of evidence for the first type of phase-shift with \( \delta_{13} \) positive is the polarization of the recoil proton and is shown in Fig. 8.

\[
\begin{array}{c|c|c}
\text{T = } 5/2 & \text{T = } 1/2 \\
\hline
\delta_3 = -10.4 \pm 8 \text{ (8), } \delta_1 = 8.4 \pm 2 \text{ (2)} \\
\delta_{23} = 52.1 \pm 3 \text{ (3), } \delta_{13} = 2.2 \pm 4 \text{ (4)} \\
\delta_{31} = -2.8 \pm 5 \text{ (5), } \delta_{11} = -3 \pm 4 \text{ (4)} \\
M = 6.2
\end{array}
\]

\[
\begin{array}{c|c|c}
\text{T = } 3/2 & \text{T = } 1/2 \\
\hline
\delta_3 = -18.7 \pm 10 \text{ (10), } \delta_1 = 8.8 \pm 3 \text{ (3)} \\
\delta_{23} = 63.5 \pm 4 \text{ (4), } \delta_{13} = 3.9 \pm 4 \text{ (4)} \\
\delta_{31} = 0.2 \pm 7 \text{ (7), } \delta_{11} = -3 \pm 7 \text{ (7)} \\
M = 9.9
\end{array}
\]

\[
\begin{array}{c|c|c}
\text{T = } 3/2 & \text{T = } 1/2 \\
\hline
\delta_3 = -14.7 \pm 6 \text{ (6), } \delta_1 = 6.6 \pm 2 \text{ (2)} \\
\delta_{23} = 109.5 \pm 8 \text{ (5), } \delta_{13} = 3.3 \pm 15 \text{ (15)} \\
\delta_{31} = -3.2 \pm 16 \text{ (16), } \delta_{11} = -5.4 \pm 14 \text{ (14.5)} \\
M = 9.3
\end{array}
\]

\[
\begin{array}{c|c|c}
\text{T = } 3/2 & \text{T = } 1/2 \\
\hline
\delta_3 = -15 \pm 5 \text{ (5), } \delta_1 = 18.9 \pm 11 \text{ (11)} \\
\delta_{23} = 112 \pm 6 \text{ (6), } \delta_{13} = -3 \pm 7 \text{ (7.5)} \\
\delta_{31} = -3.5 \pm 12 \text{ (12.5), } \delta_{11} = 2.8 \pm 12 \text{ (12)} \\
M = 3.9
\end{array}
\]

**Fig. 7.** Phase-shifts for \( \pi^-p \) scattering.
The experimental points are not on this curve but you have seen them recently (Fig. 3). At 35° they lie definitely on the positive side whereas at 15° they are compatible with either curve and so this tends to select the positive $\delta_{13}$.

The other reason for preferring the positive $\delta_{13}$ is due to a certain use of the dispersion relations just obtained by Chiu at Cornell. He has taken a dispersion relation for the derivative of the forward scattering amplitude, but the derivative is taken not at zero energy but just at the resonance. This makes the dispersion relation much simpler leaving only well known terms. Fig. 9 is only meant to show that the slope is fairly unambiguous at the resonance, that is just where it crosses the axis, and this also agrees. If one puts the numbers from the two phase-shift solutions into this derivative dispersion relation, only the one with positive $\delta_{13}$ agrees with the dispersion relation. We feel, therefore, that there is a unique set of phase-shifts and that the small $p$-wave phase-shifts are beginning to show at least what their sign is and certainly fit in with a smooth cubic behaviour. We have fairly good estimates of the errors also because of the analytic procedure used and those errors are not so large that they change the character of the phase-shifts as we have given them.

I do not want to make statements on the size of these errors, a little more work must be done on them.

**Fig. 9.** Forward scattering amplitude for negative pions against momentum.

**LIST OF REFERENCES**

There has been considerable work done to understand the interactions between high energy pions and protons. Negative pions have been investigated to the greatest extent mainly because they are easier to obtain. However, we are still in the beginning of the detailed study of the differential cross-sections for various types of interactions and of the energy and angular distribution of the secondary particles. It is difficult to collect sufficient experimental data and once obtained, the data are difficult to analyse.

### Pion-nucleon elastic interactions

First consider Figs. 1 and 2 which show the old curves of the total cross-sections. Fig. 1 shows the low energy data with the spectacular 3/2, 3/2 resonance at around 200 MeV which is visible both in the negative and positive pion cross-section since both of them involve isospin 3/2. In the intermediate energy range, as shown by Fig. 2, the behaviour is different for pions of the two signs. The
maximum in the negative pion cross-section is quite certain while the maximum in the positive cross-section is only fairly so. It has not been seriously challenged in the 2 1/2 years after it was first published but nobody has remeasured the total cross-section at that energy.

The Cornell and Cal. Tech. groups find indications for another resonant state of the $\pi p$ system which is excited by bombardment of hydrogen with $\gamma$-rays of about 750 MeV. One might expect to find this resonance in the pion-proton curves at a pion energy of $750 - 150 = 600$ MeV. The total cross-section at the maximum of the resonant peak might be expected to be as large as $\frac{3}{2} \pi \lambda^2$, (32 mb), but there is no trace of such a peak in the total cross-section curves. However, R. R. Wilson points out that possibly there are two resonance maxima, one at 600 MeV and the other at 850 MeV, with no marked valley between them, as shown in Fig. 3, which is a plot in "photon energy". When a photon of energy $W$ and a meson of kinetic energy $E_n$ collide with a proton, the total energy in the c.m. is equal if $W = E_n + 150$ MeV.

Fig. 4 shows the result of recent differential cross-section measurements of negative pions on protons which are due to Walker, Ballam et al. A noticeable feature of the curve is the backward peak which seems to increase between 460 MeV and 700 MeV and decreases again at 950 MeV as shown by Fig. 5, in which the lower curve is due to Walker and Ballam and the upper curve to Erwin and Kopp. According to Walker and Ballam it can be interpreted in terms of two separate spin-flip terms, one due to $p$-waves and the other to $d$-waves which change their relative phase somewhere around or above 700 MeV. Such a shift can also be connected with the resonance state found in photoproduction as mentioned above. Fig. 6 shows the energy dependance of the differential cross-section at specified angles. The curve for $120^\circ$ shows substantially the behaviour of the backward peak in the angular distribution and the curve at the $75^\circ$ the behaviour of the forward peak. Walker and Ballam have compiled the total elastic cross-section as
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Fig. 6. (according to Walker and Ballam)  

shown in Fig. 7 and the sum of the inelastic and charge-exchange cross-section of Fig. 8. Note that the point at 4.7 GeV in Fig. 7 and 8 is not to scale with respect to the other points.

The disappearance of the backward peak in the angular distribution is also apparent in the work of the Bologna-Trieste group shown in Fig. 9 and compared with other works in Fig. 10.

The interactions of positive pions at 500 MeV have been measured by Willis at Yale, Fig. 11. His work indicates that the $d$-wave phase-shift is less than 10°. He also obtains the following values for the $s$ and $p$ phase-shifts at this energy:

$$\alpha_s = -29.0°$$  
$$\alpha_{31} = -14.2°$$  
$$\alpha_{33} = 157.3°$$

The backward maximum is not so pronounced as for negative pions. D.A. Glaser and Rollig have made measurements of positive pion scattering at 1.1 GeV as shown in Fig. 12.

Fig. 7. (according to Walker and Ballam)  

Fig. 8. $\pi^- + p$ inelastic + charge-exchange. (according to Walker and Ballam)  

Fig. 9. (Bologna-Trieste group)
Fig. 10. \( \pi^- p \) angular distribution at 915, 950 and 1300 MeV.

Fig. 11. Angular distribution of \( \pi^+ p \) scattering at 500 MeV, analysed in terms of s-p-phase-shifts (P) and s-p-d-phase-shifts (D). (Willis)

Fig. 12. Cross-section for elastic \( \pi^+ p \) scattering. (Glaser and Rollig)

Pion-nucleon inelastic interactions

Attempts have been made to find some evidence for isobar formation in the inelastic interactions. It seems reasonable that the \( 3/2, 3/2 \) resonance should have an effect on the energy and angular distribution of the secondaries but it is by no means sure that this effect is so predominant as to allow the three-body problem to be reduced to a two-body problem. The experimental evidence collected so far appears to confirm this expectation.

McCormick and Baggett at Berkeley obtain the following values for the elastic and inelastic cross-sections for 800 MeV negative pions on protons, using a hydrogen bubble chamber.

<table>
<thead>
<tr>
<th>Process</th>
<th>Cross-section</th>
</tr>
</thead>
<tbody>
<tr>
<td>elastic</td>
<td>21 mb</td>
</tr>
<tr>
<td>( p + \pi^- + \pi^0 )</td>
<td>6.4 mb</td>
</tr>
<tr>
<td>( n + \pi^- + \pi^+ )</td>
<td>16 mb</td>
</tr>
<tr>
<td>others</td>
<td>9.9 mb</td>
</tr>
<tr>
<td>Total:</td>
<td>53.3 ± 2.4 mb</td>
</tr>
</tbody>
</table>

These data give a ratio \( (n\pi^-\pi^+)/p\pi^-\pi^0) \) of 2.5, which is in agreement with the isobar picture since \( \pi^- n \) is a pure \( 3/2 \) state while \( \pi^- p \) and \( \pi^0 p \) also involve the state with total isospin of \( T = \frac{1}{2} \). The experimental ratio would indicate that if the reaction goes only via the isobar state, the total isospin of the system should be \( T = \frac{1}{2} \). On the other hand, the data of Walker\(^5\) two years ago at 950 MeV gave about unity for that ratio, and the Bologna-Trieste group\(^4\) at 950 MeV now gives \( (n\pi^-\pi^+)/p\pi^-\pi^0) = 38/39 \).

The energy distribution of the secondaries as found by McCormick and Baggett is shown in Figs. 13 and 14.
The nucleon and its interaction

Fig. 13. Momentum distribution of $\pi^+$ and $\pi^-$ from $\pi^- + p \rightarrow \pi^+ + \pi^- + n$ at 800 MeV. (McCormick and Baggett)

Figs. 15, 16 and 17 due to the Göttingen group show the energy spectrum and the angular distribution of mesons produced by 0.96 GeV negative pions. The experimental points are compared with the curves of Lindenbaum and Sternheimer, based on the assumption that the process proceeds entirely via isobar formation. No evidence for $\pi-\pi$ interaction has been reported. If this existed the pions would probably tend to form a body analogous to the isobar state consisting of two mesons with a certain $Q$-value. The production of an additional meson in a meson-proton interaction could possibly also occur as if it were meson-meson collision. There is no evidence for the reality of this picture, but this does not constitute a definite proof against it since the kinematics of the process can only be expected to agree with the free meson-meson collision at bombarding energies which are so high that the rest energy of the pion can be neglected. At such high energies, however, the pion-pion collision will no longer be elastic, and so a clear indication for $\pi-\pi$ interaction is never to be expected.

Nucleon-nucleon collisions

A Berkeley group, Atkinson et al., have measured the elastic and absorption cross-sections in lead for neutrons of about 4 GeV, using a counter telescope with a gas Cherenkov counter. The neutrons hit a beryllium target and produce secondary pions in front of the detector. Only pions above 3 GeV are recorded by the Cherenkov counter. The experiment is thus insensitive to low energy
neutrons. 4 GeV is a preliminary estimate of the average neutron energy. The attenuation of the beam when the geometry is varied from good to poor is shown in Fig. 18, as a function of the half angle of the detector as seen from the absorber. The absorption cross-section is determined as $1.69 \pm 0.12$ b and the total cross-section $2.32 \pm 0.09$ b. These values show a considerable decrease in the elastic cross-section compared to lower energy data as shown in Fig. 19. These results are only preliminary.

Two Birmingham groups, Batson et al. 8, and Dowell et al. 9 have performed experiments on $p-p$ and $p-d$ interactions at 0.98 GeV, and on $p-p$ elastic scattering at 0.93 GeV, the first group using a hydrogen or deuterium diffusion cloud chamber and the second a propane bubble chamber. It is quite instructive to see their comparison between the $p-p$ cross-section measured on free protons in the hydrogen chambers and with bound protons in the deuterium chamber (Table II). The differences are of the order of 10 to 20%. This encouraged the group to determine the $p-n$ cross-section using of course the bound neutrons in deuterium (Table III).
The nucleon and its interaction

The shadow effect between the two nucleons in the deuterium nucleus is evaluated as 3.7 ± 3.1 mb. Table III makes it possible to compute cross-sections for the nucleon-nucleon state of isospin zero.

The authors also obtain a check on charge independence using the relation

\[ \sigma (np \rightarrow \pi^0) = \frac{1}{2} \sigma (pp \rightarrow \pi^+) + \sigma (np \rightarrow \pi^-) - \sigma (pp \rightarrow \pi^0). \]

The result of this check is not as yet definite. The present values indicate that the reaction pn -\( \rightarrow\) \(\pi^0\) occurs with an excess cross-section of 3 ± 2 mb over the value calculated with the above formula. The authors intend to improve their accuracy so as to make a more stringent check possible.

Fig. 20 shows the elastic \( p-p \) scattering at 0.93 GeV measured in a propane chamber. The experimental data fit a diffraction curve for a black disc of radius \( R = 0.8 \times 10^{-13} \) cm with an absorption coefficient \( K = 3.34 \times 10^{13} \) cm\(^{-1}\). The broken curve indicates the Coulomb effect. Fig. 21 compares the elastic \( p-p \) scattering on free protons as shown by the curve with the experimental points obtained for the bound protons in deuterium. The energy spectra of the secondary particles produced in elastic \( p-p \) interactions are shown in Figs. 22 and 23 and compared with phase-space calculations and with the predictions of the isobar model of Lindenbaum and Sternheimer. The experimental points seem to indicate some effects of the isobar state. There is also some evidence for the isobar effect in the \( Q \)-value distribution.

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**Fig. 19.** Neutron cross-section for lead as a function of energy. (Atkinson et al.)

**Fig. 20.** Elastic \( p-p \) scattering at 0.93 GeV. (Dowell et al.)

---

**TABLE II**

Comparison of \( pp \) and \( "pp" \) cross-sections (millibarns)

<table>
<thead>
<tr>
<th>Criterion</th>
<th>Free ( pp ) value</th>
<th>&quot;( pp&quot; ) (deuteron) value</th>
</tr>
</thead>
<tbody>
<tr>
<td>( pp ) elastic</td>
<td>25.9 ± 1.9</td>
<td>23.6 ± 2.2</td>
</tr>
<tr>
<td>( p + p \rightarrow p + n + \pi^+ )</td>
<td>16.3 ± 1.9</td>
<td>13.8 ± 1.5</td>
</tr>
<tr>
<td>( p + p \rightarrow p + p + \pi^0 )</td>
<td>4.6 ± 0.7</td>
<td>4.9 ± 0.8</td>
</tr>
<tr>
<td>Ratio ((np^+) / (pp^+))</td>
<td>3.6 ± 0.5</td>
<td>2.8 ± 0.7</td>
</tr>
<tr>
<td>Ratio (elastic ) (inelastic)</td>
<td>1.25 ± 0.09</td>
<td>1.26 ± 0.14</td>
</tr>
</tbody>
</table>

**TABLE III**

Proton-neutron cross-sections

<table>
<thead>
<tr>
<th>Reaction</th>
<th>Cross-Section (mb)</th>
</tr>
</thead>
<tbody>
<tr>
<td>( p + n \rightarrow p + n ) (elastic)</td>
<td>15.5 ± 3</td>
</tr>
<tr>
<td>( p + n \rightarrow p + n + \pi^0 )</td>
<td>14.5 ± 3</td>
</tr>
<tr>
<td>( p + n \rightarrow n + n + \pi^+ )</td>
<td>4.1 ± 1.2</td>
</tr>
<tr>
<td>( p + n \rightarrow p + p + \pi^- )</td>
<td>2.7 ± 0.6</td>
</tr>
<tr>
<td>total ( pn )</td>
<td>37.6 ± 3.9</td>
</tr>
</tbody>
</table>
of Fig. 24 since the full line experimental histogram for the $\pi^+ p$ agrees better with the theoretical curve B for the isobar model than with the curve A for the statistical model. The dotted histogram for $\pi^+ n$ behaves differently.

The "Q-values" recorded in this figure are obtained with the assumption that one meson and the nucleon form a single body by subtracting from the calculated mass of this body the rest masses of the pion and nucleon.

Fig. 25 shows the differential $p$-$n$ elastic scattering compared with the old results of other energies. In
The nucleon and its interaction

Fig. 25. Differential p-n elastic scattering cross-section at 903 MeV (Batson et al.19), compared with previous results, A — 90 MeV, B — 270 MeV, C — 580 MeV.

Fig. 26. Nucleon-nucleon cross-section. (Batson et al.19)

Fig. 27. Prong distribution for 9 GeV proton interactions. (Moscow group 10)

Fig. 28. Differential cross-sections for 9 GeV protons. (Moscow group 10)
TABLE IV

<table>
<thead>
<tr>
<th>$T$ in BeV</th>
<th>$\pi^- + p$</th>
<th>$\sigma_a$</th>
<th>$\sigma_{el}$</th>
<th>Authors</th>
</tr>
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<tbody>
<tr>
<td></td>
<td>$\sigma_{el}$ mb</td>
<td>mb</td>
<td>$\sigma_{el}/\sigma_{el}$</td>
<td></td>
</tr>
<tr>
<td>0.307</td>
<td>10 ± 1</td>
<td>19</td>
<td>1.9</td>
<td>Zinev and Korenchenko</td>
</tr>
<tr>
<td>0.45</td>
<td>13 ± 2</td>
<td>16.4</td>
<td>1.26</td>
<td>Walker, Ballam 5)</td>
</tr>
<tr>
<td>0.60</td>
<td>19 ± 2.5</td>
<td>17.0</td>
<td>0.9</td>
<td>McCormick, Baggett</td>
</tr>
<tr>
<td>0.75</td>
<td>20 ± 2.5</td>
<td>23.5</td>
<td>1.15</td>
<td>Erwin, Kopp 3)</td>
</tr>
<tr>
<td>0.80</td>
<td>21 ± 1</td>
<td>32.4</td>
<td>1.54</td>
<td>Walker 8)</td>
</tr>
<tr>
<td>0.95</td>
<td>19 ± 1</td>
<td>25.9</td>
<td>1.36</td>
<td>Steinberger et al.</td>
</tr>
<tr>
<td>1.00</td>
<td>20 ± 2</td>
<td>26</td>
<td>1.3</td>
<td>Shutt et al.</td>
</tr>
<tr>
<td>1.3</td>
<td>10 ± 1.5</td>
<td>19</td>
<td>1.9</td>
<td>Maenchen et al.</td>
</tr>
<tr>
<td>1.4</td>
<td>10 ± 2</td>
<td>24</td>
<td>2.4</td>
<td></td>
</tr>
<tr>
<td>4.7</td>
<td>6 ± 1.5</td>
<td>22</td>
<td>3.7</td>
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<table>
<thead>
<tr>
<th>$T_p$</th>
<th>$p + p$</th>
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<th>$\sigma_{el}/\sigma_{el}$</th>
<th>Authors</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>$\sigma_{el}$</td>
<td>mb</td>
<td>mb</td>
<td></td>
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<tr>
<td>0.81</td>
<td>26</td>
<td>24</td>
<td>1</td>
<td>Shutt-BNL</td>
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<tr>
<td>0.97</td>
<td>26 ± 2</td>
<td>21 ± 2</td>
<td>0.8</td>
<td>Batson, Culwick-Birmingham 8)</td>
</tr>
<tr>
<td>1.5</td>
<td>20</td>
<td>27</td>
<td>1.3</td>
<td>Shutt, BNL</td>
</tr>
<tr>
<td>2.75</td>
<td>15</td>
<td>26</td>
<td>1.7</td>
<td>Shutt, BNL</td>
</tr>
<tr>
<td>5.3</td>
<td>6</td>
<td>27.6</td>
<td>4.6</td>
<td>Wright, Powell</td>
</tr>
<tr>
<td>6.2</td>
<td>8</td>
<td>25</td>
<td></td>
<td>Wenzel</td>
</tr>
<tr>
<td>8.7</td>
<td>9</td>
<td></td>
<td></td>
<td>Moscow 10)</td>
</tr>
</tbody>
</table>

Fig. 29. Energy dependence of elastic and inelastic $p \cdot p$ and $\pi^- \cdot p$ cross-sections.
The nucleon and its interaction

Fig. 26 the data have been separated into the values for nucleon-nucleon interactions with isospin $T = 1$ and $T = 0$. It is to be noted that in spite of the apparent agreement of the energy spectrum with the isobar model the inelastic cross-section in state $T = 0$ is comparable in magnitude with that in the state $T = 1$. Since an isobar of $T = 1/2$ together with a nucleon of $T = 1/2$ cannot be produced from a nucleon-nucleon state of $T = 0$, the model in which the inelastic interaction occurs entirely via the isobar state does not receive a clear support from the Birmingham experiments.

The first data are now available on proton-proton interactions obtained with the 10 GeV Synchrophasotron by a group of the Joint Institute for Nuclear Research in Moscow. A stack of nuclear emulsions was exposed to 9 GeV protons in the machine. Fig. 27 shows the frequency of interactions with a multiplicity of $n_s$ light tracks. The average multiplicity was found to be 3.4 charged particles per interaction.

The differential cross-section for interactions compatible with an elastic $p-p$ scattering are shown by the full line histogram of Fig. 28. The angular distribution has the appearance of a diffraction peak in the forward direction. The integrated elastic cross-section is 9 mb. The inelastic cross-section per nucleon in the emulsion for interactions involving no heavy prongs is 25 mb.

The angular distribution of the particles produced in 9 GeV interaction has a median angle of 18° for collisions supposed to be proton-proton collisions, while the distribution for proton-nuclei collisions has a larger median angle of 25°. The median angle for the proton-proton collisions is somewhat smaller than the value 23° to be expected if all the particles were produced isotropically with extreme relativistic velocities in the centre of mass system.

The multiplicity of charged particles is 3.2 and the average energy loss of the proton in a collision is $60 \pm 20\%$. This value is derived from the multiplicity of all mesons, charged and uncharged, which is five, and the average energy of the $\pi^+$, 750 MeV.

To summarize, all elastic differential cross-sections from 1 GeV and upwards can be approximately understood as simple diffraction scattering. At the last Rochester Conference, Serber reported that a fit can be obtained with an optical model with the parameters

\[ \pi^-p \text{ at } 1.4 \text{ GeV : } R = 1.18 \times 10^{-13} \text{ opacity } 61\% \]
\[ \text{abs. coeff. } = 0.87 \times 10^{13} \text{ cm}^{-1} \]
\[ p^-p \text{ at } 2.75 \text{ GeV : } R = 0.93 \times 10^{-13} \text{ opacity } 93\% \]
\[ \text{abs. coeff. } = 2.7 \times 10^{13} \text{ cm}^{-1} \]

A Russian group, Grishin et al., obtains a fit with a unique radius for pion and protons of

\[ 1.08 \pm 0.07 \times 10^{-13} \text{ cm} \]

with sharp boundaries. The optical properties for the medium are somewhat different for pions and protons.

The inelastic and elastic cross-sections are compiled in Table IV with due reserves for errors or omissions.

Fig. 29 shows the energy dependence of the elastic and inelastic cross-sections for $p-p$ and $\pi^-p$ interactions. It is seen that the curves display simple behaviour at high energies. At 9 GeV the proton is more opaque to protons than to pions, as indicated by the larger absolute cross-section. One is tempted to attribute the large elastic $p-p$ cross-section at 2 GeV mainly to a core-core interaction whose effect might decrease with energy. The inelastic $p-p$ cross-section, and consequently part of the elastic might, on the other hand, be due to the interaction of the pion field of a proton with the core of the other proton. An interaction between the two pion fields might possibly also contribute. In either case the approximate equality of the $p-p$ and $\pi^-p$ inelastic cross-sections will follow naturally if the pion field of the proton contains about one virtual pion. The experimental errors are however still too large to allow any conclusions. It would be very nice to see the experimental errors in these curves become substantially smaller.

Cosmic ray energies, $10^{14}$ to $10^{16}$ eV

There exist, the world over, somewhat more than 50 events in emulsion with primary energy greater than $10^{12}$ eV. The physicists try to analyse these events on the basis of
relativistic kinematic relations. A fundamental tool of the trade is

\[ \gamma_c = \left( \frac{y + 1}{2} \right)^{1/2} \]

where \( \gamma_c \) is the Lorentz factor \( (1 - v^2/c^2)^{-1/2} \) of the centre of mass of the system. \( \gamma_c \) is determined from the experimental data using the relation

\[ \ln \gamma_c = \langle \ln \cot \theta_L \rangle \]

where the average is to be taken over all secondaries. A convenient method due to W. D. Walker, Wisconsin, is to plot \( y = \ln F(1 - F) \) versus \( x = \ln \tan \theta_L \) where \( F(\theta_L)/[1 - F(\theta_L)] \) is the ratio of the number of particles at angles less than \( \theta_L \) to the number of particles at angles greater than \( \theta_L \) in the laboratory frame. The graph of \( y \) vs \( x \) is a line of slope 2 for an isotropic distribution in the centre of mass. The distributions of Heisenberg and Landau give a line of slope about 1, and Fermi's distribution gives a slope 1.3, as shown in Fig. 30. Actually, when such a \( y \) vs \( x \) plot is made for the existing jets, the graph is often more complex than a line of just one slope (Fig. 31). This can be interpreted to show that the particles are emitted in the centre of mass with an angular distribution expressed by a power of the cosine, \( \cos^n \theta \).

Another important feature is that in nearly all jets the transverse component of the momentum of the emitted particles, particularly the \( \pi^0 \)'s, is close to 0.5 GeV/c.

Following a line indicated by Takagi, Kraushaar and Marks, and now newly restored by the Polish-Czech emulsion group\(^{12} \) who also observed several of the existing jets, and by G. Cocconi, who has analysed most of the existing data, it is possible to give a suggestive interpretation of the present results. In this simply empirical model the emission of the mesons takes place from two separate moving bodies of equal mass which are formed after the collision. The bodies, or centres, have velocities equal and opposite to each other in the centre of mass of the collision, and they emit mesons quite independently. No specific description of these rapidly decaying centres is given in such an empirical model, which is only meant to be an aid for the description of experimental results. For instance, the masses of the two bodies could be unequal in a slightly more sophisticated picture. If now the angular distribution of the mesons emitted from the centres were also arbitrary it would not be exciting to find a fit with the experiments. The point is that it is possible to obtain agreement with experiment by simply assuming that the centres emit isotropically in their own rest frame and that substantially all emitted mesons have the same energy in this frame. Thus there are now two parameters to be determined for each individual jet: the velocity of the centre of mass, and the velocity of the two bodies in the
Fig. 32. $y$-$x$ graphs, experimental (Polish-Czech group).
centre of mass, or equivalently, the two velocities of the two bodies in the laboratory system. In this fashion one can make a good fit to observed angular distributions. Fig. 32, due to the Polish-Czech group, shows how a computed $y$-$x$ distribution (curve) fits the experimental points (dots). Actually one can construct two such $y$-$x$ graphs for each interaction as shown in Fig. 33 (Cocconi), considering separately the jet from each of the two emitting bodies. Approximately speaking, one of the two bodies emits what appears as the narrow jet in the emulsion, and the other body emits the diffuse jet. The difference between the average angle of the two jets determines the velocity of the two bodies in the centre of mass.

Once two Lorentz factors $y_1$ and $y_2$ have been found for the two centres by fitting the angular distribution, one is able to predict the average energy of the observed mesons. In the few cases in which this energy can be directly determined by scattering measurements the model could, in principle, be tested. In practice, the errors involved are such that the angular distribution remains the sole support of the model. Another parameter that is possible to compute is the fraction of energy lost by the primary. Values as low as 10% have been measured by the Polish group, with median value of 40%.

In the energy range above $10^{12}$ eV, Teucher, Haskin and Schein have found three stars which, according to very stringent criteria (besides no heavy prongs, symmetry and momentum balance between the forward and backward cones were demanded), appeared to be nucleon-nucleon interactions. They measured the momenta of the backward emitted mesons and conclude that in interactions of energy greater than $10^{12}$ eV more than half of the mesons have energy less than 1 GeV in the centre of mass. For these interactions about 50 GeV is available in the centre of mass and the average number of charged particles is about 18. (Note that $50 \times 2/3 \times 1/18 = 1.8$ GeV.)

Pernegr, Scollak and Vrana of Prague have started the study of an electromagnetic shower of $10^{12}$ eV in emulsion.
### TABLE V

*(Vernov et al.)*

<table>
<thead>
<tr>
<th>Number of shower particles</th>
<th>Total energy of electron-photon component, $E_{\gamma} - (eV)$</th>
<th>Total energy of nuclear-active particles in core of radius 1 m, $E_{na} - (eV)$</th>
<th>Total energy of electron-photon component in core of radius 1 m, $E_{\gamma}' - (eV)$</th>
<th>$E_{na}/E_{\gamma}$</th>
<th>$E_{\gamma}'/E_{\gamma}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>$5 \times 10^5$</td>
<td>$7.2 \times 10^{13}$</td>
<td>$9.3 \times 10^{12}$</td>
<td>$1.2 \times 10^{13}$</td>
<td>0.13</td>
<td>0.17</td>
</tr>
<tr>
<td>$1.8 \times 10^5$</td>
<td>$2.6 \times 10^{13}$</td>
<td>$1.3 \times 10^{12}$</td>
<td>$1.7 \times 10^{12}$</td>
<td>0.049</td>
<td>0.065</td>
</tr>
<tr>
<td>$2 \times 10^5$</td>
<td>$2.9 \times 10^{13}$</td>
<td>$4.6 \times 10^{11}$</td>
<td>$5.2 \times 10^{12}$</td>
<td>0.016</td>
<td>0.18</td>
</tr>
<tr>
<td>$1.1 \times 10^5$</td>
<td>$1.6 \times 10^{13}$</td>
<td>$8.5 \times 10^{11}$</td>
<td>$8.4 \times 10^{11}$</td>
<td>0.053</td>
<td>0.052</td>
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<tr>
<td>$2.5 \times 10^5$</td>
<td>$3.6 \times 10^{13}$</td>
<td>$7 \times 10^{11}$</td>
<td>$5.1 \times 10^{12}$</td>
<td>0.019</td>
<td>0.14</td>
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<tr>
<td>$1.3 \times 10^5$</td>
<td>$1.9 \times 10^{13}$</td>
<td>$1.7 \times 10^{11}$</td>
<td>$7.9 \times 10^{12}$</td>
<td>0.092</td>
<td>0.41</td>
</tr>
<tr>
<td>$1.2 \times 10^5$</td>
<td>$1.7 \times 10^{13}$</td>
<td>$1 \times 10^{11}$</td>
<td>$4 \times 10^{12}$</td>
<td>0.59</td>
<td>0.23</td>
</tr>
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<td>$1 \times 10^5$</td>
<td>$1.4 \times 10^{13}$</td>
<td>$1.1 \times 10^{12}$</td>
<td>$3.6 \times 10^{12}$</td>
<td>0.078</td>
<td>0.26</td>
</tr>
<tr>
<td>$2.1 \times 10^5$</td>
<td>$3 \times 10^{13}$</td>
<td>$8.5 \times 10^{11}$</td>
<td>$3.6 \times 10^{12}$</td>
<td>0.028</td>
<td>0.12</td>
</tr>
</tbody>
</table>

**Fig. 36.** As Fig. 34 but at 690 MeV.

**Fig. 37.** As Fig. 34 but at 785 MeV.
Investigation on the cores of extensive air-showers

Vernov et al. \cite{13} have put together an apparatus with some 800 Geiger counters and 120 ionisation chambers, with individual pulse observation for all of them. With such a hodoscope they can carefully locate the position of the cores of air-showers. From 1000 hours running at sea level they have selected 9 showers (Table V) of about $10^5$ particles with the core conveniently located, so that they can study the properties of the nuclear active component (pions and nucleons). They measure the size of the electron showers that the nuclear active component regenerates locally when the soft component has been absorbed. They find that when showers of about $10^5$ particles are detected, the size of the local shower reproduced by the nuclear active component fluctuates surprisingly much. The electron-photon component in the core also shows large fluctuations. From numerical considerations \cite{14} they deduce that the harder, more penetrating, component which is responsible for the measured altitude dependence of the air-showers (140 g/cm$^2$) is simply the primary nucleon itself that originates the shower. These primary nucleons, as they show with another experimental arrangement\cite{15}, have a collision mean free path of less than 85 g/cm$^2$; but due to the partial inelasticity of their collisions and the shape of their spectrum they have an absorption mean free path of 140 g/cm$^2$.

Photoproduction experiments

From Cal. Tech. and Cornell we have an abundant supply of information on the photoproduction of one pion and two pions. Photoproduction of three pions has also been seen. The reaction $\gamma + p \rightarrow \pi^0 + p$ has been the subject of intense study. The angular distributions shown in Fig. 34 to 38 give the combined results of Cal. Tech. \cite{16} and Cornell \cite{17}. 

\begin{figure}[h]
  \centering
  \includegraphics[width=\textwidth]{fig38}
  \caption{Fig. 38. As Fig. 34 but at 940 MeV.}
\end{figure}

\begin{figure}[h]
  \centering
  \includegraphics[width=\textwidth]{fig39}
  \caption{Fig. 39. Reaction $\gamma + p \rightarrow \pi^0 + p$ Cornell data on angular distribution of $\pi^0$ in c. m. space \cite{17}.}
\end{figure}

\begin{figure}[h]
  \centering
  \includegraphics[width=\textwidth]{fig40}
  \caption{Fig. 40. Reaction $\gamma + p \rightarrow \pi^+ + n$, angular distribution of $\pi^+$ in c. m. space, as function of $\gamma$ energy. (Walker and Dixon)
These curves show clearly a rapid change in the angular distribution when the energy surpasses 800 MeV. Also the distributions of Fig. 36 and Fig. 37 are quite compatible with \((2 + 3 \sin^2 \theta)\), as required for a \(P_{3/2}\) resonance. These two experimental facts are among the indications that a resonance in a \(P_{3/2}\) or \(P_{1/2}\) state might occur, roughly around 750 MeV. This point will be amplified during the discussion.

Fig. 39 is a summary of the Cornell data on the \(\pi^0\) angular distribution. Clearly the same features of Figs. 34 to 38 are displayed in Fig. 39.

The same indication of resonance is shown by the angular distribution of \(\pi^+\), Fig. 40 (Walker and Dixon) and Fig. 41 (Cornell \(^{19}\)). The forward-backward asymmetry of positive pions at energy less than 800 MeV points to the existence of states of different parity, and the disappearance of such asymmetry at 900 MeV indicates that a change of phase, possibly in a \(P_{1/2}\) or \(P_{3/2}\) state, has taken place.

Fig. 42, due to Walker and Dixon, shows that the differential intensity of \(\pi^+\) at 90° is 8 \(\mu\) b/ster at 750 MeV. Fig. 37, however, shows that at the same angle and energy the \(\pi^0\) intensity is 4 \(\mu\) b/ster. This ratio of 2 is in agreement with a resonance state of \(T = \frac{1}{2}\), since \(\pi^+n\), like \(\pi^-p\), is one-third of the time in a \(T = \frac{3}{2}\) state, and two-thirds of the time in a \(T = \frac{1}{2}\) state. \(\pi^0\) is vice-versa.

The same indication is observed in the total cross-section curves for \(\pi^0\) (Fig. 43) when compared with \(\pi^+\) total cross-section (Fig. 41).

Clegg and Bingham, at Cal. Tech., detect neutral pions with a Cherenkov counter and obtain agreement with the counter telescope and magnet data. With such a detector they also measure double production of mesons, obtaining a cross-section of approximately \(5 \times 10^{-30}\) cm\(^2\) for a gamma energy from 800 to 1000 MeV.
Double photoproduction of mesons

Sellen et al.\(^{19}\) have measured the total cross-section for
\[ \gamma + p \rightarrow p + \pi^+ + \pi^- , \]
with a hydrogen diffusion chamber (lower histogram of Fig. 44, the upper histogram is before normalisation to the photon spectrum).

Fig. 43. Reaction \( \gamma + p \rightarrow \pi^0 + p \), total cross-section vs \( \gamma \)-energy.

Fig. 44. Reaction \( \gamma + p \rightarrow p + \pi^+ + \pi^- \). (Sellen et al.\(^{19}\))
Upper histogram, number of meson pairs observed vs \( \gamma \)-energy. Lower histogram, total cross-section vs \( \gamma \)-energy.

Fig. 45 shows the angular distribution and Fig. 46 the \( Q \) value distribution for all the possible pairs among the three produced particles. One notices some preference for high values in the \( Q (p - \pi^+) \) and for the backward direction in the \( \pi^- \) angular distribution. Block and Sands have also obtained data on double meson production by detecting \( \pi^- \). They compare their results on energy spectra at a substantially fixed angle with a computation on the basis of an isobar model. (Figs. 47, 48, 49.) At 660 MeV the points seem to follow the curve for the doubly charged isobar \((p + \pi^+)\), while at 1000 MeV the isobar \((p + \pi^-)\) is perhaps preferred.
The nucleon and its interaction

Fig. 46. As Fig. 44, but showing distribution of $Q$-values for all possible outgoing particle parts.

Fig. 47. Reaction $\gamma + p \rightarrow p + \pi^+ + \pi^-$. Distribution of $\pi^-$ energy in c. m. space compared with $(p + \pi^-)$ and $(p + \pi^0)$ isobar models: $\gamma$-energy 660 MeV. (Sellen et al.19)

Fig. 48. As Fig. 47, but at $\gamma$-energy 820 MeV.

Fig. 49. As Fig. 47, but at $\gamma$-energy 1000 MeV.

Fig. 50. Apparatus for studying $\bar{p} - p$ interaction (Coombes et al.20) $a$, $s$, ..., and $t$ represent scintillation counters.

Fig. 51. Differential elastic scattering cross-section of $\bar{p}$ on $p$ at 133 MeV. (Coombes et al.20) Dotted curve, Chew-Ball theory calculated by Fulco. Solid curve, black disc diffraction.
Antiprotons

TABLE VI

<table>
<thead>
<tr>
<th>$T$ (MeV)</th>
<th>$\sigma_{\text{Total}}$ (mb)</th>
<th>$\sigma_{\text{Elastic}}$ (mb)</th>
<th>$\sigma_{\text{Ch. exch.}}$ (mb)</th>
<th>$\sigma_{\text{an}}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>133 ± 13</td>
<td>166 ± 8</td>
<td>72 ± 9</td>
<td>$10^{+2}_{-3}$</td>
<td>84</td>
</tr>
<tr>
<td>197 ± 16</td>
<td>152 ± 7</td>
<td>$64^{+7}_{-9}$</td>
<td>$11^{+2}_{-4}$</td>
<td>77</td>
</tr>
<tr>
<td>265 ± 17</td>
<td>124 ± 7</td>
<td>$50^{+6}_{-7}$</td>
<td>$8^{+2}_{-3}$</td>
<td>66</td>
</tr>
<tr>
<td>333 ± 17</td>
<td>114 ± 4</td>
<td>$49^{+5}_{-7}$</td>
<td>$8^{+2}_{-3}$</td>
<td>57</td>
</tr>
</tbody>
</table>

The advances in the physics of antiprotons are due to a counter experiment, two bubble chamber experiments and to continued study with nuclear emulsions.

In the counter experiment of Coombes et al. a hydrogen target is surrounded with many counters, the pulses of each counter being separately observed (Fig. 50). If an interaction gave a count in only one counter it was counted as an elastic scattering, but if in two counters it was an elastic or inelastic interaction according to the position of the two counters.

The beam of antiprotons, which usually contains only one antiproton for every $3 \times 10^5$ pions at these energies, was enriched by about a factor of order 10 by passing the beam through crossed electric and a magnetic fields.

Table VI shows the data for elastic and annihilation cross-section at four energies. The differential elastic scattering data are shown in Fig. 51 to 54. One of the theoretical curves shown is Fulco's, computed on the basis of the theory of Chew and Ball; the other is the simple black disc diffraction pattern. Note that the radius of the disc fits also the value of the inelastic cross-section.

In Fig. 55 we see the behaviour vs energy of the antiproton cross-sections. This figure shows a gradual uniform variation with energy. The point at 450 MeV is

Fig. 52. As Fig. 51, but at 197 MeV.

Fig. 53. As Fig. 51, but at 265 MeV.

Fig. 54. As Fig. 51, but at 333 MeV.
The nucleon and its interaction

The inelastic cross-section measured by Segrè, Chamberlain and others\cite{Segre}, 89 ± 7 mb, and the theoretical curves are due to Chew and Ball.

Goldhaber et al.\cite{Goldhaber} have found 16 antiproton-proton scattering in emulsion representing an elastic cross-section of 77 ± 19 mb in the energy range 30 to 220 MeV. Fig. 56 shows their angular distribution. Also with plates, Chamberlain et al.\cite{Chamberlain} have collected data on elastic scattering of antiprotons with complex nuclei, Fig. 57. The curve is calculated by Glassgold using the optical model. For annihilation in nuclei they find a pion multiplicity of 5.3 ± 0.3.

Horwitz et al.\cite{Horwitz} collected and analysed 88 pictures of antiprotons annihilating in the liquid hydrogen bubble chamber of the Alvarez group. The charged pion multiplicity was found to be 3.11 ± 0.14: multiplied by 1.5 to take account of π° this gives a multiplicity of 4.8 ± 0.22 in substantial agreement with the plate work. A nice check on the number of the invisible π°'s is the fact that the energy seen in the charged pions is just about two-thirds of 2 MeV.

The average kinetic energy of the pions is 252 MeV. The emulsion work gave 200 MeV, but it is to be expected that some energy loss occurs due to interaction between the pions and emulsion nuclei.
Among the hydrogen bubble chamber pictures was one showing the production of a $\theta^0$, $\theta^0$ pair.

The propane bubble chamber group (25) obtained preliminary data on the $\bar{p}-p$ and $\bar{p}-\pi$-carbon cross-sections. In hydrogen the integrated elastic cross-section is $58 \pm 11$ mb at an average energy of 120 MeV; the annihilation cross-section is about 75 mb, the total cross-section between 125 and 150 mb. In carbon, the elastic cross-section is about 285 mb, annihilation about 550 mb, and total about 850 mb. Fig. 58 shows the result of elastic scattering in carbon. The curve is an optical model calculation by Fernbach. Some 2% of the annihilation events produced heavy mesons.

A beautiful picture of an antiproton charge exchanging into an antineutron and then annihilating is shown in Fig. 59. The important point for the interpretation of this picture is that such a star is never seen in the experiment, except at the end of an antiproton track, and must therefore be attributed to an annihilation. On the other hand, the track ending at the arrow is a certified antiproton. The interpretation of the picture is therefore quite certain.

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9. Dowell, J., Frisken, W., Martinelli, G., Musgrave, B.
17. De Wire, J. W., Jackson, H. E., Littauer, R., Rogers, K. C, Stein, P. C.
20. Horwitz, N., Miller, Murray, J. J. and Tripp, R. D.

**DISCUSSION**

*Alvarez:* The antiproton work that was done with our bubble chamber was only made possible by an electromagnetic separator built by Murray and Horwitz. This device delivered to the bubble chamber a beam enriched in antiprotons by a large factor which greatly facilitated the measurements.

*R. R. Wilson:* I would like to expand a bit on the new resonance that seems to be evident in the photoproduction.

Fig. 60 shows a level diagram of the proton. First after the ground state ($1s$, $1s$) is the familiar excited state ($2s$, $2s$) at 300 MeV. The strong peak in the photoproduc-

(*) referred to in the text and figures as Walker and Ballam.
Fig. 59. An antiproton is transformed by charge-exchange (arrow) into an antineutron which goes on to give a typical annihilation star. (Propane bubble chamber, Agnew et al.\textsuperscript{21}).
tion indicates that there is a second resonance state at 600 MeV with $T = \frac{1}{2}$ and probably $J = \frac{3}{2}$, (Figs. 40-42). We believe that $T = \frac{1}{2}$ because the ratio of $\pi^+$ to $\pi^0$, which is $\frac{1}{2}$ for the 300 MeV state in accordance with the Clebsch-Gordan coefficients, is more like $\frac{2}{3}$ for the 600 MeV state. Furthermore this resonance comes exactly at the same energy as the minimum of the $\pi^+\pi^-$ scattering cross-section and ought to show up there if $T$ were $\frac{3}{2}$, whereas this nearly zero scattering is consistent with the resonance in a $T = \frac{1}{2}$ state.

The angular momentum is believed to be $\frac{3}{2}$ because of the angular distribution of the $\pi^0$ which is consistent with $(2 + 3 \sin^2 \theta)$. If the 600 MeV state exists you would expect to excite the $\pi^0$ production entirely through it and this would, for a $J = \frac{3}{2}$ leading to a $p$-wave, give a $(2 + 3 \sin^2 \theta)$ distribution. There is a difficulty on the other hand in the $\pi^+$ angular distribution which throughout this resonance is maximum in the forward direction until you start to excite a higher resonance (Fig. 40). If the state decayed into a $p$-wave $\pi^+$, one would expect a backward distribution below the resonance changing over into a forward distribution above the resonance, just as it is for the lower $(\ell = 2, \lambda = 3)$ state because of interference with the direct production. Peierls of Cornell has suggested to me that this could be explained if the 600 MeV state had a negative parity. It would then be excited by electric dipole absorption and would decay in a $d$-wave, still with a $(2 + 3 \sin^2 \theta)$ angular distribution, but the interference would be greatly reduced, the asymmetry coming now from the tail of the $(\ell = 2, \lambda = 3)$ state.

On the other side the reduced width of the 600 MeV resonance appears to be about like that of the 300 MeV resonance. This one obtains by fitting a resonance formula, for example, to the $\pi^0$ production data. The same reduced width is also obtained by fitting the $a_{1\pi}$ phase-shift given by pion scattering, assuming the resonance to occur at 600 MeV, and a positive parity of the level.

Thus we seem to have a rich spectroscopy of the proton. What previously appeared in the $\pi^-\pi^0$ scattering to be a single $T = \frac{1}{2}$ resonance now looks like two resolved levels. The next level would, if you make this assumption, come at 800 MeV and would also be $T = \frac{1}{2}$ with an angular moment $\frac{3}{2}$ or higher. Indeed a further resonance is indicated by $\pi^-\pi^0$ scattering at 1000 MeV with $T = \frac{3}{2}$ and $J = \frac{3}{2}$ or more. So there appear to be at least 4 excited states of the proton approximately equally spaced, and narrow enough so that they can be expected to dominate the meson processes occurring at each energy. They may lead to some simplifications in the study of the photoproduction and suggest something of the proton structure.

**R.L. Walker:** I would like to make a couple of comments about what R.R. Wilson has just told you. One is that the shape of the curve of $\pi^\pm$ photoproduction with energy is so striking that I would also agree that it certainly looks like a resonance. The assignment of isotopic spin seems fairly clear, but the assignment of the angular momentum is at least in doubt. It could be $\frac{3}{2}$, but the shape of the angular distribution of $\pi^0$ photoproduction which is roughly $(2 + 3 \sin^2 \theta)$ can also arise from an interference between a $J = \frac{3}{2}$ state and the tail of the old $J = \frac{3}{2}$ resonance, or there is probably also some other background of some other states. This has been shown by some calculations of Clegg at Cal. Tech., who succeeds in reproducing fairly well the shapes of the angular distribution of $\pi^0$ if he includes this interference between a $P_{1\pi^\pm}$ resonance near 700 MeV photon energy and the tail of the old resonance.

The other point, having to do with the fact that the angular distribution on $\pi^\pm$ does not change as you go through the resonance, could also be explained in terms of the interference; there is presumably an interference with the $s$-wave, and if the $s$-wave part had a fairly large phase itself, the angular distribution will not change until the difference in the phases goes through $90^\circ$, and that could happen somewhat above the resonance, as seems to be the case. Also if you want to explain the cross-sections by a resonance, you have to account for the difference between the shape of the energy dependence of the $\pi^\pm$ production and $\pi^0$ production; and this could be done perhaps, although only very vague calculations have been made, by also taking into account the interference between this, state which is $T = \frac{1}{2}$, and a general background of $T = \frac{3}{2}$ arising in part from the tail of the old resonance. This can give the very sharp drop in the $\pi^\pm$ production curve above the resonance, and at the same time would give somewhat of a rise in the $\pi^0$ curve, and I believe that by some suitable combination of these states one could explain this difference, which is one of the puzzles, if you want to explain it without any interference terms.
R. R. Wilson: Could I make a remark on the comment of Walker, about whether the $p$-state is $^3_1$ or $^1_2$, or rather whether the excited state, if it exists, is $^1_3$ or $^3_2$. There is one other bit of evidence that comes from the scattering, that is, referring to Fig. 3 of Piccioni's report, once one has assigned the energy, the isotopic spin and the angular momentum to the state, then one should be able to calculate absolutely the $\pi$-$p$-scattering; and the lower curve in Fig. 3 shows that the calculation fits the low energy part of the single peak of Cool, Piccioni and Clark rather well. On the other hand, this is an absolute calculation, and you need all of the cross-section you can get. That is, I think you need a $p$-wave; an $s$-wave would give the dotted curve down at the very bottom and this would be too small by a large factor. I think this is some evidence that it is a $P$-state.

R. L. Walker: I will make a brief comment on that. The proposal of Clegg was not that this is an $S$-state, it is a $P$-state, but that the $J$-value be $^1_2$. There is no evidence that it is $^3_2$; we just want to point out that there is also no evidence that it should be $^3_2$.

Gell-Mann: I would like to say one or two theoretical words that may be relevant, or may not, to this question of the sudden switch in the angular distribution of the photoproduction of single pions. The situation is reminiscent of something that I think is very familiar, that is the scattering of neutrons and protons in the $^2S_1$ state, which is the state that includes the deuteron. If you plot the phase-shift as a function of energy then at zero energy, by virtue of the fact that there exists a deuteron at negative energy, the phase-shift can be considered to be $\pi$; and above zero energy the phase-shift drops down from $\pi$ towards zero, passing through $\pi/2$, very inconspicuously, at some several MeV. Nobody, of course, calls this a resonance, because the phase-shift is coming down through $90^\circ$. Now in the case of the pion-nucleon problem, in the $P_{\frac{1}{2}}$, $\frac{1}{2}$ state, which is the state that was discussed by Walker and not by Wilson, there is perhaps a similar phenomenon to be expected theoretically, which may or may not be relevant to the experimental situation. Here the bound state is the nucleon; the phase-shift at zero energy should start at $\pi$, and in every theory it does; it then comes down from $\pi$ in any theory, very slowly actually. Now there is no reason to believe that it does not come down, as in the neutron-proton case, through $\pi/2$ to zero. Specific models in field theory have been investigated by Salzman, and by Fubini and Thirring, sort of static models. In both of these pieces of work, it was mentioned that it was absolutely necessary that this occurs, but it was seen as a feature of the specific model and it was sort of disavowed. But I would like to point out that I think it is probably a very general thing that in almost any theory, without any great dependence on specific models, the phase-shift should come down through $\pi/2$ from $\pi$. Now as to whether in coming down through $\pi/2$ it gives anything like a peak in the cross-section (as opposed to the change in the angular distribution which this does perhaps explain) depends on how rapidly it comes down through $90^\circ$, and causality sets limits on how rapidly a phase-shift can come down through $90^\circ$. But it may be that a small peak can result from this kind of thing. If so, and if it should turn out that we are discussing the $P_{\frac{1}{2}}$, $\frac{1}{2}$ state here, this might be the explanation.

Adair: I would like to make a comment on Gell-Mann's statement. Such a thing is already known in nature with neutron scattering, for example, on lead. You do not there get any kind of peak. However, one thing you can say about this sort of pseudo-resonance, is that it is not in general associated with any absorption maximum when one goes through $\pi/2$. Such an effect is then not obviously related to the photoproduction discussion.

Feld: We are clearly dealing here with quite complex phenomena because if one looks at the scattering data, it is quite obvious that a single angular momentum $^1_1$, resonance, or a single angular momentum $^3_2$, resonance, will not be enough to give the observed total cross-section for $\pi^-$/protons. Indeed, it seems rather unlikely that even a single angular momentum $^3_2$, resonance would give the observed total, so if one is going to have any resonance which involves those angular momenta one needs at least two, if not all, of these states. If indeed the evidence which has been presented indicates that one of these states, let us say, the $^3_2$, has negative parity, it would lead to the emission of $d_{\frac{3}{2}}$ pions. These could easily interfere with the $P_{\frac{1}{2}}$, $\frac{1}{2}$ state, whose phase-shifts have just been discussed, to give the rapid change in the angular distribution. In addition phenomena involving the $2\pi$ production must also, of course, exhibit such resonances effects. Indeed it may be that the 2-$\pi$ production phenomena, involving the excitation of the isobar, may somehow also lie at the heart, or at least close to the heart of this business. If one thinks that the $^3_2$, $^3_2$ isobar, like the nucleon itself, interacts strongly with pions in a $P$-state, then it is possible to have inelastic scattering where the pion interacts strongly in the final state with the isobar and then the isobar decays and one gets two pions as a result. Lindenbaum and Sterneheimer have shown that the evidence on the inelastic production of pions by pions on nucleons does agree with such a model in the angular and energy distributions of the pions. If one makes such a model, then the total angular momentum which would be involved would be $^3_2$ added to $\frac{1}{2}$ which would give $^3_2$, $^3_2$ or $^3_2$ and it could very well be that one or more of these states are involved here.

W. D. Walker: I want to make a comment on the $\pi^-$/p scattering. I think the data as they stand would be quite consistent with allowing, say, $a_{11}$ and $a_{13}$, probably both, to go through 90°. The $a_{11}$ I would prefer to see go through 90° first, at about 500 or 600 MeV, $a_{13}$ later. The $a_{11}$ resonance might not be strongly damped (i.e. give rise to inelastic scattering) in order to be consistent with the differential elastic scattering. $a_{13}$ must be strongly
damped by inelastic processes. The other complicating thing in this energy range is that the \(d\)-wave pretty much dominates the scattering and it is the \(a_{11}\) and \(a_{13}\) beating against the \(d\)-wave that produces the change in the angular distribution of the \(\pi p\) scattering in the neighbourhood of 700 MeV. If the change in the angular distribution is due to a change in the interference in the spin-flip between the \(d\)-wave and the \(p\)-wave, then the condition for the change from forward to backward interference is that \((a_{11} + a_{13})\) be of the order of 180°.

**Sachs:** I agree with Adair that it is extremely important to make the distinction between a 90° phase-shift and a resonance in the sense of a quasi-stationary state. We usually think of a resonance as a quasi-stationary state, and the crucial thing is always the width of the resonance. In this connection one wants to try to determine a reduced width. I do not know how Wilson determines that reduced width. I suspect that he used the standard Wigner-Eisenbud type of determination which involves using a radius of some kind and I assume that he used the pion Compton wavelength for the radius. He says "yes". I do not think this is the right way to get the reduced width for this type of problem because we do not have a clear-cut radius of interaction. But one does have to find a way to determine a reduced width in order to tell whether we are dealing here with a true resonance or a quasi-stationary state, and the crucial thing is always the width of the resonance.

**R. R. Wilson:** I would like to add one more thing about this excited state. If you add up the \(\pi^+\) cross-section, the \(\pi^0\) cross-section and then what you would compute for the multiple production of mesons using the Clebsch-Gordan coefficients and phase-space arguments, you find that this state is excited about twice as much as the first resonance state, that is, it is a very strong state. I think this tends to be consistent with the idea that you excite it through electric dipole absorption rather than magnetic dipole absorption leading to a positive parity state.

**Gell-Mann:** I just want to clarify my remarks by one or two additional ones. I was not trying to suggest that I can account for the peak. I do not think that I can. I was just going to say that one should watch out in this region for the possibility of the \(P_{3/2}\) phase-shift coming down through 90° which can give a change in the angular distribution. I should say a second thing, which is that the phase-shift is not very well defined up here because there is a lot of strong inelastic effect in it, that is, there is one pion in and two pions out, which makes the concept of the phase-shift not very good any more. And so the theorem that the phase of the photoproduction matrix element is determined by the phase of the scattering, fails. I wanted to mention the possibility that phase-shifts coming down through 90°, as this one is expected to, might become confused with genuine resonances in which phase-shifts go up rapidly through 90° and that the situation might become so complicated that both perhaps are occurring somewhere in this region of energy.

**R. R. Wilson:** I would like to ask about Piccioni’s slide about antiproton-proton annihilation. (Fig. 55). He puts a theoretical curve on that slide which he said was derived from a proton-proton potential with change of signs in accordance with meson parity presumably. I just wanted to ask what was done about the spin-orbit coupling.

**Chew:** The spin-orbit coupling was kept as it appears in the Signell-Mashak potential. However, we did another calculation with a purely meson theoretical potential derived by Miyazawa and co-workers which has now spin-orbit terms and it did not seem to make very much difference.

**Ruderman:** Gell-Mann remarked about the theorem that when one has a bound state the phase-shift goes from \(\pi\) to 0. In the presence of inelastic processes it seems as if this same Levinson theorem still holds as long as one replaces the phase-shift by the real part of the phase-shift and as long as one has microscopic causality.

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The nucleon and its interaction with pions, photons, nucleons and antinucleons

Chairman R. E. PEIERLS

THEORETICAL
Rapporteur G. F. CHEW
Secretaries R. HAGEDORN
A. STANGHELLINI
V. WATAGHIN
I. Introduction

The subject this morning is essentially “strong interactions”, with the exclusion of strange particles. The nucleon happens to be the only strongly interacting particle from which targets for scattering can be made, and so it plays a central role in all experiments on strong interactions. In principle, however, the pion-pion interaction belongs in this morning’s category, along with the nucleon-nucleon and the pion-nucleon interactions. Antinucleons are, of course, closely related to nucleons; an understanding of one requires an understanding of the other.

Photons are included because, just as do electrons, they serve as a convenient probe into the structure of strong interactions. For practical reasons, electron-nucleon scattering has been given a separate place in the programme of this conference, but it is clear from yesterday’s theoretical discussion that one cannot draw a sharp dividing line.

It is, in fact, the most characteristic feature of the theory of strong interactions that no one problem or even subgroup of problems can ever be thoroughly understood without, at the same time, understanding all the problems in the field. The reason for this circumstance is that nowhere in the strong interaction picture is there a really small dimensionless parameter which permits a decoupling of different phenomena. In contrast, electrodynamics possesses the fine structure constant, $1/137$, and the ratios of electron mass to those of the other charged particles, which range from 1/200 to 1/2000. The chief crutches of strong interaction theory so far have been the ratio of pion to nucleon mass, $\mu/M \approx 1/7$, and the ratio of the $\Omega^-$ to nucleon rest energy, a miserable $1/10$. In addition, it seems likely that the $K$ meson coupling to baryons is substantially smaller than that of pions: $g_K^2/g_{\pi N}^2 \approx 1/4$. This fact, together with the ratio of pion to $K$ meson mass, $\mu/m_K \approx 1/7$, partly justifies the exclusion of strange particles from this morning’s discussion. It is hoped, in other words, that strange particles are sufficiently difficult to create, even virtually, that without recognizing them explicitly a meaningful discussion of the pion-nucleon problem can be carried on. It may turn out that this hope is not justified.

A second characteristic feature of the theory of strong interactions is that so far the problems are not well defined, even on a practical level. In electrodynamics the conventional objective has been to predict the results of experiments in terms of two given constants, the electron mass and charge. In strong interaction theory a well-known dilemma has always been whether, in addition to the pion and nucleon masses and the mutual coupling constant, one should include a pion-pion interaction constant as a fundamental parameter. On the other hand, it has frequently been suggested that the pion is not “elementary” and that not only its interactions but also its mass should be predictable. The existence of the strange particles further confuses the situation and causes severe doubt as to whether any absolute distinction between “fundamental” and “derived” constants makes sense in the strong interaction picture.

In this situation we must be even more liberal than usual in our definition of a “successful theory”. I think we must call a contribution a successful theory if it leads to any correlation between experiments which has not previously been recognized. Some theories may express their predictions in terms of conventional parameters such as masses and coupling constants, while others may appear phenomenological because they use the position of a resonance or scattering amplitudes at a particular energy. Theories with the smallest number of parameters are, of course, the most satisfactory, but in the present state of confusion about the meaning of the elementary particle concept, I cannot think of any other absolute criterion for judging the status of a theory. Short of a complete explanation on the level attempted by Heisenberg, all theories of strong interactions are phenomenological.
II. Review of current theory

A. Static models

There are at present two types of theoretical approach which have yielded convincing experimental correlations. The first exploits the largeness of the nucleon mass with respect to both the pion rest mass and the \((\pi/\rho)\) resonance energy. All nucleon degrees of freedom except spin and isotopic spin are suppressed and calculations are made with various cut-off field theoretical Hamiltonians. This approach has given a fair correlation of the \(p\)-wave pion-nucleon phase-shifts up to the \((\pi/\rho)\) resonance in terms of three parameters: the pion mass; the pion-nucleon coupling constant; and a cut-off energy. The first two can be independently measured. Although many attempts have been made to extend the method to include \(s\)-waves, no significant correlations have emerged. That is to say, the two \(s\)-wave scattering lengths have not yet been calculated from less than two new and adjustable parameters. Some static models make distinctive predictions about the energy dependence of the phase-shifts, but (as we heard yesterday) this dependence is not sufficiently well known experimentally to distinguish between different models.

The fixed nucleon approach has had its most impressive success in the problems of photo-pion production and photon scattering by nucleons. In the first case, one can derive a formula in terms of the same three parameters used for pion-nucleon \(p\)-wave scattering, plus the fine structure constant and the anomalous nucleon magnetic moments. This formula gives a semi-quantitative fit to experiment up to the \((\pi/\rho)\) resonance. The corresponding formula for photon-nucleon scattering is less easy to derive because there are more degrees of freedom and the calculations are more complicated, but the preliminary results obtained to date—with the same parameters as for photo-pion production—correspond in a satisfactory way to the experimental situation.

It is less easy to describe in terms of parameters the application of the fixed nucleon approach to the nuclear force problem \(^1\). All workers in the field agree that a quantitative description of the outside fringe of the nuclear force is successfully given in terms of two parameters, the pion mass and the pion-nucleon coupling constant. Nearly all workers agree that the radius of the short-range hard core in the nuclear force must be introduced as a new parameter, although an attempt has been made to derive this radius from the cut-off energy appropriate to pion-nucleon scattering. For the intermediate region, formulae have been derived in terms of \(s\)- and \(p\)-wave pion-nucleon phase-shifts, but there is disagreement about how successful these formulae are and how successful they should be. The potential which has received most attention during the past year is, in the terms of this discussion, a real bastard. The so-called Signell - Marshak (SM) potential has an almost unbelievably obscure parentage. It should be called the Yukawa - Brueckner - Watson - Gartenhaus - Case - Pais - Goldfarb - Feldman - Signell - Marshak potential. The outer fringe is due to Yukawa and is non-controversial. The core was derived by Gartenhaus and is highly controversial. The static part in the intermediate range was derived by Brueckner and Watson and is moderately controversial. The spin-orbit part is due to Case - Pais and Goldfarb - Feldman and so far has no field theoretic derivation (see below). The whole thing was put together by Signell and Marshak who demonstrated that the combination has an impressive ability to predict the results of experiments. Japanese theorists, under the leadership of Taketani, have been more cautious and systematic. They argue, with much justification, that the static model cannot be trusted at distances smaller than the pion Compton wavelength and, as we shall hear, they are sceptical of the need for a spin-orbit interaction.

The nucleon-antinucleon problem has much in common with the nucleon-nucleon, but the crucial question of the annihilation process cannot be discussed within the framework of the conventional static model. The same is true for the pion-pion interaction.

B. Relativistic dispersion relations

The second theoretical approach which has made successful predictions in the strong interaction field is that of the relativistic dispersion relations. These relations have a very peculiar status at present. In part they are derived from the theoretical principle of causality, that events at different space-time points cannot affect each other unless a signal of light velocity can pass between them. However, the dispersion relations also require a knowledge of the experimental mass spectrum of strongly interacting particles (they do not distinguish between elementary and complex particles) and the assumption is tacitly made that the theory actually has these energy eigenvalues and no others. Now the only causal relativistic theory that we know of is local field theory, but many people believe that conventional local field theory necessarily contains unphysical eigenstates; where such a circumstance would leave the dispersion relations is unclear.

I believe a reasonable attitude for this particular session of the conference is that the dispersion relations represent a conjecture that scattering amplitudes can be extended into the complex plane with the minimum number of singularities required by the unitarity of the \(S\)-matrix. That is to say, there are no singularities except where actual physical masses occur. This conjecture may be inconsistent with local field theory but it leads to definite and non-trivial experimental predictions. I think our principal task at this session is to see what predictions have so far been worked out theoretically and to what extent they have been verified. The physical meaning of dispersion relations we leave to a different session.

Four different types of dispersion relation application may be distinguished. First, the complete relations may be truncated on the basis of the dominance of the \((\pi/\rho)\) resonance, so as to exclude very high energy phenomena from explicit consideration. The resulting approximate relations may then be regarded as a relativistic generalization...
of the static model. We heard yesterday about the most useful applications of this type: to photo-pion and electron-pion production. The approximate nature of this type of application of dispersion relations must be emphasized. It represents a definite improvement over the static model in that it satisfies Lorentz and gauge invariance, but it is not a straightforward and clean-cut approach, and its failure to predict experiments with precision cannot be interpreted as a failure of the underlying dispersion relations.

All the problems which have been discussed with any success by the static model have by now been restudied through truncated dispersion relations. Generally speaking, the correlations produced by the static model can be reproduced and in most cases improved in accuracy. (In the photo-pion problem, for example, kinematical confusion over nucleon recoil, which could not be resolved by the static approach, seems to have been successfully eliminated.) In some cases the dispersion relations have led to new correlations. An impressive instance of this kind is a relation between the pion-nucleon charge-exchange s-wave scattering strength, the coupling constant and an integral over the $(1/2, \gamma_3)$ resonance.

A second example of a correlation not given by the static approach is a formula for the negative to positive photo-pion production ratio in terms of nucleon magnetic moments. We heard yesterday something about how well this formula agrees with experiment. We shall hear in this session of new calculations on photon-nucleon scattering by the truncated dispersion relation approach.

The problem which so far has yielded least to this approach is that of the nucleon-nucleon interaction. Difficulties which can sometimes be concealed in the static model are glaringly evident in the relativistic nucleon-nucleon dispersion relations. Much remains to be done in this area before a connection can be made with something as complicated as the current semi-phenomenological potential, but I personally believe that a connection will be made. It seems to me probable that the hard core and the spin-orbit force will eventually be related to other phenomena, such as pion-nucleon and nucleon-antinucleon scattering. Whether such a relation would constitute an "explanation" of these features of the nuclear force is, I think, a semantic question.

A second type of application of dispersion relations is the investigation of whether the relations are satisfied by experiment with the expected precision. We heard yesterday a discussion of this question for the case of forward pion-nucleon scattering. I shall simply remark here, that over the past year many theoretical investigations of electromagnetic effects in this problem have revealed no significant corrections to the dispersion relations when only real and imaginary parts of the measured charged pion elastic scattering amplitudes are used. Charge independence is in principle not required. It is possible, however, that when charge independence is used to supplement inadequate information on elastic scattering with data on charge exchange scattering there can be trouble. Therefore measurements of the forward pion-nucleon amplitudes which do not involve phase-shift analysis and thus, implicitly, charge independence, are highly desirable.

I think the appropriate conclusion to draw from yesterday's discussion is that a great deal more effort in precise verification of the pion-nucleon forward dispersion relations is worthwhile, but that for the present we should regard the experimental evidence as favourable.

If one accepts the validity of dispersion relations, the possibility of precise definition and measurement of the pion-nucleon coupling constant is presented, since this constant appears as the residue of poles in the pion-nucleon and nucleon-nucleon scattering amplitudes. This is a third type of dispersion relation application. Actually there are three constants: the charged, $f_2^s$; the neutral pion coupling to neutrons, $f_{\pi N}$, and the neutral coupling to protons, $f_{\pi p}$. These three should be equal according to charge independence, but because of electromagnetic corrections they are almost surely not exactly the same. The only constant for which a reasonably accurate determination has been made is $f_{\pi N}$. From yesterday's discussion it appears that if dispersion relations are valid so that a precise definition can be given to $f_2^s$, then its value must be within 10% of 0.08.

Many determinations of $f_2^s$ have been made on the basis of the static model and also of truncated dispersion relations, with varying results, although there is a definite tendency to cluster around $f_2^s = 0.08$. It should be kept in mind that these determinations do not have the same degree of validity as those based on complete dispersion relations. Among the contributions to this session will be two proposals for using nucleon-nucleon scattering data for a determination of $f_2^s$. One of these uses a truncated relation, the other method is, in principle, exact but requires some extrapolation of the data.

The fourth and last application of dispersion relations is the most interesting but so far has made the least progress. It is the attempt to use them as fundamental dynamical relations which replace the usual field equations. There has been a hope that in this way, since one does not commit oneself to regarding any particles as elementary and never speaks of point interactions and "unrenormalized" constants, many of the difficulties associated with the Lagrangian approach to field theory could be avoided. Gell-Mann pointed out several years ago that when the coupling is weak, the dispersion relations can be expanded in a power series which reproduces the content of conventional perturbation theory. This observation continues to stimulate efforts to treat the relations as integral equations which, even in the case of strong coupling, give a complete description of the system.

III. Contributions

1. The first contribution to this session is from Mandelstam and is relevant to all four aspects of dispersion
relations. This work will perhaps seem baffling to the experimenters, but I assure you that we shall come quickly to an important practical application. Mandelstam has conjectured a generalization of dispersion relations for scattering amplitudes in which both energy and momentum transfer are extended into the complex plane. Nambu proposed such an extension earlier but subsequent developments showed that the Nambu representation was too simple. Mandelstam’s representation is very simple, too, but it seems to satisfy all known general requirements and has been verified in fourth order perturbation theory.

Consider a relativistic scattering process—with no spin (Fig. 1).

Define

\[ x = (p_1 + p_2)^2 = -W^2, \]
\[ y = (p_1 - p_2)^2 = \delta^2, \]
\[ x' = (p_1 - p'_2)^2, \]

with

\[ x + y + x' + m_1^2 + m_2^2 + m_3^2 + m_4^2 = 0. \]

Mandelstam:

\[ T(W^2, \delta^2) = g^2 \left[ \frac{1}{x + M^2} + \frac{1}{x' + M^2} \right] \text{ (for pion-nucleon scattering)} \]
\[ + \int \frac{d m_1^2 \, d m_2^2 \, q_1(\delta^2, m^2)}{(x + m^2)(x' + m^2)} \]
\[ + \int \frac{d m_2^2 \, d \delta^2 \, q_2(\delta^2, \delta^2)}{(x + m^2)(y + \delta^2)} \]
\[ + \int \frac{d m_1^2 \, d \delta^2 \, q_2(\delta^2, \delta^2)}{(x' + m^2)(y + \delta^2)}. \]

The real weight functions \( q_1, q_2 \) vanish not only for masses less than those corresponding to the appropriate physical thresholds but within a boundary curve asymptotic to these limits. For example, consider \( q_1(\delta^2, \delta^2) \). (See Fig. 2.)

It is easy to derive the conventional dispersion relation for \( W^2 \) at fixed \( \delta^2 \) by performing one of the two integrations, e.g. for the second term, integrate over \( d\delta^2 \). However, one may also obtain a dispersion relation for \( \delta^2 \) at fixed \( W^2 \) by performing the integration over \( dm_1^2 \).

Mandelstam finds the boundary curve for the mass spectrum as a by-product of his novel method for “solving” these relations. He does not need to know the curve in advance. Roughly speaking, his method is as follows. Write down the two one-dimensional dispersion relations, the one for \( W^2 \) at fixed \( \delta^2 \) and the other for \( \delta^2 \) at fixed \( W^2 \). In the ordinary relation, the weight function is the imaginary part of pion-nucleon elastic scattering. In the other, it is the imaginary part of the amplitude for two pions to go to a nucleon-antinucleon pair. Use the unitarity condition to express these imaginary parts as angular integrals over bilinear combinations of various amplitudes. These angular integrations are performed in each case with the aid of the “other” representation and one is left with relations involving mass integrals which are then regarded as coupled integral equations. At this stage one may identify the boundary curve for the mass spectra.

Mandelstam is led in this way to introduce pion-pion scattering into the pion-nucleon problem, a development which, for a long time, has seemed in order but which had not been incorporated before into the dispersion relation framework. He regards his relations, when supplemented by unitarity, as dynamical equations and has developed an iteration procedure which he hopes will allow a numerical solution. This procedure is far too complicated to describe here and so far has not yielded concrete results.

2. The first practical result from the extension of the momentum transfer into the complex plane is a new method of measuring coupling constants.

This is a collaboration in which I have been involved together with Cziffra, Moravesik, Taylor and Uretsky 3) 4). Lehmann 5) recently showed that the real and imaginary parts of elastic scattering amplitudes considered as a function of \( \Lambda^2 \) for fixed \( W^2 \) are separately analytic in the complex plane at least within ellipses which include the physical region. Since

\[ \Lambda^2 = 2k^2(1 - \cos \theta), \]

where \( k \) is the barycentric system momentum, when we discuss \( \Lambda^2 \) it is equivalent to discuss \( \cos \theta \). The foci of
Lehmann’s (Fig. 3) ellipses are at ±1 and the length of the major axis depends on the energy. Furthermore, the ellipse for the imaginary part is larger than that for the real. Lehmann, however, makes no statement as to the location of the singularities except that they are outside these ellipses.

The Mandelstam representation places the singularities all on the real axis at positions which for the real part are correlated in a simple way with the physical mass spectra. (The location of the singularities for the imaginary part requires a calculation.) The Mandelstam recipe, which many others have independently guessed, is as follows.

To get the right hand spectrum, i.e., \( \cos \theta > 1 \), consider the states which can be reached considering \( p_1 \) and \( p_2' \) as incoming particles, with \( p_1 \) and \( p_1' \) outgoing (Fig. 4). For example, in nucleon-nucleon scattering we have to consider states which can be reached from the nucleon-antinucleon system. The lowest of these is discrete, the single pion, and corresponds to a pole in \( A^2 \) at \( A^2 = -4\mu^2 \) or at

\[
\cos \theta = 1 + \frac{\mu^2}{2k^2}.
\]

It may be verified that this pole is outside Lehmann’s ellipse for the real part but inside that for the imaginary part. However, the residue of the pole can be proved to be real; it is, in fact, the pion-nucleon coupling constant \( g^2 \), so the pole occurs only in the real part of the scattering amplitude.

The next least massive state is that of two pions, which forms a continuum and thus gives rise to a branch point at \( A^2 = -4\mu^2 \) or

\[
\cos \theta = 1 + \frac{9k^2}{2\mu^2}.
\]

Further branch points occur at \( \cos \theta = 1 + \frac{9k^2}{2\mu^2} \), etc., corresponding to more complicated intermediate states.

The left hand spectrum is achieved in a similar way by considering the states reached when \( p_1 \) and \( p_2' \) are incident. In the \( NN \) case the spectrum in \( \cos \theta \) is, of course, symmetric.

The application in which Cziffra and I have been interested is the determination of \( g^2 \) by extrapolation from the physical region to the neighbourhood of the poles, which come close to forward and backward scattering as the energy increases. It is possible to carry out the extrapolation with directly measured angular distributions; no phase-shift analysis is needed. For various practical reasons, backward \( n-p \) scattering seems the best bet for a determination of \( g^2 \), and the most suitable data we have located so far is from the Chicago Synchro-cyclotron at 400 MeV. The pole here occurs at \( \cos \theta = -1.05 \). Its residue leads to

\[
f_{e^2} = 0.08 \pm 0.02.
\]

New measurements at still higher energies are being planned at Berkeley. What is needed, of course, is a very accurate angular distribution at extreme backward angles. If the forward angles can be used in the same way one could determine \( f_{ON} \) and \( f_{OP} \).

Another application has been examined by Moravcik, Taylor and Uretsky, who considered photo-meson production. The right-hand spectrum here is of the same type I have just described, with the one-pion pole occurring at

\[
\cos \theta = 1/v,
\]

where \( v \) is the velocity of the outgoing pion. The residue is \( \sim e_f \). Photo-pion angular distributions at forward angles are still relatively imprecise, but data at four different energies were extrapolated to give four values of \( f_e^2 \), the average of which was

\[
f_e^2 = 0.07 \pm 0.03.
\]
The situation here also could be much improved by increasing the experimental accuracy.

**TABLE I**

(Moravcsik, Taylor, and Uretsky 4)

<table>
<thead>
<tr>
<th>$E_{\gamma}$</th>
<th>Experimental residue (micobarns/sterad)</th>
<th>Coupling constant $f^2$</th>
</tr>
</thead>
<tbody>
<tr>
<td>235</td>
<td>1.43 ± 0.90</td>
<td>0.078 ± 0.050</td>
</tr>
<tr>
<td>260</td>
<td>1.70 ± 0.52</td>
<td>0.136 ± 0.048</td>
</tr>
<tr>
<td>265</td>
<td>1.79 ± 1.25</td>
<td>0.133 ± 0.093</td>
</tr>
<tr>
<td>290</td>
<td>0.287 ± 0.350</td>
<td>0.0278 ± 0.0340</td>
</tr>
</tbody>
</table>

Average of all data 0.0716 ± 0.0302
Average without the 290 MeV data 0.111 ± 0.039

Low 4) has pointed out that there is a pole of the type discussed here in the process $\gamma + p \rightarrow \gamma + p$, corresponding to the exchange of a neutral pion between the photon and the nucleon (Fig. 6).

In the Compton cross-section the residue of this pole is the product of $f_{eP}^2$ and the $\pi^0$ decay constant. A measurement of this residue would therefore be equivalent to a measurement of the $\pi^0$ lifetime! Unfortunately the residue is small, so that an extremely accurate angular distribution measurement is required.

3. We have a contribution now from Gell-Mann and Mathews 7), which is one of the best examples to date of the ability of dispersion relations—with a little coaxing—to make detailed predictions. Their problems is that of photon-nucleon scattering, and there are two different aspects of the work which should be emphasized. Firstly, they formulate relations with no subtractions, so that no new arbitrary constants are introduced. The residues of the various poles depend only on the static nucleon charge and magnetic moment. The possibility of avoiding subtractions is tied to the choice of scalar amplitudes; there are six independent amplitudes, each of which multiplies an invariant matrix containing the dependence on nucleon spin and photon polarization. Gell-Mann and Mathews are guided in their choice of amplitudes by the zero energy limit theorems for Compton scattering; that is, they arrange things so that the single nucleon poles correspond precisely to the Powell amplitude, which is known to be exact in the zero energy limit. They are led to an essentially unique set of amplitudes that is consistent with the requirement of no subtractions and at the same time has the correct zero energy limits. The corresponding six invariant matrices are too complicated to present here. GM do not prove that subtractions are unnecessary but only show that it is possible to hope they are unnecessary.

The second aspect of the GM paper is the deduction from the conjectured dispersion relations of an approximate but specific formula for photon-nucleon scattering appropriate to the sub $(\ell/\rho, \ell_\rho)$ resonance region. The basic relations assumed to be satisfied by each of the six scalar amplitudes are of the form

$$\text{Re } T_i (\omega, \Delta^3) = h_1 \left( \frac{1}{\omega - \omega_b} - \frac{1}{\omega} \right)$$

$$+ \frac{2\omega}{\pi} \int_0^\infty \text{Im } T_i (\omega', \Delta^3) \frac{d\omega'}{\omega'^2 - \omega^2}$$

where the $h_1 \ldots h_6$ are known quantities (some of which depend on $\Delta^3$) proportional to $e^2$, $e\mu_a$ or $\mu_a^2$ ($\mu_a$ is the anomalous proton magnetic moment). The variable $\omega$ is linearly related to $W^2$

$$W^2 = 2M\omega + \frac{A^2}{2} + M^2$$

and $\omega_b$ is the position of the single nucleon pole. The imaginary part of the Compton amplitude is related to partial-wave total cross-sections for photons on nucleons, which in a good approximation are given by the meson production cross-sections alone. With a complete knowledge of photo-meson production, therefore, one could calculate the nucleon Compton amplitude. GM have carried out such a calculation in a crude static approximation which ignores everything above the resonance in the dispersion integral and below keeps only electric dipole and magnetic dipole contributions. Our knowledge of photo-production permits a better calculation than this but it will be very complicated and tedious. Let me show here the results of this first evaluation by Gell-Mann and Mathews. The important thing to keep in mind is that there are no free parameters in this theory. The agreement with experiment thus gives some support to the no-subtraction philosophy.

It is worth remarking that, as is usually the case with truncated dispersion relations handled in this fashion, one gets almost the same results as from the static model 8). The tricky question of gauge invariance, however, which is crucial in Compton scattering and which is never clear in the static model, is no problem in the relativistic relations. Also the recoil of the nucleon can be taken into account.
4. The next contribution is an application by Matsuyama and independently by Goldberger, Grisaru and Oehme of the conventional dispersion relations for nucleon-nucleon scattering, that is the relations for fixed momentum transfer, in this case forward scattering. These relations have not yet been given the kind of derivation achieved for pion-nucleon scattering and photo-pion production. One of the purposes, therefore, is to see if the relation is consistent with experiment. Matsuyama starts with the relativistic relation with one subtraction but neglects the entire non-physical "left-hand" spectrum (the states reached from the AW system) except for the single-pion pole and also neglects the high energy part of the right-hand spectrum. Goldberger et al. keep in addition the two-pion contribution. With these approximations one is left with something similar but not identical to the dispersion relation for non-relativistic potential scattering, and in the low energy $s$-wave resonance region it is found that the usual effective range formulas are consistent with the truncated dispersion relation.

An attempt was made by Matsuyama to determine the pion-nucleon coupling constant from triplet scattering, but the result,

$$f^2 = 0.15 - 0.19,$$

is about twice the usual value. This is also not surprising, since the neglected two-pion contributions to the left-hand spectrum play a large role in $s$-wave scattering. Goldberger et al. evaluated the two-pion term in perturbation theory and found satisfactory agreement with $f^2 = 0.08$. Further work to improve the accuracy of the calculations is in progress.

5. This is perhaps the place to report an important theorem due to Pomeranchuk concerning the relative size of particle and antiparticle cross-sections at very high energies. Consider, for example, the dispersion relation for forward $NN$ scattering: under the integral the total cross-sections for both $NN$ and $N\bar{N}$ occur and it is easy to show that if each approaches a constant at very high energy (and does not oscillate) then

$$\text{Re } f(E) \sim E \ln E \left[ \sigma_{NN}(\infty) - \sigma_{N\bar{N}}(\infty) \right].$$

A constant high energy total cross-section, however, implies

$$\text{Im } f(E) \sim E,$$

since $\text{Im } f = \sigma k/4\pi$ total. Thus $\text{Re } f$ increases faster than $\text{Im } f$ (i.e. faster than $E$) unless

$$\sigma_{NN}(\infty) = \sigma_{N\bar{N}}(\infty).$$

Pomeranchuk argues that $\text{Re } f$ cannot increase faster than $E$ if the scattering system has a finite radius, that is if there exists an $R$, such that for $l > kR$, no interaction occurs. Such an assumption, of course, is closely connected to the assumption of a constant limit for the total cross-sections. Thus, if we believe that the $NN$ and $N\bar{N}$ cross-sections approach constant values at infinite energy, and if we believe the dispersion relations, then these cross-section limits must be equal. The same is, of course, true for $(\pi^+, \pi^-)$ and $(\pi^+, p)$ scattering, as well as for any other particle-antiparticle pairs.

I should also remind you of an earlier observation by Pomeranchuk, that a plausible assumption about competition between elastic and inelastic processes at very high energies leads to even more general predictions about the equality of certain cross-sections. They predict for example that

$$\sigma_{np}(\infty) = \sigma_{np}(\infty).$$

6. Our next contribution is by Kaschluhn and concerns dispersion relations for the elastic scattering of pions by deuterons. Recently it has been shown independently by Nishijima and Zimmermann that local fields may be associated with particles like the deuteron, which we usually regard as complex. It is therefore possible to give the same kind of plausibility argument for the existence of dispersion relations for scattering processes involving deuterons as have been given for processes involving nucleons. The mass spectra associated with the deuteron, however, lead to extensive non-physical regions which at present make rigorous derivations impossible.

In the case of pion-deuteron elastic scattering, just as for pion-nucleon scattering, the right-hand and left-hand spectra are symmetrical. Let us consider, therefore, only the right-hand or "normal" spectrum in terms of the variable.
$W^2$, the total energy in the barycentric system. This spectrum is determined by states coupled to the $\pi + d$ system. There are no poles; the one you might expect, corresponding to a deuteron alone, is disallowed by isotopic spin conservation, since the deuteron has total isotopic spin zero. We do, however, have a continuum of two-nucleon states, starting at $W = 2M$, while the physical region does not begin until $W = M_d + \mu$ (Fig. 8).

Contrast this to pion-nucleon scattering (see Fig. 9).

For forward scattering the spectral function in the physical region is related in the usual way to the total $\pi - d$ cross-section, which is compounded of both elastic and inelastic parts. However, the partial cross-section for pion absorption,

$\pi + d \to N + N$,

does not vanish at zero kinetic energy and must be continued into the unphysical region to give the spectral function there.

Kaschluhn finds that there is not yet enough data on pion-deuteron scattering to do the complete dispersion analysis that has been possible in the pion-nucleon case. He relies, therefore, on the impulse approximation to fill in the gaps, arguing that where experiments have been done the impulse approximation has given reasonably good results. He also treats the pion absorption process by something like an impulse approximation, expressing the matrix element for absorption in terms of the pion-nucleon coupling constant. Lichtenberg \(^{10}\) some years ago showed that this procedure gives a result of the correct order of magnitude. The entire contribution from the non-physical region is then proportional to $f^2$ and may be regarded as the equivalent of the contribution from the single nucleon pole in $\pi - N$ scattering.

Kaschluhn has examined both non-spin-flip and spin-flip $\pi - d$ scattering and found that the impulse approximation is consistent with the dispersion relations. The non-spin-flip relations are insensitive to the value of $f^2$, but the spin-flip relations indicate a value $f^2 \approx 0.1$ with a 30% uncertainty.

Thus we have some confirmation of a suspicion, which has been abroad for quite a while, that dispersion relations are not tied to the elementary particle concept. They are most easily applied to the lightest particles in the strongly interacting family, but this seems to be a matter of practice not of principle. (See, however, the discussion remark by Nambu in Session 7, p. 212.)

7. We have an important contribution now from Greenberger \(^{10}\), which discusses the effects of the breakdown of charge independence on the phase-shift analysis of pion-nucleon scattering. Greenberger generalized the Hamiltonian of the usual $p$-wave static model to include the known mass difference between charged and neutral pions and a possible difference between the three coupling constants. There are two new parameters

$$\delta = \frac{1}{f_c} \left[ \frac{1}{2} \left( f_{op} + f_{op} \right) - f_c \right]$$

$$\delta = \frac{f_{op} - f_{on}}{2f_c}$$

as well as the known mass difference,

$$\Lambda = \frac{m_{n+} - m_{n0}}{m_{n+}} = 0.032.$$
Greenberger derives the Low equations for the generalized problem. Making the usual one-meson approximation, assuming the dominance of the \((\frac{3}{2}, \frac{3}{2})\) resonance and treating all other effects as perturbations, Greenberger manages to solve the equations by a self-consistent procedure. He arrives at a definite formula in terms of \(\delta\) and \(d\) for the first order deviation from the charge independent amplitudes. As one example of his results, let me show a rough sketch of the correction which Greenberger finds should be applied to the angular distribution coefficients in \((n~, p)\) scattering before an analysis according to charge independence is carried out. If
\[
d\sigma_{\text{exp}}(\theta) = A + B \cos \theta + C \cos^2 \theta,
\]
then
\[
d\sigma_{\text{CL}}(\theta) = (A + \delta A) + (B + \delta B) \cos \theta + \cos^2 \theta.
\]
The corrections \(\delta A\), \(\delta B\), and \(\delta C\) are shown in Fig. 10. The corrections to the charge-exchange scattering are also important in the subresonance region, but those for \(\pi^+\) scattering are small.

The quantities \(\delta\) and \(d\) are in principle measurable, as we have seen, but so far nothing is known about them except that they must be small compared to unity. Greenberger was forced, therefore, to attempt to determine \(\delta\) and \(d\) from the failure of the experiments to be well represented by charge independent amplitudes. In other words, he added \(\delta\) and \(d\) to the usual six \(s\)- and \(p\)-charge independent phase-shifts and repeated the least squares analysis. The only data accurate enough to be at all meaningful in such an attempt is that from Carnegie Tech. at 150 and 170 MeV. This leads to
\[
d \approx 0, 
-0.05 < \delta < -0.03
\]
that is, the two neutral coupling constants are about equal and a few per cent less than the charged constant.

The associated changes in the usual phase-shifts are as follows:
- \(\alpha_3\) is stable \((-13.2^\circ \to -12.4^\circ)\), but \(\alpha_1\) decreases substantially \((9.0^\circ \to 5.9^\circ)\)
- \(\alpha_{12}\) increases by about \(2^\circ\) \((51.8^\circ \to 53.3^\circ)\)
- \(\alpha_{11}\) is stable \((\text{remains negative and small})\) \((-2.0\to -1.8)\)
- \(\alpha_{31}\) becomes slightly more negative \((-3.9\to -5.4)\)
- \(\alpha_{13}\) which is positive in the normal analysis, decreases by its own order of magnitude and may even change sign \((2.1^\circ \to 0.3^\circ)\).

(The numbers in parenthesis refer to 150 MeV.)

The precision of the data is not sufficient to make these results quantitatively reliable, but the lesson is clear: in the sub-resonance region, at least, we must take into account the failure of charge independence in any attempt to determine from experiment accurate values of the small \(p\) phase-shifts or the \(s\) phase-shifts.

LIST OF REFERENCES — see p. 108.

DISCUSSION

Goldberger: I would like to make two comments. One has to do with another possible way of determining the coupling constant analogous to the method described by Chew. I obviously have not had time to work it out so I simply make the suggestion. Namely, in the process of meson production in a nucleon-nucleon collision there is an isolated pole associated with the one-nucleon state whose strength would certainly be proportional to two things, one is the amplitude for nucleon-nucleon scattering at whatever undoubtedly peculiar energy that would correspond to, and the other is the strong coupling constant. Now, whether this is a feasible way I have no idea. The second point has to do with the comparison with experiment of the nucleon-nucleon dispersion relations which Chew described. I would only like to say, that if one attempts to calculate the two-meson contribution in something other than perturbation theory and, in fact, uses the highly discredited method of calculating that two-meson contribution described yesterday by Drell (*) the agreement with experiment is worsened enormously. There is perhaps one interesting feature about the calculation, and that is that the algebraic sign of the contribution of the re-scattering is such as to suggest the possible existence of a hard core; that is, the re-scattering corrections make a definite repulsive contribution to the total volume integral of the potential, which is the quantity which has essentially been tested in this very crude test of the dispersion relations.

Feynman: I want to ask whether the new dispersion relations of Mandelstam permit an extension away from

(*) See session "Nucleon structure" (theoretical), p. 29.
the mass shell; that is the relation that the sum of the three variables $x, x'$ and $y$ plus the sum of the four masses equals zero. This relation seems to be independent of the dispersion relation itself which apparently has not got any of the particular masses of the particles in it. So that it might be possible to extend, in our imagination, the scattering formulae to a place, where the mass, say of the $\pi$, is not really the experimental mass. I ask this question with the following idea in mind. In the old days when we had meson theory there were two contenders, one was called the pseudoscalar coupling and the other the pseudovector. One interesting consequence of the pseudovector coupling was that if both the frequency and the momentum of the meson goes to zero (which is only possible if you can move off the mass shell) then, since the coupling is through the gradient, all amplitudes would go to zero. Although the meson theory may not be right, it might be fun to conjecture and to investigate whether the principle is right that, if we have some technique for extrapolating the amplitudes over into both zero momentum and zero frequency for the meson, then the amplitudes always vanish. If it turns out to be true, it would be perhaps a useful relationship to add to our other partial knowledge.

Miyazawa: In connection with Pomeranchuk’s relation, I would like to present a theorem on high energy limit. From the general relation of crossing symmetry you find that the imaginary part of the scattering amplitude of a particle ($p$) minus that of its antiparticle ($\bar{p}$) must be an even function of the energy. Now

\[ \text{Im} [f_p - f_{\bar{p}}] \to E (\sigma_p - \sigma_{\bar{p}}) \]

at high energies. Therefore if both $\sigma_p$ and $\sigma_{\bar{p}}$ go to constant, the difference cannot be a constant because the whole thing must be an even function. So the difference must vanish if both are to go to a constant. We can conclude

\[ \sigma_p (\infty) = \sigma_{\bar{p}} (\infty) \]  
(if $\sigma_p, \sigma_{\bar{p}} \to \text{const.}$)

quite generally.

Peierls: If I may ask a question about this: it is not obvious that an even function cannot have an asymptotic behaviour proportional to an odd power of $E$, but maybe one has to use other properties of the function?

Miyazawa: We know that the amplitudes are analytic and so I think it is all right.

Bogolyubov: I would like to ask Chew concerning the spectral representation of Mandelstam. Are they proved starting from some conventional or modified form of causality principle, or do they represent an “Ansatz” generalizing the well-known “Ansatz” of Nambu?

Chew: I would say the latter—very much—they are certainly not proved.

Klein: I would like to add some remarks on the fourth possible use of dispersion relations, namely their use as fundamental dynamical relations in field theory. I think it should be remarked that there has been some theoretical work along this line originating with the work of Dyson, Dalitz, and Castillejo which it seems to me establishes rather conclusively that there is not too much hope that these relations constitute a unique representation of a particular field theory. Rather they are a set of relations for a class of field theories with given transformation properties and stable particles.

Schiff: I want to ask Chew if the curves which he drew from Greenberger’s work are additive, that is the $\delta, \pi, \Lambda$ curves. This being the case, it would seem from the general shape of the curves as though the effect of having $\delta$ about $-0.03$ to $-0.05$ and $\Lambda$ about zero is just to cancel out the $\pi$ part. One might conclude, then, that the only effect of the difference between the charged and neutral coupling constants is in some way to compensate for the mass difference between the charged and neutral $\pi$ mesons. This is perhaps reflected in the fact that the phase-shifts are not changed very greatly. I take it that you agree with me?

Chew: Yes.

Stafford: I would like to put a bread-and-butter question to Chew concerning the determination of the coupling constant from the $n-p$ scattering data. First of all, is it purely the cross-section in the backward direction that is required; secondly, what is the energy dependence, in other words what is the suitable energy to use; and thirdly what sort of precision is required to improve the existing results?

Chew: The forward distribution is also interesting but it is more difficult to get an accurate value from the forward distribution because the residue there turns out to be only $1/4$ as large as it is in the backward direction. Also, if you go to very high energies, you bring the pole close to the physical region (which is good), but in the forward direction you eventually run into the diffraction peak. This peak is associated with inelastic processes in the imaginary part of the amplitude. The diffraction peak is not a singularity and is of no interest in this connection but, of course, it obscures the real part. Thus, my feeling is that as a practical matter the backward distribution will be much the better place to concentrate. Now, just how far from the backward direction it is worth while to work is not clear to us yet. The theory of extrapolation when you have experimental errors is a murky subject so far as I am concerned. I would think that as a general rule you have to go to a distance in cos $\theta$ from the backward direction, which is of the order of magnitude but somewhat larger than the distance that you want to extrapolate. For example, at 400 MeV you have to extrapolate 0.05; so you probably want to go down to 0.9 or 0.8 in cos $\theta$ to get a good lever-arm. I think that there is no limit to the accuracy which can be used. With more accurate experiments you will increase the precision of the coupling constant determination in proportion.
Segré: I would like to point out that to make this type of measurement it might be worth while not to rely only on measurements of cross-sections at small angles, because these are well known to be difficult and to be full of pitfalls of various kinds. It might well be worth while to take into account a large group of experiments by passing through the phase-shifts. Namely to take advantage of anything else that can be measured not only at these small angles; and then use the phase-shifts just to interpolate in the formula $f(\theta) \sim g^2 \left[ \cos \theta - \left( 1 + \frac{\mu^2}{2k^2} \right) \right]$ that Chew has written on the board.

Bernardini: Speaking about experiments, I would like to add that the results of Illinois about Compton scattering have been recently confirmed just around the threshold. The poor agreement between experiment and the theoretical predictions including those of Gell-Mann and Mathews, as was discussed just about one year ago with them, still persists. In other words the agreement you get seems very much of an illusion to me, because in the low energy region you have an agreement practically with the Klein-Nishina formula, in the high energy region you have the agreement with the one-level resonance formula, and in the intermediate region around the threshold where the situation would be more interesting and crucial the situation is very bad. The disagreement with the experiment is more than a factor two.

Gell-Mann: Of course, both the theory and experiment have to be worked on further. If our conjecture is right about the parameter-free dispersion relations, then a considerably better job can be done by putting in a more detailed analysis of photoproduction, including other processes, etc. If the conjecture is false, we have to start over again and try to find out where the extra parameters come from. I think a lot more work is needed and ultimately maybe there will be agreement.
8. We now commence a series of contributions on the nuclear force problem. A question of great current interest here is the origin of the spin-orbit interaction which some investigators have concluded is required by experiment, at least in the \( I = 1 \) state of the two-nucleon system. Because a spin-orbit interaction is proportional to the nucleon velocity it can never be given by a strict static model approach. However, when nucleon recoil is included, one finds a spin orbit term of order \( \mu/M \) in the fourth order force — although not in the second order. The first interesting corrections to the second order force turn out to be proportional to \( (m/M)^2 \) and have been shown by Okubo and Marshak to have the form:

\[
\sigma_1 \cdot \mathbf{L} \sigma_2 \cdot \mathbf{L}, \text{ or } \sigma_1 \cdot \mathbf{p} \sigma_2 \cdot \mathbf{p} \text{ rather than } (\sigma_1 + \sigma_2) \cdot \mathbf{L}.
\]

There seem to be two possible sources of the fourth order spin-orbit force. The perturbation calculation by including nucleon recoil in the usual two-pion exchange term gives a result smaller than what seems required by experiment but with the right range and sign. It is proportional to \( \frac{\mu}{M} f^4 \) and goes asymptotically like \( e^{-4\mu r} \). The contribution to this session by Okubo and Sato reports an extension of this calculation to include multiple scattering of the exchanged pions. It was hoped that the effect of the \((\frac{3}{2}, \frac{3}{2})\) resonance might substantially enhance the meson spin-orbit potential, but the correction, although substantial in magnitude, turned out to have the wrong sign.

9. A different approach to the nuclear force problem has been made by Breit, who considered the two-nucleon system according to classical pseudoscalar meson theory. It is hard to relate this calculation in a systematic way to the quantum theory, but it seems to correspond to a consideration of certain relativistic corrections to the one-pion exchange force.

Breit uses the full Dirac equation to describe the nucleons, but treats negative energy states only approximately and neglects retardation of the meson field. His Hamiltonian is

\[
H_{D_1} + H_{D_2} + g^2 \tau_1 \cdot \tau_2 \cdot \gamma_5(1) \cdot \gamma_5(2) \cdot e^{-\mu r}.
\]

Reducing in the usual way to 2-component spinors he finds a spin-orbit force of the same general character as that given by the above-mentioned recoil corrections to double pion exchange in quantum perturbation theory, that is, proportional to \( \mu/M \) times \( f^4 \) and with a range \( \frac{1}{2\pi} \). These two effects, however, are not the same. Breit argues that only the order of magnitude of the spin-orbit potential obtained from his approach is believable, and his result comes close enough to the experimental requirement to satisfy him that the meson theoretic origin of the spin-orbit force has been qualitatively explained. Okubo and Sato, on the other hand, feel that meson theory definitely does not give any appreciable spin-orbit force.

10. Well, I guess it is time now for the latest chapter in the success story of the Signell - Marshak, etc. potential. Marshak, in his report to this conference, considers the following items worthy of note.

(a) The result of a recent triplet \( p-p \) scattering experiment near 150 MeV is in agreement with the SM theory and disagrees even with the sign given by the purely phenomenological Gammel-Thaler (GT) potential. In many respects these potentials are similar, but there are differences which sufficiently detailed experiments can distinguish.

(b) There is, as yet, no experimental evidence concerning the spin-orbit interaction in the \( I = 0 \) state. The SM potential has so far taken this to be the same as in the \( I = 1 \) state; but both quantum and classical field theory predict a strong dependence on isotopic spin, even though the form of this dependence is not clear. Marshak therefore considers the strength and sign of the \( I = 0 \) spin-orbit potential as still a free parameter although the range ought to be independent of isotopic spin. So long as this is true, one cannot make arguments about anomalous contributions to the magnetic moment of the deuteron, which is an \( I = 0 \) state.

(c) The difficulty with the photodisintegration of the deuteron at medium energies has now been removed. For those who were not aware of a difficulty, let me remind you that the angular distribution predicted by the simple Bethe-Peierls formula for electric dipole photodisintegration is a pure \( \sin^2 \theta \). This is found experimentally at low energies, but at medium energies, say around 50 MeV, a large isotropic component develops which cannot be
blamed on the magnetic dipole. It was realized that
the non-central nature of the two-nucleon force can give rise
to isotropic contributions from the electric dipole, but for
many years the non-central phenomena were not thought
important enough to produce the observed effect. In
particularly, two legends grew up: (1) The $d$-state probability
in the deuteron is so small that only the $s$-wave need be
taken into account. (2) The $p$-wave final state interaction
is so weak that it can be ignored. With these assumptions
one can never get anything but a pure $\sin^2 \theta$.

De Swart and Marshak \textsuperscript{19} decided to ignore the legends
and undertook a systematic calculation of the electric
dipole photo-disintegration using the SM interaction to
generate the necessary wave functions. They found that
the $d$-wave component in the deuteron gives a large contri-
bution and that the splitting of the three final $p$-states by
the non-central forces (tensor and spin-orbit combined) is
of great importance. Furthermore, they got the right
answer, achieving a quantitative fit to both the absolute
cross-section and the angular distribution up to 80 MeV
photon energy. Thus the classical nuclear physics descrip-
tion of nucleons as point charges and magnetic moments —
with no explicit exchange currents — seems to be adequate
until one approaches the meson production threshold. Actually, Marshak points out, this successful calculation
is not a critical test of the SM potential as opposed, for
example, to the GT potential. Any model which gives
reasonable $p$-wave phase-shifts and a substantial $d$-wave
probability in the deuteron should give a decent fit to the
photodisintegration cross-section up to 80 MeV.

This statement is confirmed by a work of Brown and
Nicholson \textsuperscript{19}, who got good results from the Gammel-
Thaler potential.

At higher energies there seems no doubt that explicit
meson contributions to the electromagnetic matrix elements
must be taken into account. The maximum in the deuteron
photodisintegration cross-section around 200 MeV can
never be explained by classical nuclear physics. It must
be somehow associated with meson production, and a
calculation of the type performed by Zachariasen \textsuperscript{20}
(shown yesterday in one of Drell’s slides) is in order.

(d) Marshak and Signell find that reducing the range
of the spin-orbit force from that corresponding to an
exponential $e^{-r/r_0}$, with $r_0 = 1.07 \times 10^{-13}$ cm, to
$r_0 = \frac{1}{2} r_0 = 0.7 \times 10^{-13}$ cm, further improves agreement
with experiment. In particular a much better representa-
tion of $p$-$p$ scattering at 300 MeV is obtained, although
at such a high energy the SM potential is still not quanti-
tatively successful. The new choice of spin-orbit range
is motivated by meson theory, as discussed above.

11. We now come to a deficiency in the Gartenhaus
singlet even potential, which is the same as that of Signell
and Marshak — since the spin-orbit term occurs only in
the triplet state. The difficulty was uncovered by Fischer,
Pyatt, Hull and Breit \textsuperscript{21} in a detailed investigation of
$p$-$p$ scattering in the 0-40 MeV range. Roughly speaking,
they find that the range of the Gartenhaus singlet even
potential is too short by about 15\%. This defect is
manifested, not only by the low energy $s$-wave phase-shift,
but also by the $d$-wave phase-shift at 40 MeV which is
not large enough. In order to obtain a precision fit to
$p$-$p$ experiments in this range, it is necessary to stretch the
scale of the Gartenhaus singlet even potential by a factor
$\sim 1/0.84$ and decrease its strength by a factor $\sim 0.75$.

Since the Gartenhaus potential, like almost all recent
meson potentials, gets most of the singlet force from
two-pion exchange which can be only roughly calculated,
a defect of this kind is not surprising. It reminds us,
however, not to take too seriously this particular model
for the two-nucleon interaction.

12. Some Japanese theorists, in fact, believe that the
agreement with experiment of the SM potential is com-
pletely spurious. They have always objected to the
Brueckner-Watson treatment of the intermediate range
static force and consider Gartenhaus’ core as totally
unfounded. Recently Otsubi, Tamagaki and Watari \textsuperscript{22}
have made calculations which they claim show the spin
orbit force is unnecessary. They believe that the Gartenhaus
potential is excessively attractive in the $3 P_0$ state and
that the only essential role of the SM spin-orbit force is
to weaken this attraction. Given a correct static
potential, they argue, one would not need the $S$-$L$ coupling
at all.

Confronted by this criticism, Marshak has expressed
doubt that all the successful experimental predictions, such
as in the triple scattering at 150 MeV, could be maintained
without $S$-$L$, and there is some uncertainty as to how many
experiments the Japanese have considered. To settle this
question it seems that the Gammel-Thaler type of potential
search must be extended. Probably what should be done is
to accept the outer part of the potential as reliably given
by meson theory but to determine the core and inter-
mediate regions empirically. The Japanese have been
using this approach for many years but are greatly ham-
ered by the lack of high-speed computing facilities.

13. This completes the contributions on the two-
nucleon problem, and we turn now to the antinucleon-
nucleon ($\bar{N}N$) interaction. If we had a quantitative
meson theoretical picture of the two-nucleon forces we
could predict uniquely the part of the nucleon-antinucleon
forces which is due to pion exchange. Invariance under
charge conjugation leads to a pion-antinucleon coupling
constant exactly equal but opposite in sign to the
pion-nucleon constant. Thus, if we decompose the inter-
action according to the number of pions exchanged, the
parts involving an odd number of pions have opposite
signs in the $NN$ and $N\bar{N}$ systems, while the parts involving
an even number have the same sign.

As we have seen, our meson theoretic understanding of
the $NN$ problem is far from complete, but it seems that
outside the core most of the force is due to one-and two-pion
exchange. The one-pion part is on relatively firm ground theoretically, so it seems that a reasonable prescription for the $NN\pi$ pion exchange interaction is to take whatever $NN$ interaction you happen to like and reverse the sign of the well-defined single pion part.

There is little controversy about this procedure and general agreement that it gives rise to larger cross-sections at intermediate energies, chiefly because the $p$-wave interaction becomes much stronger. In the $NN\pi$ system the single-pion odd angular momentum interaction is on the average repulsive, while the two-pion part is attractive. The result is anomalously small $p$ phase-shifts and a correspondingly small total cross-section. In the $NN$ system, the reversal of sign of the one-pion force plus the introduction of new states, not present in the $NN$ system because of the Pauli principle, removes the cancellation and leads to cross-sections of normal size. The large ratio between $NN\pi$ and $NN$ total cross-sections can in this way be understood.

There has been severe controversy, however, about the annihilation process, which occurs in the $NN\pi$ system only, and for which we have no guide. Since I have been involved in this controversy I am not in a good position to present an impartial survey, but I shall do the best I can. The discussion period which follows is designed to alleviate just such a difficulty as this.

The controversy revolves about the theoretically expected range of the annihilation interaction. It is agreed by all that this interaction is strong, but some feel that its range must be no larger than that of the core in the $NN$ force while others believe it may extend to distances of the order of the pion Compton wavelength or greater.

One may visualize the role of annihilation in the $NN\pi$ interaction in terms of Feynman diagrams (Fig. 11). The conventional line of argument is to discuss the range of the interaction in configuration space in terms of the momentum transfer dependence of the scattering amplitude. For example, single-pion exchange is said to have a range $r^{-1}$ because the scattering amplitude is $(1 + r^2)^{-1}$, whose Fourier transform is $e^{-\sqrt{r^2}}$. If we apply this kind of argument to the annihilation diagrams it is clear that the minimum intermediate mass is $2M$ and that the asymptotic exponential behaviour of the Fourier transform is $e^{-2Mr}$, a very short range effect.

There are, of course, flaws in this argument. The effective interaction potential is not simply the Fourier transform of the scattering amplitude. In fact, the very notion of potential loses a clear meaning at short distances. Also, it may be that the strength of the annihilation contribution is so enormous that even with a dependence $e^{-2Mr}$ it reaches far out in effect. Finally, one must be prepared for a breakdown of local field theory, without which the above chain of reasoning cannot be made.

At present, those who would abandon field theory have nothing to replace it and are forced to a completely phenomenological description of the annihilation interaction. By introducing a complex potential of arbitrary strength, range and energy dependence one can presumably fit anything; so I think that as long as there is a chance of understanding the experiments within the framework of field theory one must continue to try. Lévy has constructed a model \cite{23}, motivated by field theory, which leads to the conventional real potential but a long-range annihilation mechanism. I have been unable to reconcile this model with the foregoing general arguments about momentum transfer and range and therefore would prefer to have Lévy explain his line of reasoning in the discussion which is to follow.

Fig. 11.

Ball and I \cite{24} last year undertook a calculation based on the conventional point of view that the annihilation interaction is of very short range. We used the SM potential, which has been successful up to 150 MeV, with a "black hole" boundary condition in conjunction with the WKB approximation. That is, we assumed that any partial wave would be completely absorbed if, with the aid of the meson exchange force, it could overcome the centrifugal barrier. I should say that Lévy is unhappy about this assumption, and I hope he will criticise it in the discussion period. At intermediate energies (50-200 MeV) we found that the results are independent of the position of the annihilation boundary, provided that this boundary is well within a pion Compton wavelength.
Piccioni showed the results of these calculations yesterday, as extended by Ball and Fulco (Fig. 12). For a variety of reasons the model cannot be used at very high energies, but it seems to work in the energy range where the SM potential has been successful for the $NN$ system. My personal conclusion is that there is, as yet, no experimental evidence for a long-range annihilation interaction. However, if a large annihilation cross-section persists into the multi-GeV range, the situation must be reconsidered. At low energies we use the meson cloud to catch the antinucleon with a big cross-section and draw it into the black hole at the centre. At very high energies, however, the meson cloud will not have much deflection capability and the annihilation cross-section ought to shrink to the size of the black hole, which should be smaller than $10 \text{ mb}$.

**Fig. 12.** $p\bar{p}$ cross-section. The full curves are calculated by means of a SM potential with a "black hole" inside.

A new calculation is being reported to this Conference by Gourdin, Jancovici and Verlet (GJV). Unfortunately, the recent experimental results from Berkeley, showing a normal amount of elastic scattering, were not available when this calculation was begun, so much of the theoretical effort was directed toward a difficulty which now seems not to exist. It is perhaps gratifying that no explanation of the non-existing paradox was found.

Briefly the situation is this. It was believed, on the basis of preliminary experiments at 450 MeV, that the ratio of scattering to annihilation is less than $1/9$. In order to achieve such a result, GJV found it necessary to introduce a fairly strong imaginary potential with range at least equal to the pion Compton wavelength. If at the same time, however, one maintains the SM real potential (or any similar potential obtained by correspondence with the $NN$ system), the total cross-section becomes too large. The new experimental results at lower energies of course make the ratio of scattering to annihilation almost one to one, thus removing the need for the long-range imaginary potential. What will happen at higher energies we must wait to see.

14. To conclude this session there are several contributions concerned with the phenomenological analysis of nucleon cross-sections in the multi-GeV energy range. The experiment which ought to be easiest to interpret is that of pion-nucleon scattering. Blokhintsev, Barashenkov and Grishin have analysed the total and elastic $\pi-N$ scattering at 1.3 and 5 GeV in the standard geometrical optics approximation, assuming imaginary phase-shifts only. The diffraction pattern then gives a "picture" of the nucleon density distribution which turns out to have a mean square radius of $(0.82 \pm 0.06)$ fermis. This same result has also been obtained by Ito, Kobayashi, Yamazaki, Minami and Tanaka. The largeness of this mean square radius and its agreement with that from electron scattering is remarkable. One may be inclined to interpret the result as evidence for a strong pion-pion interaction, but one is not forced to do so.

15. Brown reports a somewhat similar analysis of $p-p$ scattering between 1 and 6 GeV. The mean square radius of the absorption coefficient here turns out to be 1.1 fermis but a hard core of radius 0.45 fermis is introduced at the lower energy and then allowed to disappear as the energy grows. Brown believes that this model is more reasonable than the conventional one without any core but with an absorption coefficient that decreases with increasing energy. Because the total and elastic cross-sections fall markedly in this energy range, some change in optical model parameters is required.

16. Our last contribution is from Cerulus and Hagedorn who wish to report that they have set up a Monte-Carlo calculation of the multiple phase-space integrals which occur in the Fermi statistical model. The method needs a fast electronic computer and works from about 3 to 12-15 particles. Apart from the inherent statistical inaccuracies of a Monte-Carlo method, the approach is exact. As a first application, C and H have calculated multiple meson production at 25 GeV with and without consideration of the 3,3 final state isobar. They find that the usual approximate methods of evaluating the integrals can lead to serious errors. Also they find that at this energy the final state isobar substantially affects multiplicities of seven or higher.

This completes my report to the Conference.


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   Chew, G. F. and Csiffra. (private communication)
6. Low, F. E. (private communication)
17. Breit, G. (Yale University preprint)

DISCUSSION

Levy: I think that Chew, in his effort to be fair to everybody, has exaggerated a little bit our differences. Actually I believe we agree on the general philosophy and the differences are only in details. I would like to comment only on two points. The first one concerns the phenomenological calculations of Gourdin, Jancovici and Verlet which were started on a relatively ambitious basis mostly, as Chew said, because of the stimulation of the incorrect experimental results. Some of this phenomenological analysis is still relevant because the authors also considered the case when there is no imaginary potential at all. Now, the calculations they made were based on the same real part of the interaction as Ball and Chew used, but instead of using the WKB approximation they tried to make an exact integration of the wave equation except that, of course, there is now the difficulty to specify the boundary conditions one has to use. In other words, one has to make some essential assumption on the annihilation process. What they did was to assume at a certain value of the core radius (which is of the order of the nucleon-nucleon core) a certain number of boundary conditions. Fifteen of these boundary conditions were tried. Of course, one can only do this if one has a very high-speed computing machine. Now there are two results which make me a little bit doubtful of the very good fit of Chew and Ball. The results are as follows: first of all, one must keep in mind that it is hard to compare the two approaches because it is very difficult to reproduce the boundary condition that they use in the WKB approximation. But I think there is one which is very close to that, and this is the boundary condition where there is only an incoming wave present. This incoming wave has a logarithmic derivative equal to $-ik$ at the core radius.

Now, what this $k$ means, is a problem. I mean that one should use the so-called local wave number which is hard to define, but they used the wave number at infinity. In any case the results are

- Total: 154 mb (which I think is the correct order of magnitude of the observed cross-sections)
- Annihilation: 29 mb
- Exchange: 42 mb
- Elastic: 83 mb (all at 167 MeV)

and so perhaps the results are not so good as those of Chew and Ball. But, of course, this is not a sharp point of disagreement because the boundary conditions are not
exactly the same. There is another reason which makes me more doubtful. It was found that the results are extremely sensitive to the boundary condition chosen, at least when no imaginary potential is present. The philosophy of ignoring what happens inside the hole might well be successful in appearance, but one has eventually to face the situation and try to see what really is going on. And finally we also found that the contribution of higher states than D-states was far from negligible. I must say that this calculation was made at 167 MeV. Now, concerning the meson theoretical calculation, I am sorry that the diagrams that Chew wrote have been erased, but let me write now an exchange diagram with several mesons like this (Fig. 13):

![Fig. 13.](image)

I would like to point out that the reasoning by which you would say that these would yield a force falling off as $e^{-2M_r}$ at infinity, is correct as far as the real force is concerned, but if you are talking about the imaginary force, I think this reasoning is no longer valid. The reason for this is that the imaginary force comes actually from a residue of the pole which is present here. In other words, there will always be a $\delta$-function of the momenta involved. Now suppose you have a situation where the mesons tend to be exchanged at a fairly well-defined momentum. I believe this might be the case if they are $p$-state mesons which would like to sit in the middle of the resonance curve. If these mesons have more or less all the same momenta, this $\delta$-function, so to speak, fixes the momentum of all the mesons exchanged. You will no longer have the Fourier transform of the momentum distribution, but actually the momentum distribution at a fixed momentum. This will be multiplied by a certain Bessel function $j_1$ (because of $p$-state mesons) of the product $k \rho$, where $\rho$ is the fixed average momentum

$$f(k_1, \ldots) j_1(\rho \rho)$$

The intuition that one has of a range becomes very different here, because one is dealing with something which oscillates, but I think that a good definition would be to define the range as the first zero of this function $j_1$ which afterwards oscillates and almost goes to zero. The first zero of the Bessel function is fairly large, which is something about 2.5. If you take as the average momentum the momentum corresponding to the resonance in $\pi$-$\rho$ scattering this explains qualitatively why you get such a fairly long range. Now, the calculation I made was based on the Tomonaga approximation. I described the nucleon and antinucleon as a cloud of resonant mesons and then a core which is a fairly weak one. Then the annihilation needs only a small momentum transfer, most of the energy remaining in the cloud. When the cores have disappeared, the clouds are liberated and this explains the multiplicity which you observe. I think that in the light of the new experimental results, this view might be too extreme, and perhaps an intermediate view of the type which was proposed by Koba and Takeda may be more near to the truth. Of course, I believe it will still be a very interesting problem to give a meson theoretical foundation to this. Their view, I believe, is that half of the meson multiplicity comes from the cloud — and this agrees with the multiplicity predicted by the Chew - Low theory — and the remaining part comes from the annihilation of the core. So you do not need an interaction volume as large as you will need if you have five mesons produced from the annihilation, but you need a rather smaller one.

Now, no one knows how small is this interaction volume, and I think we have to wait for the high energy data before making any definite statement. This is what I wanted to say about the antinucleons.

Now I wish to make some comments on some of the earlier things which Chew said and I give my opinion — I do not know if Klein agrees with me — about the origin of the Breit spin-orbit term. I think that this spin-orbit term is not included in the one which comes from the fourth order fixed source theory. I would guess that it comes from the fact that — when you do the Pauli reduction of the Dirac equation — you allow at least one of your nucleons to go into a negative energy state. Therefore one can describe this term, using the old diagram representation, by a sum of two diagrams like this (Fig. 14):

![Fig. 14.](image)
The black dots represent the so-called $\phi^2$ coupling term, whereas the other vertices occur through the gradient coupling, and the sum of these two terms will clearly give rise to a spin-orbit force. This is exactly the term which I think Klein and I independently calculated a long time ago. We believed later, that it was small because of the so-called pair damping. But I guess this is the origin of the Breit term.

*Touschek:* Chew gave the impression that the question of the spin-orbit coupling is a matter of belief, and that there are people who think that it can be explained in terms of meson theory, and others who do not. But I want to give a very qualitative and naive argument for the first belief, that is that the spin-orbit coupling is really contained in meson theory. The reason for this is the following. If you consider a nucleon which is forced to move on a circle by some external force, and you consider the self-energy correction, which arises owing to the virtual emission of one meson in a $p$-state, you see that if the spin of the nucleon is up, let us say, that meson will travel as shown in Fig. 15a (for conservation of angular momentum). If the spin is down, the meson will travel as shown in Fig. 15b.

Now, these two configurations will give a different self-energy term; the nucleon will know the relative orientation of its spin with respect to its direction of motion. And, in fact, if you calculate the first radiative-corrections to the motion of a nucleon in an external potential, you find that you get quite a strong spin-orbit coupling. In fact, this has been done by Chisholm and myself a long time ago in 1952; but I wanted to make a comment now on the sign also because Chew has already commented on the sign, and the sign of this effect seems to be wrong. In fact we got the wrong sign, Chisholm and I, that is the spin-orbit force that corresponds to the electric spin-orbit forces rather than those which one actually observes. It has later been pointed out by Laing that the problem is not as elementary. The sign can be changed by taking account correctly of relativistic corrections. That is, there is a term in the radiative corrections which connects big and small components of the nucleon in such a manner that one really gets a very strong spin-orbit term. Not, of course, that one would attribute any meaning to the quantity of this spin-orbit coupling, because clearly this is subject to discussion. I also want to point out that this is a $p$-state effect and therefore should be suitable for a more detailed treatment in a gradient theory. And that, of course, makes you expect that in the two-nucleons problem is would be a sixth order contribution of the type (Fig. 16):

![Fig. 16](image)

Now, these two configurations will give a different self-energy term; the nucleon will now the relative orientation of its spin with respect to its direction of motion. And, in fact, if you calculate the first radiative-corrections to the motion of a nucleon in an external potential, you find that you get quite a strong spin-orbit coupling. In fact, this has been done by Chisholm and myself a long time ago in 1952; but I wanted to make a comment now on the sign also because Chew has already commented on the sign, and the sign of this effect seems to be wrong. In fact we got the wrong sign, Chisholm and I, that is the spin-orbit force that corresponds to the electric spin-orbit forces rather than those which one actually observes. It has later been pointed out by Laing that the problem is not as elementary. The sign can be changed by taking account correctly of relativistic corrections. That is, there is a term in the radiative corrections which connects big and small components of the nucleon in such a manner that one really gets a very strong spin-orbit term. Not, of course, that one would attribute any meaning to the quantity of this spin-orbit coupling, because clearly this is subject to discussion. I also want to point out that this is a $p$-state effect and therefore should be suitable for a more detailed treatment in a gradient theory. And that, of course, makes you expect

*Cini:* I would like to add a remark to the remark by Chew about the $\pi-\pi$ interaction which might be responsible for the large radius in the $\pi$-nucleon scattering at very high energy; it is also connected with the remark made yesterday by Gell-Mann, suggesting that also the pion has a large extension. My remark is that there seems to be at least one or two other possible indications of the existence of such a $\pi-\pi$ interaction. One is that, if one calculates the multiplicity of the pions produced in the proton antiproton annihilation with a simple statistical theory, by adding a $\pi-\pi$ interaction, one obviously gets a higher multiplicity. This is much better in agreement with the experiment than the simple statistical calculation, and it may be the indication of the existence of such an interaction. The other one is that possibly (but it has not been worked out in detail) also the slight deviation of the $\tau$-meson decay spectrum from the one that one obtains simply taking a spin zero particle, might also be due to a sort of $\pi-\pi$ interaction.

*Brueckner:* I would ask Lévy if in the calculation of Verlet it was found possible, for any choice of the boundary condition of the core, to get the correct total annihilation cross-section?

*Lévy:* With the core placed where it was, the only way we found to obtain a complete agreement, was to use a certain amount of imaginary potential in addition.

*Segré:* Complete agreement with what?

*Lévy:* With the present experimental data.

*Segré:* You mean total and/or annihilation cross-section?

*Lévy:* In order to get a ratio of annihilation to total cross-sections, which is about one half, we found that we needed a certain amount of imaginary potential.
**Marshak:** I should like to take a minute to summarize the nucleon-nucleon scattering situation as far I see it. The original, rather detailed, work of Gammel and Thaler attempted to fit the nucleon-nucleon scattering up to 300 MeV with a static potential using Yukawa shapes and repulsive cores. Within that context they tried all kinds of exchange possibilities and came out with a strongly negative answer. Now I think that the Japanese are pointing out that perhaps Yukawa shapes plus repulsive cores are restrictive and one should try other shapes. As far as I have seen the Japanese work, they have shown that up to 100 or 150 MeV they can bring down the $^3P_0$ phase-shifts to our values. I have not seen any curves of the cross-section or polarization or triple scattering parameters so that one can judge what sort of agreement with experiment has been achieved. It is hardly necessary to emphasize that even in the 100 to 150 MeV region it is not just a question of the triplet $P$ phase-shifts, since the polarization requires $F$ phase-shifts and so on. From the point view of meson theory there certainly is velocity dependence of forces. Since we do not know how to calculate this velocity dependence, we do not know at what energy the linear dependence on momentum will first appear. It is important to mention that the spin-orbit forces are unique forces when you consider the first power of momentum. When you go to quadratic powers of the momentum then the situation becomes much more ambiguous. Now as far as meson calculations of the spin-orbit force are concerned, the $p$-wave interaction gives the right sign. The diagrams which Lévy wrote down as conjectures of what Breit calculated, were also calculated by Okubo and myself and they also give the right sign as an additional contribution. However, this does not mean that the magnitude is quite right, and the calculations must be improved.

The work that Okubo and Sato did (I must say at my instigation) in order to find out if one can get a more definitive meson-theoretical derivation of the spin-orbit force, is not conclusive, because it is based on taking only certain diagrams into account. Therefore, at this stage the question is, how well does one do in comparison with experiment. We can say that the simple addition of a spin-orbit force of short range and the right sign (without any detailed knowledge of functional dependence) gives an enormous improvement in the fit with the experimental data. Now not all the triple scattering experiments have been done; only $D$ has been measured at 150 MeV. More should be done and finally one will decide whether the spin-orbit force is necessary. I would certainly encourage the Japanese workers to see how far they can push the static situation. It is useful both to continue on seeing what one can get out of meson theory, and also to continue purely phenomenological fits. I think it is also very valuable that there should be groups interested in this work, who are proceeding (whether from choice or necessity) without calculating machines, because by using simple qualitative arguments one can often see things that have been missed in the large calculating programmes.

**W. D. Walker:** I would like to make a short comment on, at least what I would consider, some evidence of pion-pion interaction. I tried to analyse the $\pi$-proton inelastic processes at the highest energy we have, namely at 4.5 GeV. In the case of small multiplicity (i.e. the production of one additional pion) which results for large impact parameter collisions (presumably), one cannot understand — at least I could not understand — the angular distribution on the basis of either some sort of statistical model or the isobar model. The simple model which was proposed by Piccioni several years ago seems to fit the data quite well. Also, at the same time, I tried an analysis similar to that of Blokhinsev, and I came to a very similar conclusion, namely that the effective range of $\pi$-proton interaction at this high energy must be quite large.

**Adair:** I think that concerning the success of the optical model, there is no real evidence that there actually exists such a potential. The peak in the forward direction is essentially a result of the optical theorem. As soon as the total cross-section is large compared to $4\pi\rho^2$ the optical theorem will guarantee a peak in the forward direction. So, if you consider any model which conserves probabilities it will give this forward peak. In fact, I think that it is probable that any model which has a sufficient number of parameters to fit the total cross-section and the absorption cross-section, automatically fits the peak and gives essentially the same correlation of experimental results as the optical model. In the backward direction the situation is more difficult. I know that Walker has made the calculation of the optical model for the $\pi$-proton interaction at about 1 GeV and Erwin and I made an attempt to fit this differential cross-section using the optical model varying the real and the imaginary potential, with no particular success.

**Takeda:** I would like to make two comments on two different subjects. First, one on the nucleon forces problem. Chew and Marshak have expressed some doubt on the quantitative aspects of Japanese work. As far as I know, they have worked out the calculation, not with a high-speed machine but by hand, at 90 MeV and 150 MeV. Their standpoint is, that if they assume that the tensor force is the main force, one can qualitatively explain the ordering of the three different phase-shifts in $P$-state. Secondly, they did more quantitative work and the phase-shifts they obtained are nearly the same as those obtained by Marshak. About the other phase-shifts, which Marshak pointed out might be important to the other quantities like the polarization and so forth, they have also made discussions on the sign of polarization and they find the sign right. This means that below 150 MeV without spin-orbit coupling, we could explain almost all the existing data. Of course, I think that it is fair to say that a 10% to 20% accuracy is good enough for us — as Chew expressed his opinion — and I have this opinion too. My second remark
is that the nuclear forces cannot be expected to express a real situation at 300 MeV and so we have not to worry too much about the nuclear potential at this particular energy or at higher energies. Coming to the problem of proton-antiproton scattering, I share with Chew the point of view that outside a certain range we have nuclear forces between nucleon and antinucleon obtained from meson theory. But we must be very careful about saying something about the core, which is an absorbing one, because even in the nucleon-nucleon problem we cannot say anything definite about the existence of an infinite core inside the range where we have the ordinary meson potential. In the proton-antiproton problem we have a more complicated situation and therefore I say that there is a lot of room for us theorists to play with at present time.

I would like to ask Chew about whether the existing preliminary experimental data seem to favour his calculation with Ball. Or if, at the present time, it is just enough to assume that the large absorbing medium is present inside a certain region. Since the data changed and the annihilation cross-section became nearly the same as the elastic one, and also the differential scattering cross-section seems to fit the data with the assumption that most scatterings are diffraction scattering; since that happened, I think, we cannot say anything definite about the theory of the nucleon-antinucleon scattering.

**Chew:** This is the comparison with the experimental data of the simple model that I explained (Fig. 12). The top curve and experimental points represent the total cross-section. The middle is the elastic scattering and the lower is the charge-exchange scattering. The other information that is available is on the elastic scattering angular distribution. It was shown yesterday by Piccioni and the agreement is quite adequate. The point that Takeda is making is a very good one. In our model, since we have a strong real potential on the outside, catching the antiprotons, you would not expect to get a typical diffraction result. In particular, you would expect that part of the time this potential is repulsive. In that case it just produces pure scattering which, of course, would not follow the diffraction picture. But it happens, for reasons which I suppose are purely accidental, that in the majority of the states (there are 28 different states \( S, P, D \) and \( F \), which we included) the interaction is attractive, leading to absorption and very little real scattering. This result was not expected when we began the calculation; it simply emerged from the meson potential.

**Morpurgo:** I would like to say something concerning the remarks of Walker about these experiments. Perhaps it should also be useful in such kinds of experiments to plot other angular distributions besides the ones which are usually plotted. For instance, the distribution of the angle between the line of flight of the two pions in their rest system with respect to the line of flight of the third particle in the total rest system. This distribution might give some indication about the angular momentum of the two pions.

**Goldberger:** I would like to make a couple of remarks about the general concept of potentials. In spite of the fact that the birth of meson theory was accompanied by a calculation of nuclear forces, I cannot think of any other aspect of meson theory that is cloudier at the present time from a theoretical standpoint. One difficulty is the fact that in most cases the method which purports to calculate the potential does not simultaneously provide an equation into which this potential should be substituted. And any good theory, I think, should fulfill this requirement. You see the reflection of this in the long-standing controversy between Brueckner and Watson on the one hand and Henley and Ruderman — and goodness-only-knows how many other people — on the other hand. The second remark I would like to make, is related to something that Drell pointed out yesterday and that is the remarkable discrepancy between the spectral function in the electromagnetic structure problem, between the relativistic theory and the static approximation. I believe that it is unrealistic to make any serious calculation of the two-nucleon problem unless recoil is taken into account in a more adequate fashion. Now naturally I have proposals about how to do that, but unfortunately the results are not yet completed. I believe that one can formulate something (which I may feel free to choose to call a potential, since nobody else knows what a potential is!) from the dispersion approach to the two-nucleon problem. One can deduce a set of truncated dispersion relations in the spirit of Chew's description today, the complete solution of which either will or will not explain the data. But nevertheless it is a self-contained system which simultaneously provides the potential and the equations into which this potential must be substituted. There are very serious theoretical obstacles in carrying out this programme; they are, in fact, identical with the obstacles that prevent the effective carrying out of the electromagnetic structure calculation. That is, one needs knowledge of the pion-nucleon scattering amplitudes in unphysical regions in unfamiliar variables. We are hard at work trying to complete this programme.

The final remark is in connection with the so-called annihilation range to which Levy referred. In this formulation of the two-nucleon problem which I described, there is an intimate coupling between the nucleon-nucleon and the nucleon-antinucleon problems. In fact they cannot be decoupled in any rigorous sense. We looked briefly into the range, to use the term very loosely, of the annihilation force. As far as we could tell, there is no indication whatsoever of a range which is as long as the pion Compton wavelength.

**Marshak:** I would like to ask this question: do you think one could more definitively know what equation to put the potential into, if one stops at the first power of the momentum dependence in a velocity dependent two-nucleon interaction? Certainly when one goes to the quadratic momentum dependence the situation becomes extremely messy. Do you think that such a distinction makes sense?

**Goldberger:** I do not know the answer to this question.
Piccioni: I think that yesterday, with that number of slides, I did not have time to make many things very clear. There has not been any real change in the values of the proton-antiproton cross-section. The cross-section which we know, that had a high value of annihilation with respect to the total cross-section, is at 450 MeV. The values of Cork, Wenzel and others are from 150 to 300 MeV. On the other hand, what we do know, is the total cross-section from 150 to 750 MeV. That is the slide shown by Chew in the second part of this report (see Fig. 12) and I would like to point out one thing. Look at the total cross-section as a function of energy, namely that curve, that has the upper points there. You see a change of slope between the white points and the black points there. If you knew nothing about any fit of potential obtained in any way you probably would make the following suggestion: that any real potential is important up to some 300 to 400 MeV and after that probably only the absorptive process takes place. I am not saying that this is the case, but I want to point out that extrapolating our knowledge about the annihilation cross-section at the energies less than 300 MeV to energies of 450 MeV and higher is not quite warranted, and the only experiment we know that measures the annihilation at 450 MeV is that of Chamberlain and Segré.

Klein: I do not want to get into an argument, and I do not want to belabour the point, but I have to take issue with the first of Goldberger’s remarks. I think one has to distinguish between whether one can define an equation into which to put something called an interaction potential and whether one has the patience and persistence to push this programme through once formulated. Now I think that there also exists in principle a well-defined method, invented by Lévy and extended by a number of people, which starts with something like the Bethe-Salpeter equation. There then exists a very plausible procedure for grinding through simultaneously to an approximate Schroedinger equation and interaction. Moreover, at the same time this procedure permits one to calculate the other quantities one has to know for the two-nucleon problem: the quadrupole moment and the effective ranges, etc. Now I still cannot guarantee (and I do not think anybody can) that this programme will ultimately succeed though it is still going forward. In any case, the question of principle raised had to be contested.

Goldberger: I hope I did not overstate my position at the beginning. I did not mean to imply that every calculation in the past has suffered from some of the deficiencies which I mentioned. And I agree completely that the original Lévy-Klein programme in principle—although it uses what I do not think is a very good method—was at least theoretically complete and sound.

Wenzel: I would like to make a comment on the question of the extent of the annihilation core as regards future experiments, say at higher energies. Obviously this is a very important way of finding out something about the core. If the annihilation cross-section turns out to be high, then clearly there is a good argument for the notion of the extended core which has been discussed somewhat today. But if the cross-section turns out to be low, then it may be very hard to say whether we have a short-range core (if the cross-section turns out to be say around 10 millibarns as Chew suggested) or whether, instead of the core being of short range, we have some transmission of a larger core. Unfortunately, in this regard the diffraction pattern in the elastic scattering may not be of much help because, of course, along with the annihilation there will be inelastic single and multiple pion production which take place in a large volume.
SESSION 4
Tuesday, 1st July, 1958

Fundamental theoretical ideas

Chairman W. PAULI

Secretaries V. GLASER
C. FRONSDAL
B. VITALE
This session is called “fundamental ideas" in field theory, but you will soon find out or have already found out that there are no new fundamental ideas. So what you shall hear are substitutes for fundamental ideas, and it works in the same way as I am the substitute for a rapporteur. So, you will also see that there are two kinds of ignorance; the rigorous ignorance and the more clumsy ignorance. You will also hear that many speakers will want to form new credits for the future. I am personally not very willing to give such credits but it is everybody’s own choice, what he wants to do in this respect. Now you shall hear what is going on and we start with the talk of Yukawa: “An attempt at a non-linear field theory.”
AN ATTEMPT AT A NON-LINEAR FIELD THEORY

H. YUKAWA

University of Kyoto, Japan

As has been pointed out by Pauli, I too, have really no new ideas. I would like to report briefly on a recent attempt made by Kita, of my Institute, and in this connection to talk a little bit about our prospects. Now, there are two main lines of thought in revising the present quantum field theory with the aim of arriving at a more satisfactory description of particles and fields. One is the extension of the concept of the Hilbert space which was already developed a great deal by Heisenberg. The other is the extension of the field concept, with which I have been more concerned. The field concept could be extended in various ways. One is the problem of non-locality, but as is well known there are many difficulties which we encounter in the development of a non-local field theory. For the moment I have not a very good idea of how to overcome these difficulties, so today I would rather like to confine our attention to the fact that the field concept which we now have, contains something more than just the field to which only the particles in the narrow sense are associated. This was known for many years in the case of the electromagnetic field, which is not a pure particle field. If we want to subject the entire electromagnetic field to the usual procedure of quantization, we have to introduce fictitious photons. Alternatively, you can take aside the part of the electromagnetic field to which you cannot associate a particle in the ordinary sense. But these things are so well known that we do not think there is a serious problem here. On the contrary, in the case of the gravitational field the situation is quite different. The gravitational field is a field which is related to the space-time structure. So we cannot apply ordinary methods of quantization, at least to a certain part of the gravitational field, because you cannot associate with it a real particle picture at all. However, one cannot exclude the possibility of having a unified picture, in which you start from a spinor field, such as introduced by Heisenberg, and include the gravitational field into this picture afterwards. This is what Kita has been trying to do. In order to achieve this aim, what we have to do is to see whether you can have the gravitational field in terms of a certain spinor field in Heisenberg's sense, i.e. as a function of \( \psi \) and \( \bar{\psi} \).

Let us begin with the vacuum, before going into the more complicated cases. Of course, we do not know beforehand what would be the form of the function, but Kita started from the assumption that the gravitational field \( g_{\mu \nu} \) is a quartic form of \( \psi \) and \( \bar{\psi} \).

The simplest form which constitutes a symmetric tensor is

\[
J_1 = \left( \bar{\psi} \gamma_\mu \psi \right) \left( \bar{\psi} \gamma_\nu \psi \right).
\]

But you may insert \( \gamma_5 \) in it:

\[
J_2 = \left( \bar{\psi} \gamma_5 \gamma_\mu \psi \right) \left( \bar{\psi} \gamma_5 \gamma_\nu \psi \right),
\]

and also an antisymmetric combination of \( \gamma_\mu, \gamma_5 \):

\[
J_3 = \left( \bar{\psi} \gamma_{\mu \nu \rho \delta} \right) \left( \psi \gamma_{\rho \delta \mu \nu} \psi \right),
\]

with

\[
\gamma_{\mu \nu \rho \delta} = i/2 \left( \gamma_\mu \gamma_\nu - \gamma_\nu \gamma_\mu \right).
\]

Of course, if you are dealing with a general gravitational field, then you have to take into consideration general covariance, by raising and lowering the suffix in \( \gamma_{\mu \nu} \). But, if we confine our attention to the vacuum, then we are just dealing with the Minkowski space, with \( g_{\mu \nu} = \delta_{\mu \nu} \). We have now to solve the equation for \( \psi, \bar{\psi} \) and \( \gamma_\nu, \gamma_5 \):

\[
\delta_{\mu \nu} = a J_1 + b J_2 + c J_3,
\]

where \( a, b, c \) are suitable constants.

In general, the \( \gamma_\mu \) themselves will depend on the coordinates as well as \( \psi \) and \( \bar{\psi} \). But, in the present case, the ordinary \( \gamma \) matrices could be used. Namely, we can reproduce \( g_{\mu \nu} = \delta_{\mu \nu} \) by choosing the constants

\[
a = -b = c = 1,
\]

and taking either of the expressions

\[
\psi = \begin{pmatrix} 1 \\ 0 \\ 0 \\ 0 \end{pmatrix}, \quad \begin{pmatrix} 0 \\ 1 \\ 0 \\ 0 \end{pmatrix}, \quad \begin{pmatrix} 0 \\ 0 \\ 1 \\ 0 \end{pmatrix}, \quad \begin{pmatrix} 0 \\ 0 \\ 0 \\ 1 \end{pmatrix}.
\]

These four cases show a kind of degeneracy of the vacuum, but we do not know the real physical meaning of this degeneracy. Kita tried to consider a more general case by starting from a general covariant equation for the \( \gamma \)-field of the form

\[
\gamma^\mu \left( \partial_\mu \Gamma_\nu - \Gamma_\mu \right) = 0
\]

where both \( \gamma^\mu \) and \( \Gamma_\mu \) are functions of \( \psi \):

\[
\gamma^\mu \left( \psi, \bar{\psi} \right), \quad \Gamma_\mu \left( \psi, \bar{\psi} \right),
\]
\[ \gamma_{\mu} \text{satisfying} \]
\[ \gamma_{\mu} \gamma_{\nu} + \gamma_{\nu} \gamma_{\mu} = 2 g_{\mu\nu}. \]

The \( g_{\mu\nu} \) are again functions of \( \psi \) and \( \bar{\psi} \). Thus, if all other quantities are expressed in terms of \( \psi \), (1) becomes a very complicated non-linear equation for \( \psi \). The problem of expressing \( g_{\mu\nu} \) by \( \psi \) and \( \bar{\psi} \) has not been resolved by Kita as yet. He could only show that the quartic form of \( \psi \) mentioned above was not appropriate for the general \( g_{\mu\nu}, \psi \), so that derivatives or integrals of \( \psi \) and \( \bar{\psi} \) were to be introduced. This is what Kita has done so far. In any case, it seems to me that it is exceedingly difficult to include the gravitational field in a picture of a unified spinor field. One reason for it is that in the case of the gravitational field we have a part of the field which can certainly not be quantized in the usual manner. This may well happen in other cases, because if we have a non-linear field then we do not know how to quantize it in general. It seems to me that it is a very ambiguous procedure to take out from the general non-linear field the particle part. Let us consider a very simple example. Suppose we have a neutral scalar field satisfying an equation of the type
\[ \left( \Delta - \frac{1}{c^2} \frac{\partial^2}{\partial t^2} \right) \psi + K^2 \psi = 0, \]
then this is not at all a particle field because of the wrong sign of the \( K^2 \)-term, but if you add some non-linear term then the situation is changed. For instance, just add a term: \(- \lambda \partial^2 \bar{\psi} \). Still no particles could be associated with a weak \( \psi \)-field. But the equation has a constant solution: \( \psi = \pm K/\lambda \). If now we start with this \( \psi \) as zero approximation and consider a small deviation \( \psi' \) from it, \( \psi = \pm K/\lambda + \psi' \), \( |\psi'| \ll K/\lambda \), then this deviation will satisfy the equation
\[ \left( \Delta - \frac{1}{c^2} \frac{\partial^2}{\partial t^2} \right) \psi' - 2K^2 \psi' = 0. \]

Now you can subject this part to quantization so as to associate with it a particle of real mass \( \sqrt{2}K \), but the whole \( \psi \) contains also some constant. This is, of course, a very ambiguous procedure. We can separate \( \psi \) into two parts in different ways. In general we may say that if you have a field \( \psi \), it will consist of two parts, i.e. the particle part \( \psi^{(p)} \) and the other part \( \psi^{(o)} \):
\[ \psi = \psi^{(o)} + \psi^{(p)}. \]
The part \( \psi^{(p)} \) must be quantized if it consists only of Fourier components with time-like directions, and the other part \( \psi^{(o)} \) should not be quantized if it consists only of Fourier components with space-like directions. Of course, we cannot do it unless we know the classical solutions of a non-linear field equation, but in principle we may say that only after separating away a certain part of the field, can we talk about the quantization in the usual sense.

**DISCUSSION**

**Chairman:** I thank Yukawa for his report and I just want to say that for me the most difficult and the most important problem would seem to be what the connection of the commutation rules and the field equations should be. This seems to have been left open still.

**Yukawa:** As for the commutation relations in the case of the boson field it is trivial, because the unquantized part has nothing to do with the commutation relation. But if one has a fermion field, then these two parts will interfere with each other in a very complicated way, and we do not know what will come out for the commutation relations for a fermion field. This I cannot answer yet.

**Moller:** I would like to ask you about the meaning of this: you had a non-linear equation and then you wrote \( \psi \) like a constant plus another function and then you found for \( \psi' \) that linear equation?

**Yukawa:** This is an approximation.

**Moller:** Oh, I see, \( \psi' \) is considered as small.

**Yukawa:** This is only true if \( \psi' \) is everywhere small compared to \( K/\lambda \). This means, that in this approximation the particle field cannot be concentrated in a region that is small compared to the linear dimension \( (\lambda/K)^{1/3} \). This limits the smallest possible extension of a quasi-free particle field. If \( \psi' \) is confined to a very small region, the additional non-linear term will be important.

**Chairman:** Let us go to the next paper of Heisenberg.
RESEARCH ON THE NON-LINEAR SPINOR THEORY WITH INDEFINITE METRIC IN HILBERT SPACE

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The present report will be confined to recent developments in a non-linear spinor theory of matter, that had been treated in several previous papers\(^2\). The theory is different from the conventional theories especially by the use of an indefinite metric in Hilbert space and by the introduction of dipole-ghost states, the rôle of which has been analysed in some detail with the help of the Lee model\(^2\).

The theory in the form that has been suggested\(^3\) in connection with Pauli’s transformation\(^4\) starts from two characteristic invariants:

\[
J = \int \bar{\psi} \gamma_\mu \frac{\partial}{\partial x_\mu} \psi \, dt \quad \text{and} \quad I = \int (\psi \gamma_\mu \gamma_5 \psi)^2 \, dt. \tag{1}
\]

Both expressions are invariant with respect to the Lorentz group, the Pauli – Gürsey group\(^5\) and the PCT-transformations; \(J\) is the conventional Lagrangian of the neutrino theory.

If one chooses any function of \(J\) and \(I\) as Lagrangian, the variation of \(\mathcal{L} = 0\) leads to the wave equation:

\[
\gamma_\mu \frac{\partial}{\partial x_\mu} \psi \pm i K^2 \gamma_\mu \gamma_5 \psi (\bar{\psi} \gamma_\mu \gamma_5 \psi) = 0. \tag{2}
\]

(The constant \(\frac{\partial \mathcal{L}}{\partial J/\partial \psi}\) has the dimension of a square of a length and may be put equal to \(\pm \ell^2\).)

The determination of some of the simplest mass eigenvalues has been carried out recently by Mitter and Schlieder\(^6\) in a first approximation by means of the Tamm-Dancoff method. This method seems for the time being to be the only method available for treating problems of a relativistic non-linear field theory with indefinite metric, in which the commutator cannot be given from the beginning but must come out as a result of the regularity conditions. While the mathematical basis of a non-linear spinor theory with indefinite metric is at present still uncertain in a similar manner as the more conventional theories involving interactions, there seems to be no reason to consider the Tamm-Dancoff method (when applied to a mathematically well-defined eigenvalue problem) as less reliable than other approximation methods.

The Tamm-Dancoff method needs to begin with an assumption about the form of the commutator, the consistency of which has to be checked later in the course of the calculations. This process has been described in detail in some of the previous papers\(^6\). In a first approximation it is convenient to restrict the calculation to the “strong interaction” terms where one has full invariance for the isospin group, and to look apart from such complications as \(\gamma\)-conjugation and degeneration of the vacuum discussed in the preprint\(^6\).

The only assumption for the commutator showing the complete isospin invariance seems to be

\[
\langle \psi_\alpha (x) \bar{\psi}_\beta (x') \rangle = -\gamma_\alpha^{\mu\beta} \frac{\partial}{\partial x_\mu} F(s) = 2 \int e^{i p \cdot (x-x')} \, dp \, dK \, \frac{p_\mu \gamma_\mu^{\alpha\beta}}{p^2 + K^2} \cdot \phi (K) \approx 2 \int e^{i (x-x')} \, dp \, \left[ \frac{p_\mu \gamma_\mu^{\alpha\beta}}{p^2 + K^2} - \frac{p_\mu \gamma_\mu^{\alpha\beta}}{p^2} + \frac{p_\mu \gamma_\mu^{\alpha\beta} \cdot K^\mu}{(p^2)^2} \right]. \tag{3}
\]

In the last line, which is meant as a first approximation, only one discrete mass eigenvalue is considered; the last two terms represent the contributions from the “dipole-ghost”. Other discrete eigenvalues and the continuous spectrum have been neglected.

If (3) is used as a “contraction function” in the Tamm-Dancoff method, the graph\(^7\) leads for a spinor particle of momentum vector \(3_\mu\) to an eigenvalue equation of the form

\[
f (3^3) \cdot 3_\mu \gamma_\mu \gamma_5 \psi_\beta = 0. \tag{4}
\]

The function \(f (3^3)\) has been determined numerically by Mitter and Schlieder and has only one zero defining a mass value\(^8\).

\((*)\) A detailed account of these calculations and other discussions connected with the non-linear spinor theory will be published by the Göttingen group in the Zeitschr. f. Naturforschung.
The solutions of (4) can be divided into two groups. The one belongs to the Dirac equation $\frac{\partial}{\partial x} \psi = 0$ and the mass value zero. It can be interpreted as defining four two-component leptons of mass zero ($e^+, e^-, \nu, \bar{\nu}$).

The other group belongs to a Klein-Gordon equation for a spinor wave function of the type:

$$\left(3_\mu^2 + K^2\right) \gamma_\mu = 0.$$  \hspace{1cm} (5)

It describes four baryons of equal mass ($P \ N \ \bar{N} \ \bar{N}$). Equation (5) can be decomposed into two Dirac equations

$$\gamma_\mu \gamma_\mu \psi = iK \hat{\psi},$$

$$\gamma_\mu \gamma_\mu \hat{\psi} = iK \psi,$$  \hspace{1cm} (6)

as had been shown earlier by Gürsey. The symmetry between the matrix elements ($\gamma$-functions) $\psi$ and $\hat{\psi}$ suggests the introduction of the $\gamma$-conjugation and the use of a spinor

$$\Psi = \begin{pmatrix} \psi \\ \hat{\psi} \end{pmatrix}.$$  \hspace{1cm} (7)

If one introduces Pauli-matrices $\Sigma$ in this spinor space, one may write $\hat{\psi} = \Sigma_1 \psi$ and replace (5) by

$$\left(3_\mu \gamma_\mu - iK \Sigma_1\right) \psi = 0.$$  \hspace{1cm} (8)

The group theoretical problems connected with the $\gamma$-conjugation and the reformulation of the commutator with the help of the $\Sigma$-matrices have been treated in detail by Duer. We mention as one of the results of the kind of the Pauli-transformations, which must — as one sees from (6) — be written as

$$\Psi \rightarrow a \Psi + b \gamma_5 \Sigma_3 C \gamma_5 \Psi,$$

$$\Psi \rightarrow e^{i\alpha \gamma_5 \Sigma_3} \Psi.$$  \hspace{1cm} (9)

Before coming to the symmetry properties of the solutions we have to discuss the three transformations P, C, T (parity, charge conjugation and time reversal). Since the Lagrangian is invariant for these transformations one would expect corresponding quantum numbers. In the present theory, however, neither P nor C commute with the Pauli-transformations describing charge and baryonic number, if P is written in the conventional form

$$P: \ x_k \rightarrow -x_k, \quad \Psi \rightarrow \gamma_4 \Psi.$$  \hspace{1cm} (10)

Therefore, this kind of parity cannot be defined for particles having a definite baryonic number; the only exception being particles with baryonic number zero, e.g. $\pi$ mesons, since their wave function is not affected by the transformation $e^{-i\pi \rho}$. One may, however, define a second kind of parity by the relation

$$P: \ x_k \rightarrow -x_k, \quad \Psi \rightarrow \gamma_4 \Sigma_1 \Psi.$$  \hspace{1cm} (11)

This operation commutes with the Pauli-transformations, since $\gamma_4 \Sigma_1$ commutes with $\Sigma_2 \Sigma_3$. Therefore, one can define a parity for heavy particles by means of (11) and it is this second kind of parity that can be used for deriving selection rules. At this point it may be added that in the present theory the transition from particle to antiparticle is not given by the operation C, but by $PC$ (first kind of $P$) or by $C \Sigma_1$.

Similar calculations as for the nucleons have been carried out by Mitter and Schlieder for the $\pi$ meson group, i.e. for particles of baryonic number zero. The method used was in both cases identical with the method applied in $\pi^0$. The calculations have so far been restricted to particles of spin zero. In this first approximation which is defined by the graph $\left(\right)$, the integral equation for the eigenvalues refers to eigenfunctions in which the two particles are at the same point. This restriction means that one has to do only with $s$-states. For particles of spin 1 one could scarcely hope to get an approximation without taking the $p$-states into account.

Table I gives the representation of the different particles in the form of the operators which annihilate ($A$) or create ($C$) the particles in question. If one wants to construct operators that only annihilate or only create, one has to combine the $\psi$- and the $\hat{\psi}$-term. (The explanation of the signs is given at the bottom of the table.)

Since, on account of the complexity of the calculations, it has so far not been possible to go to a higher approximation in the Tamm-Dancoff method, i.e. to an approximation defined by a more complicated graph, one may try to get some check on the reliability of the calculations by making use of the $\gamma$-functions. The symmetry between the $\psi$- and the $\hat{\psi}$-function suggests that one might get a better approximation, when one replaces $\hat{\psi} \gamma_\mu \Sigma_3 \psi$ in the calculations by $\frac{1}{2} \left(\hat{\psi} \gamma_\mu \Sigma_3 \psi + \psi \gamma_\mu \Sigma_3 \hat{\psi}\right)$. The numbers given in Table II as second approximation refer to this procedure. The parity of the neutron comes out as opposite to the parity of the proton, while the charged pions become scalar particles. This result is not identical with the usual description in the text books (where proton and neutron have the same parity and the $\pi^\pm$ particle is pseudoscalar), but it is equivalent to the usual description, since one can arbitrarily attach an odd parity to the charge. Therefore the result of the calculations is in agreement with the existing experimental evidence. The $\pi^0$ of the isospin-triplet comes out as pseudo-scalar with respect to the parity of the second kind in agreement with the experiments. However, its parity of the first kind is even, like that of the charged pions.

The isosinglet state $\pi^0$ represents a pseudoscalar particle with respect to both definitions of parity. Its mass comes out as considerably higher than that of the $\pi^0$ particles and
it should therefore be highly unstable (*). Hitherto there seems to be no good experimental evidence for an isosinglet $\pi_0^n$ particle.

The determination of the mass value leads to a satisfactory agreement between the first and the second approximation for the nucleons. Therefore, one may conclude that the correct mass value of the nucleons lies somewhere in the neighbourhood of $K\ell \sim 7$, which means that $\ell$ should be roughly equal to the Compton radius of the $\pi$ meson $\left(\ell \sim \frac{\hbar}{\mu_n c}\right)$, in order to yield the correct nucleon mass. For the $\pi$ mesons the two approximations give very different values. This unsatisfactory result seems to

\[
\begin{array}{cccc}
\pi^+ & \pi^0 & \pi^-\\
\begin{array}{c}
A \\psi_1 \psi_2 - \bar{\psi}_1 \bar{\psi}_2, \\
C \quad \psi_3 \psi_4 - \bar{\psi}_3 \bar{\psi}_4
\end{array}
& \begin{array}{c}
\begin{array}{c}
\psi_1 \bar{\psi}_2 + \bar{\psi}_1 \psi_2, \\
- \psi_1 \bar{\psi}_4 - \bar{\psi}_1 \psi_4
\end{array}
\end{array}
& \begin{array}{c}
\begin{array}{c}
\psi_2 \psi_4 - \psi_3 \bar{\psi}_4 - \bar{\psi}_3 \psi_4, \\
- \psi_2 \bar{\psi}_3 - \bar{\psi}_2 \psi_3
\end{array}
\end{array}
& \begin{array}{c}
\begin{array}{c}
\psi_3 \psi_4 - \psi_2 \bar{\psi}_3 - \bar{\psi}_2 \psi_3
\end{array}
\end{array}
\end{array}
\]

\[
\begin{align*}
\bar{\psi} &= \psi^* \gamma_4; & D &= \gamma_5 C \quad ; & D^{-1} &= \gamma_5 C^{-1}. \\
C \gamma_\mu C^{-1} &= - \gamma_\mu. & D \gamma_\mu D^{-1} &= \gamma_\mu.
\end{align*}
\]

\[\gamma^r : \gamma_5 = +1; \quad \gamma^l : \gamma_5 = -1.
\]

\[
\begin{align*}
\gamma^r : \sigma_3 = +1; & \quad \gamma^l : \sigma_3 = -1.
\end{align*}
\]

\[
\begin{array}{cccc}
\text{Parity 1st kind} & \text{Parity 2nd kind} & \text{Graph} & \text{Mass} K\ell \\
\text{not defined} & + & - & 7.08 \\
+ & - & + & 6.67
\end{array}
\]

(*) At the Conference the author had reported that the mass of the isosinglet state came out as equal to the mass of the isotriplet. A renewed controlling calculation has, however, revealed an error in the earlier computations of the neutral $\pi$-states, which did not affect the mass of the $\pi_0^n$-state but did affect strongly the mass of the $\pi_0^n$-state. The author regrets the error in his verbal report and wants to emphasize that an independent repetition of these rather complicated calculations by some other group — if possible by different methods — would be very desirable.

\[
\begin{array}{cccc|ccc|cc}
\text{Particles} & \text{Nucleons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons} & \text{charged mesons}
\end{array}
\]
be connected with the very high sensitivity of the meson mass in the calculations for changes in the nucleon mass. The $\pi$ eigenvalue depends on the nucleon mass and the calculation shows that a change of the nucleon mass by 10% produces a change of the meson mass by more than a factor 2. On the other hand, the nucleon mass can easily be inaccurate by 10-15%. Therefore, a somewhat accurate determination of the $\pi$ meson mass has so far not been possible. But it may be possible in this special case to calculate later a genuine second Tamm - Dancoff approximation. The singlet $\pi_0^0$ mass is not equally sensitive.

The wave equation (2) contains an indefinite sign in the non-linear term. A change of this sign is equivalent to a change of the sign of the commutator (3). For instance the transformation $x_\nu \rightarrow - x_\nu$ will change simultaneously the sign in (2) and in (3). But there is no simple transformation that leads from one sign in (2) to the other, without simultaneously also changing the sign in (3).

The only transformation that can produce this effect is a rather radical change made possible by the indefinite metric. If one changes the norm of all physical states of half-integer spin from +1 to −1 (keeping the norm of the states of integer spin as +1), one changes the sign of (3) without changing (2), and if one simultaneously replaces $x_\nu$ by $- x_\nu$, one changes the sign in (2) without changing (3).

By this transformation one gets into an entirely new set of states, which in relation to the first set can be considered as a non-combining term system. The assumption of a norm −1 of all states with half-integer spin does not interfere with the assumption of a unitary S-matrix. In this new system the particles corresponding to the nucleons get in the first approximation the same mass as those of the first system. The particles corresponding to the pions, however, get a different mass (the value is given in the third column in Table II). It is tempting to connect this non-combining term-system with the existence of the "strange particles" and especially the third column of Table II with the $\theta$-particles. But the analysis of the solutions has not yet been carried far enough to justify such an identification.

The mathematical analysis of the indefinite metric has not been carried further by the Göttingen group since the research connected with the Lee model. But I might mention at this point two recent papers by Ascoli and Minardi, in which these authors try to get a more rigorous axiomatic basis for the use of the indefinite metric. One of their results concerns the equivalence between a local and causal theory with indefinite metric and non-local theory with definite metric. But I understand that Pauli will speak in the discussion about such problems, therefore I need not go into these questions.

In conclusion, I would like to state the present situation concerning the non-linear spinor theory as follows: the question whether this attempt can lead to a theory of the elementary particles is still completely open. But I think that the possibilities offered by the indefinite metric and the results reported here are interesting enough to make it worthwhile to look still more deeply into the consequences of such an attempt.

**LIST OF REFERENCES**

5. Gürsen, V. Relation of charge independance and baryon conservation to Pauli’s transformations. (preprint)

**DISCUSSION**

Pauli: I am glad that during the last month I had occasion to discuss anew with many colleagues the problems of the elementary particles with respect to group theory and to field quantization. But the logical connection between field equations and commutation relations has not been sufficiently clarified neither by rigorous, nor by more clumsy intuitive methods. Therefore, reliable methods for calculating mass ratios of elementary particles are not yet available.

Regarding the papers of Heisenberg and collaborators on the spinor model, which appeared in the last years, after various discussions I reached the conclusion that they are mathematically objectionable.

To illustrate this, consider a spinor field $\psi(x)$ obeying a differential equation of first order

$$\gamma^\rho \frac{\partial}{\partial x_\rho} \psi(x) + F(\psi(x)) = 0$$

(1)
where the argument of the function \( F \) depends only on the value of \( \psi (x) \) at a particular point. (We assume that \( F \) and its derivatives have no singularities; for example, imagine \( F \) to be a polynomial.) Assume, further, that the field not only anticommutes for space-like points,

\[
\{ \psi_\alpha (x), \psi_\beta (x') \} = 0 \quad \text{and} \quad \{ \psi_\alpha (x), \psi_\beta (x') \} = 0 \quad (2a)
\]

\[
\text{for } (x_a - x'_a)^2 > 0,
\]

but that this also holds for \( x = x' \)

\[
\{ \psi_\alpha (x), \psi_\beta (x) \} = 0, \quad (2b)
\]

where in the usual theory a singularity of the \( \delta \)-type appears.

As one knows from the Lee model, the commutation rules \((2a)\) and \((2b)\) can be fulfilled for an indefinite metric. They are, however, in contradiction to the assumption of a differential equation of the first order (Eq. \((1)\)). Indeed, from \((1)\) and \((2a, b)\) it follows for all derivatives

\[
\left\{ \psi_\alpha (x), \frac{\partial^m}{\partial t'^n} \psi_\beta (x') \right\} = 0 \quad \text{for } t = t'. \quad (3)
\]

This is in disagreement with the usually assumed mass spectra. The same contradiction still holds for several spinor fields obeying first order differential equations if the anti-commutativity of the field at the same point is supposed for all these fields.

Now the question arises whether in the quoted papers the contradicting assumptions \((1, 2a, b)\) are actually used. In spite of the introduction of time-ordered products \((r\text{- and } \varphi\text{-functions})\) instead of the field operators themselves, it is my opinion that an assumption fully equivalent to the discussed one has been made in the course of the integration with the help of the Tamm-Dancoff method. Therefore, I do not trust the excuses which have been made for the contradicting results in question and I stress my opinion that the latter is still existing.

In this connection, I also wish to point out that I do not overlook the possibility of replacing the regularity of the anti-commutator at the same point by an oscillating behaviour with an essential singularity. This would give rise to an essentially new situation if this oscillating behaviour were explicitly applied in the course of the integration of the equations for the \( r\text{- and } \varphi\text{-functions.} \)

But as soon as the oscillations are replaced by an average zero, which seems to me not admissible, the equivalence with the assumptions \((1)\) \((2a,b)\) and this contradiction reappears.

I disbelieve all the more in the possible excuses for such a contradiction as it can very easily be avoided. For instance, it is absent in the Lee model, where instead of \((1)\) an integral equation appears. Indeed we know that the commutation relations \((3)\) are not fulfilled in the Lee model with an indefinite metric.

My remarks do not pretend to say anything new. I only wanted to clarify my own position with regard to these problems. The general problem of whether an indefinite metric can be used in physics is not yet covered by my present remarks.

Heisenberg (*) : Pauli's remark contains three different questions which I would like to answer separately and at some length in order to make this side of the theory completely clear. The three questions are:

1. Is the assumption that \( \psi (x) \) and \( \psi (x') \) anticommute everywhere on the subspace \( t - t' = 0 \), but not for time-like \( x - x' \), compatible with the non-linear differential equation?

2. Are the assumptions made about the anticommutator used in a consistent way when they are applied in the Tamm-Dancoff method in connection with the equation \( S(0) = S_F(0) = 0 \)?

3. Should not the differential equation be replaced by an integral equation in the time-variable and is not the Lee-model an example for such a replacement?

For the first question we have to remember that — as had always been assumed — the anticommutator should behave near the light-cone like that classical solution of the non-linear differential equation that vanishes for space-like distances. Such a solution has been constructed (the calculations had been carried out for the older spinor model, but not yet for the new wave equation) and it shows the following behaviour: it has infinitely frequent oscillations in the immediate neighbourhood of the light-cone for time-like distances. It vanishes for space-like distances. The statement that it vanishes everywhere for \( t = t' \) has, however, to be taken with some precaution: at the point \( x = x' \) its value is not defined, on account of the infinite oscillations. But its space integral at \( t = t' \) including the point \( x = x' \) definitely vanishes. There is no \( \delta\)-function of the space co-ordinates. Therefore, we see that this behaviour is actually compatible with the differential equation and there is no inconsistency in using an anticommutator of this (oscillating) type, which is, of course, not analytic at the light-cone.

With regard to the second question, we note that the non-linear wave equation of the operators can be considered as a compact way of writing the infinitely many differential equations between the \( r\)-functions of any number of variables. Actually, for the calculations it would be sufficient to assume that the field equation means nothing else but this infinite set of \( r\)-equations. (Instead of using the time-ordered products one could, of course, also use other kinds of products of field operators.) Now we know that the \( r\)-function

\[
\tau \left( x_1, x_2, ..., y_1, y_2, ...ight)
\]

behaves in the neighbourhood of the point, say \( x_1 - y_k = 0 \), like \( \frac{1}{2} S_F(x_1, y_k) \cdot \tau \left( x_1, x_1, ..., y_1, ..., y_k, y_k, ..., y_k \right) \) plus a

(*) This part of the discussion has not been reported verbatim but is based on a paper sent by Heisenberg after the Conference.
function which is less singular near the light-cone. Therefore, the value of a \( \tau \)-function at the point \( x_l = y_k \) is undefined in the same way as the value of \( S_F(0) \).

On the other hand, those \( \tau \)-equations that result from the wave equation contain always one \( \tau \)-function (that with the higher number of variables) in which three co-ordinates are equal, e.g. either

\[
\begin{align*}
\tau_1 &= \tau_2 = \tau_3 \quad \text{or} \\
\tau_1 &= \tau_2 = \tau_3
\end{align*}
\]

This is a necessary consequence of the postulate of microcausality. Therefore these \( \tau \)-equations have no definite meaning unless one defines explicitly what is meant in the equation by the value of the \( \tau \)-function at this point, e.g. \( x_l = x_k = y_l \).

The simplest definition is given by the assumption that in the equations one shall put

\[
\tau(x_1 \ldots | y_1 \ldots) = \lim_{x_l \to x_l} \lim_{y_l \to y_l} \left[ \tau(x_1 \ldots | y_1 \ldots) - \frac{1}{2} S_F(x_l \ y_l) \tau(x_1 \ldots | y_1 \ldots - 1 \ y_1 \ldots) - \frac{1}{2} S_F(x_l \ y_l) \tau(x_1 \ldots \ h k \ldots | y_1 \ldots - 1 \ y_1 \ldots) \right].
\]

This limiting value exists on account of the behaviour of the \( \tau \)-functions and it gives a possible interpretation of the \( \tau \)-equations. The definition can be replaced by the simpler formula \( S_F(0) = 0 \), or more accurately by the statement: "... in the infinite set of \( \tau \)-equations one can calculate as if \( S_F(0) = 0 \) was generally correct."

It is only this definition of the \( \tau \)-functions with three equal variables which gives a unique meaning to the wave equation. This meaning is compatible both with the oscillatory behaviour of the anticommutator and the postulate of microcausality. The equation \( S_F(0) = 0 \) should be considered as a statement about the wave equation (or the \( \tau \)-equations) rather than as one about the anticommutator.

In momentum space our assumptions concerning the function \( S(x \cdot x') \) or \( S_F(x \cdot x') \) will probably mean the following. The mass spectrum \( \phi(K^2) \), from which \( S(x \cdot x') \) can be constructed, will contain discrete states and a continuous part. For very large values of \( K^2 \) the continuous part will show an oscillating behaviour in order to represent the oscillating behaviour of \( S(x \cdot x') \) near the light-cone. The discrete part will contain the discrete states like nucleons, electrons, etc. and the regularizing dipole ghosts. In the lowest approximations of the Tamm-Dancoff method only the lowest discrete states and the dipole ghost can be taken into account; the higher discrete states and the continuum have to be neglected. Therefore, one obtains a reasonable approximation only if the contribution from the lowest states is considerably bigger than that of the higher ones and the continuum. This situation is well known from the optical spectra of atoms. The conditions, that the lowest lines contain the main part of the total sum of \( f \)-values (\( \Sigma f_r = 1 \) is the Thomas-Kuhn sum rule) is frequently, but not always, fulfilled. The uncertainty of the Tamm-Dancoff method lies just in the uncertainty about the fulfilment of this condition.

The answer to the third question arises from the well-known axiom of quantum field theory, according to which the field operators belonging to an arbitrarily small time-interval \( At \) must be sufficient to construct from vacuum the complete Hilbert space. Therefore, it must be possible to establish a unique relation between the operators referring to the interval between \( t \) and \( t + At \), and those belonging to the interval between \( t + At \) and \( t + 2At \). Since \( At \) can be taken as arbitrarily small, this relation can be written as a differential equation in the time co-ordinate, which may possibly contain integrations over space co-ordinates.

However, it has to be noted that in a relativistic theory the occurrence of integrations over the space co-ordinates at this point would lead to a violation of the condition of microcausality. Even if the anticommutator vanishes for space-like distances within the time interval \( t, t + At \), it would not fulfill this condition in the intervals between \( t + At \) and \( t + 2At \), if the connection would involve integrations over the space co-ordinates. Of course one could try to give up the condition of microcausality; but so far it has not been possible to drop this condition without violating at the same time the principle of macrocausality. It was one of the main results of the theory of special relativity, that causal connections have to be expressed as differential equations that are invariant for the Lorentz transformation. I cannot see how one could get away from this necessity.

Perhaps I should summarize the results of this discussion as follows: if one wants to keep microcausality, one has to connect the field operators at different times by means of a differential equation that is Lorentz invariant, or by other equations that are equivalent to such differential equations. The anticommutator between \( \psi(x) \) and \( \overline{\psi}(x') \) is zero for space-like distances and is indefinite at the point \( x = x' \); the space integral of the anticommutator between \( \psi(x) \) and \( \overline{\psi}(x') \) at the same time \( t = t' \) vanishes. Since the wave equation contains the product of three field operators at the same point one can give a definite meaning to the wave equation by defining this product or the corresponding \( \tau \)-functions by a limiting process, or more simply by \( S(0) = S_F(0) = 0 \). Without such a definition the wave equation would not contain any statement at all.

**Pauli:** It is probably not true that the operators in one time-plane define the whole Hilbert space. You will have to take into account that \( \psi \) is not a measurable field.

**Heisenberg:** That does not matter. It belongs to the Hilbert space; I mean, I can use any operators to define the Hilbert space, but I only need the assumption that the operators in a Hilbert space defined in a certain time-interval are sufficient to construct the complete Hilbert space, and I think that this assumption is completely true also in the Lee model.
Chew: I do not understand.

Heisenberg: Oh yes! There is an apparent contradiction, because in the Lee model the renormalized wave equation takes the form of an integral equation in the time co-ordinate. In spite of this, it should be possible, on account of the axiom mentioned above, to reformulate it into a differential equation, which will probably involve integrations over space co-ordinates.

Pauli: I do not think we will reach an agreement on this point. There are many other points to discuss in Heisenberg’s paper.

Stueckelberg: I just want to ask a rather trivial question. There is the $D_C$ (in Europe) or $D_F$ causality defined in terms of perturbation theory. Its basis is that we distinguish between positive and negative frequencies, and define $D_C$ or $D_F$ functions by positive frequencies. On the other hand, there is the causality, defined by dispersion relations, saying that the vacuum expectation value of the commutator $[\psi(x), \psi(y)]_\pm$ is only different from zero for time-like $x - y$. The question I am asking is whether Heisenberg or anybody in this group can answer the question: is there any relation between the condition of positive frequencies in the $D_C$, $(D_F)$ definition and the positive energy equation in dispersion relations theory?

Oppenheimer: I think I can answer the question. If you have vanishing commutators, then you may construct either a causal or an anti-causal $D$-function. If you do not have vanishing commutators, it is very doubtful that you could construct either.

Gell-Mann: There is just one more comment in answer to Stueckelberg’s question. If the causality in the sense of dispersion relations exists and you talk about the matrix elements of the commutators, then you can also talk, if you like, about the $D_C$ or $D_F$ functions for the same problem, and for positive frequencies in the Fourier transform these will agree and for negative frequencies they will disagree only in the sign of the imaginary part. So a statement about one is effectively a statement about the other, and the people working on the dispersion theories conventionally work with the commutator rather than with the $D_C$ and $D_F$ function just for greater convenience.

Stueckelberg: Does this mean that if you speak of commutators you do not really need somehow a series development?

Gell-Mann: Well, one hopes in dispersion theory not to make use of perturbation theory. Of course, nobody really knows how to work with field theory aside from perturbation theory but people try, and they have produced a certain number of results especially in dispersion theory.

Oppenheimer: The answer is the following. Of course, to know the commutators in full you need to know the theory and know whether it is linear or not, but to know that something vanishes will be true of all functions. Thus this statement does not restrict you to any linear theory.

Klein: I would like to ask a very simple question which can be actually answered. Will Heisenberg please remind us of what is the meaning of the graphs for the calculations of the masses.

Heisenberg: When you work with the Tamm-Dancoff approximation you deal with the $\tau$-function $\langle \Omega | \psi(x) | \Phi \rangle$. Then from this $\tau$-function you go over to some function with more variables, by means of a Green’s function, using the wave equation which has been written. So this is something like, say $\langle \Omega | \psi(x) | \Phi \rangle = \langle \Omega | dx' G(x, x') \psi(x') \bar{\psi}(x') \psi(x') | \Phi \rangle$.

Then you can again go over to the next step and you introduce two Green’s functions, $G(x, x') \cdot G(x', x)$.

In this way you get higher $\tau$-functions and then at the end you make the so-called contractions. Thereby these $\tau$-functions can be expressed approximately by the $\tau$-functions of a lower number of variables. The contraction function is essentially the commutator, or more accurately the Schwinger $S_T$-function. The graph states which points $x$ or $x'$ or $x''$ are to be connected by $G_F$- or $S_F$-functions in the final integral equation.

Thereby one can get a simple formal picture for such a rather complicated integral equation, just as in the conventional Feynman-graphs.

Johnston: I should like to ask Heisenberg one point about the masses of the $\pi^0_a$ and $\pi^0_b$ in the scheme he drew up on the board. He said that in the first approximation the masses became equal, and he said that perhaps if other corrections were included, mentioning particularly electromagnetic corrections, this situation would possibly change. Does the electromagnetic interaction come from the first wave equation written on the board, or is this something extra that has to be added?

Heisenberg: That part of the calculation concerning the electromagnetic fields and particles has not yet been carried out for this wave equation, but it had, in a former paper by Ascoli and myself, been done for the old nonlinear spinor equation. At that time it turned out that the electromagnetic forces can come out of such an equation; actually they are contained in it. So at this point we do not yet have any definite results for the new equation, but I think it is very probable that one can just repeat the old calculations for this model here and we will get very similar results.

Wentzel: There are so many questions to be asked that I can hardly start, but let me ask what has become of the problem that you once connected with the degeneracy of the vacuum, the necessity to explain the large number of quantum numbers one needs, for instance, in baryon physics.
Formations in Hilbert space, one needs for $A' = T^\dagger A T$, half-integral representations of the rotation group but the corresponding transformations in Hilbert space not then if one tries to replace their transformation by trans­formation of states in Hilbert space by saying: we have not only one state vector, namely a function $\psi$, but we have $\psi_1$ and $\psi_2$ and thereby we have a duplication of the states. In the same way you can say here that we start from a Lagrangian in which, of course, there is nothing to be seen of any duplication; but when we now try to find a commutator which corresponds to this Lagrangian, it may turn out that a square root is to be taken (e.g. as for the mass in the Klein- Gordon-equation), and therefore a duplication may occur in the vacuum or in the state vectors.

Wentzel: This partly answers my question, but looking at reality we have a quantum number that is called strange­ness. Is the multiplicity that is inherent in your formalism sufficient to explain or to incorporate all the quantum numbers which we need nowadays? Of course you can go on and introduce a $\sigma$ and a $\Sigma$, and by always taking another power of two you can get there. But is this a natural procedure?

Heisenberg: I think it is characteristic for these quantum numbers — you referred to the quantum numbers called $I$ and $I_N$ in the preprint — that they are apparently defined only by cyclic groups. We need no space group, no rotation in space, to explain these quantum numbers. It seemed natural to us that just the cyclic groups should be put into the Hilbert space. It was especially emphasized by Durr that actually cyclic groups may be transferred on the Hilbert space. It seemed natural to connect this result with the other empirical fact that these extra groups are actually only cyclic groups. So I think it is not unnatural to say that these two cyclic groups belonging to $I$ and $I_N$ are actually properties of the operators which can be expressed by a duplication of the vacuum or of the state vectors.

Pauli: I completely disagree with the answer of Heisenberg. I think this is not only unnatural but this is mathematically impossible. You cannot use the multi­plicity of the vacuum in order to obtain the strangeness, because then the charge of two protons is not any longer two, if the charge of one proton is one. This I discussed already in April and I wonder that you again repeat it all.

Heisenberg: Of course I again disagree completely with what Pauli said, because I do not see the slightest reason why one should not take out of the vacuum as many charges as one has protons; that is the same as in the Dirac theory. Dirac has introduced the doubling for each new electron, and for ten electrons he had also ten spins.

Pauli: The point is that the additivity of charges is different from the additivity of other operators, so that the answer was again completely false: the additivity of charges is different from the additive behaviour of other operators like spin and isotopic spins, and masses. This is the point. The number of the vacua does not multiply if you have two protons. I think this was disproved in April.

Gell-Mann: May I attempt to re-state the situation? I am not sure if I understand all points but perhaps it will come out. As I understand the idea, it would be something like this. You take a twin vacuum and an operator, which you would ordinarily call a neutron operator. You act on the twin vacuum with the neutron operator. On one state it makes a neutron and on another state it makes a $\Lambda$. So you get two states, neutron and $\Lambda$. I think the difficulty is, that if you take two neutron operators, the two vacua will give you only double the number of states but you need four times as many states now, namely neutron-neutron, neutron-$\Lambda$, $\Lambda$-neutron, and $\Lambda$-$\Lambda$. So, depending on how many particles you want to describe, you need $2^N$ vacua. This must vary with the number of particles we wish to describe. I suppose one can do it but it seems to be complicated and very much like adding another field.

Heisenberg: I agree completely with what Gell-Mann just said about this point; but at the same time, I propose to postpone the discussion for half a year and then we will know more about it.

Pauli: Well, I think that it is superfluous. I think that in half a year the answer would be the same as Gell-Mann gave just now.
THE INDEFINITE METRIC WITH COMPLEX ROOTS

W. PAULI

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I propose that we now go to the indefinite metric, and there is first my small communication on the Indefinite metric with complex roots. It was agreed between Glaser and myself that I should present it here.

In his paper on the Lee model, Heisenberg discussed the special case of the dipole ghosts and its physical interpretation. There is also the possibility of the eigenvalue equation for the V particle having two complex conjugate roots. The dipole ghost is a special case of the latter possibility, namely the case when the two conjugated complex roots coincide on the real axis.

Already last autumn I convinced myself that the complex root case could be treated as satisfactorily as the dipole ghost case investigated by Heisenberg—at least in the sectors of only one V or one N particle. Starting from a stationary state approach there is, of course, no difficulty in defining a unitary S-matrix operating only in the sub-space of physical states of positive norm. But I would like to concentrate on the possibility of a temporal description of physical phenomena, since this is necessary in order to understand causality. This is a non-trivial problem since we now have also complex energies in our system, which means that there will also be solutions with an exponential time increase as \( t \to \pm \infty \) and so this limit has to be investigated.

Let us suppose we have a self-adjoint Hamiltonian \( H = H^0 + V \), \( V \) representing the "scattering" of the constituents of the system described by \( H^0 \). Let us suppose that the spectrum of \( H^0 \) contains besides the normal eigenstates \( | n \rangle \) of positive norm

\[
H^0 | n \rangle = E_n | n \rangle, \quad \langle n | n \rangle > 0,
\]

also a pair of "ghost-states" \( | a \rangle, | b \rangle \), with complex energies:

\[
H^0 | a \rangle = E_a | a \rangle, \quad H^0 | b \rangle = E_b | b \rangle, \quad E_a = E^*_b, \quad \langle a | a \rangle = \langle b | b \rangle = \langle a | n \rangle = \langle b | n \rangle = 0, \quad \langle a | b \rangle + 0, \quad \text{e.g.} = 1.
\]

The vanishing of the norm of these two states follows from the self-adjointness of \( H^0 \). The effect of the interaction \( V \) between two finite times \( t' = -T \) and \( t'' = +T \) is described by the usual matrix \( U(t', -T) \), which satisfies the interaction representation equation:

\[
i \frac{d}{dt} U(t', -T) = V(t) U(t', -T)
\]

with the boundary condition \( U(-T, -T) = 1 \). This matrix is pseudo-unitary (for finite time intervals) in the sense of the indefinite metric. But transition elements \( \langle a | U | n \rangle \) and \( \langle b | U | n \rangle \) leading from normal states to ghost states will be different from zero.

To remedy this the idea is to define physical states as

\[
| f \rangle = | n \rangle + c_a | a \rangle
\]

containing no state \( | b \rangle \). Then the norm of such states will always be positive, or more generally

\[
\langle f' | f \rangle = \langle n' | n \rangle,
\]

and for matrix elements of observable quantities such as \( H^0 \), we will have

\[
\langle f' | H^0 | f \rangle = \langle n' | H^0 | n \rangle.
\]

In order to get again a physical state after the time interval \( 2T \), we shall try to determine the number \( c_a \) appearing in the initial state so that:

\[
U(T, -T) | f \rangle = | n_T \rangle + c_T | a \rangle
\]

(i.e. with no \( b \) state admixture). This gives

\[
\langle a | U | f \rangle = \langle a | U | n \rangle + c_a \langle a | U | a \rangle = 0, \quad c_a = -\langle a | U | n \rangle / \langle a | U | a \rangle.
\]

This is a straightforward generalization of what Heisenberg did in the dipole case.

Now, the relevant question is whether the limit \( T \to \infty \) exists. I have verified that this is indeed so to the first order of perturbation theory; Källen independently did the same calculation last autumn. The result is:

\[
c_a = \frac{e^{i(E_a - E_b)T} - e^{-i(E_a - E_b)T}}{e^{i(E_a - E_b)T} - e^{-i(E_a - E_b)T}} E^*_a - E_a \langle a | V | n \rangle
\]

which shows that \( c_a \) vanishes like \( e^{-1/2} \) for \( T \to \infty \), where we have put \( E_a = \epsilon + i \Gamma (\epsilon, \Gamma \text{ real}) \). Now the physical transition matrix element
also approaches a definite limit
\[ \lim_{T \to \infty} \langle f' \mid U(T, -T) \mid f \rangle = -i \int_{-\infty}^{\infty} e^{i(E_{f'} - E_f)T} dt \langle n' \mid V \mid n \rangle \]

provided we drop a purely oscillatory term (without singular denominators), arising from the second term in (8) and this is certainly permissible.

Here I would like to point out that we are free to choose which of the two ghost states we denote by \( \mid a \rangle \), and which by \( \mid b \rangle \). If we permute their roles, the expression (7) will reverse its sign in the asymptotic limit \( T \to \infty \). This means that the transition matrix between physical states for finite times is not invariant under time reversal, and that unitarity within the physical subspace holds only asymptotically for large \( T \).

Källén also remarked that it is not absolutely necessary to fix sharply the finite time of interaction \( t' = T \). If you replace \( U(T, -T) \) by \( U(T + \tau, -T) \), where \( c_0 \) has been fixed by the condition at \( t' = T \), and then pass to the limit \( T \to \infty \), everything in the limit remains the same. Of course this was to be expected.

It has to be seen whether the above procedure works also in the higher approximation of perturbation theory; this has not been done as yet.

\[ \langle f' \mid U \mid f \rangle = \langle n' \mid U \mid n \rangle + c_a \langle n' \mid U \mid a \rangle \quad (8) \]

\[ \langle f' \mid U(T, -T) \mid f \rangle = -i \int_{-\infty}^{\infty} e^{i(E_{f'} - E_f)T} dt \langle n' \mid V \mid n \rangle \quad (9) \]

A few days ago I received a paper by Ascoli and Minardi, who reach similar conclusions. However, some parts of the arguments of these authors need further clarification.

It is clear that the above procedure is also applicable in principle if, instead of a pair of ghost states, one has several such pairs: \( \mid a_r \rangle, \mid b_r \rangle \) with \( \langle a_r \mid a_s \rangle = \langle b_r \mid b_s \rangle = 0 \), and \( \langle a_r \mid b_s \rangle = \delta_{rs} \). In that case, instead of a single equation (6) for determining \( c_a \), one has a set of linear equations:
\[ \langle a_r \mid U \mid n \rangle + \sum_s \langle a_r \mid U \mid a_s \rangle c_s = 0 \quad (10) \]

(or a set of integral equations in the case of the continuum). Detailed questions, as to whether sets of such equations are solvable, are not answered as yet.

I want to add that Glaser and Froissart (CERN) have produced relativistically covariant models of the above type, which have been very ingeniously constructed. They generalise the case of the one pair example to several pairs. They have informed me that some objections, which occurred in the case of several pairs, have been withdrawn — something that Heisenberg will be interested to know.

However, in all these models the energy spectrum is not known if the interaction is included. Particularly it has to be seen whether or not bound states with real energy and negative norm arise.

**LIST OF REFERENCES**


**DISCUSSION — see p. 130.**
I wish to draw attention to some very simple and, I must say, rather trivial possibilities of realizing Heisenberg’s idea concerning the indefinite metric.

In the conventional field theory the scattering matrix in the interaction representation is defined by means of some local fields \( \psi(x) \). Substitute in the corresponding formulae

\[
\psi(x) \rightarrow \tilde{\psi}(x) = \psi(x) + \Sigma C_n \psi_n(x) ; \quad C_n = \text{const.} \quad (1)
\]

where some of the fields \( \psi_n(x) \) may have the commutation relations with the inverse sign. Let us introduce two Hilbert spaces: a “real” one \( H_I \) corresponding to real particles described by \( \psi \), and \( H_F \) corresponding to “fictitious particles” described by \( \psi_n \). By the substitution (1) one can get a formal expression for the scattering matrix \( S \) operating in the total Hilbert space \( H = H_I + H_F \).

The state vector \( \Phi \) may be decomposed into two parts:

\[
\Phi = \varphi + F; \quad \varphi = P \Phi, \quad F = (1-P) \Phi,
\]

one physical and the other fictitious.

The trouble with the matrix \( S \) is that the norm of the physical part \( \varphi \) of the state vector is not conserved and so we have to introduce explicitly negative probabilities.

To get rid of this well-known difficulty, one may proceed as follows:

Impose the condition

\[
F - \varphi = 0 ; \quad (2)
\]

then as we have

\[
\varphi = S \varphi - \varphi \]

we can write

\[
\varphi = PS (\varphi + F - \varphi) \]

\[
0 = F + F - \varphi = [F + (1 - P) S (\varphi + F - \varphi)].
\]

From these relations it follows that

\[
\varphi = \tilde{S} \varphi, \quad \tilde{S} = PS \left[ 1 + (1 - P) S \right]^{-1} P. \quad (3)
\]

Here, the matrix \( \tilde{S} \) operating in the space \( H_I \) may be considered as the “physical scattering matrix”.

It is easily seen that \( \tilde{S} \) is unitary; the norm of the physical part of the state vector is conserved and so are the mean values of energy, momenta etc., calculated by means of \( \varphi \).

The main difficulty of our model is related to the question whether it satisfies the requirements of macroscopic causality. The trouble is that we have no formal definition of these requirements.

In such a situation I can refer only to the case of classical, non-quantized fields. In this case one may, in fact, obtain non-local theories which present no difficulties with the macroscopic causality, by imposing the conditions of the type (2) directly on the field functions.

But as the situation with the quantized fields is quite different, the problem of the macroscopic causality in the models of the proposed type is quite open.

I hope that because of their simplicity they can be useful in order to clear the difficulties appearing with the introduction of the indefinite metrics.

In conclusion, I wish to present an intuitive argument against the theories with the indefinite metric leading to propagation and Green’s functions which are “too regular”, that is, which decrease too rapidly with the increase of momentum.

In fact, in such theories we must find that the particles become “transparent” in high energy collisions but there is now some evidence, stressed by Blokhintsev et al.\(^1\) that on the contrary they must have a kind of black core.

\( (*) \) During the Conference the author has found a necessary condition of macroscopic causality which any theory must satisfy. The considered models do not satisfy it. The condition I refer to may be stated, roughly speaking, as follows:

Suppose we have a system consisting of two non-interacting parts \( \alpha \) and \( \beta \), whose dynamical variables commute, then the physical \( S \)-matrix for the whole system must be the product of the \( S \)-matrices of the two sub-systems

\[
\tilde{S}_{\alpha + \beta} = \tilde{S}_\alpha \tilde{S}_\beta. \quad (1)
\]

The same condition is obviously fulfilled by the formal \( S \)-matrix

\[
S_{\alpha + \beta} = S_\alpha S_\beta. \quad (2)
\]

The prescription of getting \( \tilde{S} \) from \( S \) must be such that (1) follows automatically from (2).
LIST OF REFERENCES


DISCUSSION — Pauli and Bogolyubov

Pauli: I really want to say that there is a little difference in the rules of our game. Bogolyubov considered real states with negative norms. But we consider only states with complex energy or dipole ghosts. If you are dealing with states of positive and negative norm, which belong to different energies, then I think you could run into some trouble.

Bogolyubov: I admit that it may be so, but I think one should maybe add some other auxiliary conditions. Perhaps the indefinite metric is not good at all because it may give rise to some transparency phenomena, as one would intuitively expect, since it may lead to a too regular propagator. The present experiments seem not to be too compatible with transparency of nuclei and maybe of other particles.

V. Glaser: I would like to mention an exactly solvable model of the Bogolyubov type. With such examples, we could maybe decide whether Bogolyubov’s prescription automatically leads to causality. One of these examples is the following: let us take in the Bogolyubov general expression just two fields $A(x)$ and $B(x)$, $A(x)$ being a field with positive norm and $B(x)$ a field with negative norm, both having exactly the same mass. Then, let us consider the field

$$C(x) = A(x) + B(x)$$

and an interaction Lagrangian which is a local function only of this operator $C$. Let us say $g' = C^4$, to have a definite example. The operator $C$ has the property that it commutes with itself for any two space-time points (in the interaction representation, of course). This property is also shared with the function $g'$:

$$[g'(x), g'(y)] = 0 \quad \text{for all} \ x \text{ and } y.$$  

So the differential equation

$$i \frac{\delta U}{\delta x} (x) = -g'(x) U$$

can be solved as a classical equation and its solution for the $S$-matrix is

$$S = e^{\eta} \quad \eta = \int g'(x) \, d^4 x = \int C^4(x) \, d^4 x.$$  

Then, following the prescription of Bogolyubov, we write this as

$$S = \frac{1 + i \tan \eta/2}{1 - i \tan \eta/2}$$

and the expression for $\hat{S}$ becomes

$$\hat{S} = \frac{1 + i \tan \eta_1/2}{1 - i \tan \eta_1/2},$$

where

$$\eta_1 = \int A^4(x) \, d^4 x,$$

and where double dots mean the ordered product. It is easily seen that such an $\hat{S}$-matrix applied to a two-particle or a three-particle initial state reduces to

$$\hat{S} \, |2\rangle \, |3\rangle = \frac{1 + i \eta_1/2}{1 - i \eta_1/2} \, |2\rangle \, |3\rangle,$$

which means that in this theory we have only elastic scattering and no production of particles. Now such a kind of an expression was written down in the 40’s by Heitler, but not with such a deep philosophy behind it. However, people at that time rejected such types of $S$-matrices — it seems to me on the grounds of acausality. I really do not know for the moment if one could really prove that such an $S$-matrix is really macroscopically causal. What I can prove is that it is certainly not microscopically causal, since the forward scattering amplitude derived from it does not satisfy all the requirements of the standard dispersion relations. I think that it is maybe worthwhile to study such an $\hat{S}$-matrix and to see whether it is really in agreement with macro-causality.

Bogolyubov: By means of a power series development one may probably prove that the dispersion relations are not generally fulfilled in my theory. I agree that even with macroscopic causality it will not be quite all right with Glaser’s example. One should study different possibilities and maybe some of them will give macroscopic causality, but really I do not know.

Chairman: This is a thing open to further investigations. I would like to invite Kroll to say some words on his investigation on the higher sectors of the Lee model, especially on the sector dealing with two heavy particles. This is a rather difficult problem.
ON THE LEE-MODEL

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What I am going to say is perhaps not so difficult. I would like to describe a preliminary investigation of what happens when instead of having only one heavy particle you have two heavy particles, and in particular when you have one \(V\) particle and one \(TV\) particle.

Now it is clear that, if you have two such particles, there should be some force acting between them due to the exchange of the mesic particle (\(\theta\)-particle) and one can derive a Schrödinger-type of equation for this kind of problem. Now in order to maximize one's chances of escaping any difficulties which may arise, one generalizes the Lee-model in the sense that one allows the heavy particles to move and hence to have a kinetic energy. I will not discuss here the precise way in which this generalization has been carried out, but instead I will just write down the kind of integral equation you get for the bound state problem of these two heavy particles

\[
|E - Q_p - N_p| \left| \int \frac{d^2k}{\omega} \frac{1}{Q_p - \omega - N_{p-k}} |^2 (\omega - E) \varphi(p) \right| = \pm \left| \int \frac{d^2k}{\omega} \frac{1}{\omega + N_p + N_{p-k} - E} \varphi(p - k) \right|.
\]

The quantities \(N_p\) and \(Q_p\) represent the energies of the \(N\) particle and the renormalized energy of the \(V\) particle as functions of momentum. While the model permits one to choose them arbitrarily, they should be chosen so as to yield a reasonable physical relation between energy and momentum. For example, one might take \(N_p = p^2/2M, Q_p = Q^0 + p^2/2(M + Q^0)\), in which case \(M\) corresponds to the mass of the \(N\) particle and \(Q^0\) to the energy difference between the \(N\) and \(V\) states at rest. Regardless of the specific choice one should have \(N_p \to 0, Q_p \to Q^0\) in the infinite mass limit.

The two signs appearing at the right side of the integral equation correspond to two possible symmetries.

Now the question of interest is whether, having specified the theory so that one gets only dipole ghosts or complex ghosts in the one heavy particle sectors, one gets also only dipole ghosts or complex ghosts when more than one heavy particle is present. To answer this question for this special case of one \(N\) particle and one \(V\) particle, one must discuss the above equation, which I am afraid I have not done in any detail so far.

However, I would like to present an argument, which I think is quite plausible, that is intended to show that for some choices of \(M\) and \(Q_0\) bound states of negative norm occur, and that in the dipole ghost case (\(Q_0\) real) such states always occur. Hence the fact that one has only dipole or complex ghosts in the presence of only one heavy particle does not, by itself, guarantee that one will have only dipole or complex ghosts when two heavy particles are present.

If one considers the infinite mass limit, then \(N_p = 0, Q_p = Q_0\) and the integral equation can be solved by \(\varphi(p) = e^{ip \cdot r}\), which substitution then yields an explicit relation for \(E\). For sufficiently small \(r\) this equation always has a real root corresponding to a state of negative norm. Indeed, one can use the connection between \(E\) and \(r\) to calculate an effective "potential" for the negative norm states. This potential is attractive and of finite range.

The mere existence of an equation with kinetic energy which approaches this infinite mass limit would seem to show that also for finite but sufficiently large mass one would get bound states of negative norm. On the other hand, the fact that the effective potential is of finite range suggests that it might be possible to avoid these bound states by means of an appropriate choice of \(M\) and \(Q_0\). However, in the particular case of the dipole ghost, i.e. \(Q_0\) real, this does not appear to be the case.

The argument is the following.

If one is looking for weakly bound states, then \(p\) is going to be small and one should then be able, as an approximation, to neglect \(N_p\) and replace \(Q_p\) by \(Q_0\) in the energy denominators (but not in the factor outside the integral). Then by taking the Fourier transformation one gets an ordinary differential equation, which for \(s\)-states has the form

\[
\left( \frac{d^2}{dr^2} - \gamma^2 \right) U(r) = V(r) U(r)
\]

with \(U(0) = U'(0) = 0\), \(\gamma^2\) proportional to the binding energy, and the potential \(V\) having a form similar to a Yukawa potential. The extra boundary condition at the origin can be deduced from the integral equation and is necessary to obtain an eigenvalue problem. One may discuss this equation in a preliminary way by guessing a
form for \( U(r) \) satisfying the boundary conditions at the origin and behaving at infinity in a manner appropriate to a specified value of \( \gamma^2 \). Then one uses the equation to obtain a potential \( V \), the procedure being analogous to that used to obtain the Hulthén potential. By guessing \( U(r) \) properly, the resultant \( U(r) \) can be made similar in form to the potential actually appearing. Then one notices that as the binding energy is allowed to go to zero, the magnitude of the potential also goes to zero.

In order to understand better why one can get a bound state even for a weak potential, one may notice that one of the asymptotic solutions outside the range of the forces is simply

\[
re^{-\gamma r}.
\]

This satisfies the ordinary boundary condition at the origin and fails to satisfy the new one only by quantities of the order of \( \gamma^2 \). It therefore takes only a small correction to make it satisfy both boundary conditions. Therefore, it appears that this equation always has at least one bound state. The norm of the bound state can be written as the sum of two terms. One of the terms is negative and the other is of the form

\[
-K \cdot \int \frac{d^3 k}{\omega (\omega + N_p + N_{p-k} - E)^2} \psi(p-k) \psi(p) d^3 p,
\]

where \( K \) is a positive constant. If one again neglects \( N_p \) and \( N_{p-k} \) in the denominator, the sign of this term is also negative.

The occurrence of these bound states of negative norm would appear to represent a danger to the unitarity of the \( S \)-matrix, but this aspect of the problem has not yet been examined.

**DISCUSSION**

**Heisenberg:** One of our Göttingen physicists has done similar calculations and I wonder how you overcame the following difficulty. When you add the kinetic energy of the heavy particles, the problem of renormalization changes completely because at high energies the kinetic energy of the \( \theta \)-particle competes with the kinetic energy of the \( \theta \)-particle in the denominators, and therefore the powers of the energy in the denominator may become different from the ordinary Lee model. Did you simply cancel out these kinetic terms in the denominator, or did you actually carry out a new renormalization?

**Kroll:** I think that we actually carried out a new renormalization. One has a great deal of freedom; by properly choosing the cut-off procedure and the unrenormalized kinetic energy function of the \( V \)-particle, one can obtain any specified form for the renormalized kinetic energy in the cut-off limit.

**Källén:** I should like to add a comment on the calculation with indefinite metric mentioned earlier by Pauli. If you take this kind of game seriously (and personally I keep an open mind), and if you try to build up wave packets, then you are in difficulties if you try to localise those wave packets too sharply. This is intuitively quite clear. Another thing which seems intuitively reasonable is that the "scale of localisability" is given by the complex roots. This is born out by actual computation. In particular, the real and imaginary parts of those roots enter separately into the conditions for the wave packet. You have difficulties with the localization of the wave packet if you let either the real part or the imaginary part of the eigenvalue tend to zero. In particular, you have some trouble if you let only the imaginary part go to zero, which means the dipole case, and you will be in even more trouble if you let the real part go to zero as well, which would mean a dipole ghost at zero energy. Then you cannot work with wave packets at all. For that reason I would say that if you want to take this kind of game seriously you should take complex roots at very high masses.

**Glaser V.:** Indeed this is very probable in all these theories with indefinite metrics in which you can define in some way a unitary \( \tilde{S} \)-matrix. The \( \tilde{S} \)-matrix is actually a very complicated function of the indefinite \( S \)-matrix. In the case of the Heisenberg exclusion principle — if it works — the \( \tilde{S} \)-matrix is something of the form

\[
\tilde{S} = S (1 + L), \quad \text{where} \quad L = (Q_2 S P)^{-1},
\]

where \( Q_2 \) is the projection operator on the space of the "bad" ghosts and \( P \) is the projection operator on the normal states. By the way, you can reduce the question of the applicability of the Heisenberg ghost exclusion principle to the problem of the existence of the inverse of the operator \( (Q_2 S P)^{-1} \). Now, dispersion relations, cross-theorems, and these kinds, of things, hold only for the non-physical \( S \)-matrix. In order to get physical scattering amplitudes from these expressions, you have
form for $U(r)$ satisfying the boundary conditions at the origin and behaving at infinity in a manner appropriate to a specified value of $\gamma^2$. Then one uses the equation to obtain a potential $V$, the procedure being analogous to that used to obtain the Hulthen potential. By guessing $U(r)$ properly, the resultant $U(r)$ can be made similar in form to the potential actually appearing. Then one notices that as the binding energy is allowed to go to zero, the magnitude of the potential also goes to zero.

In order to understand better why one can get a bound state even for a weak potential, one may notice that one of the asymptotic solutions outside the range of the forces is simply

$$re^{-yr}.$$  

This satisfies the ordinary boundary condition at the origin and fails to satisfy the new one only by quantities of the order of $\gamma^2$. It therefore takes only a small correction to make it satisfy both boundary conditions. Therefore, it appears that this equation always has at least one bound state. The norm of the bound state can be written as the sum of two terms. One of the terms is negative and the other is of the form

$$-K \cdot \int \int \frac{d^3k}{\omega (\omega + N_p + N_{p-k} - E)^2} \psi(p - k) \psi(p) d^3p,$$

where $K$ is a positive constant. If one again neglects $N_p$ and $N_{p-k}$ in the denominator, the sign of this term is also negative.

The occurrence of these bound states of negative norm would appear to represent a danger to the unitarity of the $S$-matrix, but this aspect of the problem has not yet been examined.

**DISCUSSION**

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**Kroll:** It seems to me that when you look at the $\theta$-$N$ scattering amplitude you do not have dispersion relations in the case of complex roots. The absence of dispersion relations was pointed out as an objection in the case of real roots of negative norm, using Bogolyubov’s prescription. I think that the same situation may very well arise in the case of complex roots. Does anybody disagree with me?

**Glaser V.:** Indeed this is very probable in all these theories with indefinite metrics in which you can define in some way a unitary $\tilde{S}$-matrix. The $\tilde{S}$-matrix is actually a very complicated function of the indefinite $S$-matrix. In the case of the Heisenberg exclusion principle — if it works — the $\tilde{S}$-matrix is something of the form

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to perform a rather complicated algebraic transformation, so that analyticity properties and crossing properties may very well get lost in the course of this procedure. An example of such a situation was just the model in the spirit of Bogolyubov, which I was discussing before.

*Oppenheimer:* Can one show that a theory obtained from one with ghosts and an indefinite metric, by projecting them out in this way, is equivalent to a covariant but non-causal — that is, not microscopically causal — theory, that is a theory with interactions that are not local but which is formally covariant?

*Pauli:* As far as I know the answer is "yes".

*Glaser:* In a paper Lehmann, Symanzik and Zimmer­man have shown that if you write down any $S$-matrix, provided it is unitary and invariant under the Lorentz group, you can extrapolate it in many different ways to a non-local theory.

*Bleuler:* I would like to make a general remark about the indefinite metric. In all these examples we had the Hilbert space split into a positive and a negative part, but this is only a special case. In the general case we have three parts, namely parts with positive, negative and zero norm. The latter is the set of all states which are orthogonal to the complete Hilbert space. In this case we can add to any vector in our Hilbert space an undetermined vector of this zero norm space and that played a rather important role in our old application in quantum electrodynamics. This addition does not change the norm of the vectors but it does change certain expectation values. In our old application we found that this change corresponds to a gauge transformation. I wonder how such a space of zero norms could be introduced here and how it could be treated.

*Pauli:* We look rather differently on this question; it is merely that we have here the upper and the lower half of the complex plane. If you have only $a$-states and no $b$-states, one falls back to this case.
I want to discuss briefly a possible avenue for the future development of quantum field theory, which I believe may be fruitful. We are all accustomed to the idealization that accompanies the quantum theory of fields in its representation of physical phenomena, i.e. the characteristic quantum mechanical feature of the use of abstract vectors and operators to symbolize physical quantities. But in one respect, at least, the quantum field theory has been conservative. It continues to make use of a classical space-time background, upon which the quantum description is superimposed. I would like to suggest a slight deepening of the abstract basis for the representation of physical phenomena, which is the replacement of the Lorentz or Minkowski space by a Euclidean space. At first sight the idea of describing physical phenomena in terms of a Euclidean geometry may appear to be either trivial or incorrect. The aspect of triviality comes from our familiarity with the use of an imaginary time coordinate to bring about an appearance of the Euclidean geometry in the equations of physics. However, this use of an imaginary time coordinate is basically absentmindedness, for in classical physics one is very familiar with the fundamental difference between hyperbolic and elliptic differential equations. This already disposes of triviality. It is even less trivial in quantum mechanics because we know that the nature of states is fundamentally related to the underlying symmetry group. That is, we can say that the physical states are, in a sense, representations of the underlying Lorentz group on the one hand, or of the Euclidean group on the other, and these two groups have completely different topologies. This means that while you can certainly take a representation of the Euclidean group and from it derive a representation of the Lorentz group, you will not get all possible representations in this way. What I would like to assert is that while one does not get all the mathematical representations of the Lorentz group, all the representations of physical interest are actually obtained. The essential point to be made is that this possibility of a correspondence between the quantum theory of fields with its underlying Lorentz space, and a mathematical image in a Euclidean space — if one adopts a postulate that one should be able to do this in detail — gives results which go beyond what can be obtained from the present theory of fields. These I shall try to indicate. But besides this, by freeing ourselves from the limitation of the Lorentz group, which has produced all the well-known difficulties of quantum field theory, one has here a possibility — if this is indeed necessary — of producing new theories. That is, one has the possibility of constructing new theories in the Euclidean space and then translating them back into the Lorentz system to see what they imply. Concerning the second feature, I have done nothing. I am merely suggesting that when one finds formulations that are equivalent, one of these will be distinguished as the one that makes contact with the future theory. All we can do at the moment is to look at all the possible ways of formulating the present theory. First I must demonstrate that there is at least a possibility of replacing the description in Lorentz space by a description in Euclidean space. This depends on making use of suitable objects of correspondence. For this the well-known vacuum expectation values of time-ordered products are fundamental. These vacuum expectation values, or Green's functions, as I prefer to call them, are the basic objects which enable us to establish the correspondence between the Lorentz and Euclidean formulations. We consider a general Hermitian field \( \phi \), which decomposes into a Bose-Einstein field \( \phi \), and a Fermi-Dirac field \( \psi \). The Green's functions can be defined as vacuum state expectation values of time-ordered field operator products. There are two types,

\[
G_+ (x_1 \cdots x_p) = \langle \left( \phi (x_1) \cdots \phi (x_p) \right) \rangle \quad \epsilon_+ (x_1 \cdots x_p)
\]

and

\[
G_- (x_1 \cdots x_p) = \langle \left( \phi (x_p) \cdots \phi (x_1) \right) \rangle \quad \epsilon_- (x_p \cdots x_1),
\]

where positive or negative time-ordering implies an assignment of multiplication order in accordance with the ascending sense of time, as read from right to left (+), or from left to right (−). The quantities \( \epsilon_\pm \) are antisymmetrical functions of the time coordinates for the F.D. fields, which assume the value +1 when the time-ordered sense coincides with the written order. The connection between the two Green's functions is simply

\[
G_- (x_1 \cdots x_p) = G_+ (x_1 \cdots x_p)^*.
\]

The definitions as given are actually restricted to those
Fundamental theoretical ideas

fields for which all components are kinematically independent at a given time. In more general situations additional terms are necessary, the function of which is to maintain the non-dependence of the Green’s functions on the particular time-like direction employed in the time-ordering, which is otherwise assured by the commutativity or anticommutativity of fields at points in space-like relation.

The invariance of the formalism under inhomogeneous Lorentz transformations requires that the Green’s functions be translationally invariant functions of the space-time co-ordinates, while the homogeneous, proper, orthochronous Lorentz transformations

\[ \tilde{\chi}(lx) = L \chi(x) \]

imply the Green’s function invariance property

\[ \sum_{a=1}^{\rho} \left( \Pi \left( R_{a} \right)_{a} \right) G_{a}(l^{-1} x) = G(x) . \]

The form of the latter for infinitesimal transformations is

\[ \sum_{a=1}^{\rho} \left( \frac{x_{\mu}}{i} \partial_{\tau} - \frac{x_{\tau}}{i} \partial_{\mu} + S_{\mu a} \right) G(x) = 0 . \]

The theory is also invariant under the proper, antiorthochronous transformation \( x^{\mu} \rightarrow -x^{\mu} \), provided a transition to the complex conjugate algebra is included. Now

\[ \left( \prod_{a=1}^{\rho} \left( R_{a} \right)_{a} \right) G_{a}(-x_{1} \cdots -x_{\rho}) = \epsilon_{+}(-x_{1} \cdots -x_{\rho}) = G_{-}(x_{1} \cdots x_{\rho}) , \]

since positive and negative time-orderings are interchanged under time reflection, and, with \( n \) pairs of F.D. fields,

\[ \epsilon_{+}(-x_{1} \cdots -x_{\rho}) = (-1)^{n} \epsilon_{-}(x_{1} \cdots x_{\rho}) , \]

while the sign factor \((-1)^{n}\) is compensated by the imaginary unit contained in each matrix \( R_{a} \) that is associated with a half-integral spin (F.D.) field. Thus, for either type of Green’s function

\[ \prod_{a=1}^{\rho} \left( R_{a} \right)_{a} G(-x) = G(x) , \]

\[ R_{a} = e^{iS_{a12}} e^{iS_{a4}} , \]

which, in its union of two disjoint pieces of the Lorentz group, is a sign of the Euclidean foundation on which the Green’s functions rest.

The explicit dependence of the fields on the space-time co-ordinates is governed by the energy-momentum vector \( P^{\mu} \), according to

\[ \chi(x) = e^{-iP_{x}} \chi e^{iP_{x}} , \]

while the invariant meaning of the vacuum state is expressed by

\[ \langle 0 | e^{-iP_{x}} = 0 | , \quad e^{iP_{x}} | 0 \rangle = 0 . \]

Hence, if \( x^{(1)} \cdots x^{(p)} \) represent the time-ordered arrangement of \( x_{1} \cdots x_{p} \) \( (t_{(i)} > \cdots > t_{(p)}) \), we have

\[ G_{+}(x) = \langle \chi e^{iP_{x}}(x_{(1)} - x_{(2)}) \chi \cdots \epsilon_{+}(x_{1} \cdots x_{p}) \rangle \]

and

\[ G_{-}(x) = \langle \chi e^{iP_{x}}(x_{(p-1)} - x_{(p)}) \chi \cdots \epsilon_{-}(x_{p} \cdots x_{1}) \rangle , \]

wherein additional indices are needed to distinguish the various type of fields. The time dependence of the Green’s function \( G_{+} \) is thus governed by the operators

\[ e^{-iP_{\epsilon}(a_{a} - l(a+1))} , \]

which, in their dependence upon the differences of the consecutively ordered time co-ordinates, contain no negative frequencies \((P_{\epsilon} \geq 0)\). The alternative Green’s function \( G_{-} \), analogously constructed from the operators

\[ e^{iP_{\epsilon}(a_{a} - l(a+1))} , \]

contains no positive frequencies.

We shall now use these spectral characteristics of the Green’s functions to give a more precise meaning to the assumed existence of the Green’s functions, which is in the sense of the summability of Fourier integrals. It is described by the absolute convergence (for distinct \( x_{1} \cdots x_{\rho} \)) of the spectral representation obtained on replacing the positive frequency unitary operators in \( G_{+} \) with

\[ e^{-iP_{\epsilon}(a_{a} - l(a+1))} (1 - i\alpha) \]

and similarly inserting

\[ e^{iP_{\epsilon}(a_{a} - l(a+1))} (1 + i\alpha) \]

in \( G_{-} \), where the limit \( \epsilon \rightarrow +0 \) is to be eventually performed. This modified time dependence is also expressed by the substitutions

\[ t_{a} \rightarrow t_{a} (1 - i\alpha) \text{ in } G_{+} , \quad \text{and} \quad t_{a} \rightarrow t_{a} (1 + i\alpha) \text{ in } G_{-} . \]

The existence of the Green’s functions in this sense is equivalently described by the assumption that the various field operator matrix elements, multiplied by the densities of relevant states, possess no more than an algebraic growth with increasing energy.

The absolute convergence of the spectral representations for the Green’s functions \( G_{+} \) and \( G_{-} \) is now assured for the more general time substitution \((*)\) ;

\[ (*) \text{ The full analytic extension of the Green’s functions } G_{\pm} \text{ is produced by } t \rightarrow t + i\eta \text{ where } \eta \text{ retains the initial time order, which is to say that the otherwise arbitrary mapping function } \eta(t) \text{ is of positive slope.} \]
in which \( \theta \) lies in the open interval \( 0 < \theta < \pi \). The new variables \( \tau_\theta \) are real numbers that retain the ordering of the original time variables. We adopt a special notation to accompany the particular choice \( \theta = \frac{1}{2} \pi \), which asserts the existence of the functions \( G_+ (t \to -i x_\tau) \) and \( G_- (t \to +i x_\tau) \). In this way there emerges a correspondence between the Green’s functions in space-time and functions defined on a four-dimensional Euclidean manifold. To the extent that the two Euclidean functions thus obtained are related, there also appears an analytical continuation that connects the two distinct types of space-time Green’s functions, \( G_{\pm} \). Conversely, given one of the Euclidean functions, the substitutions \( x_4 \to e^{i(\pi/2 - \tau)} t \) and \( x_4 \to e^{i(\pi/2 + \tau)} t \) will yield functions having the space-time character of \( G_+ \) and \( G_- \), respectively, in the limit as \( \tau \to +0 \). We must now see how to supply an independent basis for the Euclidean Green’s functions, from which has disappeared all reference to the space and time distinctions of the Lorentz metric.

The significance of the latter remark can be appreciated through the form assumed in the Euclidean description by the statement of infinitesimal rotational invariance of the Green’s functions. The Hermitian spin matrices \( S_{\mu \nu} \), \( \mu, \nu = 1, \ldots, 4 \) \((x_4 = \pm i x^0)\), comprising \( S_{kl} \) and \( S_{kl} = \pm i S_{kl} \), still bear the mark of their Lorentz origin in the symmetry of these matrices; the \( S_{kl} \) are antisymmetrical, while the \( S_{kl} \) are symmetrical. Hence, one must perform a unitary transformation to unite them into six antisymmetrical, imaginary matrices that describe independent infinitesimal orthogonal transformations. The means for distinguishing between the two types of matrices is provided by the space-reflection matrix \( R_\theta \),

\[
R_\theta^{-1} S_{kl} R_\theta = S_{kl}, \quad R_\theta^{-1} S_{kl} R_\theta = -S_{kl},
\]

or, alternatively, by the time-reflection matrix

\[
R_\tau = R_{\tau \tau} R_\theta.
\]

Indeed, for integral spin the necessary transformation is produced by

\[
S_{\mu \nu}^{(E)} = e^{\pm i\frac{\pi}{4} R_\tau} S_{\mu \nu} e^{\pm i\frac{\pi}{4} R_\tau},
\]

where the plus or minus sign applies to the matrices associated with \( G_+ \) or \( G_- \):

\[
S_{kl}^{(E)} = e^{\pm i\frac{\pi}{4} R_\tau} \left( \pm i \right) S_{kl} e^{\pm i\frac{\pi}{4} R_\tau} = R_{\tau} S_{kl},
\]

Since the Hermitian matrices \( R_\tau \) and \( R_\theta \) are symmetrical, \( R_\tau \) is a real, symmetrical matrix and \( R_\theta \) is antisymmetrical. Thus, to permit the complete transformation from the Lorentz to the Euclidean metric, every half-integer spin (F.D.) field must carry a charge. Just such a general fermionic charge property, under the name of nucleonic charge or leptonic charge, is either well established experimentally, or has been conjectured on other grounds. The Euclidean formulation may be the proper basis for comprehending this general attribute of F.D. fields. If \( \phi \) is the imaginary antisymmetrical matrix representing the fermionic charge property, the required transformation for half-integer spin fields is

\[
S_{\mu \nu}^{(E)} = e^{\pm i\frac{\pi}{4} R_\tau} S_{\mu \nu} e^{\pm i\frac{\pi}{4} R_\tau},
\]

and, indeed,

\[
S_{kl}^{(E)} = \pm i R_\tau S_{kl},
\]

has the desired property of antisymmetry.

We should also note the removal of reference to the Lorentz metric from the orthogonal matrices \( R_\theta, R_\tau \), that are associated with the reflections of the individual space-time co-ordinate axes. For a B.E. field all these matrices are commutative, real, symmetrical matrices, and they are unchanged by the transformation to the Euclidean metric. On considering a F.D. field, however, we find that \( R_\tau \) occupies a distinguished position, differing from the anti-commuting, real, symmetrical matrices \( R_\theta \) by being imaginary and antisymmetrical. While the matrices \( R_\theta \) are unaltered by the metric change, the Euclidean matrix associated with the reflection of \( x_4 \) is now

\[
R_{kl}^{(E)} = e^{\pm i\frac{\pi}{4} R_\theta} \left( \pm i \right) R_{kl} e^{\pm i\frac{\pi}{4} R_\theta} = R_{\theta 4} R_{kl},
\]

which is also a real symmetrical matrix. Thus all the individual Euclidean co-ordinate reflection matrices are real, symmetrical and orthogonal, with the two classes of fields distinguished by commutativity properties, according to

\[
E : \quad R_{kl} R_{\mu} = e^{+i} S_{\mu \nu} R_{kl} R_{\mu}.
\]

To obtain an independent characterization of the Euclidean-type Green’s functions we can convert to the Euclidean metric the system of differential equations obeyed by the Green’s functions. We shall only attempt to outline this process in the following considerations. Let the Lagrange function be written as

\[
\mathcal{L} = \frac{1}{4} \left[ A^\mu \partial_\mu Z - \partial_\mu Z A^\mu \chi \right] + \frac{1}{2} B Z \chi - \delta_{\chi} (\chi),
\]

in which \( \delta_\chi \) refers to the interactions between fields. The field equations are

\[
A^\mu \partial_\mu Z + B Z = \frac{\delta_{\chi} \delta_\chi}{\partial \chi}.
\]
while the commutation properties on a space-like surface, as expressed in a local co-ordinate system, are given by

\[ [A^\mu, \chi(x), \chi(x')] \pm = i \delta^\mu (x - x'), \]

where \( \delta^\mu (x - x') \) is defined by

\[ \int d^4 \sigma \, \delta^\mu (x - x') f(x') = f(x). \]

Now, unifying the field equations and commutation properties, the differential equations for the Green's functions are obtained:

\[ (A^\mu \partial_\mu + B) G_(, x_1 \cdots x_p) + \cdots = i \delta (x_1 - x_2) G_(, x_2 \cdots x_p) \pm i \delta (x_1 - x_2) G_(, x_2 \cdots x_p) + \cdots, \]

in which the omitted terms on the left are the particular Green's function combinations needed to represent the interaction effects in the field equations, while, on the right, the summation is extended over all points that refer to the same field as does \( x_p \). The summation is symmetrical or antisymmetrical in these points, according to the statistics of the field. Thus the Green's functions obey an infinite system of equations that are linear and inhomogeneous (since the function with \( p = 0 \) is simply unity), and which incorporate fully all information concerning the interacting fields.

The analogous differential equations for the \( G_- \) can be obtained directly, or by complex conjugation of the equations obeyed by \( G_+ \) and differ from the latter only in the sign of \( i \) exhibited by the right-hand B.E. terms. It is worthy of note that, apart from the trivial situation of uncoupled fields, the two sets of differential equations are intrinsically different — the two types of Lorentz-type Green's functions cannot be characterized in detail as solutions of a common equation that are distinguished by boundary conditions. There is, however, a simple relation between the Green's functions that can be inferred from the differential equations (and from the time ordered operator definitions), namely

\[ G_-(x) = (-1)^n G_+ (x - e^{-ni} x). \]

The correspondence between Lorentz- and Euclidean-type Green's functions is now exhibited as

\[ G_\pm (x) \leftrightarrow \Pi_{B,E.} \left( e^{\pm \frac{n_1}{4} R t} e^{\mp \frac{n_1}{4} t} \right) \frac{e^{\mp \frac{n_1}{4} x}}{\pm \frac{n_1}{4} x}, \]

in which \( x^0 \) and \( x_4 \) are understood as the variables in the appropriate functions. Accompanying the imaginary relations between the variables is the transformation

\[ \delta (x) \leftrightarrow (\pm i) \delta (x^{(E)}). \]

On performing these substitutions in the differential equations we encounter the real B.E. matrices

\[ A_\mu^{(E)} = - e^{\pm \frac{n_1}{4} R t} e^{\pm \frac{n_1}{4} A_\mu} e^{\mp \frac{n_1}{4} R t} e^{\mp \frac{n_1}{4} x} = (-A_0 R_t, A_0) \]

B.E.:

\[ B^{(E)} = - e^{\pm \frac{n_1}{4} R t} e^{\pm \frac{n_1}{4} B} e^{\mp \frac{n_1}{4} R_t} e^{\mp \frac{n_1}{4} x} = - B R_t, \]

which are, respectively, antisymmetrical and symmetrical, and the real F.D. matrices

\[ A_\mu^{(F)} = \mp e^{\mp \frac{n_1}{4} R_t} e^{\mp \frac{n_1}{4} A_\mu} e^{\pm \frac{n_1}{4} R_t} e^{\pm \frac{n_1}{4} x} = (iA_\mu - A_0 R_t) \]

F.D.:

\[ B^{(F)} = e^{\pm \frac{n_1}{4} R_t} e^{\pm \frac{n_1}{4} B} e^{\mp \frac{n_1}{4} R_t} e^{\mp \frac{n_1}{4} x} = - B R_t i \]

which are symmetrical and antisymmetrical, respectively. The resulting form of the Green's function differential equations, as adapted to the Euclidean metric, is

\[ [A_\mu \partial_\mu + (1, \pm l)] B_{\frac{1}{2}} \left( x_1 \cdots x_p \right)^{(E)} + \cdots = \delta (x_1 - x_2) G_{\pm} (x_3 \cdots x_p)^{(E)} + \cdots, \]

where the choice indicated for the coefficient of \( B \) signifies:

unity, for a B.E. field; \( \pm l \), for a F.D. field. If, as we have discussed before \( 3 \), the Lorentz \( B \) matrices are constructed as the product of \( R_t \) with an invariant, symmetrical matrix that is independent of internal degrees of freedom, the Euclidean \( B \) matrices will be completely of the latter type. This remark, together with the observation that the transformation applied to \( B^{-1} A_\mu \) is unitary, apart from the F.D. factor \( \pm l \), indicates how Lorentz invariance is translated into Euclidean invariance.

The Green's functions of the Lorentz description are intrinsically complex quantities and, accordingly, there are two linearly independent sets of such functions. It is an indication of the simplification obtained through the introduction of the Euclidean metric that completely real Euclidean Green's functions can be defined — provided a certain general symmetry restriction is enforced on the field interactions. Conversely, the latter invariance property acquires substantial support through its role in
unifying the two classes of Green’s functions and eliminating complex numbers from a formulation of the fundamental laws of physics. Let us notice that, aside from the interaction terms, the differential equations for \( G_\pm^{(E)} \) differ only in the F.D. quantity \( \pm t \), referring to the fermionic charge. This sign factor can be removed by introducing the operation of fermionic charge reflection. But, when full account is taken of the variety of field interactions \( \Pi \) it appears that all types of charge are dynamically coupled, and the interconversion of the two sets of equations is possible only if the interaction terms differ merely through the effect of general charge reflection. Assuming this property, we conclude that

\[
G_-^{(E)} = \left[ \prod_{\alpha=1}^{P} (R_Q)\alpha \right] G_+^{(E)} = R_Q G_+^{(E)},
\]

where the individual charge reflection matrices \( R_Q \) are real and orthogonal. The composite matrix \( R_Q \) also describes the reality properties of the Euclidean-type Green’s functions,

\[
G_{\pm}^{(E)*} = R_Q G_{\pm}^{(E)} ,
\]

for the mutually complex conjugate relation of \( G_\pm \) still applies to the derived functions \( G_{\pm}^{(E)} \). In effect, all matrices appearing in the Euclidean formulation of the differential equations are real, with the exception of the imaginary charge matrices, and complex conjugation is equivalent to charge reflection. If we accept the interpretation \( 2^1 \) of the imaginary unit as symbolic of the charge nature of the measurement apparatus (matter – antimatter), the symmetry property we have postulated can be described as the relativistic invariance of the Euclidean formulation with respect to charge reflection, for the application of this transformation to the system under investigation and to the apparatus employed for the purpose produces no discernible change.

Before continuing, we must examine the relation between the matrices \( R_Q^{(E)} \), and the charge reflection matrices of the Lorentz description, \( R_Q \). The latter, having no reference to space-time properties, are uniformly chosen as real, orthogonal, symmetrical matrices \( (R_Q^2 = 1) \). The distinction between \( R_Q \) and \( R_Q^{(E)} \), which exists only for F.D. fields, arises from the incorporation of the fermionic charge \( t \) into \( A_\pm^{(E)} = -A_\pm R_t I \). To compensate the sign change of \( A_\pm^{(E)} \) induced by the reflection of \( t \), the F.D. matrix \( R_Q^{(E)} \) must contain the co-ordinate reflection matrix \( R_Q t = R_Q t \). Thus,

\[
R_Q^{(E)} = R_Q R_t I ,
\]

which is a real, orthogonal, antisymmetrical matrix \( (R_Q^{(E)*} = -1) \). However, the composite matrix \( R_Q^{(E)} \), which is constructed from an even number of F.D. contributions, is a real symmetrical matrix obeying

\[
R_Q^{(E)*} = 1.
\]

The hypothesis of Euclidean relativistic charge reflection invariance can now be interpreted as a property of the Lorentz-type Green’s functions and, thereby, of the Lagrange function of the interacting fields. The relation implied between the Green’s functions \( G_+ \) and \( G_- \) is

\[
G_-(-ix_i) = \Pi_{\Pi^{(E)}(R_Q R_t)} \Pi_{F.D.} (R_t i R_t) G_+(-i x_i) = (-1)^n R_t G_+(-i x_i) ,
\]

which makes explicit the analytic continuation that connects the two Lorentz Green’s functions

\[
G_-(t) = (-1)^n R_t G_+ e^{-\pi t} ,
\]

\[
G_+(t) = (-1)^n R_t G_- e^{\pi t} .
\]

When this result is compared with the previously obtained connection,

\[
G_\mp(t) = (-1)^n G_\pm(-e^{\pm \pi t}) ,
\]

we learn that

\[
R_Q R_t G(-t) = G(t) ;
\]

the Lorentz-type Green’s functions are invariant under charge and time reflection. The same assertion can be made of the combination of charge and space reflection, since space-time reflection is an invariance operation. But in the latter form we are dealing with unitary transformations of Hermitian field operators, and it can be concluded that the invariance of the Lagrange function under charge and space reflection is equivalent to the postulate that Euclidean-type Green’s functions exhibit a relativistic invariance with respect to charge reflection. It is surely significant that we are thus led to a general invariance property which is consistent with all the recent experiments on the so-called parity non-conserving interactions. The existence of an exact invariance transformation involving space reflection is now supplied with a basis that may be considered more substantial than the mere belief in the intrinsic indiscernibility of left and right.

We have not yet exhibited the real Euclidean-type Green’s functions, the existence of which is assured by the presence of a linear transformation equivalent to complex conjugation,

\[
(G_\pm^{(E)})^* = R_Q G_{\mp}^{(E)} .
\]

Indeed, functions having the required reality property are given by

\[
G_\pm^{(E)} = e^{\frac{\pi t}{4} R_q} e^{-\frac{\pi t}{4}} G_{\pm}^{(E)} = e^{-\frac{\pi t}{4} R_q} e^{\frac{\pi t}{4}} G_{\mp}^{(E)} .
\]

A second such choice is

\[
R_Q G_\pm^{(E)} = e^{-\frac{\pi t}{4} R_q} e^{\frac{\pi t}{4}} G_\mp^{(E)} = e^{-\frac{\pi t}{4} R_q} e^{-\frac{\pi t}{4}} G_{\pm}^{(E)} ,
\]

although the latter are not a linearly independent set and can be regarded as presenting \( G^{(E)} \) in a new representation.
An essential limitation of the description by real Green's
functions must be observed, however. The matrices
\[ e^{\pm R_Q} \] are not composite and the transformation that
introduces \( G^{(E)} \) has no simple significance for the differential
equations that characterize the Green's functions. Were
only B.E. fields involved, the composite transformation
formed from the individual \( R_Q \) could be employed, but
this is not possible for F.D. fields. Nevertheless, it remains
true that real Euclidean-type Green's functions exist
from which the physically meaningful Green's functions
\( G_+ \) and \( G_- \) can be inferred.

Finally, we shall indicate briefly the possibility of re­
placing the differential equations, as the characterization
of the Euclidean-type Green's functions, by an explicit if
formal construction. For this purpose we define fields
\( \chi (x) \), on the Euclidean manifold, that are completely
commutative or anticommutative, as befits the statistics,
\[ [ \chi (x), \chi (x') ]_{\pm} = 0 \],
and a complementary set of fields \( \tilde{\chi} (x) \), with the same
characteristics, which are such that
\[ [ \tilde{\chi} (x), \chi (x') ]_{\pm} = \delta (x - x') . \]
The B.E. fields \( \Phi, i\Phi \) are Hermitian, while the F.D.
fields \( \psi \) and \( \bar{\psi} \) are mutually Hermitian conjugate. The
Euclidean-type Green's functions are then given by
\[ G_{\pm} (x_1 \cdot \cdot \cdot x_p)^{(E)} = \langle W_{\pm} | \chi (x_1) \cdot \cdot \cdot \chi (x_p) | 0 \rangle / \langle W_{\pm} | 0 \rangle , \]
(ordinary operator multiplication!) where \( | 0 \rangle \) is the
right eigenvector of the operators \( \tilde{\chi} (x) \) associated with
null eigenvalues
\[ \tilde{\chi} (x) | 0 \rangle = 0 \,, \]
and the vector \( \langle W_{\pm} | \) is characterized by
\[ \langle W_{\pm} | [ A_{\mu} \partial_{\mu} + (1, \pm \ell) B ] \chi (x_1) \cdot \cdot \cdot \chi (x_p) + \cdots ] = 0 \,, \]
in which the omitted terms are the functions of the Euclid­
ean field operators \( \chi (x) \) needed to describe the field
interactions. This assertion is verified on observing that
\[
\langle W_{\pm} | \chi (x) \cdot \cdot \cdot \chi (x_p) | 0 \rangle / \langle W_{\pm} | 0 \rangle
\]
where commutation relations and the significance of
\( | 0 \rangle \) as a \( \tilde{\chi} \) eigenvector are used to obtain the stated result.

The vector \( \langle W_{\pm} | \) can be constructed from the left
eigenvector of the \( \tilde{\chi} \) associated with null eigenvalues,
\[ \langle W_{\pm} | = \langle 0 | e^{-W_{\pm}} \]
and, in turn, these Green's functions can be derived from
a single generating function, the expansion of which
produces the field operator products. In the latter form,
we make contact with previous developments employing
the action principle for quantized fields and the device
of external sources \(^1\) and subsequent work, largely
unpublished). A large variety of equivalent forms can
now be devised for the Green's functions, based primarily
upon the well-established transformation and representa­
tion theory \(^{(*)} \) for canonical variables of the first and
second kind. A discussion of these developments for
specific systems will be deferred to another publication, in
which the problem of translating quantum electrodynamics
into the Euclidean metric is examined.

Although we have emphasized the fundamental implica­
tions of the Euclidean representation, it will be evident that
the Euclidean-type Green’s functions also have practical
advantages. Indeed, the utility of introducing a Euclidean
metric has frequently been noticed in connection with
various specific problems, but an appreciation of the
complete generality of the procedure has been lacking.

LIST OF REFERENCES


\(^{(*)}\) An extended discussion is contained in \(^1\).
Chairman: I thank you very much for this inspiring report. To open the discussion I wish to say that for the audience it is perhaps a bit more interesting than for the speaker that the idea of analytical continuation has been anticipated by Wightman. Instead of more general transformations the speaker has selected a particular case of rotations of 90°, and I hope I interpret him correctly that he means that this has a special significance for physics and for the formalism in that particular case.

Yamaguchi: I just want to add a brief comment on this paper. Nakano of Osaka City University tried the same proposal and he is busy working on this proposal. That is all.

Stueckelberg: I would like to tell Schwinger that if you take a real Hilbert space, and \( i \) as an antisymmetric operator, unitary meaning now orthogonal, in which all observables are symmetric observables, you must take \( i \) commuting with all observables, \( i^2 = -1 \), meaning just that \( i \) has an inverse, and you get the \( C T P \) invariance stated in a real linear way. Pauli tells me that this is a triviality and probably it is. Now if you take an indefinite metric, does the spin statistics relation, usually a consequence of the positive definite energy — that is, of causality — now follow exclusively from the \( C T P \) invariance in this formalism?

Pauli: (to Schwinger) Perhaps you can see in general how the spin statistics connection comes out in this Euclidean interpretation. This would be interesting.

Schwinger: First, the \( T C P \) theorem is built into the theory since the fundamental way in which one understands it is through the underlying Euclidean formulation of the theory. That is, in the ordinary description one produces the time reversal essentially by going outside the Lorentz group through the Euclidean group or the complex Lorentz group, which is even more general, as Jost has done, in order to produce the \( T C P \) transformation. The \( T C P \) theorem is one of the indications for the underlying Euclidean structure of the present-day theory.

When one has presented the theory in a Euclidean form it is automatically contained, as there is no longer any distinction between the past and the future, and the proper transformation, which includes time reflection, is a consequence of the ordinary rotations of the theory. This, of course, depends upon the spin statistics connection and the whole development has already made use of it. The question of to what extent you can go backwards, remains unanswered, i.e. if one begins with an arbitrary Euclidean theory and one asks: when do you get a sensible Lorentz theory? This I do not know. The development has been in one direction only; the possibility of future progress comes from the examination of the reverse direction, and that is completely open.
I feel quite ashamed not to be speaking about any indefinite metric. I am going to confine my remarks to the old familiar relativistic local field theory and discuss the problem of creating a framework for the description of unstable elementary particles. I need not apologize for this. Of the sixteen known elementary particles, only four — the proton, the electron, the neutrino and the photon — are stable. The problem which exercised us was the problem of the construction of a state vector in a relativistic field theory corresponding to an unstable elementary particle.

Let me briefly recapitulate what one normally does in order to describe stable particles. Particles are characterized first of all by eigenstates of the energy momentum vector

$$ P_{\mu} | p, n > = p_{\mu} | p, n >. $$

(1)

Here $p_{\mu}$ is the mass of the particle and this is necessarily real. The further characterization of the state vectors is provided in general by specifying whether we are dealing with in-states or out-states. This needs an asymptotic description of these particles at plus or minus infinity of time in the conventional fashion. I shall denote these state vectors in this form:

$$ | ... k_1 ... k_N >_{in} \text{ or } | ... k_1 ... k_N >_{out}. $$

One then postulates that this set of states, both the in-states as well as the out-states, satisfy the completeness relation which I shall write in the following form:

$$ \Sigma | k_1 ... k_N >_{in} < k_1 ... k_N |_{out} = 1. $$

(2)

It is clear that for an unstable particle there is no asymptotic limit in any sense, and it is impossible to use this conventional procedure to construct or define any state whatever for this particle.

How does conventional theory cope with this? Consider a $\Sigma$-particle, which can decay into two stable $\chi$-particles $k_1$ and $k_2$:

$$ k_1^2 = k_2^2 = m^2. $$

One assumes the existence of a $\Sigma$-particle state $| M^0 >$ and calculates the transition rate

$$ \lambda = | < M^0 | k_1 k_2 > |^2 $$

as if all particles were stable. Then the attenuation of a beam of such particles is given by

$$ dn/d\tau = - \lambda n(\tau) $$

where $\tau$ is the proper time.

This may be interpreted as a wave function

$$ < \tau | \Phi > = e^{i M^0 \tau - \lambda | \tau | / 2}. $$

The particle then appears with a complex mass $M^0 + i \lambda/2$, which is certainly not consistent with (1), any more than the states $| M^0 >$ or $| \Phi >$ belong in the completeness relation.

We wish to propose a restatement of this phenomenological description of long-lived elementary particles, which is consistent with the general scheme described in (1) and (2).

The Hilbert space of field theory is spanned completely by the stable particle states $| k_1 k_2 ... >$. To describe unstable particles we define a density matrix

$$ \rho = \sum | k_1 k_2 > < k_1 k_2 | \rho (m^2) \frac{q(m^2)}{m^2} < k_1 k_2 |, $$

$$ m^2 = (k_1 + k_2)^2. $$

$\rho (m^2)$ is essentially the probability of finding the $\Sigma$-particle with mass — or $Q$-value — corresponding to $m$. 

*) Reported by A. Salam
The mean mass is then given by
$$M^0 = \frac{\text{Tr} \, \mathcal{P} \, \frac{\Phi}{\Phi} \, \partial^2 \mathcal{P} \, \Phi}{\text{Tr} \, \mathcal{P} \, \frac{\Phi}{\Phi} \, \partial^2 \mathcal{P} \, \Phi} = \int \frac{m^2 \, q \, (m^2) \, dm^2}{f \, q \, (m^2) \, dm^2}.$$  

The decay constant is given by the spread in mass values, as determined by the second moment
$$\langle M^0 \rangle^2 = \frac{\text{Tr} \, \left( \partial^2 - \mathcal{P} \, \frac{\Phi}{\Phi} \, \partial^2 \mathcal{P} \, \Phi \right)^2}{\text{Tr} \, \mathcal{P} \, \frac{\Phi}{\Phi} \, \partial^2 \mathcal{P} \, \Phi}.$$  

In order to relate these definitions to field theory we assume the existence of a field $\Phi (x)$, satisfying the causality condition
$$[\Phi (x), \Phi (y)] = 0 \quad \text{for} \quad (x - y)^2 < 0.$$  

We then have
$$\langle (k_1 + k_2)^2 \rangle = \langle 0 \mid \Phi (0) \mid k_1 \, k_2 \rangle^2.$$  

If the $\Phi$-field has to satisfy a field equation of the form
$$(\partial^2 - a) \Phi = F \mathcal{P} \Phi,$$  

then a general theorem due to Källén, Gell-Mann and Low and Lehman, states that
$$\alpha = \frac{\int m^2 \, q \, (m^2) \, dm^2}{\int q \, (m^2) \, dm^2}.$$  

Thus:
$$\alpha = M^0 \frac{q}{\mathcal{P}}$$  

as defined above.

For $F \mathcal{P} \Phi$ we may assume:
$$F \mathcal{P} \Phi = g \, x^2.$$  

If $q$ is computed by summing perturbation theory we obtain
$$q \left( m^2 \right) = \frac{\lambda (m^2)}{\left( m^2 - M^0 \right)^2 + \lambda^2 (m^2)}.$$  

where $\lambda$ is the $\Sigma-2\chi$ vertex part and $\lambda (m^2)$ gives the conventionally computed life-time to all orders. Clearly, the second moment (suitably defined) is $\lambda (M^0)$, giving us in this approximation, the conventional result.

I must mention that Peierls has conjectured that the one particle propagator
$$G \left( k^2 \right) = \int \frac{q \left( M^2 \right)}{k^2 - M^2 \, dM^2}$$  

has a pole on the "unphysical sheet" which gives the lifetime and mean mass of an unstable particle.

**DISCUSSION**

**Wataghin:** I want to remark that, in a recent paper on non-local theories, I discussed the virtual states in a manner similar to that of Salam. It will be published in Nuovo Cimento $^3$. I introduced decaying virtual states of bare particles in a non-local field theory, where an explicit reference to the proper time and the c.m. system of interacting particles is made.

**Low:** It seems to me that in your approach the probability of decaying with some energy is, in fact, given by the density function. According to this, there should therefore be a finite probability, say, for a $\lambda^0$ decaying into a proton and two pions.

**Gell-Mann:** Let us pursue this point a little further. You say, of course, that you want to consider only those states into which the particle can actually decay. Now let us consider, as Low did, the case of the $\lambda^0$ decaying into a proton and a pion. Do we consider all energies of the final state? You did not put in any cut-off on the mass.

**Salam:** Let us consider this case (Fig. 1) where there are two humps. Now this is a standard case of two resonances in nuclear theory and one has the standard problem of resolving cleanly the two resonances. I have no solution to this question. There is the very simple case (Fig. 2) where you first have a sharp hump, then a very little tail and then a large second hump. But supposing one is dealing with cases where all the humps have the same sort of widths, then you may well ask to which particle does the region between the peaks belong. I do not know. I am pointing out a matter of principle, that from this point of view there is no distinction between the peaks, and they have all to be treated on the same footing.

**Gell-Mann:** Then I cannot agree that this is a very good definition. Let us consider the $\lambda^0$-particle once more. Let me call attention to the fact that if you turn off the
The mean mass is then given by
\[ M^2 = \frac{\text{Tr} P^2 \Phi \Phi^*}{\text{Tr} \Phi \Phi^*} = \int \frac{m^2}{q^2} \frac{d\mu^2}{dm^2}. \]

The decay constant is given by the spread in mass values, as determined by the second moment
\[ (M^2)_2 = \frac{\text{Tr} \left( (P^2 \Phi)^2 - (M^2)_2 \right) \Phi}{\text{Tr} \Phi}. \]

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If the $\Phi$-field has to satisfy a field equation of the form
\[ (\Box - a) \Phi = F(x), \]
then a general theorem due to Källén, Gell-Mann and Low and Lehman, states that
\[ \alpha = \frac{\int m^2 q (m^2) d\mu^2}{\int q (m^2) d\mu^2}. \]

Thus:
\[ \alpha = M^2 \]

as defined above.

For $F(x)$ we may assume:
\[ F(x) = g x^2. \]

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\[ \varphi (m^2) = \frac{\lambda (m^2)}{(m^2 - M^2)^2 + \lambda^2 (m^2)}. \]

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**Salam:** Let us consider this case (Fig. 1) where there are two humps. Now this is a standard case of two resonances in nuclear theory and one has the standard problem of resolving cleanly the two resonances. I have no solution to this question. There is the very simple case (Fig. 2) where you first have a sharp hump, then a very little tail and then a large second hump. But supposing one is dealing with cases where all the humps have the same sort of widths, then you may well ask to which particle does the region between the peaks belong. I do not know. I am pointing out a matter of principle, that from this point of view there is no distinction between the peaks, and they have all to be treated on the same footing.

**Gell-Mann:** Then I cannot agree that this is a very good definition. Let us consider the $A^2$-particle once more. Let me call attention to the fact that if you turn off the

![Fig. 1](image-url)
weak interaction it would be a stable particle. Therefore, there would be a gigantic hump which you might have to draw up as high as the moon on your scale, corresponding to the strong interaction, corresponding to the decay of the \( A^0 \) into a proton and a \( K^- \). That you certainly do not want to include.

**Salam:** The probability for a \( A^0 \) going into a proton and two \( \pi \) mesons will be equal to

\[
\left[ \frac{G_{\text{weak}}}{G_{\text{strong}}} \right]^2.
\]

This is about \( 10^{-12} \). One in \( 10^{12} \) \( A^0 \)'s will decay into a proton and two \( \pi \) mesons, provided the experimentalist is willing to call such an object a \( A^- \)-particle.

**Gell-Mann:** Let me continue what I was saying. You will have an enormous hump corresponding to the \( A^0 \) going into a \( K \) and a proton. Of course, this is for energies far above that of the \( A^0 \). This, of course, you do not want to include in your sum.

**Salam:** No.

**Gell-Mann:** Have you defined clearly which set of states you are going to sum over? Presumably you will include only those states which violate strangeness conservation.

**Salam:** That, for example, would be a good definition. Nobody has discovered how to isolate the resonances in nuclear physics, except when you have selection rules of the type given by strangeness violation.

**Gell-Mann:** But this is the problem that you have attacked.

**Salam:** No, no. I do not claim to have attacked the problem of separating resonances when they refer to particles with the same quantum numbers.

**Matthews:** If you want that definition to apply to the \( A^0 \) you sum only over the proton + pion state.

**Gell-Mann:** What is the difference between proton plus pion and proton plus two pions?

**Matthews:** The final states consist of stable particles for which you can use the conventional Lehman - Symanzik - Zimmerman procedure to define the states.

**Gell-Mann:** I was not aware that considering the interactions you could really distinguish between proton plus pion and proton plus two pions, since a pion incident on the proton can produce two pions.

**Peierls:** The point is this. Are we talking about the unstable \( A^0 \)-particle as the experimentalist would define it, or are we talking about a physical state? Now, the \( A^0 \)-particle has not existed for ever. It must have been created in some way. It seems to me that those curves on the board relate to what happens when you produce a particle in any of the various ways that this can be done. Then of course, you get various humps. In essence the process leading to a stable particle or an unstable particle or even to a very broad resonance look very similar. The question is: can we do something else and describe the unstable particle by itself, by a wave function? This is what Salam has called \( \Phi \). He says very little about how he defines it and it is clear that it cannot be defined exactly; because it is only to the extent to which the width of such a peak is negligible compared to the distance to the neighbouring peak that you can give a meaning to such a thing.

**Gell-Mann:** I think there is still a hope that there might be an actual definition. That is what I expected when this report was started — a definition independent of arbitrary things. The point of view which Salam attributed to you, Peierls, sounds more likely to give a unique definition. To use a slightly different language: this \( \rho \)-function is the imaginary part of what would usually be called an amplitude. If this \( \rho \)-function by itself is in some way analytically continued so that the behaviour of a function reducing to \( \phi \) on the real axis can be investigated in the complex plane, I think it would be found that poles will appear off the axis and that those poles will be unique in their position. They will correspond to a particular real and imaginary part and this may some day be a real definition of the mass and lifetime of an unstable particle.

**Peierls:** I agree with that. It seems to me to be the right way of describing unstable particles. The reason I said this is only approximate, is because even when you get into regions of high energy where you get many broad resonances which cannot physically be described as particles you will still get lots of poles in the continuation of the propagator \( G \). The question of when to call such a thing a particle and use a special function to describe it, is a quantitative one and not a qualitative one.

**Salam:** I would like to point out that I think Peierls agreed to something quite different from what I understood Gell-Mann is maintaining. If the propagator is
\[ G(k^2) = \int \frac{e(M^2) \, dM^2}{k^2 - M^2}, \]

Gell-Mann conjectures there are poles in \( e(M^2) \) in the \( M^2 \)-plane, Peierls conjectures there are poles in the "unphysical sheets" of \( G(k^2) \), and both want the poles to be at the same place.

**Gell-Mann:** These two things are exactly the same.

**Salam:** Is there such a theorem?

**Treiman:** What is it the experimentalists measure? If the answer is, as it may be, that it depends on the production, then how do you divorce this discussion from the production.

**Salam:** You cannot. You have to write down a rather complicated formula in terms of \( e \)'s to take into account the production as well.

**Treiman:** I see no reference to the production.

**Salam:** The production is not simply related. If you ask me how to measure \( e \) most cleanly...

**Treiman:** In the first place the experimentalist does measure a lifetime, at least he thinks he does.

**Salam:** Oh, no, no! He measures nothing of the kind. In very special cases he measures what he thinks is an exponential...

**Treiman:** What is the connection, why is it that so many people are fooled?

**Salam:** If the lifetime in decay is very much larger than the lifetime in production, then one can talk about an exponential. If this is not the case then there is no experimental exponential. And I would like to challenge the experimentalist to produce one.

**Gell-Mann:** May I try to answer Treiman’s question? In the case of a very narrow resonance such as a weakly decaying particle, the experimentalists do not get into much trouble practically. In the case of something like the \((\frac{3}{2}, \frac{3}{2})\) resonance, which is a very broad one, certainly the answer will depend to some extent on the experimental methods. But I think that what is being sought here is the possibility that theoretically there may be a certain parameter which occurs naturally in the theory in the description of several processes which go through this same resonance. And I think that is to be found in the poles in the complex plane of \( e \).

**Treiman:** But that would have to be demonstrated.

**Gell-Mann:** Yes. Maybe someone has, I am not up to date on these things.

**Chairman:** I would propose now, the time being so late, that we close this session which has been so rich in very different reports.

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**LIST OF REFERENCES**

SESSION 5
Thursday, 3rd July, 1958

Strange particle production

Chairman C. C. BUTLER

EXPERIMENTAL
Rapporteur J. STEINBERGER
Secretaries R. BUDDE
H. FILTHUTH
J. TREMBLEY

THEORETICAL
Rapporteur M. GELL-MANN
Secretaries F. CERULUS
M. KRETZSCHMAR
H. RUEG
A) Pion-nucleon production of strange particles

1. Introduction

Since the early work of Shutt et al. and Walker with the \( \text{H}_2 \) diffusion chamber, all studies of strange particle production have been with the propane or liquid \( \text{H}_2 \) bubble chamber. The work has been actively pursued for 2-3 years now by several groups. The experimental procedure and type of result is very similar for all groups; it will therefore save time and effort to summarize the results of all groups together, rather than separately. The contributing groups and authors are listed in Table I. Because of the large number of authors which seem necessary in this work, it will unfortunately save time to refer to the institution rather than the individual authors.

The following reactions have been studied in the interval of 930-1430 MeV/c for the incident pion.

<table>
<thead>
<tr>
<th>Reaction</th>
<th>Approx. number of events studied to date</th>
<th>Threshold momentum MeV/c</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) ( \pi^- + p \rightarrow \Lambda^0 + \eta^0 )</td>
<td>(~ 1000)</td>
<td>(~ 900)</td>
</tr>
<tr>
<td>(2) ( \pi^- + p \rightarrow \Sigma^- + K^+ )</td>
<td>(~ 250)</td>
<td>(~ 1050)</td>
</tr>
<tr>
<td>(3) ( \pi^- + p \rightarrow \Sigma^0 + \eta^0 )</td>
<td>(~ 150)</td>
<td>(~ 1045)</td>
</tr>
<tr>
<td>(4) ( \pi^+ + p \rightarrow \Sigma^+ + K^+ )</td>
<td>(~ 32)</td>
<td>(~ 1040)</td>
</tr>
</tbody>
</table>

The experiments have been very fruitful in clarifying the properties of the particles (spins, decay modes and probabilities, parity conservation in decay). Those results are presented in other sessions. Here we discuss only the production cross-sections and polarization effects in production. The work has been seriously handicapped in the last months by the failure of the Brookhaven Cosmotron. The only work which has not been presented already last year at Venice or published is that of Berkeley, our own measurements at 1090 MeV/c, and the work of the Yale group.

2. Cross-sections

Representative angular distributions are given in Figs. 1-4. These are all corrected for the angular variations in the detection efficiency. The total cross-sections are given in Table II. The following results are noted:

a) All observed total cross-sections are in the interval

\( 0.1 \text{ mb} < \sigma < 1 \text{ mb} \)

b) The \( \Lambda^0 \) cross-section rises from threshold to a peak of \( \sim 0.8 \text{ mb} \) near 1100 MeV/c and then drops again to 0.2-0.3 mb in the interval 1250-1450 MeV/c. No structure is observed in the \( \Sigma^- \) and \( \Sigma^0 \) cross-sections outside of experimental error.
c) The angular distribution of $\Lambda^0$'s is similar in the interval 1000-1450 MeV/c and markedly peaked backwards. The $\Sigma^0$-distribution is also backwards, but less markedly, and the $\Sigma^-$- and $\Sigma^+$-distributions are peaked forward.

3. Polarization

The asymmetry of the pion or nucleon in the decay of a spin $\frac{1}{2}$ hyperon, relative to the production normal, is $1 + \alpha P \cos \theta$. If $\alpha$ is known, this can be used to measure $P$. The large asymmetry observed in $\Lambda^0$-decay is compatible only with $1 \gg |\alpha| > \frac{3}{8}$. The sign of $\alpha$ has now also been determined (see the report of D.A. Glaser p. 271). The decay

![Fig. 1. $\pi^- + p \rightarrow \Lambda^0 + \theta^0$ production angular distributions.](image)

![Fig. 2. $\pi^- + p \rightarrow \Sigma^- + K^+$ production angular distributions.](image)

**TABLE II**

<table>
<thead>
<tr>
<th>Momentum GeV/c</th>
<th>$\pi^- + p \rightarrow \Lambda^0 + \theta^0$</th>
<th>$\pi^- + p \rightarrow \Sigma^- + \theta^0$</th>
<th>$\pi^- + p \rightarrow \Sigma^+ + \theta^0$</th>
<th>$\pi^+ + p \rightarrow \Sigma^+ + \theta^+$</th>
<th>Group</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.927</td>
<td>$0.09 \pm 0.03$</td>
<td></td>
<td></td>
<td></td>
<td>Berkeley</td>
</tr>
<tr>
<td>1.04</td>
<td>$0.59 \pm 0.12$</td>
<td></td>
<td></td>
<td></td>
<td>Bologna</td>
</tr>
<tr>
<td>1.09</td>
<td>$0.82 \pm 0.13$</td>
<td>$0.25 \pm 0.07$</td>
<td></td>
<td></td>
<td>Columbia</td>
</tr>
<tr>
<td>1.09</td>
<td>$0.58 \pm 0.12$</td>
<td>$0.09 \pm 0.04$</td>
<td></td>
<td></td>
<td>Brookhaven</td>
</tr>
<tr>
<td>1.12</td>
<td>$0.85 \pm 0.10$</td>
<td>$0.22 \pm 0.03$</td>
<td>$0.41 \pm 0.07$</td>
<td></td>
<td>Berkeley</td>
</tr>
<tr>
<td>1.13</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Yale</td>
</tr>
<tr>
<td>1.23</td>
<td>$0.27 \pm 0.004$</td>
<td>$0.17 \pm 0.04$</td>
<td>$0.16 \pm 0.04$</td>
<td>$0.15 \pm 0.05$</td>
<td>Michigan</td>
</tr>
<tr>
<td>1.23</td>
<td>$0.26 \pm 0.004$</td>
<td></td>
<td></td>
<td></td>
<td>Berkeley</td>
</tr>
<tr>
<td>1.33</td>
<td>$0.29 \pm 0.005$</td>
<td>$0.13 \pm 0.03$</td>
<td>$0.12 \pm 0.04$</td>
<td></td>
<td>Pisa</td>
</tr>
<tr>
<td>1.43</td>
<td>$0.32 \pm 0.006$</td>
<td>$0.23 \pm 0.04$</td>
<td>$0.24 \pm 0.06$</td>
<td></td>
<td>Columbia</td>
</tr>
</tbody>
</table>
Strange particle production

Curve normalized to 80 events

\[ \frac{dN}{d\Omega} = \frac{1}{e} \frac{d\tilde{N}}{d\Omega} = 5.85 - 1.27 \cos \theta + 4.71 \cos^2 \theta \]

\[
\begin{bmatrix}
N = \text{True counts} \\
\overline{N} = \text{Observed counts} \\
e = \text{Escape correction}
\end{bmatrix}
\[
= \begin{pmatrix}
1.36 & -0.175 & -2.58 \\
-0.175 & 2.40 & -7.33 \\
-2.58 & -23 & 9.29
\end{pmatrix}
\]

Error Matrix

Fig. 3. \( p^+ + p \rightarrow \Sigma^+ + \theta^0 \) production angular distribution.

asymmetry, as function of the \( \Lambda^0 \)-production angle, is additional experimental information which can be used to find the production amplitudes. The Berkeley results at 1.12 GeV are shown in Fig. 5. Our own results, combined with those of the Bologna group and of the Brookhaven group, and a portion of the Berkeley results, in the interval 1.04—1.12 GeV/c are shown in Fig. 6.

There has been no observed asymmetry in \( \Sigma^- \)-decay in the explored energy interval (see D.A. Glaser). Since this may be due to a small value of \( \alpha \), no statement can be made with certainty about polarization in \( \Sigma \)-production.

4. Amplitude analysis of \( \Lambda^0 \)-production near 1.1 GeV

The most extensive data on any one production process are on \( \Lambda^0 - \theta^0 \) production in the region of the peak of the cross-section. (\( \sim 1.1 \) GeV/c.) At this energy \( p_{\theta} = p_{\theta^0} = \sim 280 \) MeV/c in the centre of mass and it seems fair to analyse this in terms of \( s \)- and \( p \)-wave amplitudes. Then

\[
\frac{d\sigma}{d\Omega} = \frac{1}{4\pi} \left| a_s + (a_{p_{\theta^0}} + \sqrt{2} a_{p_{\theta^0}} \cos \theta) \right|^2
\]

\[
+ \left| a_{p_{\theta^0}} \cdot \frac{1}{\sqrt{2}} a_{p_{\theta^0}} \sin \theta \right|^2
\]

\[
p = \frac{\sin \theta}{2\pi} \text{Im} \left( a_{p_{\theta^0}} - \frac{1}{\sqrt{2}} a_{p_{\theta^0}} \right) \left( a_s + (a_{p_{\theta^0}} + \sqrt{2} a_{p_{\theta^0}} \cos \theta) \right)
\]

There are five real parameters to be determined on the basis of five experimental observations, assuming \( \alpha \) to be known.

The analysis has been performed at Berkeley for their 1.12 GeV/c results and at Columbia for a combination of the Bologna results at 1040, the Brookhaven results at 1080, the Columbia results at 1090 and part of the Berkeley results at 1120 MeV/c.
One of two possible sets of solutions of Berkeley, with $\alpha = 0.95$, is the following (*):

$$
\begin{align*}
    a &= 0.149 \pm 0.044 \\
    b &= 0.286 \pm 0.033 \\
    c &= 0.212 \pm 0.033 \\
    \varphi &= 2.34 \pm 0.31 \text{ (or 0.70 \pm 0.31)} \\
    \psi &= 5.24 \pm 0.59 \text{ (or 4.18 \pm 0.59)}
\end{align*}
$$

where

$$
\begin{align*}
    a e^{i \varphi} &= a_0 \\
    b e^{i \psi} &= a_{p_{1/2}} + \sqrt{2} a_{p_{3/2}} \\
    c &= a_{p_{3/2}} - \frac{1}{\sqrt{2}} a_{p_{1/2}}
\end{align*}
$$

We find from slightly different data

<table>
<thead>
<tr>
<th>$\alpha$</th>
<th>$a_{p_{1/2}} \times 10^{-15}$ cm</th>
<th>$a_{p_{3/2}} \times 10^{-15}$ cm</th>
<th>$a_{p_{3/2}}$ ($x 10^{-15}$ cm)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1.0</td>
<td>3.4</td>
<td>-1.8 - 3.8 i</td>
<td>-3.9 + 1.7 i</td>
</tr>
<tr>
<td>0.9</td>
<td>3.6</td>
<td>-2.0 - 4.1 i</td>
<td>-3.3 + 1.7 i</td>
</tr>
</tbody>
</table>

To get some feeling for the actual constraint of the data on the parameters, we have tried to find also extreme values for the parameters. We find then that adequate fits to the data can be obtained for

$$
0.15 < f_{p_{1/2}} < 0.5
$$

where $f_{p_{1/2}}$ is the fractional $s$-wave contribution to the total cross-section. The following table is then self-explanatory

<table>
<thead>
<tr>
<th>$f_{p_{3/2}}$</th>
<th>$(f_{p_{1/2}}/f_{p_{3/2}})$ minimum</th>
<th>$(f_{p_{1/2}}/f_{p_{3/2}})$ maximum</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.15</td>
<td>0.92</td>
<td>0.92</td>
</tr>
<tr>
<td>0.165</td>
<td>0.48</td>
<td>2.3</td>
</tr>
<tr>
<td>0.225</td>
<td>0.25</td>
<td>3.8</td>
</tr>
<tr>
<td>0.39</td>
<td>0.57</td>
<td>2.6</td>
</tr>
<tr>
<td>0.45</td>
<td>0.76</td>
<td>1.9</td>
</tr>
</tbody>
</table>

The conclusion is that the data admit of a large range in the relative contribution of the three states, but that none of the states can contribute less than about 15% nor more than 50% to the total intensity. The Berkeley results give similar limits.

5. **Isotopic spin conservation in production**

If isotopic spin is conserved in production then the three reactions $\Sigma^-$, $\Sigma^0$ and $\Sigma^+$, are related by triangular inequalities. The $\Sigma^-$-data are still rather poor. But combining the Michigan and Yale results on $\Sigma^+$ on the one hand (1.13—1.23 GeV/c) and those on $\Sigma^-$ and $\Sigma^0$-production in the interval 1.1—1.23 GeV (Brookhaven, Columbia, Berkeley, Michigan) on the other, the violation pointed out some time ago by D.A. Glaser has somewhat diminished so that the result is now statistically compatible with parity conservation.

6. **Parity conservation in strange particle production**

It has been pointed out by Golovico that theoretical arguments, which can be made for the conservation of parity in pion-nucleon and electromagnetic interactions, may not apply to the production of strange particles. Lack of parity conservation in the $\Lambda^0$-production would result in a polarization of the $\Lambda^0$ in the production plane, and this would then be detected by means of the large asymmetry in $\Lambda^0$-decay. Since only the direction, and not the plane of polarization is specified, there is some liberty in the collecting of the results. In Figs. 7 and 8 we have collected the results of the Bologna, Columbia and Pisa

(*) See Appendix page 325.
Strange particle production

Fig. 9. $\pi^- + p \rightarrow \Lambda^0 + \theta$ Magnitude of $\alpha P(\theta) = \alpha(\theta)$ in the production plane.

Fig. 10. $\gamma + p \rightarrow \Lambda^0 + K^+$ differential cross-section (Cal. Tech.).

Fig. 11. $\gamma + p \rightarrow \Lambda^0 + K^+$ Excitation curve at $\theta_{CM} = 90^\circ$ (Cal. Tech.).

B) Photoproduction of strange particles

The reaction $\gamma + p \rightarrow \Lambda^0 + K^+$ is being studied both by Silverman and Wilson at Cornell and at Cal. Inst. of Tech. by Brody, Donoho, Walker and Wetherell, in the narrow interval between 960 MeV and 1060 MeV. The measurements are made by means of electronic detection of the magnetically selected $K$ mesons. The momentum and groups in the interval 1.04—1.43 GeV/c for $\Sigma^-$ and $\Lambda^0$-production. The angle that is plotted is that of the decay pion relative to the hyperon, in the centre of mass of the hyperon and projected into the production plane. No significant asymmetries are observed. The asymmetries might have been missed because of the integration over production angles and energies. The Berkeley group has analysed its results at 1.12 GeV/c as a function of production angle (Fig. 9). They find no significant asymmetries.
production angle define the incident energy. The observed cross-sections are shown in Figs. 10-13.

The mean features of the results are the following:

a) The cross-sections are in the interval $0.7 \cdot 10^{-31} \text{cm}^2/\text{sterad} < \frac{d\sigma}{d\Omega} < 1.6 \cdot 10^{-31} \text{cm}^2/\text{sterad}$ for all angles, energies and institutions.

b) The excitation function rises even more slowly than linearly with the momentum, in the centre of mass momentum interval 100–250 MeV/c.

c) The angular distribution does not differ very significantly from isotropy, although there is some indication of at least a small peaking in the region near 90°.

In addition, there is a measurement of the reaction $\gamma + p \rightarrow \Sigma^0 + K^+$. For a photon energy of 1111 MeV, at a centre of mass angle of 85°, $\frac{d\sigma}{d\Omega} = 0.61 \pm 0.17 \cdot 10^{-31} \text{cm}^2/\text{sterad}$, very nearly the same as for the $\Lambda^0$-production at a comparable energy (960 MeV). The measurement is performed by studying the $K^+$ production at a given angle and momentum as a function of the $\gamma$-ray energy (Fig. 14).

C) Nucleon-nucleon production of strange particles

The experimental situation here is nearly empty, in the mathematical sense. The only reported results are obtained by Baumel, Harris, Lee, Orear and Taylor. Magnetically selected $K$ mesons emitted at 0° from a liquid $\text{H}_2$ target bombarded by 3 GeV protons, were

![Fig. 15. $\bar{\Lambda}^0 \rightarrow \bar{p} + \pi^+$ in nuclear emulsion.](image-url)
Strange particle production

Points were obtained with errors of the order of 30% at K momenta of 280 and 625 MeV/c. The latter momentum corresponds to zero centre of mass momentum. The fact that the cross-section at 625 MeV is roughly twice that at 280 MeV demonstrates that s-wave production is important. All higher modes must give zero contribution at this momentum. The absolute values of the cross-section are not inconsistent with nucleon-nucleon cross-sections of the order of 1% of geometrical.

D) Antihyperon production

Prowse and Baldo-Ceolin have found a clear case of $\bar{\Lambda}^0$ production. In a stack exposed to 5 GeV $\pi^-$ mesons a two-prong star was found, which decays into a $\pi^+$ meson and a track of $2.1 \times$ min. ionization which interacts to produce a typical antiproton star (11-prong, visible energy $\sim 780$ MeV). The $Q$ is $35^{+2.6}_{-0.9}$ MeV from range of $\pi^+$ and ionization of proton (Fig. 15).

E) Mass 500

1. Alikhanian

The particles of mass near 500 $m_e$, announced a few years ago by Alikhanian, have been the subject of extensive search in the last year. It may be well to review the work of Alikhanian et al.\(^1\), which remains one of the strongest pieces of evidence in its favour. The most convincing data were obtained with the apparatus of Fig. 16. A 76 ton magnet separates two cloud chambers with metal plates. The curvature of a track is measured by a hodoscope arrangement in the magnet. The bottom chamber serves to measure the range (7 plates of Pb, 7 mm thick), and the top chamber to see the origin of the track. The trigger is supplied by a coincidence 1-3-5-A. The conditions of acceptance of a track were the following:

i) The track must trigger at least four of the counters determining the curvature and must lie on a circle.

ii) It must be straight in the perpendicular plane.

Fig. 16. Schematic diagram of the magnetic spectrometer with two cloud chambers.

Fig. 17. Total mass spectrum of particles stopping in the lower chamber as the result of ionization loss.
iii) The direction in the lower chamber must be a continuation of the direction determined by the hodoscope.

iv) The particle must stop in the illuminated region of the lower chamber.

v) The increase in track density near the end of the range must be consistent with the other determinations.

In a magnetic field of 4800 gauss the error in the mass determination is estimated at 9%.

The observed mass spectrum is shown in Fig. 17. In addition to the large proton and $\mu$ and $\pi$ meson peaks, 17 $K$ particles ($15^+, 2^-)$ and 11 mass 500 particles ($2^+, 9^-)$ are resolved. A spectrum of particles originating in stars in the top chamber exhibits no mass 500 events. The spectrum of particles entering the chamber from the outside is shown in Fig. 18. The $K$ particles and $\pi$'s are absent, $\mu$'s and mass 500's are present.

Only one of the 11 events has a secondary, and this is a rather questionable electron.

The frequency of the events is not so easy to determine, since the efficiency of detection is a function of the mass.

The lower the mass, the poorer the detection efficiency, since the particle is bent too much in the field. 255 $\mu$ mesons were observed in the same experiment with the 11 mass 500's. After the correction for detection efficiency it is estimated that this corresponds to 1/200 of the $\mu$ flux stopping in the same range interval.

The assignment of the mass near 500 m, was verified for the group of 11 particles by studying the scattering versus range in the bottom chamber.

2. Other electronic and cloud chamber work

The discovery of Alikhanian has excited a large effort to verify the existence of the particle. I group here three electronic and Wilson chamber attempts:

Fig. 19. Schematic diagram of Keuffel's apparatus.

Fig. 18. Mass spectrum of particles entering the apparatus from the outside.

Fig. 20. Schematic diagram of the Princeton double cloud chamber.
Strange particle production

a) *Keuffel*

The arrangement of Keuffel reported at the May meeting of the American Physical Society is about as shown in Fig. 19.

There was some evidence found, unfortunately rather meagre, that mass 500 particles do exist and that they may have a lifetime near $10^{-3}$ sec. The chief difficulty with the technique is due to the inelastic scattering of protons in the "E" counter. Such events simulate lower mass particles. At the present time the evidence of Keuffel must be considered substantially weaker than that of the Russian group. We have a telegram from Keuffel that with increased data the flux of mass 500's in excess of 0.1% relative to $\mu$'s in the same range interval is excluded.

In particular $500 \rightarrow \pi^+ + \pi^0$ and $500 \rightarrow \mu^+ + \pi^0$ are less than 0.05%.

b) *Princeton double cloud chamber (Piroué and Hendel)*

Two cloud chambers are exposed at Echo Lake (3250 m) about as shown in Fig. 20.

Absolutely no events have been found which could be attributed to mass 500. In the same time 432 $\mu$ mesons and 620 protons were found (Alikhanian had $\frac{11 \text{ m}_{500}}{255 \mu}$).

It is estimated that the recording efficiency was biased in favour of $m_{500}$ relative to $\mu$'s by a factor 2.4, so that no particle was found as compared to an effective $\mu$ flux of

---

Fig. 21. *Schematic diagram of the Pisa-Milano double cloud chamber.*
 Session 5

\[ \sim 1000. \] There is 95\% confidence that the mass 500 flux is less than 0.3\% of the \( \mu \) flux.

3. Pisa - Milano group. (Conversi, Rubbia, Torelli, Fiorini Ratti, Succi.)

The apparatus, exposed at 2550 m, is very similar to the previous one, but there is no field and the droplet chamber is not in operation (Fig. 21). Great reliance has therefore to be put on the \( dE/dx \) counter, and the experiment has great similarity to Keuffels. However, it is not plagued by the inelastic protons, since these can be detected in the bottom chamber. The apparatus has, however, the advantage of many (17) very thin (0.3 cm plexiglass) plates. In Fig. 22 good stopping tracks are points, tracks interacting or with improper ionization near the end of their range are circles. There is one dot in the mass 500 region, and this is very likely a proton, by comparison with a typical proton stopping (b) in Fig. 23. It is estimated that in the same period about 1200 \( \mu \) mesons would have stopped in the chamber, if they had not been excluded by the Cherenkov counter. According to Alikhanian, therefore, 6 mass 500 particles should have been seen. The sensitivity is therefore about the same as that of the Princeton experiment.

4. Emulsion work

There are three sets of emulsion work of comparable sensitivity:

a) Columbia group (Bierman, Harris, Orear, Rosenfeld, Taylor.)

A stack was exposed at 3300 m altitude 39° geomagnetic latitude under a copper absorber (Fig. 24).

The conditions are chosen to duplicate those of Alikhanian. All stopped particles are analysed by grain counting the last centimetre, independent of the character of the stopping. (That is, whether there is, for instance, a \( \pi - \mu \) decay or not.) 700 \( \mu \) stoppings have been found, and no mass 500's.

b) Bombay group (Durga Prasad and Sharma)

A stack was exposed at 3400 m, mag. lat. 24°N. Of 1925 tracks accepted for mass determination up to now, 1297 were of mass greater than 1100 m, and 628 comprised a meson group. Of these, 35 decayed to \( \mu \)'s, 38 produced capture stars, 13 originate in stars, and 542 come from the outside and are predominantly \( \mu \) mesons. The mass spectrum of these particles reflects the \( \mu \) mass very well. None of the 628 particles had a mass exceeding 380 m.

c) M.I.T. group (Fazio)

An emulsion stack was exposed at sea level. Only flat, well countable tracks of about twice minimum ionization were followed to their end. About 1000 \( \mu \) mesons have been found. Half of this sample has been systematically analysed; no mass 500's were found. Since the stopping interval range of mass 500's is 2\( \frac{1}{2} \) times as great as that of \( \mu \) mesons in the same ionization interval, the sensitivity of the experiment corresponds not to 500, but to 1250 \( \mu \) stoppings.

In addition, the other half of the sample has also been scanned for those particles (\( \pi - \mu \)-like) which have 600 \( \mu \) long \( \mu \) meson secondaries. No mass 500's were found. The conclusion is that if mass 500's exist, and decay with the appearance of a 600 \( \mu \) long \( \mu \) meson, there are not likely to be more than one to 2500 \( \mu \) mesons stopping in the same interval.

If we combine these last 5 negative results we have a sensitivity as follows:
Fig. 23. *Comparison of the track of the only particle in the mass 500 region (a) with a typical proton stopping (b).*
If we ask specifically for those mass 500's which decay with the emission of a 600 $\mu$ meson, the total is raised to 6000 by the work of the M.I.T. group. It would seem extremely unlikely therefore, that particles of this mass exist in cosmic rays in the abundance of 1/200 relative to $\mu$ mesons. The only possible conclusions seem to be that either the particle does not exist, or that the $\mu$ meson flux in the Alikhanian experiment has been underestimated by a factor of $\sim 20$. Whether this last is a reasonable possibility is not clear to me, but would seem to be a matter which could be settled by a more accurate evaluation of the geometry of acceptance in the Alikhanian experiment.

LIST OF REFERENCES


DISCUSSION — see p. 159.
SOME COMMENTS ON ASSOCIATED Y-K PRODUCTION NEAR THRESHOLD
BY PIONS AND PHOTONS

B. T. FELD
Massachusetts Institute of Technology, Cambridge (Mass.)

Summary: Evidence is presented that the reactions 

\( \pi^- p \rightarrow (A \text{ or } \Sigma) + K \), in the region of pion kinetic energies 0.8—1.4 GeV, are dominated by the resonances previously observed in the total \( \pi^- p \) cross-sections. Some general consequences of the resonant nature of these interactions are discussed.

1. It has been observed (Cresti, N. Y. Meeting, Am. Phys. Soc., 1958) that the cross-section for the reaction

\[ \pi^- + p \rightarrow A^0 + K^0 \]

rises very rapidly above threshold (0.76 GeV) to a peak in the region of 0.9 GeV, and thereafter follows a curve roughly parallel to the total \( \pi^- p \) cross-section. The latter is known to exhibit a \( J = \frac{1}{2} \) resonance in this region.

2. For a resonance, the relative decay probability for any mode should be independent of the mechanism of excitation of the resonance. At the pion kinetic energy \( E_\pi = 0.9 \) GeV

\[ \frac{\sigma(\pi^- + p \rightarrow A^0 + K^0)}{\sigma_{\text{tot}}(\pi^- + p)} \approx \frac{0.5 \text{ mb}}{40 \text{ mb}} = 0.012. \]

At the equivalent photon energy, \( E_\gamma = 1.05 \) GeV,

\[ \frac{\sigma(\gamma + p \rightarrow A^0 + K^0)}{\sigma_{\text{tot}}(\gamma + p)} \approx \frac{3 \mu b}{200 \mu b} = 0.015. \]

(These cross-sections are estimated from the work of a number of Cornell groups; private communications.)

3. There are a number of means of investigating the resonant \( Y - K \) production. Assuming the resonant state were known, the energy dependence of the cross-section near threshold is determined by the relative \( Y - K \) parity. However, for a resonance in the \( \pi^- p \) interaction, the interaction range is expected to be \( R_\pi \approx h/m_\pi c \), instead of \( R_K \approx h/m_K c \), which renders the angular momentum barriers rather ineffective. In the following table we give \( \eta = kR \) for the \( \pi^- + p \rightarrow Y + K \).

| \( t_\pi \) (GeV) | \( \Lambda + K \) | \( \Sigma + K \) |
|-----------------|----------------|
|                 | \( kR_\pi \) | \( kR_\pi \) |
| 0.95            | 0.57          | 2.15          |
| 1.25            | 0.91          | 3.30          |

It is clear that even relatively close to threshold (0.76 GeV for \( A^0 - K \); 0.9 GeV for \( \Sigma - K \)) it will be very difficult to distinguish, through barrier-penetration effects, the parity of the resonant state (say, between \( p_{3/2} \) and \( d_{3/2} \) meson emission).

4. Angular distributions could tell us more, but the observations are not yet sufficiently accurate. Example: Assume a \( j = \frac{3}{2} \) resonance in \( A^0 - K \) production. The final state is \( p_{3/2} \) or \( d_{3/2} \). Let its (resonance) amplitude be \( re^{i\alpha} \); since the threshold is above the resonance, \( \pi/2 < \alpha < \pi \). Let the amplitude for s-wave production be \( a \). Then

\[ \frac{\sigma}{4\pi} = a^2 + 2r^2 \]

in both cases. The "front-to-back" ratio is

\[ Q = 2ar \cos \alpha / (a^2 + 2r^2) \]

for a \( p_{3/2} \)-resonance, and

\[ Q = [ab + \frac{1}{2}(3b' + b) r \cos \alpha] / (a^2 + 2r^2) \]

for a \( d_{3/2} \)-resonance (\( b \) and \( b' \) are, respectively, the non-spin-flip and spin-flip p-wave production amplitudes). The observations indicate \( Q < 0, |Q| \geq 0 \) and increasing with \( \pi \) meson energy. Both these observations are consistent with the expected behaviour of the resonant phase-factor, \( \cos \alpha \).

5. Another tool available is the \( \Lambda^0 \)-polarization \( \langle \hat{P} \rangle \), as observed by the decay asymmetry. In the examples quoted above, we would have, for a \( p_{3/2} \)-resonance

\[ \hat{P} = \left[ \frac{\pi}{2} ar \sin \alpha \right] / (a^2 + 2r^2) \]
Strange particle production

and, for a $d_{1/2}$-resonance

$$\bar{P} = - \left[ \frac{\alpha}{6} (3b + b') \frac{r \sin \alpha}{(a^2 + 2r^2)} \right].$$

Here, the indications are of $|\bar{P}| \approx 1$ and decreasing with increasing pion energy; this is not inconsistent with the expected $\sin \alpha$-behaviour.

6. The interpretation of $\Sigma-K$ production is considerably more complicated. Here we are involved with two resonances, the $T = 1/2$ (0.8 GeV) and the $T = 3/2$ (1.3 GeV). The data indicate: $\Sigma^\pm$ go forward; $\Sigma^0$ go back; $|\bar{P}(\Sigma^-)| \ll 1$ in $\pi^- + p \rightarrow \Sigma^- + K^+$. These data are not sufficient to yield a unique interpretation, especially in the absence of any other information concerning the nature of the $T = 3/2 \pi^- - p$ resonance.

LIST OF REFERENCES


DISCUSSION — Steinberger and Feld

Morpurgo: I would like to make two remarks concerning the investigation of parity non-conservation in strong interactions. The first point is that it is very possible, in my opinion, although not necessary, that the longitudinal polarization of the produced hyperon is a relativistic effect of the order of $v/c$. To detect such effects one should perhaps go to higher energies. The second point is that pion-nucleon interactions at high energies should also be investigated and, for instance, in a reaction of double production of pions in pion-nucleon or photon-nucleon collision, there are some obvious angular distributions which may be investigated.

Adair: Perhaps it is appropriate now to say something about some of the phenomenological aspects of the strange particle production by pions and photons. These phenomenological estimates were made, using the $R$-matrix methods that Wigner used to derive threshold theorems quite a while ago. Such an analysis leads to interesting consequences. Let us regard the experimental data, particularly around 950 MeV, the energy region which was discussed most thoroughly by Steinberger. We find that the $s$-wave scattering and production amplitudes in the $T = 1/2$ state for both $A^0$ and $\Sigma$ production by $\pi$ mesons interacting with nucleons are very nearly $1/2$ to $1/4$ of the maximum allowed by the conservation laws. This immediately suggests that perhaps the $K$ meson coupling constant is not small. We can use the values of these production amplitudes then to make an extrapolation in energy of the $A^0$ cross-section will go up to a cusp at the energy of the $\Sigma$-threshold. Of course, on top of the $s$-wave cross-section are superimposed contributions from the $p$-waves. This cusp in the $s$-wave cross-section will only occur if the $\Sigma$ and $A^0$ have the same parity. The very large values of these $S$-matrix elements for the production of $A^0$- and $\Sigma$-hyperons provides some information about the final state interaction for $A^0$- and $\Sigma$-production in photon-nucleon interactions. Photo-production of $K$ mesons is related to pion production of $K$ mesons in very much the same way as the photoproduction of $\pi$ mesons is related to $\pi$-nucleon scattering. Using then the same matrix elements which will derive from the experimental values presented here, one can make some comments upon the $K$ meson photoproduction. One can say that the phase of photoproduction matrix elements, for example, the photoproduction of $A^0$'s and $K$'s, is not simply related to the $A^0-K$ scattering phase-shifts as it is when this energy dependence breaks down very near the threshold for $A^0$ production as the $s$-wave $A^0$ cross-section will go up to a cusp at the energy of the $\Sigma$-threshold.

Notice that the $A^0$ cross-section and the $\Sigma$ cross-section have a characteristic $\sqrt{E}$ dependence near threshold, but

Fig. 25. $A^0$ and $\Sigma$ production cross-section.
there is only one channel open. In fact, near threshold the phase-shift is not zero or even necessarily small. Another factor of interest is that one can make an estimate of the following reaction:

\[ \gamma + p \rightarrow A^0 + K \]

via the final state interaction of the reaction

\[ \gamma + p \rightarrow \pi + n. \]

If one takes the values of the cross-section of \( \gamma + p \rightarrow \pi + n \) from the Cornell and Cal. Tech. data and the values of the matrix elements for \( \pi + n \rightarrow A^0 + K \), one can get complete qualitative agreement with the experimental measurements of the \( A^0 + K \) photoproduction from this contribution alone. In general, the final state interaction has the property that, as compared to the perturbation calculation into plane waves, it can either decrease or increase the interaction rate by factors of perhaps 3 or 4.

**Treiman:** Two questions: it was not clear to me how the amplitudes obtained by the Berkeley group were arrived at. What was assumed about the parameter \( a \)? Secondly, can such an analysis be done for \( K \)-production in order to set limits on the polarization? This is for purposes of later discussions of up-down asymmetries.

**Good:** The way the analysis was done was simply to repeat it several times for different values of \( a \) (*) . The analysis cannot be performed for values of \( a \) less than about \( \frac{1}{3} \) because then one cannot fit the angular distribution and the polarization with \( s \)- and \( p \)-waves. I should say that the analysis is based on \( s \)- and \( p \)-waves only. As for the \( \Sigma \)-production it is not possible to set a limit, one does not know whether the \( \Sigma \)'s have no asymmetry because they are unpolarized or because they have no intrinsic asymmetry parameter.

**Reynolds:** One remark concerning the direct nucleon-nucleon strange particle production cross-section: a little information is supplied by an experiment done by the Princeton group with Bowen and others, in studying the \( Z \) dependence of \( \Lambda^0 \)-production with both pion and proton beams. One finds that if one takes the value of possible absorption cross-sections of \( \Lambda^0 \)'s in getting out of the nuclei, the nucleon-nucleon production results can be explained without involving any significant direct nucleon-nucleon production cross-section. That is, nothing is required more than \( \frac{1}{10} \) of the known pion-nucleon \( \Lambda^0 \)-production cross-section.

**Lederman:** With respect to the question of non-conservation of parity in \( \Lambda^- \)-production, I would just like to remind you that there are a large number of experiments, all done with cloud chambers on production of \( \Lambda^0 \)'s in complex nuclei and showing a forward-backward asymmetry of the pion emission relative to the \( A^0 \) line-of-flight. This kind of data would indicate violation of parity in the production process unless one wants to revive parity doublets. A recent review of the literature is given by Blumenfeld, Chinowsky and Lederman 1). Whether the agreement of so many different experiments represents a propagation of bias or not, I do not know but it is all in the literature.

**Newth:** Could I ask Steinberger whether he has any information on pair production of \( K \) mesons, even an excitation curve from machine data?

**Steinberger:** No.

**R. R. Wilson:** We have examined at Cornell our excitation curve looking for the effects just mentioned by Feld concerning the possible effects of the resonance in the pion production on the photoproduction of the \( K^-A^0 \) and looking for the implied momentum cubed variation of the cross-section with the momentum of the \( K \). The measurements do go down quite close to threshold, namely to within 23 MeV. The data indicate instead a linear variation with the momentum which means that the \( K \) meson is made in an \( s \)-wave. In trying to determine the question of the scalarity of the \( K \), the photoproduction in an \( S \)-state implies that one has magnetic dipole absorption in the case that the \( K \) meson is a scalar, or electric dipole absorption in the case that the \( K \) meson is a pseudoscalar. We can expect now to be able to make a very clear differentiation between these two possibilities if we look for the photoproduction of the \( \theta^0 \)'s on neutrons. Experiments in this direction are being started.

**Adair:** This is rather a minor point concerning Feld's discussion on the possible effects of \( d \)-waves. Even if one uses a radius so small as the \( K \) meson Compton wavelength (I made some calculations using a particular model which probably does not make too much sense) one can get enough \( d \)-wave, particularly in amplitude, to affect considerably the production amplitude calculations shown by Steinberger from the Columbia group and the Berkeley group.

**Marshak:** I just want to ask Wilson: does not a combination of the excitation function for a \( K^+ \) and your angular distribution indicate more strongly that it is a pseudoscalar than a scalar particle? That is, one can reconcile the excitation function with both possibilities as you indicated, but one needs a combination of accidents to get the rather isotropic angular distribution which goes so nicely with the pseudoscalar possibility.

**R. R. Wilson:** Yes, if we compare your weak coupling calculation and that of Moravcsik with our measurements, then we get slightly better agreement with your pseudoscalar calculations than with the scalar case. The scalar case does require a particular choice of the effective magnetic moment of the proton, of the \( A^0 \), and of the coupling constant.

(*) See appendix p. 323.
Fig. 26. Probable case of the decay of a $\Xi^0$ in the Pic du Midi cloud chamber:
$\Xi^0 \longrightarrow A^0 + \pi^0$; $A^0 \longrightarrow p + \pi^-$; $\pi^0 \longrightarrow e^+ + e^- + \gamma$. 
But the limits of disagreement are not at all beyond our experimental errors.

**Marshak**: Nor the theoretical.

**W. Powell**: For the sake of completeness we might add here the fact that we obtained two cascade particles in a beam of 5 GeV \( \pi^- \) mesons. The cross-section based on these two events is between one and two \( \mu \)barns with no corrections made.

**Kaplon**: Also for the sake of completeness I would like to point out that I have received an event, which should have been turned over to Steinberger, from the Pic du Midi group of the Ecole Polytechnique, observed in their double cloud chamber experiment. This event is best interpreted as the decay of a \( \Xi^- \) (Fig. 26). I think this is the first experimental evidence indicating the existence of this particle. I would like to make another comment with respect to Newth’s question concerning \( K^+ \) and \( K^- \) production. If my memory is correct, a group at Brookhaven some time ago ran an excitation function on \( K^- \) at the Brookhaven energies and it seemed to be, with very coarse statistics, in agreement with phase-space considerations on some simple model calculations by Sternheimer.

**Nikitin**: I should like to stress that the weakest point of the paper of Alikhanian’s group is surely the intensity of the mass 500 component relative to the \( \mu \) meson flux. It may be uncertain, at least by a factor of three, because of the large difficulties involved in evaluating the geometrical correction for the efficiency of the apparatus used.

**Reynolds**: With reference to Keuffel’s experimental set-up, I should like to point out the obvious possibility that his \( dE/dx \)-counter might be detecting a fluctuation on the high side of the Landau distribution. He minimizes this effect by having a Cherenkov-counter in anticoincidence, so that only slow particles are detected, not fast particles giving high \( dE/dx \) accidentally; so only slow particles are measured. One other remark concerns the work of Lindeberg using the upper cloud chamber of the Princeton set-up. Using only drop counting and momentum, he obtained three particles with a mass very close to 500; the internal errors are essentially the same as the external errors on the events. The total number of \( \mu \) mesons is unknown since the data were not scanned statistically. So that this is what actually led us on to continue the double cloud chamber experiments which have proved negative so far.

**Peyrou**: I do not understand exactly the point of this uncertainty in the geometrical correction of the luminosity of the apparatus of Alikhanian, because the calculation is quite straightforward; secondly the apparatus could be very well standardized by the flux of protons and \( \mu \) mesons which is known at 3200 m. So I cannot believe that there is an uncertainty of a factor of twenty or even five.

**Nikitin**: I suppose that the most difficult thing in evaluating these corrections is the stray field because the \( \mu \) mesons have quite small momenta. One has to take into account the stray field, but it is very difficult to do it, and all the uncertainties come from this point, as far as I know.

**Goldhaber**: I want to ask Nikitin if somebody knows in how many cases Alikhanian and his group saw secondaries from stopping \( \mu \) mesons, especially negative ones. The efficiency of detecting such secondaries would be useful to know in discussing conceivable decay modes of his particles.

**Nikitin**: I have no information on this point.

**W. T. Sharp**: I should just like to remark that to look for the mass 500 particle, Hincks at Chalk River is doing a cosmic ray sea level counter telescope experiment which in a few weeks’ running should give rather better sensitivity than the results quoted. Unfortunately there are no results as yet.

**Kaplon**: I would also like to comment again here, for the sake of completeness, that there exists in the literature, in JETP, the report of three events observed in emulsion, of particles which decay into \( \pi^0 \) mesons of the unique range of something like 390 \( \mu \). Two cases of \( \pi^0 \), I believe, and one case of \( \pi^- \). This is a short note in the journal but it is very difficult really to understand what the people mean to interpret from this note. But if on this basis one assumes that this is a two-body decay to a \( \pi^0 \) and a \( \pi^+ \), one again gets a mass 500 particle out of this. I would like to hear some comment if possible from the Russian people about this observation.

**Nikitin**: I have no comment.

**Butler**: From all these contributions to the discussion on the existence of the mass 500 particle we must conclude that this is a very fascinating topic, but obviously no definite conclusions can be reached at the meeting this year. Many of us have heard that Alikhanian and his colleagues have a very fine new apparatus which they are taking to the mountains this summer. We have also heard of other experiments elsewhere now being planned and started, as well as those already running. Thus I feel we can only conclude that there are still suspicions of this particle’s existence, but we must wait until the high energy conference next year for a definite answer.
STRANGE PARTICLE PRODUCTION — Theoretical

M. GELL-MANN, Rapporteur
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My report will be very brief. It is very close to the truth to say that we know nothing in the way of theory about strange particle production. I shall dismiss immediately, as too complicated, all processes except

\[ \gamma + p \rightarrow K + A, \Sigma \quad \text{and} \quad \pi + p \rightarrow K + A, \Sigma. \]

The experimental information on photoproduction is becoming fairly definite now and is well worth analysing. So far, not much theoretical work has been done, however. There are the speculations about a possible resonance, about which we have heard. Besides that, there is perturbation theory including anomalous magnetic moments (work of Fujii and Marshak and of Moravcsik).

In the case of pions, the perturbation method is all right for such low energies that the 3/2, 3/2 resonance is not dominant. Furthermore, we know that in the limit of zero total pion energy (\(\sim 140\) MeV below threshold) the perturbation theory becomes rigorous. If \(K\) is pseudoscalar (relative to \(A\) and \(p\)) the same theorem holds, but zero total energy is 500 MeV below threshold, so the argument for perturbation theory is poorer. If resonances occur, they of course invalidate perturbation theory in their vicinity. A large contribution from the sequence of processes \(\gamma + p \rightarrow N + \pi \rightarrow A + K\) can also invalidate the perturbation method. Despite these counter-arguments, it is perhaps not a total waste of time to try to estimate the coupling constant \(g_{KA}/4\pi\), assuming \(K\) is pseudoscalar, by comparing the experimental data with the perturbation formula. Since magnetic moments are included, a second parameter appears, which is \(\mu_A + (g_{K2}/g_{KA})\mu_X\). Here \(\mu_A\) is the magnetic moment of \(A\) in nucleon Bohr magnetons and \(\mu_X\) is, in the same units, the transition magnetic moment between \(A\) and \(\Sigma^0\). (The sign conventions are those of my paper on strong interactions.)

The quantity \(\mu_X\) is of especial interest since it determines the lifetime of \(\Sigma^0\) through the formula

\[ \Gamma_{\Sigma^0} = \frac{(\mu_X)^2 \omega^3}{137 \text{ M}^2}, \]

where \(M\) is the nucleon mass and \(\omega\) is the energy of the emitted \(\gamma\)-ray.

Berman at Cal. Tech. has fitted the latest data to the formula of Fujii and Marshak and obtains \(g_{KA}/4\pi \approx 1.5\) and the quantity \(\mu_A + (g_{K2}/g_{KA})\mu_X\) negative, around \(-1\). These results are probably not to be taken very seriously, but they are certainly consistent with \(K\) couplings being weaker than \(\pi\) couplings, as in the global symmetry model.

It is out of place, but perhaps interesting, to remark that in that model, with neglect of \(K\) couplings and baryon mass differences, the magnetic moments of hyperons are calculable. We have

\[ \mu_A \approx 0, \quad \mu_\Sigma \approx 0, \quad \text{and} \quad \mu_X \approx -\mu_n \approx +1.9. \]

Perturbation formulae have also been given in the case of a scalar \(K\). Here comparison with experiment indicates a value of \(g_{KA}/4\pi\) considerably smaller than 1. Such a value would probably not be consistent with our knowledge of \(K\) interactions. Of course, if the \(K\) really is scalar with a large value of \(g_{KA}/4\pi\), we have no particular reason to believe that perturbation theory would be valid, even for this problem, and so we cannot draw a strong conclusion that \(K\) is pseudoscalar.

Future analysis of \(K\) photoproduction may benefit from the current fashion, about which we heard from Chew two days ago, of looking for poles in the scattering amplitude just beyond the physical region by extrapolation of experimental data. In particular, there are the poles with coefficients \(g_{K2}/g_{KA}\) and \(g_{K2}/g_{KA}\) that have already cropped up in our discussion of perturbation theory. The importance of these poles and others has been emphasized by Low; they may help us to evaluate several fundamental constants pertaining to the strange particles.

\(K\) particle production by pions should also be analysed in terms of such poles. So far, this has not been done, and the only calculations that have been made are by straight perturbation theory. The example of pion scattering shows us by how much such calculations are likely to be wrong in the \(s\)-wave, even at threshold. Moreover, we know that the produced \(A\)-particles have almost maximum polarization, which certainly belies any validity of the Born approximation, and in fact proves that spin-flip and non-spin-flip amplitudes are out of phase by a large angle.

It is probably worth while for the time being to undertake a simple phenomenological treatment of associated
production near threshold, using \( s\)- and \( p\)-waves and conventional threshold energy dependence. Phases must be assigned to the various matrix elements. Even near threshold, these phases cannot be related to the real parts of pion scattering phase-shifts unless all the other channels, like \( 2\pi + N \), are neglected. Anyway, the phases in pion scattering are not yet known at these energies. Polarization measurements on \( A \), however, as just mentioned, give us some direct experimental information on the relative phases of associated production matrix elements in even and odd states. Such questions have been discussed by Amati and Vitale.

In connection with the phenomenological analysis of the reactions \( \pi + p \rightarrow K + \Sigma \), I now turn to a specific contribution to this session, the work of Baz and Okun. They point out the relevance to these processes of a theorem in nuclear physics that has been discussed by Breit and Wigner. It concerns the appearance of a cusp in the cross-section vs energy plot of one reaction at the threshold of another. In this case, of course, they refer to a peculiarity in the cross-section for \( K + A \) at the threshold for \( K + \Sigma \). (They argue that the \( K^0 + \Sigma^0 \) threshold is really the relevant one, rather than that for \( K^+ + \Sigma^- \).)

The state in which the phenomenon occurs evidently has \( I = \frac{1}{2} \), as \( K + A \) must have. Since at threshold for \( K + \Sigma \), we need consider only the \( s_{1/2} \)-state of that system, we see that the corresponding state for \( K + A \) is \( s_{1/2} \) or \( p_{1/2} \), depending on whether the relative parity of \( A \) and \( \Sigma \) is even or odd. In the state with this isotopic spin, angular momentum and parity, let us consider the \( S\)-matrix, which, with suitable phases for the states, is symmetric as well as unitary. We label the relevant channels \( N \) (for \( \pi + N \)), \( A \) (for \( K + A \)), and \( \Sigma \) (for \( K + \Sigma \)), and pay little attention to the rest of the channels, which are, however, not in any way neglected. Let \( k_\Sigma \) be the c.m. momentum in the \( \Sigma \) channel and let \( k_A \) be the c.m. momentum in the \( A \) channel when \( k_\Sigma = 0 \) (\( K + \Sigma \) threshold). When \( k_\Sigma \) is very small, the matrix elements \( S_{N2}, S_{A2}, S_{\Sigma2} \) etc. are all proportional to \( \sqrt{k_\Sigma} \) (and the corresponding cross-sections proportional to \( k_\Sigma \)), as is well known. Put \( S_{N2} = b_{N2}\sqrt{k_\Sigma} \), etc. Then the theorem states that for small \( k_\Sigma \) we have

\[
S_{NA} = S_{NA} \text{(threshold)} + b_{N2}b_{A2}k_\Sigma,
\]

\[
S_{NN} = S_{NN} \text{(threshold)} + b_{N2}b_{N2}k_\Sigma,
\]

These terms in \( k_\Sigma \) produce cusps in the cross-sections for \( \pi + N \rightarrow K + A \), \( \pi + N \rightarrow \pi + N \), etc. The authors concentrate on the first reaction. For this process, let \( g \) be the non-spin-flip amplitude and \( h \) the spin-flip amplitude as functions of energy and angle. Then of course we have

\[
\frac{d\sigma}{d\Omega} = |g|^2 + |h|^2
\]

and

\[
P = \text{Im} g \* h \left[ |g|^2 + |h|^2 \right]^{-1}
\]

for the differential cross-section and \( A \) polarization respectively.

If the parity of \( A \) and \( \Sigma \) is the same, then \( g \) acquires near the \( \Sigma \) threshold the term \( b_{N2}b_{A2}k_\Sigma/4i\kappa = A \) and \( h \) acquires nothing. If the parities are opposite, then \( g \) and \( h \) pick up respectively the terms \( A \cos \theta \) and \( A \sin \theta \). The difference can be illustrated in the case where just \( s\)- and \( p\)-waves occur in the \( K + A \) reaction, so that \( g \) contains \( 1 \) and \( \cos \theta \) terms and \( h \) is proportional to \( \sin \theta \). Then if \( A \) and \( \Sigma \) parities are equal, the angular distribution of the "peculiarity" contains no \( \cos^2 \theta \) term, while if they are different such a term will in general occur.

The suggestion of Baz and Okun', if it can be carried out in practice, can yield not only a proof of the relative parity of \( A \) and \( \Sigma \), but detailed information about the amplitudes \( g \) and \( h \). It may also help to distinguish between theories in which \( K \) couplings are somewhat weaker than \( \pi \) couplings and a theory of Okun', in which certain \( K \) couplings are very strong.

**LIST OF REFERENCES**

2. Baz, A. I. and Okun', L. B. (to be published in Zh. eksper. teor. Fiz.)
Miyazawa: I would like to ask Gell-Mann if he applied his global symmetry model to this production case, and if he found something.

Gell-Mann: No.

Cocconi: These cusps could fake the results of Cornell where we saw the second jump in correspondence to the threshold of the $\Sigma^0$. If this cusp corresponds to that energy, could it not be that the increase is due to the cusp and not to the production of the $\Sigma^0$?

Gell-Mann: They have to go together, of course.

Cocconi: This means then that it may be that the ratio between the cross-section of $\Sigma^0$ to $\Lambda^0$ is smaller than the ratio we see experimentally?

Gell-Mann: I think that it is possible that we see indeed a mixture between the production of the $\Sigma^0$ and the cusp in the production of the $\Lambda^0$. I think this could be a serious problem in the analysis, but I do not know very much about the experiments.

Low: I do not understand how a $\cos^2\theta$ term can be associated with a $p_{3/2}$-state.

Gell-Mann: I will explain. We may consider the production amplitude to be of the form $g + \Delta g$ for non-spin-flip and $h + \Delta h$ for spin-flip where $\Delta g$ and $\Delta h$ are proportional to the momentum $k$ in the new process. So $\Delta g$ and $\Delta h$ vanish at threshold and their interference with $g$ and $h$ gives the cusp. In calculating the angular distribution altogether we take the squares

$$|g + \Delta g|^2 \quad \text{and} \quad |h + \Delta h|^2$$

and so we get interference terms between $g$ and $\Delta g$, $h$ and $\Delta h$. If the cusp occurs in the $p$-wave, then we can pick up a $\cos \theta$ from the $g$ and another one from the $\Delta g$, so that in the interference term we get $\cos^2\theta$.

Low: Is it not so though that for the $p_{3/2}$-wave the coefficient of $\Delta h$ is the same as the one for $\Delta g$ and the coefficient of $h$ the same as for $g$?

Gell-Mann: No, not the same for $h$ and $g$. It is true that the coefficient of $\cos \theta$ in $\Delta g$ is the same as the coefficient of $\sin \theta$ in $\Delta h$, since they both come from $p_{3/2}$ only. But in $g$ and $h$ we can get contributions also from the next state $p_{1/2}$.

Adair: Perhaps this is the point to drop the discussion, but I still do not understand this. If you have only $s_{1/2}$ and $p_{3/2}$-waves, I do not see how you can have under any circumstance a $\cos^0\theta$ term.

Gell-Mann: I did not say that you have only $s_{1/2}$- and $p_{3/2}$-states, I said that you have $s$- and $p$-waves only. But this means $s_{1/2}$, $p_{3/2}$ and $p_{1/2}$, and that the peculiarity in the amplitude occurs in the $p_{3/2}$-state. The $\cos^0\theta$ arises from the interference between the $p_{3/2}$ peculiarity and the $p_{1/2}$ non-peculiarity.

Adair: This is the situation in which you just have these three amplitudes, of $s$- and $p$-waves. If you only have $s_{1/2}$ and $p_{3/2}$, then this $\cos^0\theta$ term will vanish so to speak accidentally. This can always happen in any such test. If you have $d$-waves the angular distribution analysis indicates $d$-waves and it is necessary to re-do the problem. But there is then again a difference, in general, between one case and the other.

Adair: If you have a Minami-type ambiguity you might not be able to tell the difference between $s$ and $p$. However, I agree with Okun' in practice.

Gell-Mann: You have to consider the energy dependence as well as the angular dependence.

Bernardini: Considering the importance of determining the $g_{K\Lambda}$ by this generalisation you mentioned of, say, the Kroll-Ruderman theorem, I would like to ask whether it would be possible in that case to correct properly for the recoil terms, as you can do at least in the first order in the ratio of the masses with the Kroll-Ruderman theorem.

Gell-Mann: I think we could discuss this in a slightly different language. This theorem could perhaps be discussed without reference so much to the masses as to a plot in energy and momentum transfer. In that case the theorem again holds without any reference to the mass being small. But the larger the mass the more difficult the problem of extrapolation down to the place where the pole is. If the mass is small the pole occurs not too far from the physical region of energies and angles. (Essentially an extrapolation by $\approx 140$ MeV.) If the mass is large you have to perform a much longer range, much more difficult and treacherous extrapolation. But I think that if a lot of accurate data are obtained (in ways which must be investigated theoretically to see what is the best thing to plot), then it is not impossible that one can perform an extrapolation even with a relatively large mass. This is especially true of the term which involves magnetic moments times the coupling constants.

Marshak: I think it is very important that Gell-Mann pointed out the role of the transition moment in the photoproduction of the $K$'s since this explains the differences in the published literature in connection with the angular distributions. My first question is this: would the number you wrote down for the magnetic moment on the basis of global symmetry also imply zero moments for the charged $\Sigma$'s, or only equal but opposite values? There is, of course, the general theorem that the neutral magnetic moments must be equal to half the sum of the charged moments. My second question is: is it not
true that if you neglect the mass difference between the \( \Sigma \) and \( A \) then, in the limit of global symmetry, this transition moment is also zero or the same essentially as the \( A \) or the \( \Sigma \)?

**Gell-Mann:** The results are as I have said them, that is: the transition moment does not vanish, but is equal to minus the neutron magnetic moment, in the limit of neglecting all the corrections (mass differences, \( K \) particle effects). The \( \Sigma^+ \) moment is the same as the proton, the \( \Sigma^- \) moment the same as the antiproton. So \( \mu_{\Sigma^-} = -\mu_{\Sigma^+} \) and that fits in with the theorem. I do not know what good this is, but if global symmetry should be right, these may have some relevance to the actual values.

**Biedenharn:** Would one not expect that there would be a linear term in \( \cos \theta \) because the interference that occurs comes about because the energy shift factor is discontinuous across the boundary, and if there is such a term it might be more easily discernable, is it not so?

**Gell-Mann:** Yes. I should perhaps have devoted more time to this. I was not exactly sure about how much people wanted to hear. In the paper by Baz and Okun' the thing is discussed very fully and nicely. The \( \cos \theta \) term certainly is there, but I think that with the special model that I took, namely only \( s \)- and \( p \)-waves, the \( \cos \theta \) term does not serve to distinguish one parity from the other but it should be conspicuous in the effects.

**Biedenharn:** The question whether you have a cusp or an \( S \)-shape has a very specific answer when you take a model. If you take one level then you get a one-sided cusp as Prosser and myself have shown.

**Adair:** I guess for the sake of completeness in the discussion of cusps, one should point out that one can expect cusps in \( K \) meson photoproduction in just the same way as Baz and Okun' have pointed out for \( \pi \) production and that one can expect cusps again in the photoproduction of \( \pi \) mesons at the threshold for production of \( K \)'s and \( \Sigma \)'s. In principle then if you can measure the angular distribution of these cusps and you have a little intuition whether you can separate \( s \)-waves from \( p_{3/2} \)-waves, which of course is not trivial, you have a chance of measuring the \( K \) meson parity.
CLASSIFICATION AND SYMMETRIES OF STRONGLY INTERACTING PARTICLES

B. d'ESPAGNAT, Rapporteur
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Assuming Yukawa interactions between baryons and mesons—and charge independence—the most general strong interaction Lagrangian is, as is well-known,

\[ \mathcal{L} = g_1 \bar{N} \tau N \pi + g_4 \bar{\Sigma} \tau \Sigma \pi + g_5 \bar{\Sigma} \tau \Sigma \cdot \pi + g_6 \bar{K} \Lambda + g_7 \bar{K} \Sigma + g_8 \bar{K} \Lambda \Sigma (1) \]

with \( \gamma \) matrices being omitted. Several authors have attempted to make this \( \mathcal{L} \) more definite by reducing the somewhat impressive amount of independent coupling constants and we should review now some of their most recent proposals.

As you know already, from former attempts, two approaches can be used: either the cautious one, which assumes as little as possible and tries to stick to experiment at every step, or the ambitious one which starts from an idea, deduces many symmetries from it and then compares with actual data. The first one is unquestionably the better, except that unfortunately its yield is very poor. At any rate we shall begin our review by the most cautious approaches and then go step by step to the more ambitious models.

I feel that in this spirit I should begin with Pais' recent remarks. Pais' first paper on this subject is, however, already published so that I shall only recall the essential results of it, which are as follows. Let us assume that

\[ g_5 = \pm g_3; \quad g_6 = \pm g_4; \quad g_7 = \pm e g_8 \] (2)

with correspondence of signs, then Pais shows that, to the extent that the \( \Lambda - \Sigma \) mass difference is small compared to baryon mass, this assumption is incompatible with the data on \( \Sigma K, \Lambda K \) pair production with pions on nucleons.

In a second paper, Pais shows that unfortunately none of the other possible assumptions lead to relations between the various production amplitudes (except of course to triangular inequalities): in particular, one cannot exclude in this way all the symmetries (2) which would not have the given sign correspondence \( +++ \) or \(- - -\).

Inequalities may, however, have their importance: in this connection I should like to mention here a paper by Amati and Vitale who consider \( K^-, p \) reactions with the only assumption that \( g_2 = \pm g_3 \) (renormalized) and that \( K \) interactions are rather weak.

For a review of this paper the reader is referred to Dalitz' report p. 197.

Returning to Pais' work, its great value is obviously that it postulates a priori really very little. It follows, however, from Pais' second paper that this cautious approach cannot, unfortunately, lead us very far: in fact all it could do was to exclude case (2) out of infinitely many possible cases. We might, of course, stop here but if we want to go further we must necessarily resort to assumptions.

Now one assumption that has been proposed by several people is that the baryon bare masses are all equal. Let us have a little glance at this. In fact we shall only need the weaker assumptions

\[ a) \ m_N = m_\Sigma \]
\[ b) \ m_\Lambda = m_\Sigma \] (3)

Under assumption \( b) \) case (2) is of course trivially excluded, because the whole Lagrangian is then 4-dimensionally invariant with \( \Sigma, \Lambda \) as a 4-vector, and thus no \( \Lambda \Sigma \) mass splitting can occur; but \( a) \) also gives us some indications. As a trivial example let us mention the simple cases

\[ g_1 = g_4; \quad g_5 = e g_7; \quad g_8 = e g_6 \] (4)

with

\[ e = \pm 1 \] (5)

These can be most simply excluded using the transformation...
Strange particle production

\[ N \rightarrow \Sigma, \quad \pi \rightarrow \pi \quad K \rightarrow \hat{K}, \]

\[ (\Sigma A) \rightarrow \epsilon (\Sigma A) \]  \tag{6}

which leaves the whole \( \Psi \) invariant while changing \( N \) into \( \Sigma \) and conversely. \( \Psi \) cannot therefore induce any \( N, \Sigma \) mass difference.

Nothing, of course, tells us that (4) holds. Now another use of transformation (6) and of similar substitutions could be simply to classify the strong interactions into a part that remains invariant under them and a part that changes its sign: this for further theoretical use. Before we embark on this however, I shall make the trivial remark that instead of transformation (6) we might just as well have used its product with any isotopic spin rotation—which leaves the Lagrangian invariant anyhow—and in particular with a 180° rotation around the \( I_2 \) axis. This gives

\[ N \rightarrow -i \tau_2 \Sigma, \quad K \rightarrow -i \tau_2 \hat{K} = -K^*, \]  \tag{B}

\[ \pi_i \rightarrow (-1)^i \pi_i \quad i.e. \quad \pi \rightarrow -\pi^*, \quad \pi^2 \rightarrow \pi^0 \]

\[ \Lambda \rightarrow \Lambda, \quad \Sigma^+ \rightarrow -\Sigma^-, \quad etc... \]

For practical purposes (B) is quite equivalent to (6): its one formal advantage is that the two bosons, \( \pi \) and \( K \), behave similarly under it. We already know that the full Lagrangian cannot be invariant under (B) if bare masses are equal but as I said we want to use (B) for a classification of the interaction terms, hoping that this will lead us to natural assumptions.

Of course, such a classification would look more highly promising for our purpose if, instead of being just a mathematical substitution, some physical interpretation could be given to it. This is precisely what is provided by the contribution of Budini, Dallaporta and Fonda 4. They consider to that end a kind of compound model where we have as fundamental particles

- A baryon \( A_0 \) with no isotopic spin, hypercharge, and charge

\[ I = U = Q = 0 \]

- The \( K \) meson

- The \( \pi \) meson

From these the known particles are obtained by clothing with \( K \) and \( \pi \): for instance they could use

\[ N = A_0 K, \quad \Sigma = A_0 \hat{K}, \quad \Sigma^+ = A_0 \pi, \quad \Lambda = A_0 \]

In such a scheme it appears natural to split the charge conjugation \( C \) into a product of two operations, one, \( B \), being roughly speaking a charge conjugation of the boson field only, the other, \( S \), being a particle antiparticle conjugation acting on \( A_0 \) only. Mathematically one chooses

\[ K \rightarrow K^*, \quad \pi \rightarrow -\pi^*, \quad \Lambda_0 \rightarrow \Lambda_0 \]

then

\[ \Sigma^+ \rightarrow -\Sigma^- \quad etc. \]

\[ N \rightarrow -A K^* = -i \tau_2 A \hat{K} = -i \tau_2 \Sigma^c \]

so that this is indeed just our former (B), but now with a physical interpretation. Then \( B \cdot S = C \) gives

\[ A_0 \rightarrow A_0^c, \quad K \rightarrow -K, \quad \pi \rightarrow -\pi \]

\[ N \rightarrow -A_0^c K = -i \tau_2 A \hat{K}^* = -i \tau_2 \Sigma^c \]  \tag{S}

(With the general definition \( \chi^c = C^{-1} \chi^T \)).

In the author's idea, their compound model should just be used in order to introduce the \( B \) and \( S \) (boson conjugation and spinor conjugation) in a natural and so to speak physical way. Once they have thus entered the picture it becomes believable that they play a role in nature, though what this role exactly is we, of course, do not know. Quite tentatively they suggest that the Lagrangian (1) should be invariant under \( B \) and \( S \) separately. Then the \( N, \Sigma \) mass-splitting should be attributed to some non-Yukawa interaction, for instance to a (baryon, baryon, \( K, K \)) direct interaction.

At this point I would tentatively insert a small remark 5. It was shown by Zel'dovich, Feynman and Gell-Mann and others that the representation

\[ \chi = a N \]

\[ \hat{\chi} = \bar{a} N \]

with

\[ a = \frac{1 + \gamma_5}{2}, \quad \bar{a} = \frac{1 - \gamma_5}{2} \]

is particularly suited to the description of weak interactions, because with \( V-A \) coupling, \( \hat{\chi} \) does not enter. The same of course holds if we change \( \chi, \hat{\chi} \) to

\[ \chi = a N + \bar{a} i \tau_2 \Sigma^c \]

\[ \hat{\chi} = \bar{a} N + a i \tau_2 \Sigma^c \]

provided we request invariance under \( \chi \rightarrow e^{i \theta / 2} \chi \), i.e. baryon conservation. Now in terms of \( \chi \) the "boson conjugation" \( B \) takes the simple form

\[ \chi \rightarrow -\chi^c \]  \tag{B}

In full analogy with what it is for bosons. The product of the "spinor conjugation" \( S \) with ordinary parity \( P \) takes also a simple form

\[ \chi \rightarrow -i \gamma_5 \chi, \quad \pi \rightarrow \pi, \quad K \rightarrow K \]  \tag{S.P}

if \( K \) is pseudoscalar.
From (7) it is obvious how in the new representation $\gamma$, one should write the B and S conserving terms; they are just those which do not involve a $\gamma_5$. (It may further be pointed out that a PC invariant Lagrangian, see below, could similarly be split into B and (S,P) separately conserving and non-conserving terms.) Thus with the $\gamma$ representation both weak and strong interactions take rather simple forms while B and S conjugations are straightforward and endowed with a kind of physical interpretation as reported. Whether or not these facts are an indication of any deep-lying symmetry, it is of course much too early to judge.

Votruba and Lokajíček have followed a different line. They consider two sets of vector matrices $\omega$ and $\lambda$

in a 3-dimensional isospace, which have close analogies with the $\gamma (\beta)$ and $\sigma$ matrices in ordinary space time. They subject them to a matrix algebra which is too complicated to be transcribed here, but which has the property that its only representations that involve irreducible non-zero $\omega$ matrices are

a) $\lambda = \frac{T}{2}$, $U = \frac{1}{2} \omega \cdot \lambda = 1$

b) $\lambda = \frac{1}{2}$, $U = -1$

c) $\lambda = \left(\begin{array}{cc} T & 0 \\ 0 & 0 \end{array}\right)$, $U = 0$

T being the spin one matrix. These, of course, they put in correspondence respectively with

a) $N;K$

b) $\Xi;\bar{K}$

c) $\Sigma, \Lambda; \pi, \pi'$

and, these representations of their matrix algebra being the only non-pathological ones, they are thus able to limit the number of possible baryons and mesons. This, I think, is their essential result. As far as interactions are concerned I would, however, mention the fact that they also are led to transformations which are practically identical with B and S in a very natural way.

Finally, the negative most recent Berkeley results on backward-forward asymmetry in $\Lambda$-decay make it unnecessary that we should dwell much on possible P and C non-conservation in strong K interactions. However, I would like to mention the fine theoretical point made both by Soloviev and Drell that, with non-gradient couplings, the assumption of PC conservation, together with that of charge independence, unambiguously lead to separate P and C conservation but in the $\pi$ interaction terms only, not in the K interaction terms. A theory based on such premises would therefore accommodate, and according to Drell even predict, a polarization of the $\Lambda$ in the production plane and, therefore, a front-back or left-right asymmetry of the decay pions.

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SESSION 6
Thursday, 3rd July, 1958

Strange particle interactions

Chairman D. I. BLOKHINTSEV

EXPERIMENTAL
Rapporteur M. F. KAPLON
Secretaries J. A. NEWTH
R. D. FORTUNE
C. VERKERK

THEORETICAL
Rapporteur R. H. DALITZ
Secretaries K. GOTTFRIED
P. SERGENT
L. WOLFENSTEIN
INTRODUCTION BY THE CHAIRMAN

We now start the afternoon session, the main topic of which is the strange particle interactions. We have many contributions from the different groups working in this field. It is not a simple task for Kaplon and Dalitz to cover all of this field; yet we may hope that it will be possible for them to do so at a pace less than is usual in gangster films.

The session is arranged in the following way: the experimental part will be reported by Kaplon from Rochester. Then we shall have a small but important contribution from Tripp. After this there will be questions only, followed by Dalitz who will report the theoretical part of the session. The discussion, which will be common to all three reports, will take place at the end of the session.

Kaplon, may I ask you to give your report.
Strange particle interactions

Chairman  D. I. Blokhintsev

EXPERIMENTAL
Rapporteur  M. F. Kaplon
Secretaries  J. A. Newth
            R. D. Fortune
            C. Verkerk

THEORETICAL
Rapporteur  R. H. Dalitz
Secretaries  K. Gottfried
            P. Sergent
            L. Wolfenstein
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Kaplon, may I ask you to give your report.
I should like to preface my remarks with an expression of deep appreciation for the many people who have contributed their time and effort in an attempt to both crystallize the experimental situation of the topics in this session and in the compilation and preparation of data subject to such handling. Any clarity resulting from this is due to them—any failure my responsibility. In particular, I should like to thank the secretaries for the experimental part of this session for their over-all assistance and contributions to discussions; Dallaporta and Guerriero for their aid in compilation and assessment of the $K^+$-data; Ceccarelli and Winzeler for their compilation of the $K^-H$ information from emulsion, and many people, particularly Dilworth, Burhop and Eisenberg for their patience and contributions in discussing the $K^-$-nucleon interaction; Tripp for his invaluable assistance in the $K^-H$ data from the Berkeley bubble chamber; Levi-Setti for the hyperfragment data and Fry for his recent results on neutral $K$ interactions. The opportunity to discuss the general scope of this report with Blokhintsev and Dalitz and their suggestions are deeply appreciated.

Before getting to the meat of the matter I should like to state very briefly the viewpoint we have adopted—essentially it is that theorists are naïve where experimental results are concerned. With this philosophy we have decided to emphasize those experimental results that seem quite clear and are relatively free from detailed model considerations. In this context I must apologize to certain contributors for not presenting their complete contribution but this is, I believe, my prerogative. Anyhow, on with the show.

The subjects to be presented are $K^+$ interactions, $K^-$ interactions, neutral $K$ interactions and hyperon interactions in that order. In referring to specific contributions I shall refer to the group, rather than to individuals. The experimental situation to date I should like to summarize briefly as follows. The isotopic multiplet structure of Gell-Mann and Nishijima for the mesons and baryons (at least up to and including the $\Sigma^-$ and $\Lambda^0$-hyperons) and the conservation laws for strong interactions, i.e. conservation of minimal electromagnetic interactions, have withstood the assault of time (which includes the content of this report). It appears that the $K$ mesons are bosons of unknown parity (unknown both now and to my knowledge also at the conclusion of Dalitz’s summary) and the hyperons are fermions, most probably with spin $\frac{1}{2}$. The description of the neutral $K$ particles in terms of the Pais—Gell-Mann picture seems in accord with experimental observations to date and there is abundant evidence that there exist attractive $\Lambda^0$-nucleon forces and some suggestion of similar forces for the $\Sigma^+$ hyperons.

(I) $K^+$ Interactions

I shall begin as promised with a discussion of $K^+$ interactions. The principal experimental observations to date have come from nuclear emulsions and the new results exclusively arise from that medium. I shall begin by presenting a compilation of $K^+H$ elastic scattering measurements made in emulsion. The results are necessarily averaged over finite energy intervals, these intervals being somewhat arbitrarily chosen. Fig. 1 gives the total cross-section as a function of finite energy intervals, these intervals being somewhat arbitrarily chosen. Fig. 1 gives the total cross-section as a function of $K^+H$ elastic scattering measurements made in emulsion. The results are necessarily averaged over finite energy intervals, these intervals being somewhat arbitrarily chosen. Fig. 1 gives the total cross-section as a function of $K^+H$ elastic scattering measurements made in emulsion. 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The results are necessarily averaged over finite energy intervals, these intervals being som
fortunately we are unable to obtain any information about the sign of the $K^+$-proton potential from the above data. More of this in a moment, however.

The $K^+$ nuclear scattering has to date been fairly informative. The nucleus contains neutrons in addition to protons and with our knowledge of the $K^+ - p$ scattering we might hope to obtain some information about the $K^+ - n$ scattering. In emulsion $K^+$ mesons are seen to decay both in flight and at rest (the decays do not concern us here), to scatter elastically and inelastically and to scatter with charge exchange. The inelastic scattering is characterized most strongly by the fact that the relative (kinetic) energy loss $\Delta T_K/T_K$ is small, particularly so at low $K^+$ energies. The striking thing is the correlation of $\Delta T_K/T_K$ with respect to the laboratory scattering angle. Fig. 2, from the work of the Padua group, shows the correlation of $\Delta T_K/T_K$ with $\theta$. For comparison the full line shows the dependence expected for interaction with a stationary nucleon. The dependence on $\theta$ is less marked than for interaction with a stationary nucleon. Fig. 3, also from the work of the Padua group, shows $\Delta T_K/T_K$ vs $T_K$ and here the energy dependence shows a decrease of inelasticity with decreasing energy. This data, which is well supported by that of many other groups strongly suggests a repulsive $K^+$-nucleon potential. Additional evidence relating to this potential appears in the literature and from the results of an optical model analysis of the elastic scattering. Experimentally the elastic nuclear scattering interferes constructively with the Coulomb scattering but at lower energies (40-100 MeV) the optical model analysis cannot differentiate between a repulsive or an attractive real part of the potential. However, the attractive potential obtained is so large ($\sim -45$ MeV) as to be inconsistent with the low $\Delta T_K/T_K$. At higher energies, $\langle T_K \rangle = 150$ MeV, the optical model analysis uniquely predicts a repulsive nucleon potential. At both energies the real part is $V_N + V_C \approx 25 \pm 5$ MeV while the imaginary part is $-10.7 \pm 1.4$ at $\langle T_K \rangle = 150$ MeV and $-5.7 \pm 1.1$ for the interval 40-100 MeV.
Strange particle interactions

To proceed to obtain information about the \( K^+ - n \) interaction there are two paths open; the first and least informative involves working from the optical model analysis alone from which one obtains the average cross-section per nucleon from the imaginary part of the potential, and the forward scattering amplitude (averaged over protons and neutrons) from the real part of the potential. Further deductions utilizing the known \( K^+ - p \) data involve assumptions which are not necessary if the second route is taken. This second procedure has been explored by the Padua group in great detail; their work comprises the principal part of the following.

They have attempted to obtain the \( K^+ - n \) cross-sections by a detailed analysis of the inelastic scattering utilizing the known \( K^+ - p \) scattering results. Basically two assumptions are made: 1) Each inelastic event is due to a single \( K^+ \)-nucleon collision; the justification for this rests upon the observation that the mean free path in nuclear matter deduced from the \( K^+ - n \) scattering is \( \geq 11 \) f for the energy interval under consideration. 2) The \( K^+ \) and nucleons are considered as being dynamically free in a rectangular well of suitable depth; \( +15 \) MeV for \( K^+ \) (in addition to the Coulomb potential), and for the nucleons a Fermi model with \( P_{\text{max}} = 241 \) MeV/c corresponding to a nuclear radius \( R = 1.25 \) f is assumed.

For a given scattering angle in the c.m. system the angle and energy loss distributions to be expected in the laboratory system have been calculated taking into account the conservation laws and a complete set of possible values for unobserved parameters and taking due account of the Pauli principle. Thus \( d\sigma/dQ_{\text{lab}} \) and \( \Delta T_K/T_K \) distributions are obtained corresponding to any angular distribution \( \sigma_K(\theta) \) in the c.m.s. The analysis is restricted to events with \( T_K > 80 \) MeV and \( \Delta T_K/T_K > 20 \% \) in order to reduce experimental uncertainty derived from the latter (i.e. confusion with elastic scattering).

A c.m. angular distribution \( d\sigma/d\Omega = (a + b \cos \theta)^2 \) was assumed and \( \chi^2 \) calculation performed for three different values of \( V_N - V_C \) vs \( b/a \) for two energy intervals. Fig. 4 shows the probability that the experimental distribution fits the given parameters for the range indicated. It is seen not to be too sensitive to the value of \( V_C + V_N \) and \( V_C + V_N = 25 \) MeV \( (V_N = 15 \) MeV\) as the best fit and \( b/a \approx -5/8 \). It seems clear that an isotropic distribution is extremely unlikely and since \( d\sigma/d\Omega(K^+ - p) \) is observed to be isotropic the anisotropy must be attributed to the \( K^+ - n \) interaction. Since the \( K^+ - p \) interaction is purely I-spin 1 and if we assume charge independence as we do, the anisotropy must be due to the \( I = 0 \) isotopic spin state. Assuming \( d\sigma/d\Omega_{\text{cm}}(K^+ - p) = 1/4\pi (14.5 \pm 2.2) \) mb the distribution of events due to \( K^+ - p \) scattering subtracted taking into account all effects and a resulting distribution due to \( K^+ - n \) inelastic scattering is obtained and a value of \( b/a_n \approx -5 \) is found as the most probable value. When all statistical uncertainties are folded in, it is found that \( b/a_n < -1 \) with a 90\% confidence limit.

Once the angular distribution in the c.m.s. is known then, and only then, can the \( \sigma_{K^+ n} \) (free) be evaluated since the effect of the Pauli principle is \( \theta \) dependent, biasing against forward scattering and favouring backward. It is found that \( 5.4 \leq \sigma_{K^+ n} \) (tot) \( \leq 6.1 \) mb for \( -1 \geq b/a \geq -5 \).

At this point one can attempt an analysis to deduce some information about the scattering amplitudes. The following assumptions were made.

1. The amplitudes are real, (i.e. the phase shifts are small).
2. \( a_{11} = 0 \), i.e. \( K^+ - p \) is isotropic (the first subscript is the total I-spin \( T \) and the second the angular momentum \( J \)).
3. Spin-flip scattering was neglected (i.e. \( P_{1/2} = P_{3/2} \)).

![Fig. 4. The angular distribution of \( K^+ \)-nucleon scattering. The curves show the results of \( \chi^2 \) tests to fit the experimental data with a c.m.s. angular distribution of the form \( \frac{d\sigma}{d\Omega} = (a + b \cos \theta)^2 \) and different nuclear potentials. (Padua group) ](image)

Table I shows the three reactions and the differential cross-section with assumptions made in terms of the amplitudes \( a_f \). In the actual evaluation the known results on charge-exchange scattering were folded in (which are Pauli principle sensitive) and the values obtained for \( \langle T_K \rangle_{\text{lab}} = 110 \) MeV are given in the table.

These results correspond to \( b/a_n = -1.2 \) and imply a charge-exchange cross-section which practically vanishes at \( \theta_{\text{cm}} = \pi \) and is peaked strongly forward.

One can still be slightly worried about the seemingly low ratio of charge-exchange to non-charge-exchange \( (\approx 0.1) \) observed at lower energies \((\approx 40-80 \) MeV\). This may be a result of poor statistics, but is somewhat hard to explain.
in terms of the above model from a preliminary qualitative look. It would appear that more work both experimentally and of the above nature should be undertaken at lower energies.

**TABLE I**

Phase shift analysis of $K^+$-nucleon scattering.

<table>
<thead>
<tr>
<th>Possible reactions</th>
<th>Cross-section analysis</th>
</tr>
</thead>
<tbody>
<tr>
<td>$K^+ + p \rightarrow K^+ + p$ (I)</td>
<td>$\frac{d\sigma}{d\Omega} = \Lambda^2 (\alpha + \beta \cos \theta + \gamma \cos^2 \theta)$</td>
</tr>
<tr>
<td>$K^+ + n \rightarrow K^+ + n$ (II)</td>
<td>Total cross-section: $\sigma_{(tot)} = 4\pi \Lambda^2 (\alpha + \frac{1}{3} \gamma)$</td>
</tr>
<tr>
<td>$K^+ + n \rightarrow K^n + p$ (III)</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>(I)</th>
<th>(II)</th>
<th>(III)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\alpha = a_{11}$</td>
<td>$\alpha = \frac{1}{4}(a_{10} + a_{00})^2$</td>
<td>$\alpha = \frac{1}{4}(a_{10} - a_{00})^2$</td>
</tr>
<tr>
<td>$\beta = 0$</td>
<td>$\beta = \frac{1}{4}(a_{10} + a_{00})a_{01}$</td>
<td>$\beta = \frac{1}{4}(a_{10} - a_{00})a_{01}$</td>
</tr>
<tr>
<td>$\gamma = 0$</td>
<td>$\gamma = \frac{1}{4}a_{01}^2$</td>
<td>$\gamma = \frac{1}{4}a_{01}^2$</td>
</tr>
</tbody>
</table>

Results of analysis:

a) Padua group $\langle T_K \rangle = 110$ MeV

$\delta_{10} = -23^\circ$  $a_{10} = -0.39 \pm 0.03$

$\delta_{11} = 0^\circ$  $a_{11} = 0$ (hypothesis)

b) UCLA results $\langle T_K \rangle = 105$ MeV

$\delta_{10} = -25.3^\circ$  $\delta_{00} = -13^\circ$

$\delta_{11} = 0$ (hypothesis)  $\delta_{01} = \sim 18^\circ$

$\delta_{12} = 0$ (hypothesis)  $\delta_{02} = \sim 15^\circ$

In the Padua group analysis $a_1$ was taken -ve as $K^+$-$p$ potential was assumed repulsive from $K^+$-$p$ nuclear scattering measurements.

A preliminary analysis by Prowse and collaborators (UCLA) at a somewhat higher energy $\langle T_K \rangle = 150$ MeV assuming again $a_{11} = 0$, $a_0$ negative and taken from $K^+$-$p$ data and using the observed charge-exchange cross-section, the $\sigma(\theta)$ at $\pi$ and $\pi/2$ and limits on $f(\theta)$ from an optical model analysis plus some reasonable and educated rejections of possible solutions arrives at quite similar phase-shifts. The UCLA results are shown for comparison in Table I.

To proceed to higher energies, $T_K > 200$ MeV and up to $\sim 350$ MeV, there is a preliminary optical model analysis by Prowse and collaborators on the elastic scattering. They find the best fit to date is $V = (+13-i19)$ MeV. The point to note here is the increased imaginary part of the potential which implies an increasing $K^+$-nucleon scattering cross-section. The situation at the higher energies is both simplified in some aspects and complicated in others. On the one hand, the higher energy reduces the importance of the Pauli-principle at forward scattering angles but the indication of increased cross-section leads to the added implication of secondary scattering inside the nucleus.

The next striking phenomenon observationally is the increase in charge-exchange scattering and the incidence of $\pi$ meson production. In Fig. 5 we present compilations of $\sigma_{K^+N}$ (total scattering uncorrected) as well as the total charge-exchange and inelastic scattering cross-sections, all uncorrected, as a function or energy.

In Fig. 6 the corrected data are shown. At the lower energies appropriate corrections have been made which have not been done as carefully at the higher energies due to lack of time, though corrections have been made for Coulomb repulsion. It is seen that the $\sigma_{K^+N}$ (no charge exchange) increases with energy and then tends to level off. At the same time is shown the variation of $\sigma$ (CE), with energy. This has to be taken with a grain of salt since here again at higher energies the effect of the Pauli principle and double scattering are not taken into account (it is estimated that double scattering at this energy would change a value of $\sigma_{CE}/\sigma_{nonCE}$ of 0.2 to $\sim 0.3$; this assumes only isotropic $T = 1$ state). If one then just goes blindly ahead and subtracts $\sigma_{K^+p}$ from $\sigma_{K^+N}$ (no CE) to obtain $\sigma_{K^+n}$ (no CE) we have the resultant behaviour of $\sigma_{K^+n}$ (no CE), while $\sigma_{K^+n}$ (CE) increases on up in energy. Since $K^+p$ is still isotropic, the enhancement of the charge exchange must come from the $T = 0$ state. Since the increase must result from $p$-wave we can say that the $T = 0$ state enhancement results from $p$-wave effects and if the reduced optical model potential (from that at lower energies) $V_N$ (real) is taken somewhat seriously, we may infer that this state is attractive which is in agreement with the lower energy analysis.
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Fig. 6. Collected emulsion data on K⁺ scattering by nucleons; corrected cross-sections. The upper graph shows the total K⁺-nucleon cross-section (full line) and the non-charge-exchange cross-section (interrupted line). The lower graph shows the deduced →-neutron cross-sections, non-charge-exchange (triangles) and charge-exchange (squares).

The corrections applied to the data of Fig. 5 are a) Coulomb potential (10 MeV); b) Screening effects (r₀ = 1.25) and c) the exclusion principle (assuming an isotropic cross-section). To obtain the →-neutron cross-sections a (K⁺p) was taken as

\[ 14.5 \pm 2.6 \text{ mb for } T_K < 200 \text{ MeV and } 21.0 \pm 4.5 \text{ mb for } T_K > 200 \text{ MeV.} \]

It is also somewhat interesting that the enhancement in the cross-section sets in at about the threshold for π production (~ 220 MeV for a K⁺ meson on a free nucleon). In 331 m of track of K⁺ with T_K > 200 MeV and in 652 inelastic events, 3 charged π⁺'s have been observed (2π⁻, 1 π⁺) and 2 possible cases of π⁰ (inferred from dynamical considerations). This corresponds to

\[ N(\pi^+)/N(\text{Inel.}) = (0.45 \pm 0.20)\% \]

and

\[ N(\pi^0)/N(\text{Inel.}) = (0.77 \pm 0.35)\%. \]

These figures have not been corrected for secondary nucleon interactions or for re-absorption of π mesons in nuclear matter.

(2) K⁰ interactions

Logically in discussing K mesons we should proceed down the isotopic spin ladder which brings us to neutral K mesons. Here we have only one new but extremely interesting contribution. Fry and his collaborators have done the following experiments at the Bevatron (see Fig. 7).

The purpose of the experiment was to measure the interference effect due to the possible mass difference between K⁰₁ and K⁰₂ mesons. The stacks were exposed below a target bombarded by K⁺ of 750 MeV/c from the Bevatron. C—2 emulsions were scanned for hyperfragments in order to measure the amplitude of the K mode. The preliminary results are

<table>
<thead>
<tr>
<th>Area scanned</th>
<th>Events interpretable as HF</th>
</tr>
</thead>
<tbody>
<tr>
<td>105 cm²</td>
<td>60 events</td>
</tr>
<tr>
<td>77 cm²</td>
<td>15 events</td>
</tr>
</tbody>
</table>

(\(\lambda = 1/r_0^2\) and \(t = \text{time of flight}\))

Of the 75 possible hyperfragments, 51 have connecting tracks with \(L > 10\mu\) and 41 with \(L > 20\mu\) which are clearly hyperfragments.

In the monitoring stack an area of 25 cm² was scanned and only two events found. If events in A and B were due to sprays of K⁰ other than from the CE scattering of K⁺ then the number of hyperfragments expected in the monitoring stack should be greater than that in A or B by a factor of about fifteen, therefore the background can be neglected.
Though no conclusions can be drawn about the mass difference, this experiment is an extremely clear demonstration of \( \Delta S = 2 \) via strong interactions and comprises the strongest support of the Gell-Mann - Pais scheme to date.

(3) \( K^- \) interactions

In this field we have the very interesting results on \( K^- - H \) and \( K^- - D \) interactions from bubble chambers as well as \( K^- - H \) from emulsion. Tripp will report on the \( K^- - D \) at the end of this talk and I will now summarize the \( K^- - H \) interactions starting first with the Berkeley hydrogen bubble chamber data and proceeding to the emulsion data for free protons and then to the emulsion data on nuclear capture.

There is essentially no new data on \( K^- - H \) at rest from the bubble chamber and we shall be concerned with \( K^- - H \) in flight. The principal part (~95%) derives from the 15 in. chamber which has a field of 11 kG. About 4000 \( K^- \) tracks have been observed corresponding to \( \sim 2/3 \) km of track. The events selected were those corresponding to interactions in flight and the effort was put on \( K^- - p \) elastic, \( K^- - p \) inelastic \( \rightarrow \Sigma^+ + \pi^- \) and \( K^- - p \) (charge exchange). No effort was put on \( K^- - p \rightarrow \Sigma^0 \) or \( \Lambda^0 \) in flight. The \( K^- \)'s that came to rest were identified by their interactions and ionization and those which left the chamber after elastic scattering by ionization and kinematics.

In Fig. 8 we show the momentum dependence of the total cross-section for elastic scattering for the momentum interval 50 MeV/c (2.5 MeV) \( \leftrightarrow \) 240 MeV/c (55 MeV) as well as the over-all c.m. angular distribution for this interval. The total cross-sections are based only on “red” events (those \( K^- \) which would have stopped in the chamber if they had not interacted) while the angular distribution contains both red and green (those which would have left if they not interacted). In the total cross-sections Coulomb scattering is incoherently subtracted out and the value portrayed was obtained by extrapolating the angular distribution flat to 0°. Since it is difficult to obtain bias free data below 50 MeV/c, none is presented below that limit.

The angular distributions more finely divided with respect to momentum intervals are shown in Figs. 9 and 10 for the intervals 100-150, 150-200 and 200-240 MeV/c. In these the theoretical Coulomb cross-section without interference is drawn in. The over-all data appear consistent with isotropy in the upper two momentum intervals, while the data in the lowest interval can be fitted with a
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Fig. 11. Cross-sections for the reactions \( K^- + Z^- \rightarrow n^- \). The upper curve shows the cross-section for producing \( E^- \) and the lower that for producing \( E^+ \). Berkeley bubble chamber results.

\[
\sin^6 \theta \text{ distribution. However, the emulsion data do not support this so it may be considered a statistical fluctuation.}
\]

Next we consider \( K^- + p \rightarrow \Sigma^- + \pi^+ \). In Fig. 11 we show individually the cross-sections and data for \( K^- + p \rightarrow \Sigma^- + \pi^+ \) and \( \Sigma^- + \pi^- \) as a function of \( P \) for \( 50 < P < 240 \text{ MeV}/c \). Perhaps the best way to describe these is to say that the ratio \( \Sigma^-/\Sigma^+ \) which at \( P = 0 \) is 2, seems to increase rapidly to 7 at about 100 MeV/c and then levels off to a value of \( \sim 1/1 \); this change is principally due to \( \Sigma^- \) while \( \Sigma^+ \) seems fairly constant with \( P \). The emulsion data of comparable statistics do not support this rapid variation of \( \Sigma^-/\Sigma^+ \) at low \( P \) and this could be attributed to a fluctuation of two standard deviations from the value \( \Sigma^-/\Sigma^+ = 2 \) at zero energy. The angular distributions are shown in Fig. 12 for \( \Sigma^- \) and \( \Sigma^+ \) separately over the entire intervals and are consistent with isotropy.

Fig. 12. Centre of mass angular distributions for the reactions \( K^- + p \rightarrow \Sigma^- + \pi^+ \). The upper curve refers to the production of \( \Sigma^- \) and the lower to that of \( \Sigma^+ \). \( \theta \) is the angle of emission of the \( \Sigma \)-hyperon. Berkeley bubble chamber results.

Next we come to the \( K^- + p \) charge-exchange scattering. Here 15 observations of the mode \( \theta_0 \rightarrow \pi^+ + \pi^- \) have been observed. To obtain the total \( K^- + p \rightarrow \pi^+ + n^- \) one must correct for the unseen \( \theta_0 \) mode (2\( \pi \)°) as well as the long lived \( \theta_0 \) mode. The correction factor is taken as 3 which corresponds to \( 2\pi/2\pi = 1/4 \) and \( \theta_0^2 \theta_0 = 1 \) yielding 45 CE scatterings and a value of \( \text{CE/el. scatt.} = 1/5.5 \) which is remarkably similar to that observed for \( K^- + n \) scattering (but at a higher momentum interval). This value is to be viewed as a lower limit since some of the 660 \( \Lambda_0 \) observed may turn out on further inspection to be \( \theta_0 \). The \( \theta_0 \) observed did not contain a mono-energetic group. From this it is concluded that all the charge-exchange occurred in flight and that \( K^- + p \rightarrow \theta_0 + n \) appears to be energetically forbidden at rest.

Fig. 13. Elastic scattering of \( K^- \) mesons on free protons in emulsion. The total number of events, collected from different groups, is 203.

We can now turn to a comparison with the emulsion compilation of \( K^- - H \) interactions. Here we can see only \( K^- - H \) elastic scattering and \( K^- + H \rightarrow E^- + \pi^+ \). Fig. 13 shows \( K^- + p \) elastic total cross-section from 5 to 130 MeV \( K^- \) energy in the laboratory, and is characterized by a rise from 5 MeV to 30 MeV and then a decrease to \( \sim 100 \text{ MeV} \) and a suggestion of a rise again. In Fig. 14 we show the \( K^- + p \) elastic angular distribution in four energy intervals. It is clear that it is consistent with isotropy at the lower intervals and at the upper intervals one can let one's prejudices dominate, though a statement of consistency with isotropy could not be called wrong. Fig. 15 shows the energy dependence of \( K^- + p \rightarrow E^+ + \pi^- \). It appears to parallel somewhat the elastic scattering at the higher energies but to rise more rapidly at lower energies like a characteristic reaction cross-section. Fig. 16 shows two angular distributions for two energy intervals.
of $\Sigma^+$ in the c.m. system and one can say that it is not inconsistent with isotropy. Fig. 17 shows the energy dependence of the ratio $\Sigma^-/\Sigma^+$ which is consistent with the statement that $\Sigma^-/\Sigma^+ = 2$ from 0 to 20 MeV and then descends to 1 and is quite consistent with the Berkeley bubble chamber data if they are grouped in equivalent momentum intervals. Unfortunately, we cannot from the data so far presented draw any firm conclusions about the angular momentum dependence of the zero-energy $K^-$ capture since the statistics are much too small to rely on the 7:1 $\Sigma^-/\Sigma^+$ ratio in the bubble chamber being significant at 2-10 MeV. Dalitz will comment on this later.

I would like to underline the statement that we have no evidence from the $K^-+p$ elastic scatterings concerning the sign of the potential.

We now come to the observations arising from $K^-$ capture on nuclei. Here I would like to re-emphasize my thanks to the contributors for the amount of time they have spent in discussing this problem with me, and apologise again for my treatment of their work. My feeling is that the principal problem of interest, namely the $K^-+n$ interaction is more clearly treated from the observations of the deuterium chamber. However the evidence from emulsion reinforces this (or vice versa depending on your taste) so let us get to the subject at point. There is a single large contribution from the European Collaboration Group on $K^-$ captures in emulsion at rest and one from the Bern Group on $K^-$ interactions in flight. The detailed reasoning involved in arriving at the conclusions reached by these works gets rather dense and would involve showing and explaining a very large number of slides. The conclusions reached are not surprising a posteriori after seeing the reaction data and the $K^--D$ data which is to be presented and I therefore do...
Strange particle interactions

not feel it justified to go through the arguments except for one or two cases which may have been misleading in the past.

The first concerns the mode of capture at rest. The implications of published work (though this is not required) is that the dominant capture mode is by a single nucleon. The collaboration experiment has analysed in detail 3023 \( K^- \) captures at rest looking in particular at the \( \Sigma \)-hyperon energy spectrum and the \( \pi \) mesons associated with \( \Sigma \)-hyperon emission. Fig. 18 shows the energy spectrum of all \( \Sigma^- \)-hyperons, while Fig. 19 shows the spectrum of those accompanied by \( \pi \) mesons and Fig. 20 that of those not accompanied by \( \pi \) mesons. The cut-off at \( T_{\Sigma} = 60 \) MeV is just that expected from reaction \( K^- + p \rightarrow \Sigma^+ + \pi^- \) for a nucleon with Fermi momentum of 240 MeV/c, while the higher energies can arise only from capture with a more massive body and in which no \( \pi \) mesons are required for momentum conservation. A similar situation holds for the \( \Sigma^- \)-hyperons as shown in Figs. 21 and 22. From these data it appears quite clear that a finite number of captures are not single nucleon captures. The lower limit to the proportion of multi-nucleon absorp-

---

Fig. 18. Energy spectra of \( \Sigma^- \)-hyperons emitted from \( K^- \)-capture stars (\( K^- \) at rest). Fig. 18 shows the spectrum for all \( \Sigma^+ \)-particles. Fig. 19 that for \( \Sigma^+ \)-particles accompanied by \( \pi \) mesons and Fig. 20 that for \( \Sigma^- \)-particles unaccompanied by \( \pi \) mesons. Emulsion results from the "European collaboration".

Fig. 19.

Fig. 20.

Fig. 21 and 22. Energy spectra of \( \Sigma^- \)-hyperons emitted from \( K^- \)-capture stars (\( K^- \) at rest). Fig. 21 gives the spectrum of \( \Sigma^- \) accompanied by \( \pi \) mesons and Fig. 22 that of \( \Sigma^- \) unaccompanied by \( \pi \) mesons. Emulsion results from the "European collaboration".
tion of 15% is obtained by attributing all hyperons above 60 MeV to multinucleon processes, while a perhaps more reasonable estimate of 30% is obtained by a more detailed analysis taking into account the over-all energy spectrum with and without π mesons.

The next observation concerns the site of the capture. Here there are two aspects. First one can make a comparison between the excitation energy of K− capture, from stars in flight and stars at rest. One finds that the fraction of events in which there is a Σ or a Σ+π from K− interaction in flight with no stable prongs is about 1/3 to 1/4 of that found at rest. This seems to indicate that the π meson accompanying Σ production has a greater chance to be absorbed or inelastically scattered when produced in flight and suggests a longer available nuclear path in flight than at rest. The implication is that the absorption in flight is more probably accomplished uniformly throughout the nuclear volume, while at rest it is more a surface phenomena. Strictly speaking, this observation refers only to the single nucleon capture mode. Additional evidence supporting this view is obtained by calculating the π absorption coefficient for K− captures at rest. For π− this is done by comparing the number of Σ+ with E < 60 MeV with π mesons and those without. The probability of absorption is 0.09 ± 0.03 (this is to be considered a lower limit since there is a possibility of bias favouring the observation of Σ accompanied by π−).

One cannot do this for π± alone but only for π± and π0 together since Σ− can come from K−+p→Σ−+π± or K−+n→Σ−+π0. Comparing Σ− of energy < 60 MeV with and without π± one finds P_{π−,π}=0.34 ± 0.14. Principally the argument is that if the capture is throughout the volume one would expect P_{π−} ≈ 0.5 so that the low value observed implies a surface capture (if P_{π±} is assumed equal to P_{π−}, one deduces P_{π±} is 0.7). No such inference can be made for the multinucleon mode but it would appear that it most probably is a surface phenomenon (as would be expected theoretically).

The next observation concerns the π−/π+ ratio as a function of energy and their energy spectra. Table II shows the data on this ratio; there appears no evidence for a change of ratio with energy and a value of 4 seems reasonable. (See Table II.)

This ratio is influenced by the π absorption, K−−N interaction strength and the competition for various final states yielding π− and π+. Superficially one would say that such a ratio would pick out preferentially the Σ−+π− state in K−−p capture and the Σ− or Σ0+π− state in K−−n capture. The detailed interpretation is quite lengthy and involves corrections from the observed π−/π+ ratio to the ratio actually emitted (influenced by absorption) and an accounting at the same time of the Σ−/Σ+ ratio, which must also be corrected. It has previously been the fashion to confront the emulsion ratios (Σ−/Σ+ ≈ 0.8) and π−/π+ ≈ 4 with the hydrogen bubble chamber results and say that a real disagreement exists. However, it seems clear that one should not do this since, as first pointed out by the Bern group, the capture takes place in a nucleus and if the relative weights of the final states are momentum dependent, one should not expect an agreement. Principally, then, one concludes that the transition amplitudes are momentum dependent and there appears no real disagreement with the known facts (Tripp will show evidence for a large amount of Λ0 production in K−−n captures).

The shape of the π energy spectrum is relevant to Λ0 production from which π’s > 90 MeV can only arise from a capture at rest) but is obscured again by the absorption problem. Gilbert and White wish to attribute rc’s > 90 MeV to the existence of an attractive Σ-nuclear potential using the bubble chamber argument that Λ0 production is small. However, the validity of their model is now open to question and I think one can fairly say that some finite Λ0 production cannot be ruled out on the basis of the energy spectrum. Fig. 23 shows the Bern results for the spectrum which have less bias than the European collaboration group.

We now come to K− capture in flight. Here the most striking fact presented by the Bern Group is the increased

### Table II

<table>
<thead>
<tr>
<th>Energy (MeV)</th>
<th>European collaboration</th>
<th>Bern</th>
<th>Total</th>
</tr>
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<tbody>
<tr>
<td>0-30</td>
<td>+ -</td>
<td>+ 7</td>
<td>24</td>
</tr>
<tr>
<td>30-60</td>
<td>+ -</td>
<td>10 48</td>
<td>29 118</td>
</tr>
<tr>
<td>60-90</td>
<td>6 -</td>
<td>13 25</td>
<td>19 47</td>
</tr>
<tr>
<td>90-115</td>
<td></td>
<td>2</td>
<td>2</td>
</tr>
</tbody>
</table>

#### Combined results

<table>
<thead>
<tr>
<th>Energy (MeV)</th>
<th>Ratio π−/π+</th>
</tr>
</thead>
<tbody>
<tr>
<td>0-30</td>
<td>4.6 ± 0.9</td>
</tr>
<tr>
<td>30-60</td>
<td>4.1 ± 0.8</td>
</tr>
<tr>
<td>60-90</td>
<td>2.5 ± 0.6</td>
</tr>
</tbody>
</table>

![Fig. 23. Energy spectrum of π mesons coming from K− capture stars (K− at rest). Emulsion data from the Bern group.](image)
Fig. 25. The production and decay of a \(^A\text{He}^{4,5}\) hyperfragment. The decay may be either \(\text{He}^4 \rightarrow \pi^0 + \text{He}^4\) or \(\text{He}^{4,5} \rightarrow \pi^0 + \text{He}^{4,5} + n\). Chicago (EFINS) group.
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\[ \frac{\pi^-/\pi^+}{\pi^-} \] ratio from captures in flight (\( \sim 10/1 \)) and evidence that the multi-nucleon capture is less important. Fig. 24 shows the observed \( \Sigma^-\) hyperon spectra from the interaction of \( K^- \) mesons (in flight) with emulsion nuclei. The theoretical curve is calculated for \( \langle T_K \rangle = 110 \text{ MeV} \) and assuming interaction with a single nucleon in a Fermi gas. Data from Bern group.

Fig. 24. Energy spectrum of \( \Sigma^-\) hyperons produced in the interactions of \( K^- \) mesons (in flight) with emulsion nuclei. The theoretical curve is calculated for \( \langle T_K \rangle = 110 \text{ MeV} \) and assuming interaction with a single nucleon in a Fermi gas. Data from Bern group.

\[ \frac{dN}{dE} \] per 10 MeV

\[ \int_{T_k}^{} \] with \( T_k > 90 \text{ MeV} \)

\[ \text{Probably from two nucleon reaction} \]

We now turn to the question of hyperon interactions. We have at first the observations from the Berkeley hydrogen bubble chamber of 130 \( \Sigma^- \) interactions at rest; 76 produced \( \Lambda^0 \) (or \( \Sigma^0 \to \Lambda^0 \)) decaying by charged modes and 54 \( \Lambda^0 \) escaped or decayed by the neutral mode. There are five \( \Sigma^- \) interactions in flight which yield a cross-section of \( \sim 130 \) mb for \( 1 < T_K < 14 \) MeV. Of these 5 events, 3 had the \( \Lambda^0 \) observed and two were \( \Sigma^- \) in flight. Only one known \( \Sigma^- - \Lambda \) elastic scattering has been observed.

Of the 73 \( \Sigma^- - \Lambda \) interactions at rest yielding an observable \( \Lambda^0 \), 48 were \( \Sigma^- + p \to \Lambda^0 + n \) (i.e. consistent with a unique momentum \( P_{\Lambda^0} = 288 \text{ MeV/c} \)) and 25 ranged from 33 to 130 MeV/c and thus represent \( \Sigma^- - p \to \Sigma^0 + n \). Making a correction for escape, they find \( \Sigma^0, \Sigma^+ - \Lambda^0 = 0.34 \pm 0.05 \) (the phase-space factor is 0.26 for s-wave, p-wave making it even smaller). From the \( \Sigma^0 \to \Lambda^0 + \gamma \) decay, if \( \Sigma^0 \) has spin \( \frac{1}{2} \) the \( \Lambda^0 \) energy spectrum should be a square pulse lying between appropriate limits depending on the \( \Sigma^0 \) mass. The analysis of \( \Lambda^0 \) energies yields \( 1184.5 < M_{\Sigma^0} < 1191.9 \text{ MeV} \). A likelihood function based on the 25 events is strongly peaked at the upper end dropping by a factor of 100 for \( M_{\Sigma^0} = 1190 \text{ MeV} \). If the 25 events are divided into 3 energy intervals they fall as \( 5:11:9 \) consistent with the square pulse and allowing the statement of no evidence against \( \frac{1}{2} \) spin for \( \Sigma^0 \).

From emulsion observations we have both direct observation of the \( \Sigma \)-interactions in flight and estimates of \( \Lambda \)-absorption in nuclear matter. The European Collaboration Group has observed 4 exothermic interactions of nucleonic mass prongs in flight from \( K^- \) capture which yield a mean free path in emulsion of \( (34 \pm 5) \text{ cm} \) (geometric is about 27). From comparisons of \( K^- \) capture stars showing both \( \Sigma \) and \( \pi \) with those showing \( \pi \) but no \( \Sigma \) one can infer \( \Sigma \) absorption probabilities in a similar fashion as for the \( \pi \) absorptions. These are determined as \( P_{\Sigma^-} = 0.54 \pm 0.10 \) and \( P_{\Sigma^- + \Lambda} = 0.65 \pm 0.05 \). One notes that these results contain the effect of the Coulomb field in them. Similar results are obtained by the Bern Group from their analysis of interactions in flight. Since these \( \Sigma \)-s are produced most probably near the nuclear surface and thus have a reduced path in nuclear matter one can say that there is an indication of a fairly strong interaction with nuclear matter.

For the \( \Lambda^0 \)-interactions we have principally the hyperfragment data plus the observations of the Berkeley hydrogen bubble chamber of two high energy \( \Lambda^0 \) (momenta about 700 MeV) produced by \( \pi^- + \Lambda \) reactions, giving rise to the charged reaction \( \Lambda^0 + p \to \Sigma^+ + n \). The hyperfragment results arise from the Chicago Group and a combined Chicago and North Western University Group (EFINS-NU). I will first mention in passing the observation by the Chicago Group of the decay \( \Lambda^0 \to \pi^0 + \Lambda \) or possibly \( \Lambda^0 \to \pi^+ + \Lambda^0 \). Just to get one picture in, the event is shown on Fig. 25. The essential

Fig. 25. The binding energies of \( \Lambda \)-particles (\( B_{\Lambda^0} \)) in light hyperfragments. Data from the Chicago (EFINS)-Northwestern University collaboration.

Fig. 26. The binding energies of \( \Lambda \)-particles (\( B_{\Lambda^0} \)) in light hyperfragments. Data from the Chicago (EFINS)-Northwestern University collaboration.
new things in hyperfragment decay are the results of the EFINS-NU collaboration for mesic decaying hyperfragments from 39000 $K^-$ captures, yielding 280 mesic hyperfragments (of which 130 have been analysed). The stopping power of the stacks was calibrated from $\Sigma^+\rightarrow p$ decays, $\pi^+\rightarrow \mu$ decays and in some cases by independent density measurements. The events were divided into three classes, two-body decays, $\alpha(Z, A) \rightarrow (Z+1, A) + \pi^-$ ($\pi^-\pi$ events), three-body decays $\alpha(Z, A) \rightarrow (Z, A-1) + p + \pi^-$ ($\pi^-p\pi$ events), and other many-body decays where neutrons were emitted. In two-body decays colinearity was used as the criterion and in three-body decays coplanarity was used and $P_r$ evaluated by momentum conservation. The decays were analysed by an electronic computer with appropriate instructions as to the range of permissible parameters. An event was then classified as unique if only one given assignment of prong identities was consistent with zero momentum balance, within errors. Fig. 26 shows the results. On this, in black, are the $\Lambda$ binding energies from the world survey using $Q = 37.22 \pm 0.2$ MeV. The over-all results are shown in Fig. 27 in which $B$ is plotted versus $A$. The fact that in $\alpha^3_3$, $B_3 < 0$ suggests a systematic error, possible in the $Q$-value of $\alpha^4_4$. In this context Fig. 28 shows the $Q$-value for $\alpha^4_4$ obtained from the Johns Hopkins Group from a study of 2-prong $\pi^-\pi$ events in emulsion exposed to $K^-$ mesons. The preliminary value is $37.5 \pm 0.3$ MeV and this would lead to an increase of all $B_4$ by approximately 0.3 MeV which is just enough to bind $\alpha^3_3$. Additional evidence for an increase in the $\Lambda^4_4$ $Q$-value comes from the Pic-du-Midi Group of the Ecole Polytechnique who quote a value of $37.9 \pm 0.4$ MeV. It is to be pointed out that the $\alpha^7_7 \rightarrow \pi^-+Be^+$ events cannot be mistaken for $\alpha^5_5$ decays since the average recoil range is inconsistent with that. The thing to emphasize is, that the previous possible $\alpha^7_7$ events now appear to be uniquely $\alpha^3_3$ and it seems clear that $\alpha^3_3$ is unbound. Similarly, since no $\alpha^5_5$ have been observed, $\alpha^3_3$ is an isotopic singlet.

![Fig. 27. The binding energies of $\Lambda$-particles ($B_\Lambda$) in hyperfragments plotted as a function of mass number. Combined data from the EFINS world survey and the EFINS-Northwestern University collaboration.](image)

![Fig. 28. $Q$-value of $\Lambda^4_4$-hyperon. The apparent $Q$-value of $\pi^-\pi$ events observed in emulsion exposed to $K^-$ mesons. Data from the Johns Hopkins group.](image)
Strange particle interactions

$K^-$ interactions:

BERKELEY: W. H. Barkas; P. C. Giles; H. H. Heckmann; F. W. Inman; C. J. Mason; F. M. Smith.

BERN: E. Lohrmann; M. Nikolic; M. Schneeberger; P. Waloschek; H. Winzeler.

BROOKHAVEN: J. Hornbostel; G. T. Zorn.


ILLINOIS: G. Ascoli; R. D. Hill; T. S. Yoon.

LIVERMORE: F. C. Gilbert; C. E. Violet; R. S. White.

NAVAL RESEARCH LABORATORY: R. G. Glasser; N. Seeman; G. Snow.

UPPSALA: A. G. Ekspong.

BOLOGNA, GÖTTINGEN, PARIS and PARMA-COLLABORATION: W. Alles; N. N. Biswas; M. Ceccarelli; R. Gessaroli; G. Quareni; Göing; K. Gottstein; Piischel; J. Tiettge; G. T. Zorn; J. Crussard; J. Hennessy.

EUROPEAN COLLABORATION: G. Alexander; M. Bacchella; B. Bhomwik; A. Bonetti; E. H. S. Burhop; D. H. Davis; C. Ditworth; D. Evans; D. Falla; M. Grilli; L. Guerriero; F. Hassan; R. H. W. Johnston; A. A. Kamal; D. Keefe; R. C. Kumar; W. B. Lasich; L. Leprince-Ringuet; F. Muller; C. O'Ceallaigh; D. J. Prowse; M. Rene; A. Salandin; M. A. Shaukat; F. R. Stannard.

BERKELEY (bubble chamber): L. W. Alvarez; H. Bradner; P. Falk-Vairant; J. D. Gow; A. H. Rosenfeld; F. T. Solmitz; R. D. Tripp.

Hyperfragments:

CHICAGO (EFINS): R. Ammar; R. Levi-Setti; W. Slater; V. Telegdi.

NORTHWESTERN: S. Limentani; P. Schlein; P. Steinberg.

JOHN HOPKINS: A. Pevsner.

ÉCOLE POLYTECHNIQUE — PIC-DU-MIDI: C. D’Andlau; R. Armenteros; A. Astier; H. C. de Staebler; B. P. Gregory; L. Leprince-Ringuet; F. Muller; C. Peyrou; J. H. Tinlot.

$A^0 = Q$-value:

Treiman: I was not clear about the $\Sigma^-/\Sigma^+$ ratio. There were figures from $K^-$ capture in hydrogen at rest, in flight and in emulsion. The captures at rest, both in hydrogen and emulsion, agreed but both disagreed with the flight results. I have in mind the question whether you can determine if the capture is from $P$-state or $S$-state or both.

Kaplon: The answer here is that you cannot really draw any firm conclusion, unless you want to put yourself out on a limb. You can argue in the following way: if you take the Berkeley bubble chamber data seriously, which indicates that the $\Sigma^-/\Sigma^+$ ratio increases at really low energy above zero, and then decreases again to a value of 1 : 1, then you might argue that at this energy, i.e. low, but finite, you are looking at pure $s$-waves, and that since you have 1 : 1 at higher energies, what you are observing at rest, which is a value 2 : 1, means you have captures from a mixture of $S$- and $P$-states. I think it would be dangerous to try to infer this at the moment. The point is that if you look at the $K^-$-hydrogen interactions as observed in emulsion and look at the Berkeley data averaged over the same energy interval, then they are in complete agreement and show that up to 20 MeV the $\Sigma^-/\Sigma^+$ ratio is 2 : 1.

Gatto: May I know whether the value of 30% for multinucleon absorption was calculated with some assumption for the $K$ parity?

Kaplon: No, it was not. The nice thing about the European collaboration data is that their results were all inferred from experimental observation. They had essentially no model built into their analysis.

Gatto: The branching ratio might depend rather strongly on the $K$ parity and be larger for a pseudoscalar $K$ than for a scalar $K$.

Kaplon: No comment.

Peyrou: I would like to know how is the $K^-$ identified in the hydrogen bubble chamber $K^-$ elastic scattering data—by kinematics or by ionization?

Kaplon: I am told that the $K^-$ elastic scattering is identified by ionization and kinematics.
K⁻ INTERACTIONS IN DEUTERIUM

R. D. TRIPP, Rapporteur
University of California Radiation Laboratory, Berkeley (Cal.)

I would like to spend a few minutes describing an experiment performed several weeks ago at the Bevatron involving the interaction of K⁻ mesons with deuterium. The experiment was done with the 15 inch chamber of the Alvarez group and the same people mentioned by Kaplon in connection with the K⁻-hydrogen experiment are associated with the work.

There are several reasons why K⁻-D capture is of interest in strange particle physics. One is the possibility of the formation of a mass 2 hyperfragment. Another is to test charge independence, and a third is to study the K⁻-neutron interaction.

We now have about 1,500 K⁻ interactions in deuterium. So far we have had time to look at only a fraction of these events and have classified them essentially by inspection, and not by any detailed analyses (we are now in the process of doing this).

Therefore the data is preliminary, but we feel sufficiently trustworthy to report.

The bubble chamber is the same as used for the hydrogen experiment, except filled with deuterium. The operating conditions are similar to hydrogen. The K⁻ beam is also the same and since it has been quite successful for K⁻ and anti-protons, I would like to show a slide of the layout for those interested in beamology. (See Fig. 29.)

The 450 MeV/c beam is momentum analysed by the Bevatron field. A second momentum analysis removes the dispersion of the beam introduced by the first bend. The beam then passes through the Murray electrostatic separator where the pions are deflected outward and the K's are transmitted undeflected. The separator is a long cylinder with a radial electric field and a perpendicular magnetic field generated by a current flowing down a central conductor. The K's are focussed by quadrupoles and made to pass through a tiny aperture and then fan out into the bubble chamber. The rejection ratio is 650, and we have one K⁻ in the chamber per 70 background tracks (mostly muons).

The K's are almost unambiguously identified in the chamber by their characteristic curvature and high ionization. The decay of a hyperon in most cases of K⁻ interaction, confirms the identification.

We stopped the tabulation at 274 K's and Table III shows the result. (See Table III.)

The number of "K decays" is consistent with the K lifetime. The observed number has been corrected slightly for K⁻-D reactions which sometimes look like K decays.

Σ⁺ are Σ's which are so short that we cannot distinguish the decay pion from the production pion and thus cannot tell the sign. Y⁰ means either A⁰ or Σ⁰. In the Y⁰-π⁻-p events we shall be able to distinguish the two cases when we measure the events. In the Y⁰-π⁰-π⁻-n events the spectra overlap so the separation will not be clean.

"K₀" are principally Y⁰-π⁰-π⁻-n events and are consistent with that number. There is only one clear case of non-mesonic capture. There may be a few more when we measure the events—but the number seems small as expected theoretically.

We have seen no evidence for hyperfragment formation among these events. Three kinds are possible (Σ⁻-n), (Λ⁰-n), (Λ⁰-p). The latter two are already convincingly ruled out by extrapolation of the binding energy vs atomic weight (A) curve to A = 2. The (Σ⁻-n) case could well have existed. Pais and Treiman have calculated that if the binding energy is greater than 100 KeV or so, then there would be large probability for its formation in K⁻ capture, for either S- or P-state capture and pseudo-

Fig. 29. Experimental arrangement of the 15-inch bubble chamber for studying K⁻ interactions in hydrogen and deuterium. (Berkeley group)
TABLE III

\textit{K$^-$-Deuteron Interactions} (Bubble Chamber).

<table>
<thead>
<tr>
<th>Final State</th>
<th>No Observed</th>
<th>Final State</th>
<th>No Observed</th>
</tr>
</thead>
<tbody>
<tr>
<td>&quot;K decay&quot;</td>
<td>37</td>
<td>$\Sigma^- + \pi^0 + p$</td>
<td>7</td>
</tr>
<tr>
<td>$\Sigma^+ + \pi^- + n$ and $\Sigma^+ \rightarrow \pi^0 + n$</td>
<td>12</td>
<td>$\Sigma^- + \pi^+ + n$ and $\Sigma^- \rightarrow \pi^- + n$</td>
<td>43</td>
</tr>
<tr>
<td>$\Sigma^- + \pi^- + n$ and $\Sigma^- \rightarrow \pi^+ + n$</td>
<td>21</td>
<td>$\Sigma^- + \pi^+ + n$ and $\Sigma^- + D \rightarrow Y^0$ and $Y^0 \rightarrow A^0 \rightarrow \pi^- + p$</td>
<td>4</td>
</tr>
<tr>
<td>$\Sigma^+ + \pi^- + n$ and $\Sigma^+ \rightarrow \pi^0 + p$</td>
<td>17</td>
<td>$\Sigma^- + \pi^+ + n$ and $\Sigma^- \rightarrow \Sigma^0$</td>
<td>2</td>
</tr>
<tr>
<td>$Y^0 + \pi^0 + n$ and $Y^0 \rightarrow A^0 \rightarrow p + \pi^-$</td>
<td>48</td>
<td>$K_0^-$</td>
<td>33</td>
</tr>
<tr>
<td>$Y^0 + \pi^+ + p$ and $Y^0 \rightarrow A^0 \rightarrow p + \pi^-$</td>
<td>30</td>
<td>$\bar{\Sigma}^0 + n + n$ and $\bar{\Sigma}^0 \rightarrow \pi^+ + \pi^-$</td>
<td>1</td>
</tr>
<tr>
<td>$Y^0 + \pi^+ + p$ and $Y^0 \rightarrow A^0 \rightarrow p + \pi^-$</td>
<td>18</td>
<td>$A^0 + n$ and $A^0 \rightarrow \pi^- + p$</td>
<td>1</td>
</tr>
</tbody>
</table>

scalar or scalar $K$. Since no likely candidates for a ($\Sigma^- - n$) hyperfragment have been seen among 54 $\Sigma^- - \pi^- - n$ events then either the binding energy is extremely small or the ($\Sigma^- - n$) hyperfragment does not exist.

The $A^0$-nucleon hyperfragment is also not seen among a comparable number of events.

Since the $K^- - D$ system is initially in a pure I-spin state, a number of relations can be written relating various possible final states. These always require distinguishing a $A^0$ from a $\Sigma^0$, which we cannot do without careful analysis of the event. Fortunately, however, one can combine the relations in such a way that only the sum of $A^0$ and $\Sigma^0$ appear so that they need not be distinguished.

The two relations given below should be equal if I-spin is conserved for $\Sigma$ and $A$ production. Within statistics it seems to hold.

The relations used to test conservation of I-spin are:
- Branching ratio for charged mode of decay of $A$ assumed to be $0.60 \pm 0.04$
- \[ \frac{1}{2} \left[ R(\Sigma^+ + \pi^- + n) + R(\Sigma^- + \pi^+ + n) + R(\Sigma^0 + \pi^- + p) + R(\Lambda^0 + \pi^+ + p) \right] = 75 \pm 4 \]
- \[ R(\Sigma^- + \pi^0 + p) + R(\Sigma^0 + \pi^0 + n) + R(\Lambda^0 + \pi^0 + n) = 87 \pm 13 \]

Assuming I-spin conservation and a crude impulse approximation (no interference between the $K^- - p$ and the $K^- - n$ interaction), one can go from the former tabulation to the following result.

<table>
<thead>
<tr>
<th>In deuterium</th>
<th>In hydrogen</th>
</tr>
</thead>
<tbody>
<tr>
<td>total numbers</td>
<td>relative numbers</td>
</tr>
<tr>
<td>$K^- + p \rightarrow \Sigma^-$</td>
<td>54</td>
</tr>
<tr>
<td>$\rightarrow \Sigma^+$</td>
<td>44</td>
</tr>
<tr>
<td>$\rightarrow \Sigma^0$</td>
<td>58</td>
</tr>
<tr>
<td>$\rightarrow A^0$</td>
<td>22</td>
</tr>
<tr>
<td></td>
<td>178</td>
</tr>
<tr>
<td>$K^- + n \rightarrow \Sigma^-$</td>
<td>7</td>
</tr>
<tr>
<td>$\rightarrow \Sigma^0$</td>
<td>43</td>
</tr>
<tr>
<td></td>
<td>57</td>
</tr>
</tbody>
</table>

Thus in this approximation:

1. The hyperon production ratios have changed significantly for $K^-$ capture in deuterium to that observed in hydrogen. This may be due to different angular momentum states occuring in the capture.

2. Proton captures are three times more frequent than neutron captures.
DISCUSSION

Yamaguchi: How could you distinguish between proton capture and neutron capture in the deuteron?

Tripp: If we see a proton coming off then we say it was a capture on a neutron; if we do not see a proton coming off then we say it was a capture on a proton.

Yamaguchi: I do not understand. If you have a strong final state interaction you cannot distinguish them.

Tripp: We assume that there is no final state interaction.
In discussing the information which has become available on strange particle interactions, we shall keep several goals before us: the determination of the relative parities of the $K$ particle and the hyperons and the determination of the coupling parameters for the various interactions between them. The $K$ particle will be assumed to have zero spin, the hyperons spin $\frac{1}{2}$, as is consistent with all the data available. A further feature of interest is the possibility of symmetries between the various interactions, for example whether the hypothesis of a universal pion-baryon coupling as proposed at the Rochester Conference last year by Gell-Mann and by Schwinger is in accord with the data available.

**K-nucleon interactions**

**$K^+$-nucleon interactions.**

First we consider briefly the $K^+$ scattering evidence. As we have just heard, the main features appear as follows:

(i) The $T = 1$ interaction is a short-range $s$-wave repulsion, corresponding to a cross-section essentially constant up to about 150 MeV. Beyond this energy, there is some evidence for an increasing $T = 1$ cross-section. This rise in cross-section may be due to some $p$-wave interaction, although there is no real evidence for a non-isotropic angular distribution.

(ii) The $T = 0$ $s$-wave interaction appears relatively weak, but the evidence for a $p$-wave interaction appears quite clearly from the appearance of a backward peaking in the $K^+$-neutron cross-section for $K^+$ energies of 100 MeV and higher.

The appearance of both $s$- and $p$-wave scattering in this energy region seems rather difficult to understand in terms of a scalar $K$ meson, the $\Lambda$- and $\Sigma$-hyperons being assumed to have the same parity. However, Ceolin, Dallaporta and Taffara have remarked that the appearance of both $s$ and $p$ scattering could be explained in a fairly direct way in terms of pseudoscalar $KN\Lambda$ and $KN\Sigma$ interactions. In lowest order perturbation theory, a repulsive $s$-wave interaction for $K^+$-proton scattering appears naturally through the excitation of virtual antihyperon nucleon pairs.

The corresponding pair terms for the pion-nucleon system are well known to be very much greater than the observed $s$-wave scattering allows; however, the suppression of these terms is not well understood for the pion-nucleon case (it may possibly be due to the effects of virtual strange particle interactions), and it is quite possible that the mechanism for this suppression does not operate in the case of $K$-nucleon scattering. The observed $K^+$-proton cross-section of 14 mb is obtained with a value of about 2 or 3 for $(g_A^2 + g_\Sigma^2)/4\pi$. The $T = 0$ scattering amplitude is proportional to $(g_A^2 - 3g_\Sigma^2)/4\pi$, so that a weak $s$-wave scattering in this state may be obtained for suitable choice of the ratio $g_A/g_\Sigma$. Further, Barshay has suggested the use of a repulsive direct $K$-pion interaction: $\lambda\bar{K}K\pi\pi$, which would contribute a repulsive $T$-independent term to the $K^+$-nucleon scattering amplitude; in this case, weak $T = 0$ $s$-wave scattering would correspond to a different choice of the relative coupling strengths $g_A$ and $g_\Sigma$. From this pion exchange process, one might expect an $s$-wave amplitude falling off with energy beyond about 100 MeV, but the total $K^+$-proton cross-section might be held up to the observed value by some rising $p$-wave cross-section.

From the positive energy transitions
the pseudoscalar coupling gives rise to a pseudovector form of interaction, as we are now familiar with in pion physics, so that \( p \)-wave scattering is also to be expected. Ceolin and Taffara have pointed out that, in lowest approximation, this interaction is attractive in the \( T = 1 \), \( p_{3/2} \) and the \( T = 0 \), \( p_{3/2} \) states. Together with Dallaporta, they have carried out more detailed calculations in the static limit to bring out these qualitative points more clearly; however, there is no detailed agreement with the experimental results.\(^*(\star)\)

It must be emphasized that the above remarks are very qualitative and that they could be modified very considerably by the existence of the lighter strongly-coupled pion. For this reason we cannot definitely say that the scalar \( K \) meson could not account for the evidence, although in lowest approximation the interaction which it leads to is attractive and includes very little \( p \)-wave term \((a)\). The most cautious statement one could make is that the pseudoscalar interaction is not ruled out, but appears capable of giving the right qualitative behaviour.

A new feature of the \( K^+ \) data this year is the appearance of \( \pi \) production in \( K^+ \) collisions above the threshold at 220 MeV, the rate observed being of the order 1 \(?/\text{MeV}^2\). Some calculations have been reported by Ceolin, Dallaporta and Taffara on this process.\(^*(\star)\) Assuming the elastic \( K^+ \) scattering to be due to pseudoscalar \( KY\pi \) interactions, they obtain the results \((a)\) from a perturbation theory calculation, which are very much below the production rate actually observed. The second possibility considered \((b)\), was that the \( \overline{K}K\pi\pi \) interaction was responsible for the elastic scattering.

\[
\text{In this case the elastic scattering gives no charge exchange, and } f_{K\pi}/4\pi \sim 2 \text{ is needed to give the observed } K^-\text{-proton cross-section. The values obtained for } R_n \text{ are larger by a factor of } 10, \text{ but are still well below the number suggested experimentally.}
\]

\[
\begin{align*}
(E - E\text{th.}) &= 50 \text{ MeV} \quad R_n = 4 \times 10^{-5} \quad 1 \times 10^{-3} \\
&= 100 \text{ MeV} \quad = 2 \times 10^{-4} \quad 2 \times 10^{-3}
\end{align*}
\]

\(*\) In a paper received at the end of the Conference, Barshay has remarked that the appearance of both \( s \)-and \( p \)-wave scattering could also be explained if only one of the \( K\Lambda\Lambda \) and \( K\Sigma\Sigma \) interactions were pseudoscalar, the other being of scalar form. Of course this requires opposite parities for the \( \Lambda \)-and \( \Sigma \)-hyperons.

Actually, at 350 MeV \( K^+ \) energy, it seems reasonable to expect a rate \( R_n \) of at least \( 10^{-8} \) for \( K^+ \) interactions in complex nuclei, owing to secondary pion production by recoil nucleons from the backward scattering of \( K \) mesons, and this effect may be sufficient to account for the experimental observations. However, it is clear that the observation of \( \pi \) production in \( K^-\)-proton collisions would be of interest in giving some information bearing on the relative strengths of the various kinds of \( K \)-particle interaction possible.

\( K^-\)-proton interactions

Data on \( K^-\)-proton interactions is now available in some detail from emulsion studies and especially from the work of the Alvarez bubble chamber group. For \( K^- \) capture at rest from hydrogen, the data has increased greatly from that available a year ago, but is still compatible with the ratios \( 4 : 2 : 2 : 1 \) for the \( \Sigma^- : \Lambda \): \( \Sigma^0 \) reaction ratios (although the \( \Sigma^0 \) and \( \Lambda \) events have not yet been separated in the new data).

The new bubble chamber data on interactions in flight over the energy range of about 5 to 35 MeV is of the greatest interest. This data shows no clear indication of any other than \( s \)-wave interactions; the angular distributions of the scattering and the reactions are all compatible with an \( s \)-wave capture. This appears reasonable for such low incident momenta, the \( K^-\)-proton interaction being expected to have a range of about \( h/m_{\text{Be}} \sim 0.4 \). There is evidence in the emulsion data that the \( \Sigma^-/\Sigma^+ \) ratio has changed to a value \( \sim 1 \) at 100 MeV from the value 1.8 for the low energy interactions in flight, but we shall see later that this is not at all incompatible with an \( s \)-wave reaction. In discussing this data we shall therefore assume that \( s \)-wave capture is predominant. The analysis I shall give is very preliminary and has been carried out only since my arrival at the Conference. I give it here with much hesitancy, but I feel that at least it is instructive and will indicate the kind of additional experimental data we now need.

For discussion of the scattering, two complex phase-shifts \( \delta_T = a_T + i\delta_T \), that are four parameters, are needed for the two channels \( T = 0 \) and \( T = 1 \). The bubble chamber data has been collected together by an averaging potential of about 135 MeV/C, a \( K^- \) energy of 18.5 MeV. The value of \( \Sigma^0 \), at this energy is 160 mb. The elastic cross-section is then 64 mb and the charge-exchange cross-section has been taken as 0.2 \( \text{el} \) corresponding to the observed charge exchange events (this may be somewhat of an underestimate since some \( K^0 \) decays may be confused with \( \Lambda \)-decay events; however, the proportion of \( K^0 \) decays giving such configurations may be expected.
Strange particle interactions

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to be small). To obtain the reaction cross-sections for $T=0$ and $T=1$, more data is needed than just for the $\Sigma^-$ and $\Sigma^+$ reactions. In fact, in terms of the reaction amplitudes $M_0$ and $M_1$, and their relative phase $\varphi$, the relative proportions for $\Sigma$ and $\Lambda$ reactions are

$$
\sigma(\Sigma^-) \sim \frac{1}{6} M_0^2 + \frac{1}{4} M_1^2 + \frac{1}{\sqrt{6}} M_0 M_1 \cos \varphi ,
$$

(1a)

$$
\sigma(\Sigma^+) \sim \frac{1}{6} M_0^2 + \frac{1}{4} M_1^2 - \frac{1}{\sqrt{6}} M_0 M_1 \cos \varphi ,
$$

(1b)

$$
\sigma(\Sigma^0) \sim \frac{1}{2} N_1^2 ,
$$

(1c)

$$
\sigma(\Lambda) \sim \frac{1}{2} N_1^2 .
$$

(1d)

From these expressions, the ratio of $T=1$ and $T=0$ reaction cross-sections is

$$
\frac{\sigma_{ab}^0(T=1)}{\sigma_{ab}^0(T=0)} = \frac{M_1^2 + N_1^2}{M_0^2} = \frac{\sigma(\Sigma^+) + \sigma(\Sigma^-) - 2\sigma(\Sigma^0) + \sigma(\Lambda)}{3\sigma(\Sigma^0)} .
$$

(2)

To make progress, we have assumed that the $\Sigma^0$, $\Lambda$ production bears the same ratio to the $\Sigma^-$ and $\Sigma^+$ production as for the $K^-$ capture from rest. This is dangerous since it is not at all clear how much of this $K^-\text{-}P$ capture in hydrogen is from the $p$-states. Jackson, Ravenhall and Wyld 3 have pointed out that the competition of the $2p$ capture with the $2p-\text{Is}$ radiative transition could be obtained when the $p$-wave absorption in flight is known. At 100 MeV the absorption cross-section appears relatively small, and it is still quite consistent with $s$-wave capture; if 50% of the observed cross-section was $p$-wave, this would allow only about 30% of the $K^-$ capture from rest to be from the $p$-states. There is, however, some support for this assumption from the $\Sigma^-/\Sigma^+$ ratio which is 1.8 for the low energy interactions in flight, quite in accord within statistics with the ratio of 2 observed for the captures from rest. In this way a ratio $\sigma_{ab}^0(T=1)/\sigma_{ab}^0(T=0)$ of about 0.4 is obtained, the total reaction cross-section at 18.5 MeV then being $\frac{1}{2} \sigma_{ab}^0(T=1) + \sigma_{ab}^0(T=0) = 62 \text{ mb}$. Since the reaction cross-section is directly related to the imaginary part of the phase-shift,

$$
\sigma_{ab}^0(T) = \pi\lambda_0^2 \left( 1 - \exp (-2\beta_T) \right) ,
$$

(3)

values may be obtained for $\beta_0$ and $\beta_1$ from these numbers. Next, the real parts of $\delta_0$ and $\delta_1$ may be obtained from the elastic and charge-exchange cross-sections,

$$
\sigma_{el.} = \pi\lambda_0^2 \left( 2 - e^{-2\beta_0} e^{2\delta_0} - e^{-2\beta_1} e^{2\delta_1} \right) ^2
$$

(4)

$$
\sigma_{e,e} = \pi\lambda_0^2 \left( e^{-2\beta_0} e^{2\delta_0} - e^{-2\beta_1} e^{2\delta_1} \right) ^2 .
$$

(5)

There are two distinct solutions for $\alpha_0$ and $\alpha_1$; for each of these solutions the relative sign of $\alpha_0$ and $\alpha_1$ is determined but their over-all sign is still indeterminate. At this point, it is appropriate to follow the suggestions of Jackson, Ravenhall and Wyld, and to adopt the zero range approximation, since even at 100 MeV the wavelengths are still reasonably long relative to the range of the interaction. Zero-energy scattering lengths are then convenient to use and are defined as usual by

$$
k \cot \delta_T = \frac{1}{\alpha_T + i\beta_T} ,
$$

(6)

with neglect of the term $\frac{1}{2} (\rho_T + i\eta_T) k^2$ in the usual effective range treatment. The complex values obtained for these zero-energy scattering lengths are shown in Table I.

**TABLE I**

<table>
<thead>
<tr>
<th>$K^-\text{-}P$ zero-energy scattering amplitudes (*)</th>
<th>$a_0 + ib_0$</th>
<th>$a_1 + ib_1$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Solution A</td>
<td>0.28 + i 0.54</td>
<td>1.19 + i 0.22</td>
</tr>
<tr>
<td>Solution B</td>
<td>1.28 + i 0.71</td>
<td>0.43 + i 0.18</td>
</tr>
</tbody>
</table>

The following points are now of interest:

(i) $a_0$, $a_1$ have the same sign in both of these solutions. This follows really from the largeness of the elastic cross-section, which requires a reasonably large value of $(a_0 + a_1)$, and the smallness of the charge-exchange scattering, which then requires that $a_0$ and $a_1$ have the same sign to give a small value for $(a_0 - a_1)$. This is of interest especially because of the strong argument Ceccarelli gave last year for the conclusion that the $K^-$ nuclear potential is attractive (at least 20 MeV) for low energy $K^-$ mesons. This conclusion then requires that, since $a_0$ and $a_1$ have the same sign, $a_0$ and $a_1$ should both correspond to attractive interactions.

(ii) The energy dependence of these cross-sections is next of interest. Jackson, Ravenhall and Wyld have emphasized that the presence of strong absorptive processes gives rise to a downward cusp in the energy dependence of $ka_{ab}/4\pi$. There is also a cusp in the elastic cross-section. As a result, extrapolation of the data to zero energy must always be done with care. In Fig. 1 the elastic cross-sections corresponding to solutions A and B of Table I are plotted from the expression

$$
\sigma_{el.} = \pi \frac{a_0 + ib_0}{1 + kb_0 - ik\alpha_0} + \frac{a_1 + ib_1}{1 + kb_1 - ik\alpha_1} ^2 ,
$$

(7)

(* There are also two corresponding solutions $A_+, B_-$ which are obtained from $A_-, B_-$ by reversing the signs of $a_0$ and $a_1$. The unit of length is $10^{-13}$ cm.)
COMBINED EMULSION DATA (203 EVENTS.)

**Fig. 1.** Combined emulsion data (203 events) for elastic $K^-p$ scattering and comparison with the calculated cross-section.

and are compared with the emulsion data. Both solutions agree in giving an $s$-wave cross-section which falls rapidly with increasing $K^-$ energy; the two curves agree at 18.5 MeV, of course, but solution B rises about 30% higher than solution A at zero energy. Both curves reproduce the general trend of the emulsion data, which reflects the excellent agreement of the emulsion data with the bubble chamber data. In Fig. 2 the absorption cross-section $[\sigma (\Sigma^+) + \sigma (\Sigma^-)]$ given by 0.7 $\sigma_{abs}$, where

$$\frac{k \sigma_{abs}}{4\pi} = \frac{b_0}{1 + 2kb_0 + k^2 (a_0^2 + b_0^2)} + \frac{b_1}{1 + 2kb_1 + k^2 (a_1^2 + b_1^2)},$$

is compared with the emulsion data. Again the general trend of this cross-section is reproduced quite well, although the predicted curve lies a little high compared to the emulsion data (due partly to the somewhat high value of the fitted bubble chamber cross-section relative to the general trend of the poorer known cross-sections from emulsion studies). The general agreement found provides some support for this simple interpretation of the data up to 100 MeV in terms of a predominantly $s$-wave interaction. It will be of interest later to examine how sensitive this fit is to the assumption leading to the relative weight of the $T = 1$ and $T = 0$ absorption processes. Direct evidence on the $\Sigma^0$ and $\Lambda$ production from $K^-p$ reactions is very desirable at this stage.

(iii) The energy dependences of $\sigma_{abs} (T = 0)$ and of $\sigma_{abs} (T = 1)$ may be quite different, owing to the fact that their strengths correspond to different values of $b$. For solution A, the $T = 0$ and $T = 1$ absorption cross-sections have an almost constant ratio, but for solution B, the $T = 0$ absorption drops by about 50% between 20 MeV and 100 MeV relative to the $T = 1$ absorption. This will be reflected in a corresponding energy dependence of the $\Sigma^-/\Sigma^+$ ratio for solution B. But even if the ratio $M_0/M_1$ is constant, the $\Sigma^-/\Sigma^+$ ratio depends sensitively on the phase angle $\phi$; a value $\phi = 70^\circ$ gives the observed $\Sigma^-/\Sigma^+$ ratio at 18.5 MeV and a change to $\phi = 90^\circ$ at 100 MeV would lead to a $\Sigma^-/\Sigma^+$ ratio of unity there. Since the phase is due to the scattering interactions in the $K^-p$ initial state and especially in the $\pi$-hyperon final state, it is not at all unreasonable to find a change in $\phi$ by $\sim 20^\circ$ between zero energy and 100 MeV.

It is also of interest to mention briefly the reaction which has not yet been observed, although about 3000 $K^-p$ captures have been examined

$$K^- + p \rightarrow \Lambda + \pi^+ + \pi^-.$$  

(9)

Fujii and Marshak $^4$ have estimated from perturbation theory that $S$-state capture of a scalar $K$ meson (which leads to $s$-wave motions in the final state (9)) would lead to a branching ratio of several per cent for this reaction. Okun' and Pomeranchuk $^5$ give an estimate of about 0.2% for this branching ratio on the basis of phase-space considerations. For a pseudoscalar $K$ meson, these estimates must be reduced by a factor $\sim 50$ owing to the $p$-wave motions which are then necessary in the final state. Okun and Pomeranchuk have pointed out that the

**Fig. 2.** Combined emulsion data (75 events) for the reactions $K^- + p \rightarrow \Sigma^\pm + \Sigma^\mp$. 

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energy distribution in the final state of this reaction would be of considerable interest for the determination of $K\Lambda$ parity if the reaction is ever observed.

**Dispersion relations for $K$ mesons**

It appears that the use of dispersion relations for $K$-nucleon scattering offers a very powerful means for the determination of $K\Lambda$ and $K\Sigma$ parities and coupling constants when the data available on $K^\pm$ scattering becomes more extensive. Their application for this purpose has been discussed recently by Amati and Vitale $^6$, by Igi $^7$, by Goebel $^8$ and by Matthews and Salam $^9$. The form of these dispersion relations for $K$-proton scattering is as follows:

\[
D_+(\omega) = \frac{p_A}{m_A} X(\Lambda) \omega_A + \frac{p_e}{m_e} X(\Sigma) \omega_e + \frac{1}{4\pi^2} \int_{m_K}^{\infty} \frac{k'^2 \text{d}k' \omega'}{m_k^2} \left[ \frac{\sigma_+(\omega')}{\omega' - \omega} + \frac{\sigma_-(\omega')}{\omega' + \omega} \right] + \frac{1}{\pi} \int_{\omega_A}^{m_K} \frac{\text{d}\omega' A_-(\omega')}{\omega' + \omega}, \tag{10a}
\]

\[
D_-(\omega) = \frac{p_A}{m_A} X(\Lambda) \omega_A - \frac{p_e}{m_e} X(\Sigma) \omega_e + \frac{1}{4\pi^2} \int_{m_K}^{\infty} \frac{k'^2 \text{d}k' \omega'}{m_k^2} \left[ \frac{\sigma_+(\omega')}{\omega' + \omega} + \frac{\sigma_-(\omega')}{\omega' - \omega} \right] + \frac{1}{\pi} \int_{\omega_A}^{m_K} \frac{\text{d}\omega' A_+(\omega')}{\omega' - \omega}, \tag{10b}
\]

where $\omega_a = (m_a^2 - m_p^3 - m_K^3)/2 m_p$, and $m_a$ is the rest mass in the system $a$. The first terms on the right are the poles at $\omega_A$ and $\omega_e$ corresponding to the isolated $\Lambda$ and $\Sigma$-particles. The point of special interest here is that the sign of the residues at these poles is proportional to the parity, $\eta_A$ or $\eta_\Sigma$, of the corresponding $(K\Lambda)$ or $(K\Sigma)$ pair, whilst the magnitude of the residue is related to the corresponding coupling constants $g_A$ and $g_\Sigma$. The expressions for the residues are

\[
p_A = +1: \quad X(\Lambda) = (g_\Lambda^2/4\pi) [m_A + m_p^3 - m_K^3]/4m_pm_A, \tag{11a}
\]

\[
p_A = -1: \quad X(\Lambda) = (g_\Lambda^2/4\pi) [m_K^2 - (m_A - m_p^3)/4m_pm_A, \tag{11b}
\]

with corresponding expressions for $p_\Sigma = \pm 1$, $A$ being replaced everywhere by $\Sigma$. It is of interest to add that the $K$-neutron dispersion relations have the same form except that $X(\Sigma) = 0$, the term $X(\Sigma)$ is doubled relative to the expression given by the analogue of (11); since the $K^-N$ system has $T = 1$, only the $\Sigma^-$ state can contribute a pole, so that the $K$-neutron dispersion relations relate to the $K\Sigma$ parity alone.

In expression (10), $A_-$ denotes the imaginary part of the $K^-$ forward scattering amplitude. The unphysical region here includes a continuous stretch from $\omega_A$ to $m_K$, corresponding to the fact that $\Lambda\pi$ and $\Sigma\tau$ states of positive kinetic energy can still be reached from states of unphysical energy for the $K^-$ proton system.

Now let us discuss some of the difficulties in the use of these dispersion relations. Firstly, it has been pointed out to me by Tuan (Berkeley) and by Oehme (Chicago) that the present techniques for establishing the validity of dispersion relations fail to achieve their purpose for the $K$ meson case, even for forward scattering. The mass inequalities are only just violated: if the $\pi$-particle were about 5% heavier the necessary condition would be satisfied. However, not all that we know about the possible intermediate states has been put into this calculation and there is every reason to expect that they will be rigorously demonstrated in due course.

Next, the unphysical region may be quite complicated and there is little that we can learn about this from experiments in the physical region. Although $A_-$ is required to be positive in the physical region, it is permitted to take on negative values in the unphysical region. We have already mentioned that cusps will generally occur at $\omega = m_K$, for

\[
A_-(\omega) = \frac{1}{2} \left[ \frac{b_0 + k (b_0^2 + a_0^2)}{1 + 2kb_0 + k^2 (a_0^2 + b_0^2)} \right] + \frac{b_1 + k (a_1^2 + b_1^2)}{1 + 2kb_1 + k^2 (a_1^2 + b_1^2)} \right). \tag{12}
\]

With the parameters of Table I, $A_-$ will have an S-shaped energy dependence across $\omega = m_K$. For $\omega < \omega_{\Lambda\pi}$, $A_-$ is zero, of course; at $\omega = \omega_{\Lambda\pi}$, $A_-$ has a branch point of type $(\omega - \omega_{\Lambda\pi})^{1/2}$ and may become either positive or negative, and there will also be some kind of cusp in $A_-$ at $\omega = \omega_{\Sigma\tau}$, where the competing $(\Sigma + \pi)$-states begin to play a role. Two of the many possible curves for $A_-(\omega)$ in the unphysical region are shown on Fig. 3 to illustrate these remarks.

![Fig. 3](image_url) Two of the many possible energy dependences of $A_-(\omega)$ in the unphysical region.
In the form (10), the integrals over the cross-sections will not converge if the cross-sections $\sigma_+^0$ and $\sigma_-$ approach constant values at infinite energy, and some subtractions will clearly be necessary. Goebel and Matthews - Salam have suggested making use of the form obtained by subtracting $(10\alpha)$ from $(10\beta)$. With $\omega = m_K$, this has the form (assuming $A$ and $\Sigma$ parities to be the same)

$$m_K [D_+ (m_K) - D_- (m_K)] = \frac{m_K^2}{4\pi^2} \int \frac{d\omega'}{k'} (\sigma_+^0 - \sigma_-^0)$$

For scalar $K$ meson,

$$\approx \left\{ -2 \frac{(m_K/2m_p) (g_1^2 + g_2^2)}{4\pi} \right\}$$

for pseudoscalar $K$ mesons.

Here the hope is that $\int d\omega' (\sigma_+^0 - \sigma_-^0) / k'$ may be convergent. Pomeranchuk has shown that it is reasonable to expect $(\sigma_+ - \sigma_-) \lesssim C/\log \omega$, but this is not sufficient to ensure convergence. Even if the integral is convergent, this convergence is slow and its value will depend on contributions from energies far beyond those for which experimental information exists. Even so, it is of interest to follow a little the arguments of Goebel and of Matthews-Salam. As far as the experiments go, $\sigma_-$ is larger than $\sigma_+$, and it is reasonable to expect this to continue to be the case up to fairly high energies since the $K^-$ interaction has many more reaction channels available than the $K^+$ interaction; it seems likely that the first integral (if it exists) is positive. Now $D_-(m_K)$ is known to be negative, but fairly small, whereas $D_+(m_K)$ is quite large. The second integral, over the unphysical region, is unknown even in sign, but may be expected to be moderately small (as is the case for simple extrapolations from the physical region). Hence if $D_-(m_K)$ were negative, that is the $K^-$-proton interaction is repulsive, then the expression (13) would almost certainly be positive and the $K$ meson parity would be even. But if $D_-(m_K)$ is positive, for an attractive $K^-$-proton interaction, the matter becomes more quantitative, but it appears rather likely that (13) would be negative corresponding to a pseudoscalar $K$ meson. However, owing to the question of the validity of the relation (13), there is some doubt concerning its use in this way for determination of the $K$ meson parity.

A modification of the relation (13), analogous to the method proposed by Haber-Schaim \(^{10}\) for the use of pion-nucleon dispersion relations, has been proposed by Igi. After the approximations $\alpha_+ = 0.11 m_K \approx 0$, $\omega_2 = 0.27 m_K \approx 0$, and assuming the $A$ and $\Sigma$ parities to be the same, Igi was led to the expression

$$1 \over 2 \omega (D_+ - D_-) = \frac{m_K}{\omega} \int \frac{d\omega'}{\omega'^2 - \omega^2}$$

$$\left[ \frac{\omega^2}{4\pi^2} \int \frac{k'}{\omega'^2 - \omega^2} \left( \frac{\sigma_+^0 - \sigma_-^0}{\omega'^2 - \omega^2} \right) \right]$$

$$- \frac{\omega^2}{4\pi^2} \int \frac{k'}{\omega'^2 - \omega^2} \left( \frac{\sigma_+^0 - \sigma_-^0}{\omega'^2 - \omega^2} \right)$$

$$= - \left[ X(A) + X(\Sigma) \right] + \frac{\omega^2}{4\pi} \int \frac{k'}{\omega'^2} (\sigma_+^0 - \sigma_-^0)$$

(14)

If the left-hand side of this expression is plotted as a function of $\omega$ (note that the integral over cross-sections is now rapidly convergent), it should follow a straight line which may be extrapolated to $\omega = 0$ to obtain the desired indication of the $K$ parity and the $K$-hyperon coupling constants. However, the unknown integral over the unphysical region will not be unimportant, also the extrapolation to $\omega = 0$ is a very large step relative to the energy range over which experimental data is at present available, so that the use of this interesting relation may be rather uncertain.

However, Igi has suggested the use of the following form

$$\omega \left[ D_+ (\omega) - \frac{1}{2} (m_K + \omega) D_+ (m_K) - \frac{1}{2} (m_K - \omega) D_- (m_K) \right]$$

$$- \frac{1}{4\pi^2} \int \frac{d\omega'}{k'} \left( \frac{\sigma_+^0}{\omega' - \omega} + \frac{\sigma_-^0}{\omega' + \omega} \right)$$

$$- \frac{1}{4\pi} \int \frac{d\omega'}{\omega'_2} \left( \omega' + \omega \right)$$

$$= - \left[ X(A) + X(\Sigma) \right]$$

(15)

which is weighted against contributions from the unphysical region. The cross-section integrals again converge rather rapidly and depend on the $K^+$ cross-sections to a far greater extent, owing to the large denominator which goes with $\sigma_-$. To pay for these advantages, the formulae have a correspondingly greater dependence on the energy dependence of the forward scattering amplitude for $K^-p$ scattering. Igi considers two possibilities,

(a) $\sigma_+$ is constant at 15 mb up to $\omega = 4m_K$. In this case it finds that if $D_-$ is attractive then the expression on the left of (15) is positive, corresponding to a pseudoscalar $K$ meson (assuming $\Sigma$ and $A$ to have the same parity) with $(g_1^2 + g_2^2) / 4\pi \approx 4$, whereas if $D_-$ is repulsive, the $K$ meson must be scalar with $(g_1^2 + g_2^2) / 4\pi \approx 0.8$. To obtain these conclusions, the known cross-sections had to be extrapolated far beyond our region of knowledge, and the unphysical contribution was estimated by a simple smooth extrapolation into the region $\omega < m_K$, an estimate which did not contribute at all strongly to the final expression. It is difficult to be very definite about how sensitive such calculations are to the cross-section assumptions without having had the opportunity of repeating the
calculations oneself — some of the data used at low energies deviates considerably (for example, the values for \[ \sigma_{abs} \]) from our present knowledge at this meeting, but on the other hand, there appears to be some degree of compensation between the various terms when the K^- cross-sections in the low energy region are modified. It is of interest to note that, on the basis of the same cross-section assumptions, together with the additional strong assumption that \( (\sigma_+ - \sigma_-) \rightarrow 0 \) reasonably quickly with increasing \( \omega \), the work of Goebel and Matthews - Salam reached essentially the same conclusion.

(b) a second assumption considered was that \( \sigma_+ \) followed a smooth curve running through the \( K^-p \) scattering cross-sections published \(^{11}\) by the Michigan bubble chamber group, which were consistent with a cross-section falling off by a factor two between 50 MeV and zero energy. In this case, the expression was found to be negative for either attractive or repulsive \( D_- \), corresponding to a scalar \( K \) meson and \( (g_x^+ + g_x^\pm) / 4\pi = 4 \).

My main purpose in discussing this matter in detail when the data is at such a preliminary stage is to stress the urgent need for data on \( K^+ \) and \( K^- \) cross-sections for interaction with protons over wide energy ranges, up to energies very much above those for which data is now available. For the use of Igi's relation \((15)\), more accurate studies on \( K^- \)-proton scattering cross-sections and angular distributions at low energies (say 50 MeV and below) would be very helpful. Also, as Matthews and Salam have emphasized to me, it would be of very great interest to obtain data bearing on the \( K^- \)-neutron cross-sections, for example on total cross-sections at higher energies by scattering off deuterium; information on \( K^- \)-neutron forward scattering can be deduced from sufficiently detailed data on \( K^- \)-proton interactions, on the lines indicated above for the discussion of the \( K^- \)-proton data now available, but information on \( K^- \)-neutron forward scattering will clearly need more direct experiments. From this information it would be possible to draw deductions concerning the \((K \Sigma N)\) interaction alone, which could then be used in conjunction with the \( K^- \)-proton scattering analysis by dispersion relations to lead to more clear conclusions on the parity and strength of the \((KA \Lambda N)\) interaction.

Next, we turn to discuss some points concerning the new data on \( K^- \)-deuteron interactions, which Tripp has just reported from the work of the Alvarez bubble chamber group. This data has given us our first clear and very welcome verification of a charge-independence equality. There has not been sufficient time for the measurements to go so far as to distinguish \( \Sigma^0 \) and \( \Lambda^0 \) events in the cases where this may be possible. However, it is of interest to note that there are only 7 \((\Sigma^- \pi^0 p)\) events recorded relative to 48 \((Y^0 \pi^- p)\). Since charge-independence requires the number of \((\Sigma^- \pi^0 p)\) and \((\Sigma^0 \pi^- p)\) to be equal, it appears that the relative production of \( \Sigma^0 \) and \( \Lambda^0 \) from \( K^- \)-neutron interactions is \( \sim 7/41 \), so that \( \Lambda^0 \) production is dominant here, in contrast to the situation for \( K^- \)-proton captures.

This is rather reminiscent of the conclusion reported by the Bern group \(^{13}\) that \( \Lambda \) production appears to be comparable with the total \( \Sigma \) production for \( K^- \) interactions in flight at 90 MeV. However, for deuterium this result could quite well arise from secondary interactions in which a \( \Sigma \)-particle produced interacted with the neighbour nucleon, transforming to a \( \Lambda \)-particle and causing an increase in the \( \Lambda/\Sigma \) ratio observed.

The \( \Sigma^-/\Sigma^0 \) ratio appears to be rather lower than that known for \( K^- \)-proton capture, although some increase might have been expected owing to the additional neutron capture events. Since the energy dependence of the \( \Sigma \) capture scarcely comes into play here, it may be necessary to attribute this change (and perhaps part of the increase in \( \Lambda \) production) to an increased rate of \( K^- \) capture from the 2p state in deuterium, relative to that for hydrogen. This may result from the additional capture channels now available through the neutron interactions, as well as from the larger reduced mass in the \( K^- \)-deuterium system. For the cases discussed by Fuji and Marshak \(^{4} \), it turned out that the rate of 2p absorption in deuterium was about three times larger than in hydrogen. To clarify the situation, it would be desirable to study in some detail the energy spectra and correlations in \( K^-D \) reactions. Okun' and Smushkevich \(^{13} \) have submitted a theoretical study of such correlations on the basis of the impulse approximation, but taking into account elastic scattering between the final baryons; however, there is no data available at present, except in one negative respect, namely that no bound states of type \( \Lambda \) or \( \Sigma n \) have been detected among the \( K^-D \) reaction products. Estimates for the rate of formation of such bound states (if they exist) were made by Pais and Treiman \(^{14} \) some time ago. Generally speaking, they found that these bound states should be formed about as frequently as the corresponding unbound systems, for a binding energy of 1 MeV. For smaller binding energies \( B \), the branching ratio falls off about as \( \sqrt{B} \). On this basis, the absence of these states in the \( K^-D \) reactions implies that their binding energies (if positive) are unlikely to exceed several tenths of an MeV. One important qualification is necessary: if these systems were bound only in the \( 1S \) state (and we shall later see reason to believe that this is the most favourable state for binding), then capture of a pseudoscalar \( K \) meson from the \( 1S \) state of deuteron could not give rise to this bound state, owing to the selection rules of angular momentum and parity conservation, although capture from the \( 2p \) state could.

Finally the rate of two-nucleon capture modes in deuterium, e.g. \( K^- + D \rightarrow \Sigma^- + P \), is of great interest, in view of the observation that the two-nucleon capture of the \( K^- \) meson takes place in perhaps 15% of \( K^- \) nuclear capture events in emulsion. From experience with the process of two-nucleon capture for pions, it would seem reasonable to expect this figure to imply that about 2% of \( K^- \) captures in deuterium should involve both nucleons. This is not at all excluded by the present preliminary data; when the data has increased to the point where a statistic-
ally significant comparison can be made between the deuterium and the emulsion data on this point, this will be of considerable interest for nuclear physics since there has been no test of the correlation functions in deuterium and in complex nuclei for such large momentum transfer to date. It is of interest to add that the perturbation calculations of Fuji and Marshak lead to a branching ratio ~ 0.1% for two-nucleon capture in the case of pseudoscalar $K^-$-capture from $s$-states, and ~ 10% for scalar $K$ meson capture; for $2p$ capture, the proportions were ~ 1% for pseudoscalar $K$ meson and ~ 0.1% for scalar $K$ meson.

**Hyperon-nucleon interactions**

Strong interactions between a hyperon and a nucleon may be transmitted by the exchange of $K$ mesons and of pions between them. Exchange of an odd number of $K$ mesons and any number of pions involves the transfer of strangeness between the particles and gives rise to an "exchange" interaction, whereas exchange of pions alone or with an even number of $K$ mesons gives an "ordinary" interaction. Now, since the hyperons are coupled at least moderately strongly to $K$ mesons and nucleons, and the nucleons very strongly with the pions, there must certainly exist pion-hyperon interactions of the type

$$ A \leftrightarrow \Sigma + \pi, \quad \Sigma \leftrightarrow \Sigma + \pi. \quad (16) $$

The isotopic spin forms of these Yukawa-type interactions are determined by charge independence, and the coupling parameters will be denoted by $g_{A\Sigma}$ and $g_{\Sigma\pi}$, respectively. If these interactions (16) are reasonably strong, they will dominate over the $K$ meson interactions in hyperon-nucleon interactions at low energies, owing to the larger range associated with the pion exchange process.

At this conference last year, Gell-Mann and Schwinger each put forward the hypothesis of a "global symmetry" involving a universal pion-baryon coupling. In order to allow a comparison between the pion coupling for the $T = \frac{1}{2}$ nucleon doublet and the $A$, $\Sigma$ multiplets of integer isotopic spin, the $A$, $\Sigma$ states were re-arranged into two doublets

$$ Y = \left( \begin{array}{c} \Sigma^+ \\ (A - \Sigma)/\sqrt{2} \end{array} \right), \quad Z = \left( \begin{array}{c} (A + \Sigma)/\sqrt{2} \\ \Sigma^- \end{array} \right). \quad (17) $$

Of course this makes sense only if the $A$ and $\Sigma$ multiplets have the same parity, as is here assumed. In terms of these $Y$ and $Z$ doublets, the form of the pion-baryon coupling was

$$ G (\bar{N} \tau N \cdot \pi + \bar{Y} \tau Y \cdot \pi + \bar{Z} \tau Z \cdot \pi + \bar{\Sigma} \tau \Sigma \cdot \pi), \quad (18) $$

where $G$ is the known pion-nucleon coupling parameter and these interaction terms each have the same form in spin and space variables, so that the $Y$ doublet and the $Z$ doublet each behave in the same way as the nucleon doublet as far as pion interactions are concerned. The use of this form of coupling is equivalent to the choice $g_{A\Sigma} = g_{\Sigma\pi} = G$. It is also possible to consider the choice $g_{A\Sigma} = g_{\Sigma\pi} = -G$, corresponding to a minus sign before the $Y$ and $Z$ terms of (3), but it appears very unlikely (see below) that this can be compatible with the experimental facts.

This "global symmetry" obviously cannot correspond completely with the observed facts, of course, since there is a mass difference of ~ 80 MeV between $\Sigma^0$ and $A$ states; this symmetry can hold valid only to the approximation that the mass difference $A = m_{\Sigma^0} - m_A$ can be neglected. Further, if the $K$ couplings were also very strong, they would be expected to distort this symmetry between the pion-baryon couplings quite severely, so that the proposal was put forward that the $K$ couplings might be regarded as a moderately strong interaction whose effects on the pion-hyperon interaction might be neglected as a first approximation. This appears fairly reasonable, as a coupling strength of about one-tenth of that for the pion-nucleon interaction appears reasonable for the $KYN$ interactions, assuming these to be of pseudoscalar form.

It has appeared attractive to seek for a possible symmetry between the strong interactions $N \leftrightarrow A + K$, $N \leftrightarrow \Sigma + K$ also; for example there was the hypothesis $g_{AK} = g_{\Sigma K} = g$. This would then lead to the interaction form

$$ \sqrt{2} g \left[ \bar{N} (YK^0 + ZK^+) + h.c. \right]. \quad (19) $$

However, it has been pointed out by Pais that this possibility is excluded by the experimental data if the pion-baryon symmetry $g_{A\Sigma} = g_{\Sigma\pi}$ holds. For example, since $K^+$ is coupled only with $Z$, and since there are no couplings which mix $Z$ and $Y$, it is clear that there are no interactions which can lead to the charge exchange process

$$ K^+ + n \rightarrow K^0 + p, \quad (20) $$

whereas this process is well known. Another counter-example is the reaction

$$ K^- + p \rightarrow \Sigma^+ + \pi^-. \quad (21) $$

Here the $K^-$ is coupled only with $Z$-states, and the pion-hyperon couplings cannot modify this. Hence the $\Sigma^+$ state (a $Y$-state) cannot be reached from an initial $K^-$ particle and so this reaction is forbidden, contrary to the evidence. So if the pion-baryon symmetry $g_{A\Sigma} = g_{\Sigma\pi}$ holds, then $g_{AK} \neq g_{\Sigma K}$.

The limitation that this pion-baryon symmetry can be fully effective only in situations for which the $\Sigma^0 - A$ mass difference is unimportant severely restricts its applicability to many strange particle reactions without rather detailed considerations, so that the possibilities for clear-cut tests of this symmetry hypothesis seem relatively few at present. For this reason it is of interest to mention one situation where this symmetry principle makes a rather clear prediction, although there is no data available yet for a check. This concerns the magnetic moments of the
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27- and \(L\)-particles. It appears reasonable (cf. Drell, Session 1) to expect these to receive much greater contributions from the pion currents than from the \(K\) currents within each baryon. For the terms associated with pion processes alone, the prediction is that \(\mu(\Sigma^+) = \mu(Y^+)\) should just be \(\mu(p)\), the proton magnetic moment, whereas \(\mu(\Sigma^-)\) should equal \(-\mu(p)\). In this approximation, the \(\Sigma^0\) and \(A\) moments should each be zero and the matrix element for the process \(\Sigma^0 \rightarrow A + \gamma\) should have the form

\[\mu_\gamma \sigma \cdot H,\]

where the transition moment \(\mu_\gamma\) is the negative of the neutron magnetic moment.

Now we shall look at several situations where the pion-baryon interaction is of obvious importance and for which there exists a little empirical evidence. The first of these concerns the interactions between \(\Sigma\)-particles and nucleons. In the low energy region (where the pion interactions will predominate) these interactions may be represented by potentials, to a sufficiently good approximation at present. The neglect of the mass difference \(\Lambda\) in intermediate states will affect the calculation of these potentials relatively little (by less than 10%), since the intermediate states which contribute most have relatively high energies. To this approximation, the \(Y-N\) and \(Z-N\) potentials are predicted to be identical with the \(N-N\) potentials. On this basis one may then hope to proceed phenomenologically and to deduce the hyperon-nucleon potentials from the evidence on nucleon-nucleon scattering. Comparison of these potentials with the data on hyperon-nucleon interactions would then provide a test for the global symmetry hypothesis. For example, the \(2\Sigma\) state, the \(\Sigma^- p\) or \(\Sigma^- n\) potential is predicted to be the same as the \(1S n-n\) or \(p-p\) nuclear force and therefore almost resonant at zero energy (see below), but for the \(3S\) state, the prediction is by no means unambiguous since the Pauli principle forbids the \(3S\) state for the \(p-p\) or \(n-n\) systems. Only triplet states with odd \(l\) are permitted for identical particles, and it is therefore necessary to extrapolate the interaction in these states to the triplet states with even \(l\). In principle, this extrapolation is not possible since it is possible to construct a potential which vanishes for states of low odd \(l\), although finite for states of low even \(l\) (note that position exchange terms are excluded in the potential in so far as it is due to pions alone), although such potential terms do not appear very reasonable from the viewpoint of simple meson theory. With a potential limited to simple static and spin-orbit forms, the \(3S\) \(\Sigma^- p\) potential may be obtained unambiguously if the odd-\(l\) triplet potential is sufficiently well-established.

Bryan, de Swart, Marshak and Signell have made calculations (see Figs. 4 and 5) of the scattering cross-sections and polarization properties for \(\Sigma^+ p\) scattering on the basis of the Marshak - Signell potential which fits well the \(p-p\) scattering below 200 MeV. However this calculation illustrates the practical difficulties of an extrapolation from the observed triplet \(p-p\) scattering to the \(3S\) \(\Sigma^+ p\) potential. Since the triplet \(p-p\) scattering is due to \(p\)-states and higher, it is not sensitive to the form of the triplet potential at short distances, whereas the properties of the \(s\)-wave are quite strongly affected by the form of the potential at short distances. The Marshak - Signell potential actually predicts a \(3S\) bound \(\Sigma^+ p\) state, but it is found possible to modify the triplet state potential at short distances sufficiently to remove this bound state, without affecting appreciably the fit of the potential for the \(p-p\) scattering.

The question whether bound states should be expected to exist for the \(\Sigma^- n\) and \(\Sigma^+ p\) systems is an interesting one. At present there is no clear empirical evidence to indicate the existence of such bound states; in fact, as we have

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Fig. 4. *Theoretical differential cross-sections for \(\Sigma^+ p\) elastic scattering as computed by Bryan, de Swart, Marshak and Signell*.  
Fig. 5. *Theoretical polarization in \(\Sigma^+ p\) scattering as computed by Bryan et al.*
seen above, the evidence from $K^-D$ capture experiments is that a $1S$ bound state for $\Sigma^-n$ is very unlikely, although a $1S$ bound state is not yet excluded. On the basis of the global symmetry hypothesis, meson-theoretical calculations by Lichtenberg and Ross (1957) and by Ferrari and Fonda (1957) indicate that the $\Sigma^-n$ potential has most attraction in the $1S$ state, for which the potential is closely related to the nucleon-nucleon potential. Ferrari and Fonda have pointed out that, in the static limit ($G\alpha \cdot p / 2M_p$) of the universal interaction $G\gamma\mu$, the $\Sigma^-n$ potential may be expected to be weaker than the $n-n$ potential since the hyperon mass $M_p$ is about 25% greater than the nucleon mass. This is partly balanced by the appearance of a larger reduced mass in the $\Sigma^-n$ system, but the net effect is that the $\Sigma^-n$ system will be further from binding than the $n-n$ system, as far as forces due to pion exchange are concerned. Ferrari and Fonda emphasize that $K$ meson exchange would contribute an additional attractive interaction for a pseudoscalar $K$ meson (repulsive for a scalar $K$ meson), so that observation of a $\Sigma^-n$ bound state would not still conflict with the global symmetry hypothesis.

When a $\Sigma^-$-particle comes to rest in hydrogen, the $\Sigma^-p$ capture reaction is observed to lead to the reactions

\begin{align}
\Sigma^- + p &\rightarrow \Sigma^0 + n, \quad (22a) \\
&\rightarrow A + n, \quad (22b)
\end{align}

in the ratio $A/\Sigma^0 \approx 2$. Now since the $\Sigma^-$-particle is a $Z$ particle, the pion-hyperon interactions cannot lead to the $(A - \Sigma^0)/\sqrt{2}$ state, which belongs to the $Y$ doublet, so that the global symmetry principle would suggest

\[
\begin{align}
\langle \Sigma^- p \mid S \mid \frac{A - \Sigma^0}{\sqrt{2}} n \rangle &= 0 \\
&= \frac{1}{\sqrt{2}} \left\{ \langle \Sigma^- p \mid S \mid A n \rangle - \langle \Sigma^- p \mid S \mid \Sigma^0 n \rangle \right\}.
\end{align}
\]

On this basis, one might expect the matrix elements for the processes (22a) and (22b) to have the same form, in which case the ratio $A/\Sigma^0$ would be expected to have the value $(p_h/p_e)^{21/2} \sim (4)^{21/2}$ for capture from a state of the $\Sigma^-p$ atom of orbital angular momentum $l$, the outgoing momenta in processes (22a) and (22b) being $p_h \sim 280$ MeV/c and $p_e \sim 70$ MeV/c. This conclusion does not agree with the data, but the situation is more complicated than this simple argument assumes. To a good approximation, the argument of equation (23) may be used to deduce that the potentials $\langle \Sigma^- p \mid V(r) \mid \Sigma^0 n \rangle$ and $\langle \Sigma^- p \mid V(r) \mid A n \rangle$ are equal, on the basis of global symmetry. However the wavelength of the outgoing $A$-particle is shorter than the range $(\hbar/m_mc)\Sigma^0$ of these potentials by about a factor 2, whereas the $\Sigma^0$ wavelength is longer than this range by about the same factor. This means that the matrix element $M_f(p) \sim \int V(r) r^l j_l(p_r) d_2(p_r)$ relevant to these processes when the potentials are treated in lowest order approximation has to be evaluated for quite different momenta in the two cases and may be expected to be a good deal smaller for process (22b) than for (22a); in fact a first order calculation on these lines leads to a $A/\Sigma$ ratio of less than unity. For capture at rest, the direct application of the global symmetry argument to the matrix elements for processes (22a) and (22b), as given following equation (8), would be valid only if $\Sigma$ were less than $\sqrt{2}/2M$. In a more exact treatment of the matrix elements from the potentials, the hyperon-nucleon system must be discussed in terms of states of definite isotopic spin $T$, since the global symmetry is broken down by the $A-\Sigma$ mass difference. The $\Sigma^-p$ system is then split into a $T = \frac{3}{2}$ state for which the potential is the nucleon-nucleon $T = 1$ potential $V_1$, and for which the transformation (22b) is not possible, and a $T = \frac{1}{2}$ state for which the potential is represented by a matrix between $\Sigma$ and $\Lambda$ states, expressible in terms of the nucleon-nucleon potentials $V_0$ and $V_1$. For the $T = \frac{1}{2}$ potential, this form is explicitly

\[
\begin{align}
V_{\Sigma\Sigma} &\equiv V_{\Sigma,1} = \left( \frac{1}{2} V_1 + 3 V_0 \right), \\
V_{\Sigma\Lambda} &\equiv V_{\Lambda,1} = \left( \frac{\sqrt{3}}{4} \right) (V_0 - V_1).
\end{align}
\]

The potential $V_1$ is strongly attractive, so that there is strong scattering in the $T = \frac{3}{2}$ channel. In the $T = \frac{1}{2}$ channel, the mass difference $A$ is important and a pair of simultaneous equations are to be solved, to give the amplitudes of the outgoing $\Sigma^0n$ and $\Lambda-n$ waves for the $T = \frac{1}{2}$ state. If, for example, the $\Sigma^-p$ $T = \frac{3}{2}$ state were just in resonance at zero energy for the capture state considered, then the process (22a) would dominate strongly, process (22b) then being possible only through the non-resonant $T = \frac{1}{2}$ channel. Detailed calculations of this kind have been carried through by Lichtenberg and Ross (1957) and by Weitzner (see Appendix, p. 329), but from these remarks it will be clear that the conclusions reached will depend a great deal on the precise treatment of the hyperon-nucleon potentials. Also the conclusions will depend by how much capture occurs from the $2p$ state of the $\Sigma^-p$ atom; owing to the long range of the hyperon-nucleon potential, it appears probable that $p$-state capture may be predominant. The situation is quite complicated and a prediction of the $A/\Sigma^0$ ratio in reaction (22) cannot be made on the basis of global symmetry alone, without detailed consideration of the effect of the mass difference $A$.

Global symmetry can make a clear statement about these reactions only for $\Sigma^-p$ reactions of high energy (so that the final kinetic energies are little affected by the mass difference) and of low momentum transfer (so that the reactions do not explore the region of the potential where $K$ meson processes contribute).

In a first order calculation of the $\Sigma$-nucleon potential (Lichtenberg and Ross, 1957; Ferrari and Fonda, 1957), this
Strange particle interactions

potential is found to be attractive in the $^3S$ and $^5S$, $T = \frac{3}{2}$ states and in the $^3S$, $T = \frac{1}{2}$ state for $g_{\Sigma A} = g_{\Sigma A} = +G$, but repulsive for the $^1S$, $T = \frac{1}{2}$ state. With $g_{\Sigma A} = g_{\Sigma A} = -G$, the signs of these first order potentials are to be reversed. Gilbert and White (1957) have argued from a comparison of the pion spectra observed in $K^-$ capture in emulsion nuclei with and without an accompanying $\Sigma^-$ emission, that the $\Sigma^-$ nuclear potential is attractive and about 30-40 MeV deep. This conclusion appears difficult to reconcile with coupling parameter $-G$ for the pion-hyperon coupling and appears to require the choice $+G$ as made by Gell-Mann and Schwinger.

Another situation of interest for the global symmetry hypothesis is the $K^-p$ capture reaction, since the pion-hyperon interactions in the final state will affect the branching ratios for these reactions. The most extensive discussion on this basis has been given by Amati and Vitale. These authors note that the final pion-hyperon system may be expressed in terms of $\pi Y$ and $\pi Z$ scattering states, and that the scattering properties of the $\pi Y$ and $\pi Z$ states are identical, being characterized by the same $T = \frac{1}{2}$ and $\frac{3}{2}$ scattering phases (equal to the pion-nucleon phase-shifts if $g_{\Sigma A} = g_{\Sigma A} = G$). The $T = 0$ $\Sigma$-state corresponds to $T = \frac{1}{2}$ $\pi Y$ and $\pi Z$ final states; for $K^- + p$ capture from rest the phase of this amplitude (assuming time reversal invariance) is precisely the $T = \frac{1}{2}$ phase-shift $\delta_{1/2}$. Two orthogonal $T = 1$ pion-hyperon states are then formed, each of which corresponds to $T = \frac{3}{2}$ $\pi Y$ and $\pi Z$ systems; the two amplitudes which lead to each of these final states then both have the phase $\delta_{3/2}$. After forming the expressions for the branching ratios in terms of these three real amplitudes and the relative phase ($\delta_{1/2} - \delta_{3/2}$), Amati and Vitale show that they imply the inequality

$$ (\Sigma^+ + \Sigma^- - 4\Sigma^0)^2 + 4(\Sigma^0 - \Sigma^+\Sigma^-) \geq 0. \quad (25) $$

The standard ratios $\Sigma^+ : \Sigma^0 : \Sigma^+ : A \sim 1 : \frac{1}{2} : \frac{1}{2} : \frac{1}{4}$ observed for $K^-p$ capture from rest fail to satisfy this inequality, giving the expression (25) the value $-\frac{3}{16}$. This explains in a general way why Kawarabayashi and Yamaguchi, who carried through detailed calculations on the static model for pseudoscalar and for scalar mesons respectively (assuming lowest order perturbation theory for the $K$ interaction), were unable to find agreement with the data for any values of the free parameters in their calculations. It is unclear how significant this discrepancy is to be considered. For one thing the violation of inequality (25) is fairly sensitive to the proportion of the $\Sigma^0$ reaction, which is relatively poorly known; for another, it is far from clear at present how large an effect the $\Sigma-A$ mass difference will produce in the final state. However the derivation of the inequality (25) does not involve the assumption that the $KYN$ interactions are weak (only that they do not appreciably modify the symmetry between the pion-hyperon interactions) but does require that the capture reaction occurs from a state of definite angular momentum, either from a bound state or from a continuum state of very low energy.

Finally, we turn to the $A$-nucleon interaction below the $\Sigma$ threshold, for which there is a good deal of evidence from observations on $A$-hyperfragments. Here the two-body $A$-nucleon forces are due firstly to exchange of two pions, the simplest exchange compatible with $T = 0$ for the $A$-particle, and of three pions or more, as well as to the exchange of $K$ mesons with or without pion exchange. The forms to be expected for these potentials have been calculated in the static limit, in varying degrees of approximation and for the various $KA$, $K\Sigma$ and $\Sigma A$ parities, by Dallaporta and Ferrari, Lichtenberg and Ross (1957) and by Ferrari and Fonda. These calculations will be discussed briefly after some remarks on the phenomenological analysis, but we may anticipate this comparison here by the remark that it appears necessary to assume that the forces are due very considerably to the pion exchange processes, as required by the global symmetry hypothesis.

Several authors, Spitzer and Weitzner (see Appendix p. 329) and Bach have pointed out that in this situation, three-body forces may be expected to occur in the same order of approximation of perturbation theory (see Fig. 6). In lowest approximation they will necessarily have the form

$$ V_3(1, 2, A) = \sigma_1 \sigma_2 \tau_1 \tau_2 W(r_{1A}, r_{2A}) + \text{non-central terms,} $$

where $W(r_{1A}, r_{2A}) \sim \exp \left( -\mu r_{1A} - \mu r_{2A} \right)$ when the separations $r_{1A}, r_{2A}$ are both large.

Next we note that if the two nucleons have $s$-wave relative motion, then $\sigma_1, \sigma_2, \tau_1, \tau_2 = -3$, so that these three-body forces do not depend on the spin or isotopic spin for interaction of the $A$-particle with a pair of $s$-shell nucleons. The various calculations reported for these three-body forces have given rather different results. Weitzner's calculation assumes an interaction $F \Lambda A \pi \pi \pi$ to be responsible for both processes (A) and (B) of Fig. 6 and finds the central part of the three-body potential to be repulsive; Spitzer begins from a pseudovector $A \leftrightarrow \Sigma + \pi$ interaction and finds three-body forces with an attractive central part, whereas Bach's calculation on a similar basis found a weakly attractive central term. In each calculation, the three-body potential obtained had a complicated non-
central form. The implications of the existence of such three-body potentials will be discussed below.

Dalitz and Downs \(^{21}\) have given a phenomenological analysis of hypernuclear binding energies based on two-body forces alone. The \(A\)-nucleon potentials were assumed to be central and of Gaussian shape; these simple potentials are to be understood as potentials equivalent to the actual \(A\)-nucleon potentials as far as their low energy scattering properties are concerned. For the \(^4\text{He}\) system, the result obtained for the total \(A\)-nucleon potential (see Table II) seems fairly reliable owing to the rigidity of the \(a\)-particle and our detailed knowledge from electron scattering experiments of the nuclear parameters for \(^4\text{He}\). For the \(^4\text{He}\), \(^2\text{He}\) doublet, the value given is somewhat less certain since there is no direct measure available for the nuclear parameters of \(^3\text{He}\) or \(^3\text{He}\) — the value given for \(U_3\) in the table was obtained assuming values which seem reasonable for the \(^3\text{He}\), \(^3\text{He}\) radii. At this point it is of interest to remark that it seems definite that the \(^4\text{He}\), \(^2\text{He}\) doublet observed has zero spin — this conclusion follows from the high proportion (about 45\%) observed for the two-body decay modes among \(^4\text{He}\) decay events. This means that the singlet \(A\)-nucleon potential must be more attractive than the triplet potential; this conclusion is not affected by the possible presence of three-body forces since to a good approximation these do not depend on the \(A\) spin.

The hypertriton \(^3\text{He}\) is of special interest. It has very low \(B_A\), not more than a few tenths of an MeV, so that it has a very open structure. In this situation the three-body forces can have relatively little effect, so that this case should allow a clear estimate of the strength of the two-body force. However with such light binding, a rather flexible trial wave function is needed and the lower values for \(U_3\) shown in Table II are those obtained recently by Dalitz and Downs for \(B_A = 0.25\) MeV, the larger values being those obtained earlier with a simpler form for the trial function. These values are substantially below those given elsewhere in the literature, which were generally obtained using the simplest possible wave functions. Note that the well-depth parameter \(S\) for the mean \(A\)-nucleon potential in the hypertriton is no more than \(3/2\); of course the mean potential here is a combination of the singlet and triplet potentials, \((3U_3 + U_t)/4\). But since the \(a\)-particle gives quite a strong attractive potential, and this potential is given by \((3U_t + U_s)\), the triplet potential cannot be repulsive: from this it follows that the well-depth parameter for the singlet state cannot exceed about 0.9. With the values of \((3U_t + U_s)\) and \((3U_t + U_s)\) given in Table I, the well-depth parameter obtained for the singlet state is actually 0.75, whether the potential is due to pion processes, or to \(K\) exchange, or to both. From these remarks, it follows that there can be no bound state for the \(A-p\) or \(A-n\) system, in accord with the lack of any such evidence. The main assumption here has been the neglect of three-body forces.

Weitzner has pointed out that these volume integrals \(U_2\), \(U_3\) and \(U_4\) could also be fairly well represented by the assumption of a repulsive three-body force, such as he has computed, together with a \(A\)-nucleon two-body potential having only little spin dependence. Including a three-body force, the expressions given above (Table II) for the volume integrals of the \(A\)-nuclear potentials should be replaced by

\[
\begin{align*}
3U_t + U_s + 6W &= U_4 & (27a) \\
\frac{3}{2}U_t + \frac{3}{2}U_s + 3W &= U_3 & (27b) \\
\frac{1}{2}U_t + \frac{1}{2}U_s + W &= U_2, & (27c)
\end{align*}
\]

**TABLE II**

\(A\) — Nucleus interaction potentials

<table>
<thead>
<tr>
<th>Hypernucleus</th>
<th>Range Parameter</th>
<th>Potential</th>
</tr>
</thead>
<tbody>
<tr>
<td>(^3\text{He})</td>
<td>(\frac{h}{m_Kc} = 490 \rightarrow 422)</td>
<td>(U_2 = \frac{3}{2}U_4 + \frac{1}{2}U_t)</td>
</tr>
<tr>
<td>(^3\text{He})</td>
<td>(S = 0.66)</td>
<td>(U_3 = \frac{1}{2}U_4 + U_s)</td>
</tr>
<tr>
<td>(^4\text{He})</td>
<td>(\sim 650)</td>
<td>(U_4 = 3U_t + U_t)</td>
</tr>
<tr>
<td>(^2\text{He})</td>
<td>(\sim 870)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(910)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(420 (S = 0.74))</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(240 (S = 0.75))</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(150)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(165)</td>
<td></td>
</tr>
</tbody>
</table>

*(Volume integrals are specified in units of MeV. \(f^3\))
where \( W \) denotes the volume integral of the three-body potential over \( A \) positions for two nucleons in a distribution with the standard nucleon density. In equations (27b) and (27c), the coefficients of the \( W \) term should be modified a little by amounts depending on the average nucleon density in He^3 and H^3, respectively, relative to that for He^4: however these modifications are not important within the present uncertainties. It is then clear that the three equations (27) involve only the quantities \( U_s \) and \((U_t + 2W)\) and that they are consistent only if \( U_s = 3(U_t + 2U_s)/8 \), a condition satisfied by the values given in the table II, within their uncertainties. In all cases the singlet potential \( U_t \) is given by \((U_s - 1/2U_t)\). However the best estimate \( U_t \) comes from \( U_s \) and \( U_t \) and is that given in the table, corresponding to a well-depth parameter \( S \simeq 0.75 \) for either \( h/m_Kc \) or \( h/2m_Nc \) range. The triplet potential \( U_t \) itself cannot be determined from the equations (27): as pointed out by Weiztnzer, it is possible to assume a repulsive three-body potential with \( W \) about 72 MeV \( f^2 \) and to obtain \( U_t \sim U_s \). It appears that the only clear way to decide what are the relative strengths of the two- and three-body potentials is to obtain information directly on \( \Lambda \)-proton scattering for particles of energy much less than 150 MeV. Probably hydrogen bubble chamber evidence on \( \Lambda \)-particles will give some information on the size of these cross-sections before too long.

The strength of the \( \Lambda \)-nucleon potential obtained for the singlet state may be compared with the \( \Lambda \)-nucleon potential computed from meson theory by Lichtenberg and Ross (1957) assuming symmetry for the pion-hyperon coupling. The calculated potentials agree with the potentials found empirically in that they predict the stronger attraction to be in the singlet state; the empirical strength for the singlet potential corresponds to a \( \pi \)-hyperon coupling constant a little larger than the pion coupling constant. Unfortunately it is not possible to make such a direct comparison with the extensive calculations of Ferrari and Fonda \(^{22}\) to which I now wish to refer. These authors have calculated \( \Lambda \)-nucleon forces in the static approximation up to fourth order in the coupling parameters for all combinations of relative parities of \( K \) mesons and hyperons, including the exchange of both \( K \) mesons and pions. Of course, these potentials are very singular and must be cut off, the core radii being chosen equal to those for the Brueckner - Watson nucleon-nucleon potential. If one wishes to use these potentials directly in a calculation of the \( \Lambda \)-nuclear binding energies, it is clear that the trial functions must be quite complicated. The trial functions they used for the \( \Lambda \)-particle motion were of the form

\[
\psi_A(r) = F(r) \prod_{i=1}^{A-1} (1 - e^{-\beta(|r-r_i|-r_c)}) ,
\]

for all \( |r-r_i| > r_c \),

with any \( |r-r_i| < r_c \),

where the product is taken over all \( A-1 \) nucleons of the core nucleus and \( \beta \) is a variational parameter, the distribution of nucleons in the core nucleus being taken from experiment as far as possible. Ferrari and Fonda find that if they neglect \( K \)-meson exchange they can obtain a reasonable value for the binding energies only with the same parity for \( \Sigma \) - and \( \Lambda \)-particles and a coupling parameter \( g_{\Sigma K}^2/4\pi \simeq 16 \), perhaps a little larger than the value known for the pion-nucleon coupling. However, if the \( K \)-meson exchange is also included, a pseudoscalar \( K \)-meson gives rise to an additional attraction and a coupling parameter \( g_{\Sigma K}^2/4\pi = g_{\Sigma K}^2/4\pi \simeq 2 \) provides sufficient attraction to allow a fit with \( g_{\Sigma K}^2/4\pi = 13 \). However, since the higher order pion potentials are not included, nor any estimate of three-body potentials, it is difficult to take this last refinement very seriously, although Ferrari and Fonda remark that scalar \( K \) meson would contribute repulsion, leaving more attraction to be made up by the higher order terms. With the inclusion of \( K \) exchange, Ferrari and Fonda find that they can also obtain a qualitatively satisfactory potential assuming negative parity for \( (\Sigma A) \), and either scalar or pseudoscalar \( (K A) \) parity, by suitably choosing the signs and magnitudes of the various coupling parameters; for these cases, however, about half of the potential must be provided by \( K \) exchange, the fit obtained appears somewhat artificial and Ferrari and Fonda have not investigated the binding energy situation in detail for these cases.

Some direct evidence that the the two-body potential has a range of order \( h/2m_Nc \) is provided by a comparison of the potentials \( U_s \) and \( U_t \) derived from data on the \( p \)-shell hypernuclei \( \Lambda \)Li\(^7\) and \( \Lambda \)Li\(^8\) with those given in Table I for the light hypernuclei. Using parameters recently obtained by the Stanford group for the shape and radius of Li\(^4\), the values obtained for \( U_s \) and \( U_t \) greatly exceed those expected for a \( \Lambda \)-nucleon potential of range parameter \( h/m_Nc \). Agreement between \( U_s \) and \( U_t \) of Table I requires a range parameter perhaps 10% larger than \( h/2m_Nc \) for the \( \Lambda \)-nucleon potential, which gives qualitative support for the conclusion that the \( \Lambda \)-nucleon potential arises mainly from the pion processes.

On the basis of the phenomenological analysis it is of interest to note that it appears quite probable that the \( \Lambda \)H\(^4\) system should have an excited bound state of spin 1. The value of its binding energy will depend on the degree of spin-dependence for the \( \Lambda \)-nucleon potential. If there is a repulsive three-body potential, this will reduce the degree of spin dependence necessary to account for the \( B_\Lambda \) data, and therefore make it more certain that this state should be bound. This may be somewhat unfortunate for the \( K \)-He\(^4\) capture experiment which has looked so promising for the determination of the \( K \)-A relative parity. For a scalar \( K \) meson, the direct formation of ground state \( \Lambda \)H\(^4\) is strictly forbidden, but the selection rules permit the formation of \( \Lambda \)H\(^{**\prime}\) with spin 1. If the excited state \( \Lambda \)H\(^{**\prime}\) is formed in this capture reaction, it will then decay by \( \gamma \)-emission to ground state \( \Lambda \)H\(^4\), so that normal \( \Lambda \)H\(^4\) decay events may be observed, even when the direct formation of \( \Lambda \)H\(^4\) is completely forbidden. This situation may prove quite difficult to sort out.
Finally, there are some calculations to report on a correlation effect which was reported last year in the three-body decay mode for hypernuclei,

\[(A + A) \rightarrow A + P + \pi^-.\]  \hspace{1cm} (28)

The configuration of this decay mode may be characterized conveniently by giving the recoil momentum \(p_A\) of the residual nucleus \(A\) and the angle \(\theta\) defined in Fig. 7. The distributions in \(p_A\) and \(\cos \theta\) obtained for various hypernuclei in the recent EFINS-NU collaboration are shown in Fig. 8. The anisotropy in the \(\cos \theta\) distribution, especially for the case of \(^5\text{He}\) decay, is very striking since an isotropic distribution would follow from an \(s\)-wave motion for the initial \(\Lambda\)-particle if the nuclear interactions between the final proton and the nucleus \(A\) could be neglected. For the case of \(^5\text{He}\), the effect of this final state interaction is especially strong, owing to the low energy resonance in \(p\)-\(^4\text{He}\) scattering, as pointed out by Cottingham and Byers \(^{23}\) who have recently completed a calculation of the distributions to be expected for \(^5\text{He}\) decay as a result of this resonant interaction. As shown in Fig. 8, their calculation agrees remarkably well with the observed \(\cos \theta\) distribution for \(^5\text{He}\) decay events. The comparison of the \(p_A\) distribution with experiment for \(^5\text{He}\) suffers from a bias in the identified \(^5\text{He}\) events owing to the fact that it becomes very difficult to distinguish between \(^4\text{He}\) and \(^5\text{He}\) decay events when the residual \(\text{He}\) nucleus has a momentum of 100 Mev/c or less. However this bias against low \(p_A\) values disappears when the data on all \(^5\text{He}\) decays of type (28) are taken together.

The \(p_A\) distribution \((*)\) for about 150 \(^5\text{He}\) decay events is given in Fig. 9 and shows some evidence for the double-peaked structure (curve A) predicted for \(^5\text{He}\) decay by Cottingham and Byers. On this Fig. 9, curve B shows the \(p_A\) distribution expected when the final nuclear interaction is neglected, only the final phase-space and the momentum distribution in the initial state being taken into account.

\(*)\) Note added in proof: In Fig. 9 the abscissa has been labelled erroneously. Instead of \(p_A\) read \(Q_A\), where

\[Q_A = [(M_A - B_A - p_A^2 / 2M_A^2) - P^2_A]^{1/2} - M_P - m_\pi\]
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DISCUSSION

Alvarez: This photograph (see Fig. 1) was taken with the hydrogen bubble chamber at Berkeley and, like the work of Fry, is a confirmation of the Pais - Gell-Mann scheme. It is a bit more complete, because it shows the production of the $K^0$, in addition to its interaction as a $K^0$. At 1), a $\pi^-$ interacts with a proton, and produces a $\Lambda^0$ (2) and a $K^0$. (The angle and momentum of the $\Lambda^0$ show that no $\Sigma^+$ was produced, so one can predict accurately the direction and energy of the $K^0$ produced.) The neutral $K$ particle interacts with a proton at 3), to give a $\Sigma^+$. In order to conform to the strangeness selection rules, the $K^0$ (of positive strangeness, since it accompanied a $\Lambda^0$) must have changed to a $\bar{K}^0$ (of negative strangeness, since it interacts to produce a $\Sigma^+$). Every angle and measured momentum is in excellent agreement with the kinematics of the assumed reactions.

Okun': I would like to make some comments about the Gell-Mann - Pais - Piccioni oscillations in connection with Fry's experiment. These comments were published by Pontecorvo and myself.42 It is essentially a remark that if you have along with a $\Delta S = 1$ weak interaction also weak interactions which change $S$ (the strangeness) by two, then the most suitable way to see this is to study the Pais-Piccioni oscillations, because the other evidence for or against $\Delta S = 2$ is not so strong.

Such an interaction will give a very large mass difference between $K^0$ and $K^±$ mesons. This is easy to see if you have only the $\Delta S = 1$ interaction this transition will proceed only according to diagrams of the following type:

\[
\begin{array}{c}
\text{\textbullet} \quad \text{\textbullet} \quad \text{\textbullet} \\
K \quad g \quad \bar{K} \\
\end{array}
\]

in which the weak interaction is present twice. If you have an interaction with $\Delta S = 2$ then the $K \rightarrow \bar{K}$ transition represented by such diagrams as:

\[
\begin{array}{c}
\text{\textbullet} \quad \text{\textbullet} \quad \text{\textbullet} \\
\Lambda \quad g \quad \Lambda \\
\end{array}
\]
in which the weak interaction is present only once; and so if the \( \Delta S = 2 \) and \( \Delta S = 1 \) interactions are comparable then the rate of oscillations will be essentially not of the order of the lifetime of the \( K^0 \) meson \( (10^{-15} \text{s}) \), but of the order of \( 10^{-17} \text{s} \).

**Dallaporta:** Comment on the \( K^+ \) scattering. The analyses that were presented were all made on the assumption that \( K^+P \) scattering \( (T = 1 \text{ state}) \) had a constant cross-section. However, it seems now that this cross-section is increasing somewhat with energy. This can perhaps allow the following interpretation: we determine \( s \)-wave amplitudes by total cross-sections at low energy: \( a_{10} \; (T = 1, \; l = 0) \) dominant and negative. Then assuming that the two amplitudes \( a_{01} \) and \( a_{11} \; (T = 0, \; l = 1 \text{ and } T = 1, \; l = 1) \) are of opposite sign, one is taken to be positive in order to give a backward peaked \( K \)-nucleon cross-section and to become important around 150 MeV; the other one, to be negative and to increase above 200 MeV more quickly than the first one. We then obtained that the charge-exchange cross-section increases steeply while the \( X \)-neutron scattering increases afterwards; this is perhaps indicated now by the data. It does not seem that elementary theory should be able to explain this behaviour.

**Fry:** I would like to make one small correction. The experiment on the mass difference — the University of Padua contributed more than Wisconsin, in reality, and was not mentioned.

**Treiman:** I would like to ask once again about this question which seems to be important in several respects about whether the \( K^- \) capture is in an atomic \( S \)-state or \( P \)-state. Dalitz assumed \( S \)-state, I believe. A casual observer looking at the data might have expected that it indicated some \( P \)-state. This seems to be an important question.

**Dalitz:** Certainly it is relatively important. The only thing is that I would say that Jackson et al. have certainly argued very well that it cannot be entirely captured from the \( P \)-state; this may be well be only 10% or less of the capture processes. In order to make progress with some kind of analysis it is necessary, of course, to make some simplifying assumptions and to assume that the capture is entirely from \( S \)-states which is not inconsistent with the data. I agree with you that it is an important question but I think it can only be settled when there is more data in the high energy region (100 to 200 MeV) when one will actually see angular distributions and be able to measure the amount of \( P \)-wave interaction which is effective in this reaction.

**Oppenheimer:** This is just a question. I understood your discussion of the reactions \( K^- + P \) going to \( \Sigma \)'s and \( A \)'s to involve global symmetry for \( \Sigma \)'s and \( A \)'s for \( K \) and \( \pi \) both. It would seem to be involved.

**Dalitz:** As far as the \( K \) meson is concerned, the global symmetry, as Pais has pointed out, is inconsistent with the data. The essential assumption we made in that discussion was that in the final state interaction there is global symmetry for the pion-hyperon system. This means that the four parameters which you need to describe these reactions occur in a different way from the four parameters used in the description based only on charge independence. This leads to a certain inequality because of the relation between certain phases involved in this representation. This inequality is quite independent of any assumption about the \( K \)-meson-proton interaction, whether it is scalar or pseudoscalar or strong or weak.

**Oppenheimer:** So these \( Y \)'s and \( Z \)'s are not directly usable.

**Dalitz:** I am not sure what you mean by that.

**Oppenheimer:** Well, they are split by the \( K \) interaction.

**Dalitz:** Oh, certainly. There are two assumptions which are important. One is that the pion-hyperon symmetry is not strongly modified by the \( K \) meson interaction. This does not mean, of course, that the \( K \) meson interaction must be treated in perturbation theory. The other is that the mass difference between \( \Sigma \) and \( A \) should not affect the reaction amplitudes too strongly. The mass splitting does not appear quite so serious for this reaction as for the \( \Sigma^- \) proton capture, but this needs further investigation.

**Yamaguchi:** I would like to add a comment on the \( K \overline{K} \pi \) reaction. Though this interaction is favoured in explanation of \( K \) nucleon scattering, if this \( K \overline{K} \pi \) interaction is very strong you may expect substantial \( K \overline{K} \) production and this would be in contradiction with experiments.

**Lomon:** With regard to the three-body interaction in hyperfragments. In a thesis by Bach, this was investigated using the same pseudoscalar theory as has been used for the two-body interaction. Applying this in the nuclear calculation it was found that for the hypertriton the effect was indeed only a few per cent and unimportant, and furthermore even this effect was mostly due to strong correlations which exists in the hypertriton and which are not likely to exist to such an extent in the heavier hyperfragments. Therefore, it was thought that in the heavier hyperfragments the three-body interaction would not amount to more than a per cent or so.

**Dalitz:** I would like to apologize for not having mentioned this calculation. There is also a published calculation by Spitzer of Columbia on exactly the same question. In my haste I forgot to mention this. The situation concerning the role of the three-body forces is a little unclear at present; Weiztner suggests fairly strong repulsion. Spitzer obtains moderately strong attraction, and Bach has a moderately weak attraction.

**Eisenberg:** I have a remark about the \( K^- \) interactions in flight at a relative \( K^- \)nucleon momentum of about 350 MeV/c. We seem to have indications from the Berne experiments.
Strange particle interactions

emulsion group data \textsuperscript{12b)} that the total $T = 1$ state interaction (the sum of the $\Lambda$ and $\Sigma$ in $T = 1$ state) is quite strong and as a matter of fact is perhaps stronger than that in the $T = 0$ state or at least comparable with it.

\textit{Lomon:} I would like to make two further comments, or rather one comment and one question concerning the three-body question. The first is that Bach does get repulsion if he includes some $D$-state in the wave function. Could Dalitz please explain a little more about what Weitzner assumes in obtaining his results?

\textit{Dalitz:} From the results of the European collaboration on $K^-\bar{K}$ interactions at rest with emulsion nuclei we think we can infer a rate of production of $\Sigma$-hyperons smaller on neutrons than on protons. This could imply that the production of $\Sigma$ from the $T = 1$ state is small. This of course does not apply to the $\Lambda$ production.

\textit{Klein:} I did notice in Weitzner’s letter to the Physical Review that he used a pair interaction of the mesons with the hyperons. Do the other authors use a similar Hamiltonian, or do they use the Yukawa interactions?

\textit{Dalitz:} No. Bach and Spitzer both assume that the interaction is of the Yukawa type $\Lambda \leftrightarrow \Sigma + \pi$. I agree that the calculations are not directly comparable.

\textit{Klein:} That might account for the difference.

\textit{Dalitz:} Certainly it would, and probably the true situation lies somewhere between the results of Weitzner and of Bach and Spitzer.

\textit{Chairman:} I see that it is time to summarize the discussion. We see that the strange particles are getting to be more and more familiar and maybe after one year at the next conference we should ask to change the names of these particles. We have seen no sensational developments such as breakdown of parity conservation or isotopic spin conservation in the strong interactions. There is a great deal of experimental information and now we have time to think about its interpretation.
SESSION 7
Friday, 4th July, 1958

Special theoretical topics

Chairman        T. D. Lee

Secretaries     A. Peterman
                G. Wanders
DISPERSION EQUATIONS

M. L. GOLDBERGER, Rapporteur
Princeton University, Princeton (N.J.)

I have been asked to report on a number of loosely related theoretical topics. The common bond is dispersion theory. The subjects are as follows:

1. Derivation of dispersion relations for the production of pions by photons and electrons, and for $\gamma - p$ scattering, by Oehme and J. G. Taylor.

2. Perturbation theoretic derivation of dispersion relations for the vertex function and for nucleon-nucleon scattering, by Symanzik and independently Nambu.

3. Solution of certain integral equations which arise in dispersion theory, by Omnes.


1. A detailed discussion of the work of Oehme and Taylor would be quite impossible to present here. Furthermore, the material upon which the new results have been based is largely already published. Since the individual contributions will be published later, only the conclusions will be stated here. I thought it might be worth while to make a table summarizing the current status of dispersion relation derivations as I know them. In all cases we consider a process $k+p \rightarrow k'+p'$ where $p$ and $p'$ are nucleons ($p^2 = p'^2 = m^2$) while $k$ and $k'$ are the "projectiles". In general $k'^2 > 0$ though $k^2$ may not be so restricted. (See Table I.)

It is perhaps worth remarking that there is no very firm knowledge in many cases of what is required in the way of subtractions. This depends on precisely what invariant amplitudes are used in the various cases and on how optimistic one happens to be. It is also true that frequently information can be obtained on the structure of the subtraction terms by considering the "crossed" dispersion relations in which other than the usual quantities are held constant.

I would like to emphasize that these ordinary dispersion relations, both normal and crossed, will be far inferior to the two-dimensional Mandelstam relations if these can be brought to a practical form.

2. Nambu has discussed parametric representations for various Green's functions based on an examination of the structure of these quantities in perturbation theory. He has shown, in particular, that the spectral representation of the vertex function discussed by Drell is in fact obtained. In this problem which is the discussion of a quantity $\langle p' | j | p \rangle$, where $j$ is some current—either electric or mesonic—one readily obtains in every order in perturbation theory a spectral representation of the form

$$F[(p - p')^2] = \int_{\sigma_0^2}^{\infty} \frac{d\sigma^2}{\sigma^2 - (p - p')^2 - i\epsilon} \phi(\sigma^2).$$

What is not at all readily obtained is the value of the lower limit $\sigma_0^2$ and the precise identification of $\phi$ with a physical absorptive process. Nambu has pointed out that in certain circumstances, depending on the masses of all the particles involved, the intuitive values for $\sigma_0^2$ may not be obtained. This has also been noted by Karplus, Sommerfield and Wichmann, by Oehme, by Symanzik and is implicitly contained in an appendix to a paper by Källén and Wightman.

It is perhaps worth spending a moment elaborating on this point. Consider the lowest order perturbation diagram for the electromagnetic nucleon vertex:

---

nucleon $p'$  
nucleon  
nucleon $p$
### TABLE I

**DISPERSIONS RELATIONS**

1. **Proved relations**

<table>
<thead>
<tr>
<th>Process</th>
<th>Limitation in invariant momentum transfer</th>
<th>Continuation of absorptive part into the unphysical region by convergent partial wave expansion</th>
</tr>
</thead>
<tbody>
<tr>
<td>$k + p \rightarrow k' + p'$</td>
<td>$\Delta^2 \equiv \frac{1}{4} \left[ (k - k')^2 - (k_0 - k_0')^2 \right]$</td>
<td>$0 \leq \Delta^2 &lt; \Delta^2_{\text{max}}$</td>
</tr>
<tr>
<td>$\pi + N \rightarrow \pi + N$</td>
<td>$\Delta^2_{\text{max}} = \frac{8\mu^2}{3} \frac{2m + \mu}{2m - \mu}$</td>
<td>$0 \leq \Delta^2 &lt; \Delta^2_{\text{max}}$</td>
</tr>
<tr>
<td>$\pi + \pi \rightarrow \pi + \pi$</td>
<td>$\Delta^2_{\text{max}} = 7\mu^2$</td>
<td>$0 \leq \Delta^2 &lt; \Delta^2_{\text{max}}$</td>
</tr>
<tr>
<td>$\gamma + N \rightarrow \gamma + N$</td>
<td>$\Delta^2_{\text{max}} = \mu^2 \left{ \frac{(2m + \mu)^2}{4(m + \mu)^2} + \frac{2m + \mu}{m} \right}$</td>
<td>$0 \leq \Delta^2 &lt; \Delta^2_{\text{max}}$</td>
</tr>
<tr>
<td>$\gamma + N \rightarrow \pi + N$</td>
<td>$\Delta^2_{\text{max}} = F(0)$</td>
<td>$\left{ \begin{array}{l} 0 \leq \Delta^2 &lt; \Delta^2_{\text{max}} \ \Delta^2_{\text{th}} = \frac{m}{m + \mu} \frac{\mu^2 - \gamma}{4} \end{array} \right.$</td>
</tr>
<tr>
<td>$e + N \rightarrow e + \pi + N$</td>
<td>$\Delta^2_{\text{max}} = F(\gamma)$</td>
<td>$\left{ \begin{array}{l} 0 \leq \Delta^2 &lt; \Delta^2_{\text{max}} \ \Delta^2_{\text{th}} = \frac{m}{m + \mu} \frac{\mu^2 - \gamma}{4} \end{array} \right.$</td>
</tr>
</tbody>
</table>

2. **Some unproved relations**

<table>
<thead>
<tr>
<th>Process</th>
<th>Mass restrictions appearing in proof based upon causality and spectrum; $\Delta^2 = 0$.</th>
<th>Perturbation theory (every finite order)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$N + N \rightarrow N + N$</td>
<td>$\mu &gt; (\sqrt{2} - 1)m$</td>
<td>proved for $\Delta^2 &lt; \frac{\mu^2}{4}$</td>
</tr>
<tr>
<td>$K + N \rightarrow K + N$</td>
<td>complicated; not fulfilled by narrow margin</td>
<td></td>
</tr>
<tr>
<td>$\pi + D \rightarrow \pi + D$</td>
<td>$\varepsilon &gt; \frac{\mu}{3}$; $M_D = 2m - \varepsilon$</td>
<td></td>
</tr>
</tbody>
</table>

etc.

---

(*): lowest order in the electromagnetic interaction

(**): for $\gamma \gg -3\mu^2$:

\[
F(\gamma) = \frac{m}{m + \mu} \frac{\mu^2 - \gamma}{4} + \frac{8\mu^2}{3} \frac{2m + \mu}{2m - \mu} \frac{4\mu^2 - \gamma}{2} \frac{2m + \mu}{2m} \times \frac{1}{\sqrt{1 + \frac{\mu^2 - \gamma}{m}}} \frac{1}{\frac{4\mu^2 - \gamma}{(m + \mu)^2} \frac{(2m + \mu)^2 - \gamma}{2m + \mu}}.
\]

for $\gamma \ll -3\mu^2$: $F(\gamma)$ can be calculated numerically.
The least massive absorptive process would appear to be the virtual production of the pair of $\pi$ mesons indicated by the cut — which becomes possible as soon as the momentum transfer is $4\mu^2$. This is, in fact, the case, and the lower limit $a_0^2$ takes on the "intuitive" value. If, on the other hand, one considers the electromagnetic structure of a $\Sigma$ particle, with the diagram

then the value of $a_0^2$ turns out to be less than $4\mu^2$ and the absorptive process corresponds to the cut shown. The rule seems to be that there is danger, so to speak, whenever there is a baryon of mass lighter than the external one into which the external one may "decay" (albeit with negative kinetic energy—recall that such negative kinetic energies are common in dispersion theory: the bound state in the pion nucleon relations corresponds to the "physical" process of the absorption of a negative kinetic energy pion by a nucleon). There is a precise criterion which for the above simple case is that the limit is less than $4\mu^2$ if $m_{\pi^2} > m_{\pi^4} + m_{\pi^2}$ which is in fact true. Another interesting example is that of the vertex function for a deuteron where the lower limit of the dispersion integral is set by the diagram

<table>
<thead>
<tr>
<th>Vertex</th>
<th>Mass restrictions</th>
<th>Remarks</th>
<th>Perturbation theory (finite order)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\langle N</td>
<td>A_{\pi}</td>
<td>N \rangle$</td>
<td>$\mu &gt; (\sqrt{2} - 1) m$</td>
</tr>
<tr>
<td>$\langle N</td>
<td>(\Box + \mu^2) \phi_{\pi}</td>
<td>N \rangle$</td>
<td>$\mu &gt; (\sqrt{2} - 1) m$</td>
</tr>
<tr>
<td>$\langle \pi</td>
<td>(\gamma \frac{\delta}{\delta x} + m) \psi</td>
<td>N \rangle$</td>
<td>proved</td>
</tr>
<tr>
<td>$\langle \pi</td>
<td>(\Box + \mu^2) \phi_{\pi}</td>
<td>\pi \rangle$</td>
<td>proved</td>
</tr>
</tbody>
</table>
The first diagram yields the reasonable value \( \sigma_\epsilon^2 \sim 16 \text{ me} \) where \( \epsilon \) is the binding energy; this limit may be associated with the spatial extension of the deuteron.

Symanzik has refined Nambu’s methods and has presented for the first time a derivation of dispersion relations for nucleon-nucleon scattering, as yet for rather limited values of momentum transfer. These limiting values seem rather smaller than what one might expect \( (\Lambda^2 < \mu^2) \). Symanzik comments that with his method one cannot derive the relations for equal mass bosons for as large values of \( \Lambda^2 \) as have been obtained by the general non-perturbative approach. For the case of pion-nucleon scattering, he finds dispersion relations for \( \Lambda^2 = \mu (M + \mu) / 2 \) though possibly as much as \( 8 \sqrt{15} \) times larger. The first limit is smaller than that given by the general theory, the latter is much larger.

It is Symanzik’s feeling that considerably more information may be extracted from the perturbation theory by more careful investigation.

Furthermore, incorporation of the special features of definite models into the general theory might be very helpful.

3, & 4. In the interest of saving time I shall discuss the third and fourth topics together. This is feasible since in the study of the pion nucleon vertex we encountered an equation of precisely the same class as was treated by Omnes. The simplest one of the type was solved for us by Dyson.

Consider the quantity \( J \) defined by

\[
J = \left( \frac{p_\epsilon \cdot \bar{p}_\epsilon}{m^2} \right)^{1/2} \langle 0 | j_i | p, \bar{p}, \text{in} \rangle \\
= i [ \mu^2 + (p + \bar{p})^2 ] D_{F_c} \Gamma_{s_i} (-\bar{p}, p) u(p) \\
= i g \bar{v}(\bar{p}) \gamma_i \gamma_5 u(p) K ((p + \bar{p})^2) .
\]

Here \( j_i \) is the source of the meson field, \( \phi_i \),

\[
[\mu^2 - \Box] \phi_i = j_i
\]

\( D_{F_c} \) is the Feynman propagation function, \( \bar{v}(\bar{p}) \) is a negative energy spinor,

\[
\bar{v}(\bar{p}) \left[ i \gamma p + m \right] = 0,
\]

and \( u(p) \) satisfies

\[
[ i \gamma p + m u(p) = 0.
\]

The state \( | p, \bar{p}, \text{in} \rangle \) represents an “in” state of a nucleon-antinucleon pair, and \( g \) is the renormalized coupling constant: \( K (-u^2) = 1 \). The scalar function \( K \) is the dispersion relation of the form

\[
K(\xi) = 1 - \frac{(\xi + \mu^2)}{\pi} \int_0^\infty \frac{d\xi'}{[\xi' - \mu^2]} \frac{\text{Im} K(-\xi')}{[\xi' + \xi - i\epsilon]}
\]

where we have deliberately made a subtraction to show the normalization \( K(-\mu^2) = 1 \). We will return to the question of the need for such a subtraction later.

We may quite generally introduce a phase angle \( \varphi \) by the definition

\[
\text{Im} K(-\xi) = \tan \varphi(\xi) \text{ Re} K(-\xi)
\]

making some sort of convention about defining \( \varphi \) at some one point. Now regard \( \tan \varphi(\xi) \) as a known quantity. The dispersion relation becomes then a kind of integral equation which incorporates certain boundary conditions at infinity in order that the equation, as written, shall exist. Now imagine that \( \xi \) is a complex variable and that \( K(\xi) \) is to be extended by the equation to a function in the complex plane. We find immediately that

\[
\frac{K(\xi)}{K(\xi)} = 1 - i \tan \varphi(\xi) \frac{\theta(\xi - 9\mu^2)}{1 + i \tan \varphi(\xi)} .
\]

It is now apparent that if we take the logarithm of this relation we have the standard Hilbert problem of the known difference of a function above and below a cut.

The general solution is well known and it is (now letting \( \xi \) be real)

\[
K(\xi) = P(\xi) \exp \left\{ - \frac{(\xi + \mu^2)}{\pi} \int_0^\infty \frac{\varphi(\xi')}{[\xi' - \mu^2]} d\xi' \right\}
\]

where \( P(\xi) \) is an entire function in which we have the condition \( P(0) = 1 \). The possibilities open for \( P(\xi) \) depend on the detailed behaviour of \( \varphi \) and the boundary conditions at infinity. That there may be many solutions to dispersion equations is an old story. Omnes has shown how the more general case, where the inhomogeneity is replaced by a function \( f(\xi) \), may be solved, but since his paper has been published \(^1 \), I shall not take the time to write out the solution.

Instead, I wish now to turn to the question of what the angle \( \varphi \) may be in our special example. For this we return to

\[
J = i \left( \frac{p_\epsilon}{m} \right)^{1/2} \int \, dx^4 \bar{v}(\bar{p}) \langle 0 | [j_i(0), f(x)] \theta (-x_a) | p \rangle e^{i \bar{p} x}
\]

where \( f(x) \) is the source of the nucleon field \( \psi 

\[
[\gamma \partial + m] \psi = f.
\]

The absorptive part \( A \) is
The states $|s\rangle$ all have zero nucleon number, odd parity, and angular momentum zero. The least massive state is that of three pions; to evaluate this contribution we need the full analytic dependence on all momentum vectors in the process of pion-pion scattering and also the full dependence for the process of pair annihilation into three pions. It is unnecessary to say that these things are unknown. The first tractable state is that involving a nucleon antinucleon pair and I wish to discuss the consequences of assuming that only this state need be taken into account.

It is clear that the necessary matrix element $\langle N\bar{N}|f|p\rangle$ will involve only the $1S_0, I = 1$ phase-shift of the nucleon-antinucleon system. The other matrix element $\langle 0|j_j|N\bar{N}\rangle$ is, of course, our original vertex. We extend the sum over $|\phi\rangle$ symmetrically over both “in” and “out” states in order to preserve the proper reality conditions at all stages of approximation.

An elementary calculation then yields the result

$$\tan \varphi(\xi) = \frac{\text{Re} e^{i\delta} \sin \delta}{1 - \text{Im} e^{i\delta} \sin \delta}$$

and $\delta$ is a function of the wave-number $(\xi/4 - m^2)^{1/2}$.

We have studied some simple models for the complex phase-shift $\delta$, such as $\tan \delta = k(a + ib)$, which give the correct behaviour near $k = 0$; we also choose $\delta(k = 0) = 0$, and find that for this class of models we may set the entire function $P(\xi) = 1$ and obtain solutions for $K(\xi)$ which approach zero at infinity. This behaviour is, of course, more general than our simple models: provided only that $\varphi$ approaches a positive constant, $\varphi_\infty$, at infinity, $K$ will approach zero like $(\xi)^{-\frac{1}{2}}$. Such a contingency, namely $K \to 0$ at infinity, raises the possibility of using no subtraction in the original dispersion relation. Such a “no subtraction” philosophy has been discussed earlier in this meeting and has some very attractive aspects in that all reference to the usual perturbation theoretic ideas about a nucleon core have disappeared.

If the nucleon-antinucleon scattering is treated in perturbation theory, one has a kind of ladder approximation of the variety discussed some years ago by Edwards. There are two diagrams:

If the first only is taken, one has a proper vertex part and we find:

$$K \to \left(\frac{4 m^2}{\xi}\right)^{1/2} \tan^{-1} \frac{g^2}{16\pi}$$

I would like to emphasize that we are quite aware of the treacherous nature of approximation methods in which an infinite number of states is replaced by a finite number; we present this calculation only in the spirit of a model (which has some interesting practical consequences which will be discussed this afternoon), one which may perhaps have more general validity.

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5. Symanzik, K. (preprint)
**DISCUSSION**

**Oehme:** I would like to make a brief remark concerning the relevance, or perhaps the irrelevance, of the momentum transfer restriction obtained in the general proof of the dispersion relations. These restrictions are not necessarily characteristic for the assumptions which we have made, and which have been made by Lehmann and Bogolyubov. The reason is that there are certain symmetry properties of the four-body Green's function which we have not used. These properties are connected with the Jacobi identity between the fourfold commutators and we do not know whether or not an improvement is possible on the basis of the axioms of field theory alone (minus unitarity). The second comment is on the vertex function. All the special cases of the vertex function which we saw on the blackboard are, of course, very specific cases of the general three-point function which has been studied without mass spectrum, but on the assumption of positive energy, by Wightman and Källén. It is hoped that a more clear understanding of this function will be possible once Wightman and Källén's calculations are extended to the case of arbitrary mass restrictions. Furthermore, I want to say that the usually assumed representations for the meson-nucleon or photon-nucleon vertex function are not a consequence of the axioms of field theory without unitarity. This has been first demonstrated by the example of Jost which shows that if one uses only these assumptions one cannot exclude singularities in the complex plane. There are also examples which are constructed from perturbation theory. These give only singularities on the real axis, in the sense that they only prolongate the cut below the threshold, which one might expect to be at \((2\rho)^2\) or \((3\rho)^2\).

**Miyazawa:** Is it possible to prove from quantum field theory that a finite number of subtractions is sufficient to make the integrals convergent?

**Källén:** No.

**Bogolyubov:** I wish to draw to your attention that the dispersion relations for Compton scattering and photon-meson production were also proved by Lagunov using the new technique of Vladimirov and they succeeded in getting some higher maximum momenta. Lagunov considers not only two-particle states as initial states and two-particle states as final states, but he succeeded also in proving the dispersion relations when, for instance, we have a nucleon and a meson in the initial state and then one nucleon and two pions in the end state. Surely he could not prove it for all angles, but he generalizes the notions of backward and direct scattering, and in this way he was able to prove dispersion relations for states with \(n\) particles at the end, for quite special angles. Lagunov considers also dispersion relations for virtual elements, which could be of use in considering strong interactions with the electromagnetic interaction.

**Oppenheimer:** I would like to ask Oehme a question, not for Jost's counter-example, but where one finds, you said, in some use of perturbation theory that there are singularities below the cut. Can one understand what possible physics this corresponds to? Is it a process which goes on, corresponding to such momentum transfers, or is this not interpretable?

**Oehme:** In the case of the pion-nucleon vertex, when I have said that we have examples from perturbation theory, this does not mean perturbation theory with the usual \(\gamma_5\) interaction, and the intermediate states containing only mesons and nucleons. These are examples constructed with the help of perturbation theory only and they are mathematical examples, where one uses intermediate lines in the graphs which have nothing whatsoever to do with physical particles. All we want, is to understand whether the limitations which we have obtained in the general proof can be seen from examples which satisfy all the mathematical axioms. We know that the information contained in these axioms has not been completely exhausted and we want to understand whether we can hope to get further, on the basis of these axioms alone, or whether we have to introduce additional assumptions. One important axiom which we have left out is, of course, the unitarity condition. In fact I would say that these examples give a certain indication that we may perhaps improve our mass restriction \(\mu > (\sqrt{2} - 1)m\) using unitarity. But whether we get beyond the point where Jost’s example starts, I do not know.

**Feynman:** Are the axioms of quantum field theory, from which you start the proof, consistent? I have always thought that if you start with these axioms you find some infinities somewhere, and then the axiom that the amplitudes are finite is not consistent with the other axioms—is it, or is it not so? Do you have an example, or anything, any model, which is compatible with the axioms which shows that they are consistent?

**Wightman:** There are such examples, but so far there are no really non-trivial physical examples. You have all kinds of strange vacuum fluctuations, but no example where the scattering matrix is really non-trivial.

**Polkinghorne:** I just want to say for the sake of completeness that the proof of the dispersion relations for the inelastic process pion plus nucleon goes to two pions plus nucleon has also been discussed by Kibble from Edinburgh.

**Nambu:** I would like to make a comment about the pion-deuteron scattering. I have briefly looked at the perturbation calculations. It seems that also in this case the mass spectrum of the continuum goes below the normal threshold. The condition is very similar to the one which is obtained for the vertex function.

**Miyazawa:** May I continue my argument? The convergence has to be proved or accepted as an axiom.
FOUNDATION OF DISPERSION RELATIONS

A. BOHR

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ABSTRACT

A. Bohr reported on an attempt by Mottelson and himself to give an elementary derivation of dispersion relations, based on a criterion of macroscopic causality. Only forward elastic scattering is considered, and the method employs a correspondence treatment of the asymptotic behaviour of the incident and scattered particles, in terms of a classical field description. Signals of incident particles propagating with a sharp front are constructed by including field components of all frequencies and the analyticity condition on the scattering amplitude is derived from the condition that no signal travels faster than light. The physical significance of the field components and the scattering in the non-propagating region $|\omega| < mc^2$ is discussed.

The approach indicates that the main limitation in the derivation of dispersion relations for forward scattering may be associated with the tails of the interaction which provides difficulties for the definition of the asymptotic fields in the non-propagating frequency regions.

DISCUSSION

Kroll: I have an elementary question to ask. Is it not true that if you are just looking for a photon in the detector, you may find one arising from the vacuum fluctuations, instead of a causally propagated one. How do you distinguish them?

A. Bohr: We are dealing with a small signal above the vacuum fluctuations and this means, of course, that we have to do a big number of measurements. But there is a clear signal above the vacuum fluctuations.

Oppenheimer: I am not going to ask a question but I am going to make two pronouncements. I hope that they will be sufficiently wrong so that we will get a contradiction. It seems to me that in what Bohr has just told there is, in two respects, a very different general view than in the ordinary approach, in the present approach (let us say) of dispersion relations. One, which I think is not the deeper one, has to do with the so-called unphysical region. If one thinks that quantum field theory is a branch of quantum mechanics in which it is possible to introduce all manner of classical sources and potentials, then it should be easy possible to make the unphysical region physical, because one could change the relation between energy and momentum by these means and that is a very great discrepancy from the point of view which underlies the present effort to get rigorous results in the light of the actual mass spectra, the actual processes which can occur in the atomic domain. It seems to me unbelievable that this is just a question of mass. Just going back to light quanta, as Kroll said, one is not measuring here a quantum and we know that this cannot be done with any kind of meanings from us in space-time with precision. But one is rather using a postulate which is very much more general than the postulates of quantum field theory. In quantum field theory one makes two statements. Signals cannot propagate faster than light and a field measurement can be used for signals. But here, I think, Bohr and Mottelson say: signals cannot propagate faster than light and any linear function of the fields, whether local or not, can be measured and therefore used for signals. This, in both respects, as it seems to me, is in a quite different world than the world in which Goldberger, Källén and Oehme live. And I do not wish to argue that this is a bad world, but only indicate that it is in these points, perhaps, that the difference is large and I believe that the important ones all come out if we think of nothing but light, because light also is non-local, and light also has structure, and the vanishing of the rest mass removes certain of the technical complications which we discuss.

Low: Some time ago Thirring and I attempted to carry through a similar programme to this one, and we ran into a difficulty which I believe invalidates this proof, and which is the following. The formula which you have written down for the wave function is only an asymptotic
formula. You have dropped terms going as \( r^{-2} \) and certainly exponentials, whereas the analytical properties that you obtain for the scattering amplitude are only obtained by requiring that this wave function be exactly zero for \( r > t \). Now in fact, even if there are singularities in the upper half-plane then, as long as the rest mass is greater than zero, you get nothing but decaying exponentials from them. So that you cannot rule these out since you have dropped terms which are even larger than them already. Therefore you cannot rule out poles in the upper half-plane on the basis of your argument.

A. Bohr: If there are poles, rather than singularities at infinity, one gets into trouble at a very early stage in the sense that you really cannot use this representation for the wave function to form wave packets which for \( t \) going to minus infinity really are incoming states.

Low: I believe that if you discuss the scattering of particles this is a difficulty, but I do not think that it is in the case of scattering of classical fields. Then you can talk about the field which exists everywhere in space, which comes in, and then is scattered out according to the stationary states solutions which you can write down.

Lee: Low's question is just the mathematical question: if you use only the asymptotic expression for the amplitude, how can you deduce the types of analytic conditions which you need. It seems that this is a rather relevant point.

Jost: It is clear that it would be very desirable to have a different proof of this dispersion relation and to get out of this mathematical marasmus in which one is with the actual proof of the dispersion relation. However, I do not know whether this approach of Bohr is really an approach which is tenable. Now to illustrate my point I would like to make a model. The model I take is just the dispersion relation for potential scattering in non-relativistic quantum mechanics. It is well known from the work of Khuri that it does not make sense to discuss the forward scattering because in the forward scattering you have the dispersion relation for very weak assumptions on the potential. But I do not completely agree with Bohr that the non-forward scattering is really a very much deeper foundation, and I think on this point we all agree.

Lee: This comment is certainly very interesting. It is very important to have a different approach from the continuation of the Legendre expansions the imaginary part in the domain where you need it. Now the thing which I would like to discuss, is the point which Bohr mentions in his paper and which he mentioned at the end of his talk, namely the continuity argument in the cut-off which you introduce. Now imagine that you introduce a cut-off of the potential. You cut it off at the distance \( R \), when you would have such a relation

\[
\text{Re} \ T_r (E, \Delta) = \frac{1}{\pi} \int \frac{\text{Im} \ T_r (E', \Delta)}{E' - E} \ dE'
\]

with functions of \( R \) and of course now you will have dispersion relation for all \( \Delta \)'s. And you now want to make a continuation in \( R \) and appeal to some continuity arguments. Now I have to admit that I do not know of any example for a potential for which the dispersion relation does not hold for every momentum transfer. However, I think that it is probably quite simple to make such an example. Therefore let us assume that such an example exists! I do not think that this is a deep point, it is only a technical point. Let us discuss the formula as a function of \( R \); it is quite clear that if you go to infinity with \( R \), there must be very little change in the physical region, which extends from \( \Delta \) to infinity. But what happens is that poles come out of the unphysical region of real axis, into the upper half-plane for some value of \( R \) and these poles may have an arbitrarily big residue. There is no way of avoiding this. So Bohr's relation can get as wrong as it wants and this, I think, is probably a criticism which I can hardly avoid.

A. Bohr: I want to mention the possibility that things may go very much better for forward scattering. We know from potential scattering problems that we do have the famous redundant zeros which are just dependent on whether you make a cut-off or not. But these do not, it seems, appear in the case of forward scattering.

Jost: My point was the following. I do not see any qualitative differences between the non-physical region which enters into the forward scattering of these more sophisticated field theoretical considerations and the non-physical region which you have here in the case of the potential scattering. This I cannot substantiate by anything, but I think that this can serve as a model which shows that certainly some of your conclusions need really a very much deeper foundation, and I think on this point we all agree.

Bogolyubov: I quite agree with the statements of Oppenheimer, Jost and Lee. I think that the main problem in the use of such kinds of techniques for proving dispersion relations may be stated as follows. Physical intuitive reasoning is very good when you are restricted to real values of impulse and energy, but when you pass to complex values the validity of all intuitive reasonings just fails.
current way the experts are doing in dispersion, and what Bohr says raises a multitude of problems which are not only mathematical but also physical. Therefore discussions concentrated on the physical aspects will be extremely welcome.

**Peierls**: I think one can look at this problem in two different ways. Firstly, in the framework of local field theory in which we have also other mathematical derivations available. We do not, of course, know if such a formalism exists, but if it exists we can also proceed in the conventional way. In this context the advantage of the method of Bohr is, if one can get over all the mathematical limitations, that one can see in a more transparent way what one is doing. I would not agree with Oppenheimer that one is making here more postulates than in the conventional theory because the conventional theory is based on the whole operator and measurability picture of quantum mechanics and, while it may well be true we cannot make the tools with which one would be able to measure these things, it is nowhere written in the theory that this is excluded. We can couple the fields with an infinitely weak coupling to some new fields and we would not get out of that any inconsistency. I think one can carry this argument through.

But it seems to me also very important to use this kind of reasoning to see what happens if some of the axioms of the present formalism are not right. We know that the dispersion relations can be derived from a microscopically causal field theory. Would they necessarily fail if the theory was not microscopically causal but still was a reasonable physical theory? After all, we may soon face the situation that an experiment appears to contradict the dispersion relations. Do we then conclude that the experiment must be wrong, or do we conclude that the physics is a little different from the axioms we normally use? It seems to me that from Bohr’s arguments one can see that we would be very surprised if the dispersion relations failed, say, in pion-nucleon scattering. But if their range of validity does not correspond to what we derived from the present local field theory in, let us say, nucleon-nucleon scattering, we might well believe that this was due to a breakdown of the local formalism.

**Noyes**: In connection with the restriction on momentum transfer for the dispersion relations for potential scattering mentioned by Jost, Gasiorowicz has shown that, by following the Bogolyubov method, it is possible to extend the proof to inelastic processes. The restriction then includes the masses as well as the exponential fall-off distance of the interaction region. In particular, for $K + P$ scattering, it is impossible to prove the dispersion relations even in the forward direction if the interaction region has an exponential range as large as the $\pi$ meson Compton wavelength (as would result from a $KK\pi\pi$ interaction). This restriction also occurs in Bohr’s proof.

**Symanzik**: I have only a rather technical point: namely, in the signal state you have got down on the blackboard in the second part of your talk

$$|s\rangle = |0\rangle + e^{\sum_{k>0} \sqrt{2\omega_k} a^+(k)} |0\rangle,$$

if you neglect terms in $e^2$, then you not only make no attempt to normalize your state, but also to give it any reasonable energy, which diverges in a very high degree.

**A. Bohr**: The $\delta$-function signal should be regarded as the limit of a normalizable finite wave-packet, say of Gaussian type.

**Symanzik**: But in going from such arguments to statements about the complex plane and infinity, the theory would need a more careful discussion, I would think.

**A. Bohr**: The dispersion relations are, after all, relations between physical quantities, and only the relations may be expressed in terms of complex values of the frequencies.

**Jost**: I think I disagree with this standpoint in as far as the non-physical region is involved. The non-physical region is as bad and as treacherous as any point in the complex plane!
FORBIDDING OF $\pi - \beta$ DECAY

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Ordinarily in experimental subjects the rapporteur is not supposed to be someone who did most of the work. This is impossible as you see in theoretical papers because it is very hard to find a rapporteur who can understand any of that particular work of the others. The choice is made this way so that at least one-third of the work is understood. There are three people with three different contributions that I report on, of which I understand one-third. Those are: J.C. Taylor\(^1\), a contribution by Gatto and Ruderman\(^2\), and a contribution by myself\(^3\). In my lack of understanding it appears to me as though we are all saying exactly the same thing. So I will say that, and if the others say there is something else that they said, then I hope they will bring it up in the discussion.

The problem that we are discussing is the problem of the disintegration of the $\pi$ into an $e$ and a $\nu$ in relation to its rate for disintegration into a $\mu$ and a $\nu$. In accordance with the universal $V,A$ $\beta$-decay, the coupling of both the electron and the $\mu$ should be exactly the same to nucleons and other strange particles. So that although we do not understand the disintegration of the $\pi$ directly, but only in terms of some kind of a picture in which the $\pi$ virtually becomes say a proton and antineutron, and then those decay into each other. Whatever it is, we believe that if the coupling to everything of both the $e$ and the $\mu$ is the same, then the rates should have a definite proportion independent of the impossibility of calculating the necessary closed loops, etc.

Assume, then, that the universal theory is right. We can deduce that the rate of $\pi \to \mu + \nu$ to $\pi \to e + \nu$ is equal to the ratio of the phase-space available, which is better for the $e + \nu$ than for the $\mu + \nu$ multiplied, however, by one additional factor, which was originally pointed out many years ago by Ruderman for axial vector coupling, and that is this: in the universal theory this is an antineutrino and that is an electron, then all particles in the relativistic limit come out spinning to the left, antiparticles to the right. So if, for the moment, we take the relativistic limit, this particle should come out spinning to the left and this particle spinning to the right, and they just go in opposite directions, one spinning to the left and the other to the right, and the result is that there is a total angular momentum, but the $\pi$ has no angular momentum, so the result is impossible. It cannot decay at all, except that when the particle is not relativistic the probability of finding its spinning counter-clockwise, that is oppositely to the rule about the left, is $(1 - v/c)$, so this is an additional factor for the two processes. But since the electron is so much lighter than the meson, its $v$ is so much closer to $c$ that it makes the rate of the electron disintegration much less theoretically than the rate of the other reaction. It turns out then that the amplitude is proportional to the mass of the electron, the probability is proportional to the square of the mass, and when you work it out numerically for the known masses, this ratio should be $13.6 \cdot 10^{-3}$. Therefore, this should safely predict no electron decays. But not too safely—the experimenters are too ingenious and apparently have pushed the experimental conditions down until they can say that as a matter of fact the $\pi$ disintegrates less than $1 \cdot 10^{-5}$ as reported by Lattes and Anderson.

I am going to assume that the theory of universality is correct and I am also going to assume that the experimental result is correct, although the true explanation of this difficulty can lie in either of these two places. Suppose it does not, however, what can we do? Well, there is one thing left and that is the electromagnetic corrections to the decay. So one happily goes along and calculates the electromagnetic corrections to the decay because after all, if you calculate the electromagnetic corrections you find that if the amplitude was originally proportional to $m_e$ then the electromagnetic corrections are of order $e^2 m_e$. That is, the electromagnetic corrections also go to zero as $m_e$ goes to zero, and so at first sight the electromagnetic corrections are incapable of making any appreciable modification. All of the three contributions struggle on, in spite of this, by pointing out that if the mass of the electron were zero in the beginning somehow, then the electromagnetic corrections are of order $e^2 m_e$. That is, the electromagnetic corrections to the decay because after all, if you calculate the electromagnetic corrections you find that if the amplitude was originally proportional to $m_e$ then the electromagnetic corrections are of order $e^2 m_e$. That is, the electromagnetic corrections also go to zero as $m_e$ goes to zero, and so at first sight the electromagnetic corrections are incapable of making any appreciable modification. All of the three contributions struggle on, in spite of this, by pointing out that if the mass of the electron were zero in the beginning somehow, that this rate would not occur at all, and maybe that is the explanation of it. So they all try to get out by saying that there is something wrong somewhere with our calculations either of this factor or that the mass of the electron is all electromagnetic or some such a reinterpretation. I must remind you that the mass of the electron cannot be all electromagnetic according to this same quantum electrodynamics; the corrections to the mass are also proportional to the mass of the electron times certain logarithms

$$\Delta m_e = e^2 m_e \ln (\ldots).$$

When I say electrodynamics, I am going to cut all intervals off at the Compton wavelength of the proton, more or
less. Otherwise everything is infinite and you do not have anything to worry about. For instance, it has been pointed out that this correction that we have here is in fact directly proportional to $\Lambda m$. If this somehow could be all of the mass we could correct the whole of the reaction away or something. So therefore I should like to discuss, in a more or less organized fashion, the question of whether the mass of the electron can be entirely electromagnetic.

Before I do that, let me try to explain why the amplitude here is $e^2m_e$, in other words why the electromagnetic corrections to the decay rate go also to zero as the mass of the electron goes to zero. The physical reason is this: if the mass of the particle is zero it satisfies some kind of a wave equation like this:

$$(i\gamma^\alpha - \tilde{A})\psi = 0$$

after it has escaped from its source. This equation has the property that if I make the transformation $\psi' = \gamma^\alpha\psi$ then I get the same equation again. That means that something that is the spinniness or spirality or chirality of the particle once liberated cannot be modified by the electrodynamics. So that if a particle comes out spinning to the left—while the antineutrino having zero-mass must necessarily spin to the right—this spinning to the left cannot be altered by all the interactions with the electromagnetic field, if the mass is really zero. So there cannot be any corrections due to electrodynamics of finite size to undo the finite size from this mass $m_e$.

Well, now I would like to discuss the question, can the mass of the electron be entirely electromagnetic? We know that there are electromagnetic masses almost certainly. There are lots of examples of little differences in masses which we now interpret as electromagnetic. The mass of the electron is very small, it is a great hope that it could be entirely electromagnetic. But it is easy to prove that the mass of the electron on conventional quantum electrodynamics with a cut-off cannot be purely electromagnetic, because you want to convert the above electron equation to an equation for a particle with mass, which looks like this:

$$(i\tilde{\gamma}^\alpha - \tilde{A})\psi = m\psi.$$  

Now, this equation no longer has this property of invariance under $\psi' = \gamma^\alpha\psi$. Starting from an equation with zero mass it is impossible in any order, no matter how you struggle, to produce one which does not have the symmetry which the original one has. Therefore it is impossible, starting with the zero mass for the electron, to obtain by electrodynamics a finite mass, if the electrodynamics is not altered in some way. This is the group of the $\gamma^\alpha$ transformations. There is another group which also says that the mass cannot come purely from electrodynamics, and that is, the scale of the system can be changed arbitrarily—some scale has to be determined to determine the mass. We have already allowed that symmetry to go by saying that there is a cut-off at some particular momentum, that involves a scale. So I would like to propose this: that—let us make the hypothesis that the mass of the electron is electromagnetic—then we have to conclude that not only is the scale change violated as we go up to higher momentum but this symmetry, the $\gamma^\alpha$ symmetry, is violated also when we go to higher momentum. In other words, the modification of electrodynamics at higher momentum has to have two diseases, if the mass of the electron is all electromagnetic, the scale symmetry of the equation must be lost and furthermore the $\gamma^\alpha$ symmetry must be lost. Now you see that the same $\gamma^\alpha$ symmetry which I now know must be lost at high momentum will permit the spirality of the electron to be altered by electrodynamics, because the symmetry is no longer going to be in existence in my new unknown electrodynamics.

Well, I tried to make a number of models of methods of altering electrodynamics so that it would not only have the scale in it, the cut-off, but also do something to this $\gamma^\alpha$ symmetry at high momentum. The easiest one to calculate and work with, is to simply say that there is an anomalous magnetic moment added on to the coupling, a Pauli moment multiplied by some form factor $G(k^2)$, to give the new coupling

$$\gamma_{\mu} + \frac{\mu}{4}(\gamma_{\mu}k - k\gamma_{\mu})G(k^2).$$

This has the property that it destroys the symmetry and also contains a scale factor. Now if we do something like this we have to have an extra cut-off in order to make the integrals converge; the most obvious one is this:

$$G(k^2) = \frac{\Lambda^2}{\Lambda^2 - k^2}.$$ 

It turns out that this produces an anomaly in the moment beyond the experimental errors in the present day knowledge. So we can always fix it by putting in a thing like this

$$\frac{\Lambda^2k^2}{(\Lambda^2 - k^2)^2}$$

so that this term only comes in at high momentum, when the $k$ is very small I suppose there is no correction. It is very ad hoc, it is very unsatisfactory; but this is an example to see what happens, to see if it really can be done. So this is a term which has no effect on the moment, but does produce a failure of the symmetry at high momentum, and when you put this kind of a term in, it is very easy to show that the rate of $\pi - e$ decay decreases. Assuming that the decay into a $\mu$ is not changed in any way—that the formula for the $\pi - \mu$ decay is right—that the origin of the mass of the $\mu$ is something else—maybe it just exists; but that the electron mass is due to this electrodynamics, is rather interesting in that for any term of this kind, no matter what the cut-off factors, etc., are, the amplitude for $\pi - e$ decay is reduced to exactly $1/2$ of what you would have
calculated from the corresponding mass. You see we determined the coefficient $\mu$ to get the mass right and then we put it in to calculate the rate, and then it is easy to show that the amplitude is cut by exactly a factor of 2 and then the probability is cut by a factor of 4. Then $13.6 \cdot 10^{-5}$ becomes $3.45 \cdot 10^{-5}$. It is not 3.45 so they say, so that this model is not sufficient—it is just one example of how to do it.

Incidentally the numbers come out as follows: if you write $\mu$ is of order $1/A$, so you have the right scale for the magnetic moment, it turns out that if for instance $A = 0.7$ times the nucleon mass, which is a reasonable cut-off, then everything works out reasonably. But it is completely ad hoc, this method of arranging things so that you do not change the magnetic moment at low momentum, and it is unsatisfactory, also, because it still does not quite agree with experiment.

I tried another way. Electromagnetic masses can, as was pointed out by Weisskopf, be represented as corresponding to two kinds, an electric energy, which is positive from the charge, and a magnetic energy which Weisskopf thought was positive but Gell-Mann and Goldberger, I think, proved was negative. And in the case of the proton, with a high enough magnetic moment, the magnetic energy is so much bigger than the electric energy, that that is why, I guess, the proton is lighter than the neutron. And that is why we are all here! Now, the natural mass of an electron calculated for a spin zero electron is

$$\Delta m_0^2 = \frac{3e^2}{2\pi} A^2.$$  

The correction for spin 1 is some number, which I do not remember, times $\frac{e^2}{\pi} A^0$. For spin $\frac{1}{2}$, it is the same sort of thing. For spin $\frac{1}{2}$, $\Delta m_{1/2}^2$ comes out zero to this order and the next term is of order $m^2$. The supposition is that this is an accident—that this is due to a balance between the positive electric energy of the charge and the negative energy of the magnet, which is so finely balanced by luck that it comes out zero. The natural value of the mass is more like these $\frac{e^2}{\pi} A^0$ factors, and that if we could only make this balance slightly inaccurate at high momentum we would get a finite mass for a zero mass electron.

Energy, the energy that comes from the second term you could call the magnetic energy—there is no cross term, and it turns out that the electric and magnetic energies are equal and opposite in sign to the first order in $A^2$. So, all right, we will just put in a cut-off in the second, magnetic, term, which makes the two terms not exactly balance when the momentum goes up. In other words we suppose that the cut-off is a little bit different for the electric part of the coupling and the magnetic part. Incidentally, this makes the equations not unitary or something like that, but so does any cut-off anyhow, as soon as you use a cut-off you are in trouble anyway. With this result we get $\frac{3e^2}{2\pi} (A_1^2 - A_2^2)$ where the first cut-off is for the charge and the second cut-off is for the magnetic moment. And as far as I can tell, although there seem to be some ambiguities, when I apply this idea to calculating the rate of the decay, I can get zero, but I am not absolutely sure, because there are some ambiguities in what you do with the coupling of the $\beta$-decay itself, but I think I can get zero.

The disadvantage of this technique, however, is this: what could be natural would be for these two cut-offs to be of the order of the proton mass, but not equal to each other; that is the whole idea, but they should not differ by a very small percentage. If they differ by any reasonable fraction, then $\Delta m^2$ comes out say about $4000^2$. There must be a small number in the works, I do not know where it comes from, so that there is an ad hoc item there too.

At any rate, these were two attempts to understand how this rate could be smaller. Two serious questions come up. A theoretical question of great seriousness, which has always been here, what is the $\mu$ meson mass—what is the $\mu$ meson? And why does it have a mass? You cannot say that the mass of the electron is electromagnetic, and that of the $\mu$ meson is also electromagnetic—at least at first sight. Because, if you do that, you correct both of these rates—perhaps. Or, another way of saying it: what is the difference between the $\mu$ and the electron? If they are both electromagnetic, why do you get two different answers for two identical particles with just spin $\frac{1}{2}$? It could be, of course, that it is two roots of some quadratic equation and so on, but I leave that for the Rochester Conference five years from now, in which it comes out so beautifully that there are three roots and one of them has mass $500^2$. However, the question of great seriousness from the experimental standpoint is this: to establish as well as possible whether there is any rate at all here, what the number is here, if there is any. For example, it is still possible that it actually is $3.5$. It would help us if we could learn a little bit, you see. So one thing is to determine this rate as well as possible, if it exists. And the second thing is the magnetic moment of the $\mu$, as a piece of information about its structure. For example, one
might guess that the $\mu$ meson has a mass not due to coupling with electrodynamics, but to coupling with some mysterious other particle—of mass 500—and if this coupling is what makes the energy of the $\mu$ meson, this hypothesis would mean that the magnetic moment of the $\mu$ would be a bit different, even much more off than the experiments already said. So it would be very interesting to know how accurately the magnetic moment of the $\mu$ is given by the electromagnetic theory—that and the rate of $\pi - e$ decay. That is what I would like to see the experimenters give me more information on, in order to discover more about these charged leptons.

**LIST OF REFERENCES**

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**DISCUSSION**

*H. L. Anderson:* I would like to make clear the situation on the experimental side of the $\pi - e$ decay. The experimental statement is that the ratio of the $\pi - e$ decay to the $\pi - \mu$ decay is $0 \pm 10^{-5}$. The theoretician may always multiply such an uncertainty by a factor 3 to be on the safe side, so that for Feynman's 3.5 there exists a small possibility that it might even be right.

*Marschak:* I am glad to see that Feynman is now talking about chirality instead of the two-component theory, but that's just a crack! There certainly is a point in emphasizing the difference between the electron and the muon, in that here you have two particles of spin $\frac{1}{2}$ which have exactly the same interactions with other particles; they both seem to enjoy only weak interactions and electromagnetic, of course, and yet they have such different masses. Therefore it may really be that there is an asymmetry in the axial vector weak coupling, so that you have to discriminate between the two particles. Now we have tried to look at the problem in a somewhat different way, in terms of asymmetric conditions on the four-dimensional divergence of the axial vector strangeness-conserving current as between the muon and the electron. You might say that the divergence of this axial vector current, in the case of the muon, is equal to a constant times the pion wave function while, in the case of the electron, it is equal to something like $\left( \mathbf{D} - m^2 \right)$ times the pion wave function ($m$ is the pion mass). Under these assumptions you predict zero probability for the electron decay and a finite value for the muon decay. There seems to be a fundamental difference between the two leptons there and perhaps one could introduce the dissimilarity as indicated rather than use the approach of Feynman. It is extremely important to be sure of the $(\pi \rightarrow e + \nu) / (\pi \rightarrow \mu + \nu)$ experiment.

*Thirring:* I would like to ask Feynman how much this anomalous magnetic moment would influence the high energy electron scattering because then one would imagine that it will come in.

*Feynman:* Yes! I evidently should have figured it out; I have not done it.

*Treiman:* Just a technical point: How did you get this reduction by a factor of two? Did you not have to know something about the closed loops, $\gamma$-rays coming out of the closed loops?

*Feynman:* I do not think I did. I think I just used gauge invariance, but I may have made an error.

*Treiman:* Oh! but this is a rigorous statement, whatever is the structure of the $\pi - e$ decay.

*Gatto:* I only want to make a brief remark about the question of the universal Fermi interaction. In $\pi \rightarrow e + \nu$, you cannot in principle demonstrate directly that you have a breakdown of universality, if you are willing to assume also a small pseudoscalar interaction. However, you have to assume that it is about one-thousandth times smaller than the axial and the vector part. It does not give rise to inconsistencies with experiment, as was shown by Low and Huang. So one could say that the question is mainly one of physical taste. In fact we do not feel that we should simply introduce a number of the order 1000, but rather try to produce it by some mechanism. This number is of the order of the electron mass to the nucleon mass which is the relevant mass for the intermediate states in $\pi \rightarrow e + \nu$, so it seemed rather appealing to us that it could be connected to the mechanism producing the electron mass. This is the motivation, I mean.

*Feynman:* It is true that if one adds the right amount of pseudoscalar, you can make the thing just cancel for the electron, but it must be appreciated that, in order for the electron rate to be reduced by this large factor, the quantity of pseudoscalars has to be very precise and with the right sign. This kind of an accident could exist, but when the argument for the existence of a term is solely to explain a single experiment, it is a question of physical taste.
There is another point there, the origin of mass is a very interesting question. The existence of electromagnetic mass is almost obvious in the differences of the masses for the proton and neutron. Therefore surely some mass is of field-origin or interaction-origin, namely electromagnetic masses. The possibility then exists that all masses are of interaction origin.

This seems very interesting to me to follow out the consequences of this hypothesis, which comes from a place different from the $\pi \to e + \nu$ problem. We are led to ask "where does the mass of the electron come from?" and we discover that it will affect this ratio. So this is physical taste, but it does seem to come from a different place. But I must still keep on my blackboard at home a little sign which I have had for the last three or four years, and which says: "Why does the $J^\mu$ meson weigh?"

Moller: When you talked about cutting off $A_\mu$ and $F_{\mu\nu}$ in a different way, can you do that in a consistent relativistically invariant way? And how do you do it then?

Feynman: It is easy: you just put in the following factor, which multiplies the amplitude for the coupling $g_{\mu\nu} F_{\mu\nu}$,

$$A_2^2 (A_2^2 - K^2)^{-1}$$

where $K$ is the four-momentum of the field that is coming in, and every time the $A$ is coupled, you multiply the amplitude for each coupling by

$$A_1^2 (A_1^2 - K^2)^{-1}$$

with a $A_1$ which is different; on the idea that these two $A$'s are different. You see it is very dangerous, if I choose $A_2$ bigger than $A_1$, I get a negative value for $m_2^2$, and other such horrors! So, the lack of unitarity, etc., in this, plus the ambiguities which I seem to find when I try to calculate the rate of the $\pi \to e + \nu$ decay, makes it not at all pleasant and the difficulty of the small number makes all these things suspicious, so I do not like this model too much.

I do not like the other one because it is a little ad hoc, I only wanted to indicate this: if all the mass of the electron is electromagnetic, then the electromagnetic symmetry for $\gamma_5$ must break down for high momenta. How? I do not know in detail; if it does, it is quite possible that the $\pi \to e + \nu$ rate is changed. That this rate is changed if a good fraction of the electron is electromagnetic is the contribution made by all three contributors.

Budini: I want to observe that if you put in the Fermi coupling a form factor, with the same parameters as Lee and Yang took, for the decay of the $\mu$ meson, you obtain a further reduction of a factor of about two, because the electron is coming out with higher impulse than the $\mu$ meson.

Feynman: The idea is that the $\beta$-decay is not a point coupling, but is momentum dependent. Since the momentum of the electron is different from that of the $\mu$ it could be that this ratio is off because of the momentum dependence of the matrix elements. First: if the momentum dependence is as strong as this, it does alter the spectrum of the electrons in $\mu$ decay a little, so that it lies almost beyond experiment. Second: in the disintegration of the $K$ particle into three particles, one of which is an electron in one case, and a $\pi$ meson in the other case, the ratio is reasonably close to the right value, whereas the momenta are so much higher there that there would be even more variation to be expected. So that the disintegration of the $K_{e3}$ and $K_{\mu3}$ ratio does not show up such a momentum dependent term, or, at least, so it seems.

Lee: I would like to remark that if one likes to put in arbitrary structure factor, one can always arrange the rate to be anything you like, because there are a large number of ways you can put into those different vertex functions. One can even make $K_{e3}$ at whatever rate one wants to be phenomenologically right. But the question is: does one get more understanding this way as compared to the mere statement of the experimental facts?
EXPERIMENTS ON PARITY CONSERVATION IN STRONG INTERACTIONS

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This is a brief report on two experiments on parity conservation, one done at Rochester and one at Liverpool. The Rochester experiment, by Heer, Tinlot, and A. Roberts, is a measurement of the up-down asymmetry in \( \pi^+ \) mesons of 40 MeV produced at 92° with respect to a beam of 209 MeV protons polarized at 89° ± 2%. The limitation on the accuracy of this experiment was the available time, although the inherent asymmetries of the equipment are such that improvement on the present results by a factor much greater than 10 is not to be expected. The result of 80 hours running time is an up-down asymmetry of

\[ \varepsilon = 0.014 ± 0.037. \]

If this is attributed to a term in the scattering matrix of the form \( \varepsilon = F \langle \sigma \rangle \cdot k \), then it follows from this that \( F^2 \lesssim 2 \cdot 10^{-3} \). This experiment is a direct measurement of possible parity non-conservation in the fundamental Yukawa interaction. It is related to a similar experiment on the virtual Yukawa interaction by Garwin, Gidel, Lederman and Weinrich, who showed that the absence of circular polarization in the gamma-rays from \( \pi^0 \) decay permit an upper limit of \( F^2 \lesssim 8 \cdot 10^{-3} \).

The other experiment is one by D. P. Jones, Murphy, and P. L. O'Neil, in which a search is made for a component of longitudinal polarization in the neutrons produced at 0° in a beryllium target by 385 MeV unpolarized protons inside the Liverpool cyclotron. The analyser was a CH\(_2\) or C target, from which the recoil protons corresponding to neutrons of energy above 340 were detected. The neutrons emerged from the cyclotron and then traversed a long magnet with a transverse magnetic field, antiparallel to the cyclotron field. The neutrons, if longitudinally polarized, would precess through 120° in emerging from the cyclotron. This precession angle was corrected to either +90° or −90° by the external magnet. The experiment then sought to detect not the asymmetry in the neutron-proton scattering, which is necessarily relatively large because of unavoidable equipment inaccuracies, but a change in the instrumental asymmetry when the hypothetical neutron polarization was precessed through 180°.

The results on hydrogen, from CH\(_2\) − C differences, indicate a change of asymmetry of \( \Delta \varepsilon = 0.6 ± 3.2 \times 10^{-4} \), or with 95% confidence that \( \Delta \varepsilon \lesssim 7.1 \cdot 10^{-4} \). Taking into account the analyser efficiency of 18%, the longitudinal component of neutron polarization is \( P \lesssim 3.8 \cdot 10^{-3} \), and the coefficient \( F \) in the term \( F \sigma \cdot k \), giving rise to parity non-conservation, is, with 95% confidence, \( F^2 \lesssim 3.6 \cdot 10^{-4} \).

There is a difference of a factor of two between the definition of \( F \) in these two experiments, arising from a difference in the interpretation of \( \langle \sigma \rangle \) in the expression as a spin or polarization. Thus, to compare the two values, the Rochester value for \( F^2 \) should be divided by 4. It is clear from the comparison that such asymmetry experiments are most accurate when performed on neutral particles; however, the interactions investigated are then not necessarily the same.
**Telegdi:** I would like to ask if there is anybody here who recalls the experiment which has been done in the last year in low energy physics which says, as far as I recall, even more stringent limits on $F^2$ than has been mentioned here. It is not a matter of energies. If you want to go into strong interactions you can do it at 1 MeV. I think that either Wilkinson or his collaborators have done such measurements and I just wanted for completeness to call attention to this.

**Hillman:** I would just like to add, complementing Robert's talk, that we have done an experiment on time reversal in strong interactions at Uppsala and by comparing the polarization produced in scattering of protons by protons at 180 MeV with the asymmetry in the same scattering measured by other people, we have attained a limit of about 2% on the fraction of the interaction which is non-time reversible at the moment.

**Morpurgo:** I would like to remark again that since we have a partial explanation, assuming time reversal and charge independence, of the conservation of parity in strong pion-nucleon interactions it would be perhaps interesting also to do experiments in which either real or virtual hyperons are involved.

**Peierls:** I can answer the question of Telegdi. Wilkinson has a low-energy experiment which would give a limit on $F^2$ of less than $1 \cdot 10^{-7}$. I do not know whether this is with or without the factor 2 which Roberts pointed out.

**Roberts:** With respect to experiments done at low energies on parity conservation, it ought to be pointed out that these do not necessarily measure the same thing as in the scattering of fundamental particles, like $p-p$ scattering. It is assuming that we know a great deal more than we do of the interaction of nucleons in a nucleus, to suppose that the experiments of Tanner or Wilkinson, in which they look for example at a system which consists of a nucleus and an $a$-particle, tell you the same thing that you get when you observe direct meson production. I think it is worth while emphasizing that one must do both kinds of experiments.
MASS REVERSAL AND COMPOUND MODEL OF ELEMENTARY PARTICLES

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ABSTRACT

The consequences that follow from the invariance of the Lagrangian of interacting elementary particles with respect to the transformation of mass reversal:

\[ \psi \rightarrow \gamma_5 \psi \cdot m \rightarrow -m \]

for each particle separately, are examined. In interactions which conserve parity owing to mass reversal invariance elementary particles cannot be created nor destroyed singly. Thus conservation of strangeness and conservation of parity are interrelated.

In electromagnetic interactions, mass reversal invariance results in the principle of minimal electromagnetic interaction. Extension of the invariance to strong interactions results in an elementary particle model in which the truly elementary particles are either \( \Xi^-, \Xi^0, \Lambda \), or \( p, n, \Lambda \) (Sakata's model). In the framework of this model the peculiarities of strong interaction of strange particles and their systematization are discussed qualitatively.

For weak interactions, as has been shown earlier by other authors, invariance with respect to mass reversal results in non-conservation of parity, in the \( V-A \) type of interaction and in conservation of combined parity.

Besides, in the case of weak interactions, a number of strangeness and isotopic spin selection rules arise. Within the framework of the Sakata's model there naturally arises non-renormalization of the \( \beta \)-decay vector coupling constant with allowance for strong interaction.

But in the above papers the strong interactions were not invariant with respect to (2).

We shall extend mass reversal invariance to strong interactions as well and find what conclusions result from this.

1. The question of the chirality transformation

\[ \psi \rightarrow \gamma_5 \psi, \quad \bar{\psi} \rightarrow -\bar{\psi} \gamma_5 \]  

meaning, essentially, a change of inner parity of the particle, and of the consequences of the invariance of the Lagrangian of interaction with respect to this transformation has lately become a point of extensive discussion \(^1\text{--}\text{7}\),. Tjomno \(^4\) noticed that the Lagrangian of a free Dirac particle and of a Dirac particle in an electric field is invariant with respect to this transformation, if the latter is supplemented by the substitution \( m \rightarrow -m \). Indeed, in this case the Lagrangian has the form:

\[ \bar{\psi} (\hat{p} - e \hat{A} - m) \psi \]

and, as can easily be seen, is not changed by the transformation

\[ \psi \rightarrow \psi' = \gamma_5 \psi, \quad \bar{\psi} \rightarrow \bar{\psi}' = -\bar{\psi} \gamma_5, \quad m \rightarrow -m \].

\( \text{(2)} \)

Tjomno suggested the term mass reversal to describe transformation (2).

Sudarshan and Marshak \(^4\) and Sakurai \(^5\) investigated the limitations imposed on the Lagrangian of weak interaction by invariance with respect to transformations (1) or (2), and arrived at the \( V-A \) coupling in weak interactions.

But in the above papers the strong interactions were not invariant with respect to (2).

We shall extend mass reversal invariance to strong interactions as well and find what conclusions result from this.

2. Let there be a system of \( N \) elementary particles between which there exists an interaction of an arbitrary type (electromagnetic, weak or strong). We require that the Lagrangian of this system be invariant with respect to transformation (2) for each elementary particle separately. Namely, we require that

\[ \mathcal{L} \rightarrow \mathcal{L}' = \mathcal{L} \]

if \( \psi_i \rightarrow \psi_i' = \gamma_5 \psi_i, \quad \bar{\psi}_i \rightarrow \bar{\psi}_i' = -\bar{\psi}_i \gamma_5, \quad m_i \rightarrow -m_i \)

\[ \psi_k \rightarrow \psi_k' = \psi_k, \quad \bar{\psi}_k \rightarrow \bar{\psi}_k' = \bar{\psi}_k, \quad m_k \rightarrow m_k \]

\( i, k = 1, 2, 3 \ldots N \quad k \neq i \).

Let us find the limitations imposed by condition (3) on the Lagrangians of electromagnetic, strong and weak interactions, under the restrictive assumption that the constants of these interactions are not changed by transformation (3).

3. It should be pointed out that condition (3) in combination with parity conservation in electromagnetic
interactions leads to the so-called principle of minimal electromagnetic interaction \(^9\). According to this principle the only interaction between the electromagnetic field and the particles is the interaction with the electrical charges of the latter. Indeed, other gauge-invariant interactions would have the form

\[
I_1 \overline{\psi}_i \gamma_\mu \gamma^\nu \psi_j F_{\mu \nu}
\]

\[
I_2 \overline{\psi}_i \gamma_\mu \gamma^\nu \gamma^\rho \psi_k F_{\mu \nu}
\]

where the subscripts \(i\) and \(k\) refer to different particles. However, transformation (3) reverses the sign of the first of these expressions and the sign and parity of the second. (It may be pointed out that the value of \(I_1\) is proportional to \(m\) in the expression for the anomalous magnetic moment of the fermion appearing as a result of radiative corrections, and so condition (3) is not violated.)

The absence of terms of type (5) accounts, in particular, for the conservation of strangeness in electromagnetic interactions. (These terms might have led to decays of the types \(p \to \pi + \gamma\), \(A \to n + \gamma\), etc.)

4. It can easily be seen that the strong meson-baryon interaction usually employed does not satisfy condition (3). Terms of type \(\overline{\psi}_i \gamma_\mu \psi_j\) reverse their sign under transformation (3), while terms of type \(\overline{\psi}_i \gamma^\mu \gamma^\nu \psi_k\) change their parity as well. This difficulty can be overcome by assuming that strongly interacting mesons (\(\pi\) and \(K\)) are not elementary particles but bound states of baryons and antibaryons, as was done by Fermi and Yang \(^9\) and by Levy and Marshak \(^10\) and by Markov \(^11\). If we accept this viewpoint the mesons should be excluded from the initial Lagrangian and the latter should be substituted by a Lagrangian containing only interacting fermions.

If we confine ourselves to four-fermion interactions it will easily be seen that not every strong interaction between fermions satisfies condition (3). Indeed, in the case of the type \(\overline{\psi}_4 O_{\psi^k}\) (for example, \(\overline{\psi}_p \gamma_4 \psi_p\)) reverse their sign under transformation (3), while terms of type \(\overline{\psi}_4 \gamma^\mu \gamma^\nu \psi_k\) (say \(\overline{\psi}_p \gamma_4 \psi_n\)) change their parity as well. This difficulty can be overcome by assuming that strongly interacting mesons (\(\pi\) and \(K\)) are not elementary particles but bound states of baryons and antibaryons, as was done by Fermi and Yang \(^9\) and by Levy and Marshak \(^10\) and by Markov \(^11\). If we accept this viewpoint the mesons should be excluded from the initial Lagrangian and the latter should be substituted by a Lagrangian containing only interacting fermions.

If we confine ourselves to four-fermion interactions it will easily be seen that not every strong interaction between fermions satisfies condition (3). Indeed, in the case of the type

\[
(\overline{\psi}_4 O_{\psi^k})(\overline{\psi}_0 O_{\psi^e}), (\overline{\psi}_4 O_{\psi^e})(\overline{\psi}_0 O_{\psi^e}), (\overline{\psi}_4 O_{\psi^e})(\overline{\psi}_0 O_{\psi^m})
\]

condition (3) and conservation of parity are incompatible. But it is precisely such terms that describe fast parity conserving processes such as

\[
\Sigma^0 + n \to A + n, \quad \Xi^- + p \to A + A, \quad \Sigma^+ + n \to A + p.
\]

To overcome this difficulty we are obliged to assume that not all known baryons are truly elementary particles \(^(*)\) and to confine ourselves to the interaction of truly elementary baryons only.

The selection of elementary baryons is somewhat arbitrary at present. There are two possibilities which satisfy condition (3). As a first one, the \(\Lambda\)-hyperon and the \(\Xi^-\) and \(\Xi^0\)-hyperons can be selected as elementary particles. Another possibility is to select a \(\Lambda\)-hyperon, a proton and a neutron. A composite model with \(p, n\) and \(A\) as the elementary particles was suggested by Sakata \(^13\) and examined afterwards by a number of authors \(^11-17\). That is the model we shall deal with in the following.

5. The following strong four-fermion interactions satisfying condition (3) and the laws of conservation of charge and number of baryons, are possible between the \(\Lambda\)-hyperon, the proton and the neutron:

\[
\begin{align*}
&\overline{\psi}_p O_{\psi^p}(\overline{\psi}_n O_{\psi^p}), (\overline{\psi}_n O_{\psi^p}), (\overline{\psi}_4 O_{\psi^p}) (\overline{\psi}_4 O_{\psi^p}) \quad (7) \\
&\overline{\psi}_p O_{\psi^p}(\overline{\psi}_n O_{\psi^p}), (\overline{\psi}_p O_{\psi^p}),(\overline{\psi}_4 O_{\psi^p}), (\overline{\psi}_4 O_{\psi^p}) \quad (8) \\
&\overline{\psi}_4 O_{\psi^p}(\overline{\psi}_4 O_{\psi^p}) \quad (9)
\end{align*}
\]

where \(O\) are known operators of four-fermion interaction. Expressions of the type \((\overline{\psi}_p O_{\psi^p})(\overline{\psi}_n O_{\psi^p})\) can be represented by means of Fierz’s relationships \(^18\) as a linear combination of terms of type (8).

It can easily be seen that expressions (7) and (8) conserve strangeness which in the model under consideration equals minus the number of \(\Lambda\)-hyperons. Expression (9) changes strangeness by two. Hence the impossibility of changing the strangeness by one in strong interactions is a direct consequence of the model under consideration and condition (3), and is not related to the isotopic properties of strong interactions. This makes it possible to introduce new elementary particles into the model if such necessity arises in the future.

6. Now let us consider the limitations imposed within the framework of the Sakata’s model on the operators in (7), (8), (9) and on the coupling constants of strong interaction by condition (3), by the requirement of isotopic invariance and by the Pauli principle. The most general isotopic invariant expression for the strong interaction Lagrangian has the form:

\[
\mathcal{L} = \mathcal{L}_{NN} + \mathcal{L}_{N \Lambda} + \mathcal{L}_{\Lambda \Lambda}
\]

\[
= \sum_i \left[ f_i (\overline{\varphi}_i \tau O_{\varphi_i}) (\overline{\varphi}_i \tau O_{\varphi_i}) + g_i (\overline{\varphi}_i O_{\varphi_i}) \right] \left( \overline{\varphi}_i O_{\varphi_i} \right) + h_i (\overline{\varphi}_i O_{\varphi_i}) \right] \left( \overline{\varphi}_i O_{\varphi_i} \right), \quad (10)
\]

where \(\varphi = \psi_{\varphi}\) is the isotopic spinor and \(\varphi = \psi_{\varphi}\) is the isotopic scalar.

Making use of the conceptions of the Yukawa interactions the terms in (10) proportional to \(g, h\) and \(i\) correspond to exchange of a neutral meson which is an isotopic scalar, while the terms proportional to \(f\) correspond to exchange of isotopic vector mesons.

\(*)\) Judging by the note added in proof to \(^13\) a similar programme has been developed in an unpublished paper by K. Iwata and K. Fujii.
It should be pointed out that expression (9) and hence fast processes with $\Delta S = \pm 2$ prove to be forbidden already on the strength of isotopic symmetry, because there is no such interaction for the proton.

Writing down (10) in explicit form we have:

$$
\sum_k \left\{ f_1 (\bar{\psi}_p O_l \psi_p - \bar{\psi}_n O_l \psi_n)^2 + 4 f_2 (\bar{\psi}_p O_l \psi_n) (\bar{\psi}_n O_l \psi_p) + g_1 (\bar{\psi}_p O_l \psi_p + \bar{\psi}_n O_l \psi_n)^2 + I_1 (\bar{\psi}_p O_l \psi_p) (\bar{\psi}_n O_l \psi_n) + h_1 (\bar{\psi}_p O_l \psi_p + \bar{\psi}_n O_l \psi_n) (\bar{\psi}_n O_l \psi_p) \right\} .
$$

(11)

Now we modify the second term in (11):

$$(\bar{\psi}_n O_l \psi_n) = - \sum_k a_{lk} (\bar{\psi}_p O_k \psi_p) (\bar{\psi}_n O_k \psi_n)$$

(12)

where $a_{lk}$ are the known Fierz factors. Substituting (12) into (11) and requiring invariance of (11) with respect to each of the transformations $\psi_p \rightarrow \gamma_5 \psi_p$, $\psi_n \rightarrow \gamma_5 \psi_n$, $\psi_A \rightarrow \gamma_5 \psi_A$, separately, we obtain the following relations:

$$
2 g_2 - 2 f_2 - (f_2 + 4 f_4 + 6 f_3 + 4 f_A + f_P) = 0
$$

(13)

$$
2 g_2 - 2 f_2 - (f_2 + 4 f_4 + 6 f_3 + 4 f_A + f_P) = 0
$$

$$
h_2 = h_p = h_A = 0
$$

It is easy to see that for symmetrical (in the $q$-number theory) couplings

$$
V - A, \quad V + A - 2(S - P), \quad S + P - T
$$

and their linear combinations one has

$$(\bar{\tilde{z}} \tau O_{1 \tilde{z}}) (\bar{\tilde{z}} \tau O_{1 \tilde{z}}) = (\bar{\tilde{z}} O_{1 \tilde{z}}) (O_{1 \tilde{z}}) .
$$

For antisymmetrical couplings

$$
V + A + 2(S - P), \quad 3(S + P) + T
$$

and their linear combinations one has

$$(\bar{\tilde{z}} \tau O_{1 \tilde{z}})(\bar{\tilde{z}} \tau O_{1 \tilde{z}}) = -6 (\bar{\psi}_p O_{\tilde{N} \tilde{p}}) (\bar{\psi}_n O_{\tilde{N} \tilde{n}}) = -3 (\bar{\psi}_n O_{\tilde{N} \tilde{n}})(\bar{\psi}_p O_{\tilde{N} \tilde{p}}) .
$$

In the latter case terms like $(\bar{\psi}_p O_{\tilde{N} \tilde{p}})(\bar{\psi}_n O_{\tilde{N} \tilde{n}})$ are equal to zero.

The existence of exchange forces in $p-n$ interaction implies that the Lagrangian (10) contains both symmetrical and antisymmetrical couplings.

7. Now let us turn to composite particles in the Sakata's model and enumerate the possible isotopic states.

Mesons

Pions are bound states of a nucleon and an antinucleon:

$$
\pi^+ = p\bar{n}, \quad \pi^- = \bar{p}n, \quad \pi^0 = \frac{1}{\sqrt{2}} (p\bar{p} + n\bar{n}), \quad T = 1 .
$$

$K$ mesons are bound states of a nucleon and an antihyperon (or an antinucleon and a $\Lambda$-hyperon)

$$
K^+ = p\bar{\Lambda}, \quad K^0 = n\bar{\Lambda}, \quad K^- = \bar{p}\Lambda, \quad \bar{K}^0 = \bar{n}\Lambda, \quad T = \frac{1}{2} .
$$

Besides, two more neutral mesons are possible with zero strangeness and isotopic spin: $\phi_4^0$ and $\phi_2^0$. These mesons are mixtures of the states:

$$(p\bar{p} - n\bar{n}) \Lambda \bar{\Lambda}, \quad T = 0 .$$

There are two more singly-charged mesons for which there are empty spaces in the Gell-Mann scheme. These are the $\omega^+$ and $\omega^-$ mesons. The strangeness of the first of them is $+2$ and of the second $-2$. These mesons can be represented as follows:

$$\omega^+ = \frac{1}{\sqrt{2}} (p\bar{n} - p\bar{n}) \Lambda \bar{\Lambda}, \quad \omega^- = \frac{1}{\sqrt{2}} (p\bar{n} + p\bar{n}) \Lambda \bar{\Lambda} .$$

Hyperons

The known hyperons can be represented as follows:

$$
\Sigma^+ = p\bar{n} \Lambda, \quad \Sigma^- = \bar{p}n \Lambda, \quad \Sigma^0 = \frac{1}{\sqrt{2}} (p\bar{n} + p\bar{n}) \Lambda \bar{\Lambda}, \quad T = 1
$$

$$
\Xi^- = \bar{p} \Lambda \Lambda, \quad \Xi^0 = \bar{n} \Lambda \Lambda, \quad T = \frac{1}{2} .
$$

Besides there are empty spaces in the Gell-Mann scheme for two singly-charged hyperons:

$$Z^+ = \frac{1}{\sqrt{2}} (p\bar{n} - p\bar{n}) \Lambda \bar{\Lambda}, \quad T = 0
$$

$$\Omega^- = \frac{1}{\sqrt{2}} (p\bar{n} - p\bar{n}) \Lambda \bar{\Lambda}, \quad T = 0 .$$

The strangeness of the first of these equals $+1$ and of the second $-3$.

The question as to why not all the particles enumerated above exist will be discussed below.

8. Now let us turn to the question of parity of elementary and composite particles.

It is easy to see that the transformation $\psi \rightarrow \psi$ corresponds to transition to a particle with a different inner parity. Indeed, if $\beta = \pm \psi$ where $\beta$ is the parity operator, then $\beta (\gamma_\gamma \psi) = \mp (\gamma_\gamma \psi)$. Therefore requirement (3) infers that the physical processes do not depend on the inner parity of the elementary particle. This is actually the case. In strong and electromagnetic parity conserving processes $\psi$-operators of elementary particles occur twice. In weak processes where particles are created or destroyed singly parity is not conserved. Thus not only the absolute inner parity of elementary particles, but their relative parity as well, lack physical sense, as they cannot be determined by experiment. For brevity we shall in the following accept the parity of all elementary particles as equal to $+1$. Then the parity of all antiparticles will equal $-1$. 

The existence of exchange forces in $p-n$ interaction implies that the Lagrangian (10) contains both symmetrical and antisymmetrical couplings.
Unlike elementary particles, the parity of composite particles is a physically measurable quantity. If we assume that the elementary particles contained in the composite particles are in the $S$-state we find that the parity of $\pi$, $K$, $\varrho$ mesons and $\Sigma^\pm$, $\Xi^0$ and $Z$-hyperons should be equal to $-1$ while the parity of $\omega$ mesons and the $\Omega$-hyperon equals $+1$.

9. There are no arguments at present against the existence of $\varrho_1^+$ and $\varrho_2^+$ mesons. If they had a zero spin these mesons would be analogues of the $\pi^0$ meson. The assumption $^{19}$ that the masses of $\varrho$ mesons coincide with the mass of the $\pi^0$ meson should be rejected if we take into account the strong dependence of nuclear interactions on the isotopic spin.

It is also very improbable that their masses are at all comparable with the mass of $\pi$ mesons, as in such a case the presence of $\varrho$ mesons would tell on the results of phase analysis of the interaction of pions with nucleons. Besides, such $\varrho$ mesons would arise during $K$ meson decay. It is possible that the $\varrho$ mesons (or one of them) are responsible for the existence of the maximum in $\pi^\pm$ meson scattering in the 900 MeV region. In this case the masses of the $\varrho$ mesons (or one of them) should be close to the mass of the nucleon.

The fast decay of pseudoscalar $\varrho$ mesons into two $\pi$ mesons is forbidden on the strength of parity conservation. The rapid decay of pseudoscalar $\varrho$ mesons into three $\pi$ mesons is forbidden on the strength of the isotopic and charge conjugation invariance of strong interactions $^{20}$ (this forbidding is not indicated in $^{17}$). Apparently the most probable decays of $\varrho$ mesons must be the fast decays $\varrho \rightarrow 4\pi\varphi$, $\varrho \rightarrow 2\pi\gamma$.

10. It is known that no multicharged elementary particles exist in nature. It is known also that nature has not filled all the vacancies in the Gell-Mann scheme within the framework of singly-charged particles: $Z$- and $\Omega$-hyperons and $\omega$ mesons are evidently absent. What qualitative conclusions concerning the nature of the interaction between truly elementary baryons can be drawn from the fact that composite elementary particles of one kind exist and composite elementary particles of other kinds do not? From the existence of pions and $K$ mesons it should be concluded that there is a very strong attraction between elementary baryons and antibaryons at small distances. Furthermore, from the fact that the mass of the $K$ meson is considerably greater than that of the pion, it follows that the attraction between the nucleon and the antihyperon is weaker than the attraction between the nucleon and the antinucleon (at least in the state with $T = 1$) or has a smaller radius. From the absence of composite particles of high strangeness and charges greater than unity it should be concluded that there is a strong repulsion between two baryons (or between two antibaryons) at small distances. This conclusion is in agreement with the widely known data on the existence of a “hard core” in the interaction of two nucleons. The repulsion between two $A$-hyperons is obviously weaker than that between two nucleons, as a particle containing two $A$-hyperons exists (the $\Xi$-hyperon), whereas particles containing even two nucleons (such particles would be $\omega$ mesons, $\Omega$- and $Z$-hyperons, and doubly-charged particles) do not.

11. The above considerations give grounds to believe that nucleon-nucleon interaction $\mathcal{P}^{NN}_N$ is stronger or has a larger radius than nucleon-$A$ interaction $\mathcal{P}^{NA}_A$ and $A-A$ interaction $\mathcal{P}^{AA}_A$.

Let us see what conclusions the symbolic inequality

$$\mathcal{P}^{NN}_N \geq \mathcal{P}^{NA}_A, \mathcal{P}^{AA}_A \quad (14)$$

leads to ($\ast$).

An interaction $\mathcal{P}^{NA}_A$ is necessarily included in the processes of creation of strange particles (particularly, $K$ mesons) as a result of collisions of $\gamma$-quanta, pions and nucleons with nucleons. This is due to the fact that in the model in question the formation of any strange particles by collisions of ordinary particles passes through a process of creation of the virtual pair $A + \bar{A}$. According to (14) the cross-sections of these processes should be smaller than the cross-sections of the corresponding processes of pion creation. This conclusion is confirmed by abundant experimental material concerning the photocreation of $K$ mesons and the formation of $K$ mesons by collisions of pions and nucleons with nucleons.

According to (14) when nucleons are bombarded with nucleons or $\pi$ mesons the creation cross-section of the real pair hyperon $+ \bar{\text{anti}hyperon}$ should be smaller than the creation cross-section of the pair nucleon $- \text{antinucleon}$, provided both these processes are considered far enough above their thresholds.

On the other hand the processes of scattering and mutual transformation of strange particles may proceed at the expense of strong nucleon-nucleon interactions $\mathcal{P}^{NN}_N$. Thus, the formation cross-section of the pair antihyperon $- \text{nucleon}$ by collisions of $K^+$ mesons with nucleons may prove to be not so small, as in this case the reaction of $K^+$ meson “break-down” is possible, this reaction not including the comparatively weak interaction $\mathcal{P}^{NA}_A$:

$$K^+ + p = (\bar{A} + p) + p \rightarrow \bar{\Sigma}^+ + p + p.$$  

The same holds also for the scattering cross-sections of $K^+$ and $K^-$ mesons. The absorption cross-section of $K^-$ mesons (reactions of the type $K^- + p \rightarrow \Sigma^- + \pi^-$) may also not be small.

Thus, in the scheme under consideration smallness of the strange particle creation cross-section does not necessarily involve small cross-sections of scattering and transition into other strange particles, as mainly different interactions are responsible for these processes. This conclusion agrees qualitatively with numerous experimental data according

$^{(\ast)}$ It seems very attractive to assume that in the Lagrangian (10) $f = g = h = l$ and the difference between $A-A$, $N-A$ and $N-N$ interactions is the combinatorial nature and is connected with the presence of matrices $t$ in $\mathcal{P}^{NN}_N$. 

to which strange particles have small creation cross-sections but large scattering and absorption cross-sections.

It should be pointed out that this peculiarity of strange particles is not reflected in the model suggested recently by Gell-Mann [21]. According to this model the interactions of all baryons with pions are of equal strength and their interaction with $K$ mesons is comparatively weak. The conclusions obtained on the basis of this model differ sharply from those we arrived at above. Particularly, according to Gell-Mann’s model the scattering cross-sections of $K^\pm$ mesons and the absorption cross-section of $K^-$ mesons should be small. The formation cross-section of the pair hyperon — antihyperon in a beam of pions or nucleons should be larger than the formation cross-section of the nucleon — antihyperon pair in a beam of $K^+$ mesons.

According to (14) the interaction between two $\Lambda$-hyperons is weaker than that between two nucleons. If this is so, the matrix element for the formation of four strange particles should be smaller than that of formation of two strange particles (at the same time, the matrix element of creation of the $\Xi$-hyperon is smaller than that of creation of $\Lambda$- and $\Sigma^-$ hyperons).

12. In the model under consideration all slow processes are described by weak four-fermion interactions. All slow processes known at present can be described by the following interactions:

$$G^1 (\bar{\psi}_p \psi_n) (\bar{\psi}_n \psi_p), \quad (15)$$
$$G^2 (\bar{\psi}_p \psi_n) (\bar{\psi}_p \psi_n), \quad (16)$$
$$G^3 (\bar{\psi}_p \psi_n) (\bar{\psi}_n \psi_p), \quad (17)$$
$$G^4 (\bar{\psi}_p \psi_n) (\bar{\psi}_n \psi_p), \quad (18)$$
$$G^5 (\bar{\psi}_n \psi_n), \quad (19)$$
$$G^6 (\bar{\psi}_p \psi_n), \quad (20)$$

Expressions (15), (16), (17) and (18) describe the interaction of baryons with leptons. Expression (19) describes a weak interaction between baryons responsible for non-leptonic decays of strange particles. Expression (20) describes the interaction responsible for the decay of the $\mu$ meson. It seems reasonable not to go any further in examining other possible types of weak interaction unless necessitated by experiment.

Experiments show that all the constant $G$ are close to each other in value. Gell-Mann and Feynman [23] pointed out, for instance, that the $\beta$-decay vector coupling constant and the vector coupling constant of decay of the $\mu$ meson equal each other very accurately. There are no data at present on the strict equality of all constants $G$.

We may point out that in writing down weak interactions in the form of (15) to (20) we tacitly assume that $p, n, \Lambda, \mu^-, e^-, \nu$ are particles while $\bar{p}, \bar{n}, \bar{\Lambda}, \mu^+, e^+, \bar{\nu}$ are antiparticles.

As shown by Marshak and Sudarshan [6] and also by Sakurai [5] condition (3) results in a unique form of operator $O$ in expressions (15) to (20):

$$O = (1 - \gamma_\mu) \gamma_\mu . \quad (21)$$

Gell-Mann and Feynman [22] arrived at the same form of operator $O$ by a different line of reasoning. Thus, condition (3) results in non-conservation of parity in weak interaction, ensures conservation of combined parity and selects the $V - A$ type of interaction.

13. Now we shall show that the vector coupling constants of the $\beta$-decay interaction $G_\mu$ and of the interaction between $\mu$ mesons and nucleons $G_\nu$ do not vary in the model under consideration if allowance is made for strong interaction corrections.

In the conventional theory of interacting pions and nucleons, as Gershtein and Zel’ dovich [23] and Feynman and Gell-Mann [23] have demonstrated, this property of the vector interaction arises only if a direct interaction of pions and leptons is introduced. Taking strange particles into account, direct interaction between these particles and leptons should also be introduced [24]. It is easy to see that in the model under consideration this property of the vector type of weak interaction is a direct consequence of the isospin invariance of strong interactions. Indeed, neglecting electromagnetic and weak interactions, we have the following expression for the total Lagrangian:

$$L = \frac{i}{2} \left( \overline{\psi}_\mu \frac{\partial \overline{\psi}_\mu}{\partial \xi} - \frac{\overline{\psi}_\mu}{\partial \xi} \gamma_\mu \overline{\psi}_\mu \right) + \frac{i}{2} \left( \overline{\psi}_\mu \frac{\partial \overline{\psi}_\mu}{\partial \xi} - \frac{\overline{\psi}_\mu}{\partial \xi} \gamma_\mu \overline{\psi}_\mu \right)$$

$$- m_{\Xi \Lambda} \overline{\psi}_\mu - m_{\tau} \overline{\psi}_\mu + L_{\text{int}} . \quad (22)$$

where $L_{\text{int}}$ is the Lagrangian of strong interaction in the form (10). Taking advantage of (22), we can easily prove that for the nucleon current of $\beta$-decay interaction $j^V_\mu = G_\nu \overline{\psi} \gamma_\mu \overline{\psi}_\mu$ the following relation is fulfilled:

$$\frac{\partial j^V_\mu}{\partial \xi} = 0 . \quad (23)$$

It was shown in [22, 24] that relation (23) results in the conclusion that the value $G_\nu$, like an electrical charge, does not change when corrections connected with the strong interaction are taken into account. A similar proof can be given for $G_\mu$. The above-obtained result is true only in an approximation which makes no allowance for virtual slow and electromagnetic processes.

Unlike the leptonic interaction of nucleons, the leptonic interaction of $\Lambda$-hyperons (expressions (17) and (18)), as can easily be seen, does not possess the property of non-renormalizability of the vector coupling constant.
the main point is in this respect for us, that we take parti­
charge, we have this physically entirely meaningless ambi­
relative parity. Whenever we have the conservation law

tical model gave only a few times as many

K
meson production process indicates a small coup­
of the

K
meson interaction volume, you can bring this
smaller
minor ways, such as using a smaller
K
meson range, and

K
meson is regarded as a bound
state of an antinucleon and a

A.

And if this is so the

A
transformation.

Lagrangian with respect to the

h
y
transformation. And we must add mass

terms ad hoc or have a chirality non-invariant interaction
which will be responsible for mass terms. I would like to
make a small remark in reply to Adair. I am not quite sure what Okun' said about
Yamaguchi: I am not quite sure what Okun' said about mass terms. Is he proposing something like mass reversal as Tjomno and Sakurai have tried? Does his mass term have different transformation properties under \( \gamma_5 \)? Or, if he is proposing an entirely \( \gamma_5 \) invariant Lagrangian, what is the origin of mass?

Okun': The total Langrangian is not invariant with respect to the \( \gamma_5 \) transformation. And we must add mass terms ad hoc or have a chirality non-invariant interaction which will be responsible for mass terms. I would like to make a small remark in reply to Adair. I have not made a statement that the coupling constant in the interaction of \( \Lambda \) with nucleon is small. I said that this interaction is weaker than the nucleon-nucleon interaction. It may be that it has a smaller range.
PHENOMENOLOGICAL THEORY OF THE S-MATRIX AND T, C AND P INVARIANCE

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Phenomenological relativistic theory of the S-matrix has been adapted so far to processes between so-called elementary particles, whose description by local field operators seemed to be more justified. There are strong arguments, however, that in the S-matrix formalism both elementary and composite particles (e.g. nuclei) can be described quite strictly by local field operators which satisfy just the usual free field equations of motion and free field commutation relations. This seems to be true even in the case of non-local interactions.

If one makes this assumption one can give the most general expression for the transition matrix \( M \) which describes any given process between \( n \) particles of given spin values and masses and:

1) is invariant under proper homogenous Lorentz transformations,
2) conserves energy and momentum,
3) conserves electric and nucleonic charges.

General expressions for the transition matrices describing all possible processes between four particles of arbitrary spins have been investigated in some detail. The \( c \)-number part of the transition matrix has been written in the form of a sum of some known covariants multiplied by unknown scalar expansion coefficients. It has been shown that the covariants satisfy some important identities which make it possible to express the \( T, C \) and \( P \) invariance conditions in the form of some simple equations between the mentioned scalar coefficients. In order to obtain TCP theorem one must make some assumptions about the commutation relations between kinematically independent fields. In the case of occurrence of two kinematically connected fields, whose commutator (or anticommutator) is a \( c \)-number different from zero, one must symmetrize (or antisymmetrize) the matrix \( M \), with respect to the corresponding field operators. If one makes this assumption one finds that TCP invariance is satisfied by any transition matrix. It is worth while stressing that conservation of energy and momentum and the charge conservation are not necessary for the validity of TCP theorem.

The general expression for the transition matrix can be used for polarization calculations in various reactions between elementary and composite particles. The examples of \( K_{\pi N} \) and hyperon decays have been studied in some more detail.

Relations between cross-sections which follow from \( T, C, CP \) and TCP invariance requirements or hermiticity condition have been put in a general form independent of the number and spins of the interacting particles.
SESSION 8
Friday, 4th July, 1958

Weak interactions — Leptonic modes

Chairman    A. SALAM

EXPERIMENTAL
Rapporteur  M. GOLDHABER
Secretaries  A. LUNDBY
             B. LEONTIC
             J. P. STROOT

THEORETICAL
Rapporteur  L. MICHEL
Secretaries  Y. YAMAGUCHI
             C. ENZ
             D. SPEISER
WEAK INTERACTIONS: LEPTONIC MODES — Experimental

M. GOLDHABER, Rapporteur
Brookhaven National Laboratory, Upton (N. Y.)

The experiments on which I shall report can be quite naturally divided into two parts, comprising on one hand the low energy field and on the other hand the high energy field. Let us consider low energy phenomena first, i.e. \( \beta \)-decay. At the time of the last Rochester Conference we had already learned that parity is not conserved in \( \beta \)-decay. We also thought that we knew that the \( \beta \)-interactions were tensor and scalar, and that the neutrino was a right-handed screw, and the anti-neutrino a left-handed one. A vague notion appeared to persist that a double definition could be used: the anti-neutrino, say, being defined either as the particle emitted together with a negative electron in \( \beta \)-decay or as a left-handed screw. For a while it must have looked as if a definition had replaced a measurement!

The chief progress during the last year consisted in the conclusive proof that the \( \beta \)-interactions are actually axial vector and vector, and that the neutrino is the left-handed particle and the anti-neutrino the right-handed one.

The first evidence came from an experiment at the University of Illinois, carried out by Herrmannsfeldt, Maxson, Stähelin, and Allen. The angular correlation between \( \beta \)-rays and neutrinos depends on the form of the \( \beta \)-interaction in a manner shown in Table I.

\[
\begin{array}{|c|c|}
\hline
\text{G-T} & \text{F} \\
\hline
T & 1 + \frac{1}{3} v/c \cos \theta \\
A & 1 - \frac{1}{3} v/c \cos \theta \\
S & 1 - v/c \cos \theta \\
V & 1 + v/c \cos \theta \\
\hline
\end{array}
\]

Fig. 1. Scheme of apparatus for measurement of energy spectrum of negative recoil ions in electron-neutrino angular correlation experiment.
Allen and his collaborators investigated the recoil spectrum of the positron emitter $^{35}\text{A}$, which from its $Q$ value was expected to be a nearly pure Fermi transition between mirror nuclei. As Table I shows, the greatest difference between angular correlations is to be found for Fermi transitions. The apparatus is shown in Fig. 1. They studied the energy distribution of the recoil ions, using two spherical electrostatic spectrometers in series. Their results are shown in Fig. 2. They expected $S$ interaction, but found instead $V$ interaction. If we compare this perplexing result with the results from previous $\beta$-recoil experiments (Fig. 3(*)), we see that all positron emitters show interactions compatible with $A$ and $V$, whereas all negative electron emitters indicate $T$ and $S$.

Theoreticians, with near unanimity, said that this was not compatible with any simple theory of $\beta$-decay. A different interaction for $\beta^-$ and $\beta^+$-emitters would have seriously contradicted present field theoretical views. The question therefore arose: Is one of the two “anchor” experiments wrong, $\text{He}^6$ or $^{35}\text{A}$, and if so, which? As we may hear in the next report by Michel, the principle of universal Fermi interaction was used by Marshak and Sudarshan and by Feynman and Gell-Mann to decide in favour of $^{35}\text{A}$ and against $\text{He}^6$. But from an experimental point of view it was still necessary to reach a clear-cut, direct decision! Because of the apparent mutual contradictions of “classical” $\beta-\nu$ recoil experiments a new approach seemed worth considering. Fortunately, thanks to parity non-conservation in $\beta$-decay, it turns out that the $\beta$-interaction can also be studied in a non-classical way. Let us use the language of the two-component neutrino theory without committing ourselves, at this point, as to its degree of validity. We know from the experiment of Wu, Ambler, Hayward, Hoppes and Hudson that the $\beta$-rays from oriented Co$^{60}$ nuclei are preferentially emitted in the direction opposite to that of the Co$^{60}$ spin (Fig. 4). We see from this that the electrons are polarized opposite to their direction of motion, i.e. they have negative helicity, in agreement with experiment. Keeping this fact in mind, we see that the helicity of the antineutrino (emitted together with a negative electron, which is conventionally defined as a particle) is seen to be correlated with the $\beta$-interaction. The helicity is negative for $T$ (left-handed antineutrino), but positive for $A$ (right-handed antineutrino). For positron emitters the situation reverses. From an oriented nucleus the positrons are emitted in a pure $G-T$ transition in the direction of the spin; they have thus positive helicity.

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(*) Prepared by P. Stähelin.
again in agreement with experiment, and the neutrinos have a helicity opposite to that for the antineutrinos, positive for $T$ and negative for $A$. If we were dealing with a pure Fermi transition, say from a nucleus of spin 0 to another spin 0 nucleus, then we have no preferred direction of electron emission, but depending on whether we have $S$ or $V$ interaction, the electron and antineutrino will preferentially go in opposite or parallel directions respectively. Since they must carry opposite spins, their helicities are the same for $S$ and opposite for $V$. For a given antineutrino helicity the electron helicity thus depends on the interaction in Fermi decay. If lepton conservation holds, i.e. if the same antineutrino is emitted in $F$ and $G-T$ transitions, then we see that $S$ and $T$ lead to negative electron helicity, and that $V$ and $A$ lead to positive electron helicity. Experiments carried out by Deutsch, Gittelman, Bauer, Grodzins and Sunyar\(^{5}\), as well as by others, show the same helicity for positrons from a $0 \to 0$ transition as is found for a pure $G-T$ transition, i.e. positive helicity. Similarly, mixed Fermi-$G$-$T$ transitions ($J \to J$) show the same positron helicity, as shown by Boehm et al.\(^{5}\). These results are compatible with lepton conservation. The same conclusion was reached by Boehm and Wapstra\(^{7}\), Schopper\(^{9}\), Lundby, Patro and Stroot\(^{9}\), Steffen\(^{10}\) and others, from the study of $\beta-\gamma$ circular polarization correlations. Here, interference terms such as $C_S C_T$ or $C_V C_V'$ show up, in agreement with lepton conservation (Fig. 5 (a)).

The existence of such interference terms also indicates that time reversal invariance is not completely violated. Let us remember, then, that the facts described lead to the following helicities:

<table>
<thead>
<tr>
<th>$e^-$</th>
<th>$\bar{\nu}$</th>
<th>$e^+$</th>
<th>$\nu$</th>
</tr>
</thead>
<tbody>
<tr>
<td>$T, S$</td>
<td>$-$</td>
<td>$+$</td>
<td>$+$</td>
</tr>
<tr>
<td>$A, V$</td>
<td>$+$</td>
<td>$-$</td>
<td>$-$</td>
</tr>
</tbody>
</table>

We see thus that a measurement of the neutrino helicity would lead to a unique determination of the $\beta$-interactions. Such an experiment was carried out last fall by Grodzins, Sunyar and myself\(^{11}\). We used a radio-active nucleus which decays by orbital electron capture (thus emitting neutrinos, just as in positron emission, which corresponds to absorption of negative electrons from the Dirac sea). We measured the neutrino helicity by a method illustrated in Fig. 6, which shows the connection between the helicity of a $\gamma$-ray emitted opposite to a neutrino of given helicity for the spin sequence $0 \to 1 \to 0 +$. The only nucleus which appears to have a suitable decay scheme is Eu\(^{137m}\) (Fig. 7).

(*) Prepared by Steffen,
Eu\textsuperscript{152} has a meta-stable state of 9.3 hours which we believed to have spin zero, and negative parity. At the time we published our first results we said that this is the most plausible spin assignment and in order of decreasing plausibility, we could also have 1— or 1+. It fortunately turns out, from the magnitude of the effect found, that we would get the same answer for the neutrino helicity if the spin were 1—. However, if the spin were 1+ we would not have an allowed but a first forbidden transition 1+ to 1—, and in that case we would not be able to discuss this experiment intelligently, because there are many matrix elements involved which we would not know. It was therefore important to rule out this implausible but still possible spin 1+ and this has been recently done by Grodzins and Sunyar\textsuperscript{12}) who studied the $\beta$-$\gamma$ correlation in the decay Eu\textsuperscript{152m}$\rightarrow$ Gd\textsuperscript{152.} If the meta-stable state of Eu\textsuperscript{152} were 1+, the $\beta$-transition would be allowed, and an allowed $\beta$-transition followed by a $\gamma$-transition gives an isotropic angular distribution. They found, in fact, a very anisotropic angular distribution compatible with a spin of 0— but again not excluding 1—, although making it still less plausible; so let us assume that the spin is 0— since it does not matter for the following discussion. Another fortunate fact about this nucleus is that the energy release in electron capture is quite comparable to that of the $\gamma$-ray emitted to the ground state. This means that the recoil energy very nearly compensates for the loss in emission of energy in the $\gamma$-ray and one can do a resonance experiment which singles out those $\gamma$-rays where the recoil due to neutrino emission has compensated for the loss of energy due to the $\gamma$-emission. The positron energy spectrum has been recently determined accurately by Alburger and Ofer\textsuperscript{13}) who give the energy within 5 KeV. The energy of the 961 KeV $\gamma$-ray is known to an accuracy of about 1 to 2 KeV. The energies of the neutrino- and $\gamma$-transitions are so nearly alike that it is rather important to know whether you are dealing with $K$-electron capture, $L$ electron capture or $M$, or even higher, because as you come closer to the full energy you get much closer to resonance, and this would be an interesting effect to learn more about for this nucleus. It does not matter in detail for the discussion which follows.

The lifetime of the (1—)-state is the mean of two measurements, one by Grodzins\textsuperscript{10}) and one by Moon, Shute and Sood\textsuperscript{10}) who showed that if they heat their source there is a thermal effect from which they can deduce a lifetime somewhat longer than that given by Grodzins.

Resonance scattering of nuclear $\gamma$-rays is a very subtle phenomenon. Due to the need of making up the energy loss from nuclear recoil in emission and absorption, the resonance condition can usually only be fulfilled in special circumstances, and as a rule requires special techniques, as developed by Moon, Malmfors, Metzger and others\textsuperscript{15}). Here we have the unique situation of a state of sufficiently short lifetime so that the neutrino recoil is available to make up the energy loss, even in a solid source material. But since the neutrino energy is not sufficient to compensate for the energy loss completely, as shown schematically in

**Fig. 8. Schematic of energy distribution in resonance scattering by Sm\textsuperscript{154.}**

Fig. 8, the resonance scattering arises partly from the overlap of the resonance wings, partly from $K$-$X$-ray or Auger electron recoil and partly from thermal Doppler broadening. But because of the complicated effects involved, it is difficult to estimate how much the percentage of circular polarization is reduced for the resonance scattered $\gamma$-rays from the ideal case of $\gamma$-rays emitted at exactly 180° to the neutrino direction. At present the reduction factor can only be estimated under idealized assumptions. In any case the helicity will have the same sign as the neutrino helicity. To measure the sense and magnitude of the circular polarization of the $\gamma$-rays we used the arrangement shown in Fig. 9.

The scattered radiation transmitted through the iron is shown in Fig. 10.

The helicity results are shown in Fig. 11.

The helicity of the $\gamma$-rays is found to be negative, thus the helicity of the neutrinos is also negative and the $\beta$-inter-
Weak interactions — Leptonic modes

Fig. 10. Resonant scattered γ-rays of Eu\textsuperscript{152m}. Upper curve is taken with arrangement shown in Fig. 9 with unmagnetized iron. Lower curve shows non-resonant background (including natural background).

Fig. 11. Measured circular polarization for each individual run.

actions are \( A \) and \( V \), in agreement with the work of Allen and collaborators.

The results plotted in Fig. 11 contain the summary of nine independent runs lasting each from three to nine hours. The first three were done with liquid sources, the last six with solid sources. The first six were done with the magnet shown in Fig. 9, the last three with a short magnet where the source could be put on top. You see clearly that the sign of the helicity is quite definite and the effect is reasonably large: \( (67 \pm 10) \% \). The particular number is really more, I believe, not a property of neutrinos but a property of Eu oxide and Sm oxide and therefore need not be discussed at this conference. So we have in this experiment decided that the photon is of negative helicity and this means that the neutrino is of negative helicity and if you recall Table II that means that we have an axial vector interaction for this Gamow - Teller transition. This confirms Allen’s\(^1\) result which states that vector and axial vector are compatible with the recoil experiments for positron emitters.

Now you might ask what has happened in the last few months on the negative electron side. I did not know that very much had happened till I arrived here. But then things seemed to start happening. I shall just discuss the contributions which have been handed to me in order of increasing mass number: neutron, He\textsuperscript{6}, Li\textsuperscript{8}, Na\textsuperscript{24}. There are extremely important experiments finished in time for this conference on the \( \beta \)-decay of the free neutron. The first paper on which I would like to report is by Sosnovskij, Spivak, Prokofiev, Kutikov and Dobrynin\(^1\). They have measured very accurately the life-time of the free neutron, and this is a very important quantity because it allows a calculation of the relative proportions of Gamow - Teller and Fermi interaction. Fig. 12 shows the arrangement. A slow neutron beam coming out of a reactor passes under a long vertical tube through which the recoil protons from neutron decay, which happens in the central volume, can be detected. By making the tube to the detector very long, about 80 cm, they were able to define the solid angle much better than had been done in previous work on the lifetime of the neutron. By also measuring the neutron density very accurately, they could calculate the absolute neutron decay rate. The recoil protons go through a field free space, defining the solid angle and are finally accelerated between two electrodes and then detected in a proportional counter with a very thin window. The acceleration potential is 20 kV and the window only absorbs 2 kV energy and therefore the efficiency of detection is high. They made many tests and believe they know all these efficiencies very well. The result is that the lifetime of the neutron is \( 11.7 \pm 0.3 \) min. The \( \beta \) value which follows from this is \( 1170 \pm 35 \). Taken together with the \( \beta \) value of 0\textsuperscript{14}, which is a pure Gamow - Teller transition, they can calculate the ratio of the Gamow - Teller to the Fermi coupling constants The square of
this quantity is $1.55 \pm 0.08$. This is, of course, very exciting news. When I was preparing this report I was going to say at this point that it would be very important to decide whether this ratio is really different from 1, because all the old experiments had a sufficiently large error that it might have been 1. Now in future conferences we shall probably discuss how important it is to see if this ratio is really different from 1.5 or whatever theory will give!

An interesting point about this $f_I$ value is that it compares well with the $f_I$ value for $\text{H}^3$ which has been recalculated by Kistner and Rustad in Brookhaven from a new determination of the energy difference of $\text{H}^3$ and $\text{He}^3$ measured with the help of a mass spectrograph (this measurement was carried out by Friedman and Smith at Brookhaven). These are very similar transitions, spin $\frac{1}{2}$ to spin $\frac{1}{2}$. The $f_I$ value of $\text{H}^3$ is 1132 ± 40. This agreement is, of course, of great interest for people who discuss nuclear models.

Whether one can, from now on, use this Gamow - Teller to Fermi ratio for the fundamental constants to work out empirical matrix elements which, then, the models ought to fit, I do not know; there still are the questions whether there are more complex effects going on in complex nuclei, besides the old-fashioned effects due to the matrix elements. This should now be a more definite and a very interesting question to pursue. Fortunately, when I arrived here, I also learned of the latest value of $A$ in the Argonne-Chicago experiment on the up-down asymmetry of electrons emitted from polarized neutrons; the value is now $A = -0.11 \pm 0.02$, and this allows one also to calculate independently the ratio of Gamow - Teller to Fermi constants. This new value, very fortunately, agrees exactly with the Russian one. So we have here an independent fortification of each result, which is very gratifying.

Fig. 13 illustrates, in a manner which Telegdi has invented, what it means when one says that Gamow - Teller and Fermi transitions have opposite phase, i.e. that the ratio is negative for the couplings.

Let us consider only the parts in the figure in which we are interested. Let us assume that we have a neutron whose spin is looking up and that it decays by a Gamow - Teller transition which carries away one unit of angular momentum leaving the proton looking downward. It can also go through the other channel, the Fermi transition, which leaves the nucleon spin the same. The thin arrows represent momenta, the heavy arrows spins of the leptons.

One can see from the schematic representation that the $A - V$ combination, in case of equal strength of both couplings, gives an $A$ asymmetry coefficient = 0 and a $B$ asymmetry coefficient = 1 and slightly different in the actual case. The contrary is true for the $A + V$ com-

(*) According to a recent communication from L. Mikaelian, $\left( \frac{G_{GTR}}{G_F} \right)^2 = 1.42 \pm 0.08$ if the latest $f_I$ value for $O^{14}$ $(3103 \pm 62)$. 

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**Fig. 12. Scheme of apparatus for measurement of neutron decay.**
Fig. 13. Correlations in neutron decay.

so this figure illustrates what we mean when we say that Fermi and Gamow-Teller transitions have opposite phases.

Fig. 14 shows the arrangement for the measurement of the neutrino asymmetry coefficient $B$. The protons pass through a venetian blind arrangement and are detected in the proton counter. By varying the direction of the spin of the neutron (up or down) one can show which way the neutrino prefers to be emitted. It prefers to be emitted in the direction of the spin, and this confirms the $V-A$ interaction. This was the first confirmation of $V-A$ on the $\beta^-$ side, and established the minus sign. Since then I have learned of many others. Fig. 15 gives a result just received from Herrmannsfeldt et al.\textsuperscript{19} in collaboration with the Argonne laboratory. They use the same apparatus which they used at Illinois for $A^{35}$. The experimental points lie higher than the theoretical $A$ curve at low energies ($< 700$ eV). This is believed to be due to the fact that doubly charged ions of high energy will appear to have half the energy because of the stronger deflection. This is not a serious deviation. The experiment looks like a good proof that $\text{He}^6$ is axial vector. As you know, before they did this experiment, Rustad and Ruby, and especially Wu and Schwarzchild\textsuperscript{22} discussed in great detail the situation in the old $\text{He}^6$ experiment. Their conclusion was that the effective volume of the $\text{He}^6$ source was not taken into account correctly and if this had been done in the original experiment it would very probably not have been concluded that the interaction is tensor. It is satisfying that one can understand why the old experiment went wrong, and that the new Argonne-Illinois result confirms axial vector coupling.

The experiment on $\text{Li}^8$ has been made by Lauterjung, Schimmer and Maier-Leibnitz\textsuperscript{20} in Heidelberg. $\text{Li}^8$ is a $\beta$-emitter (13 MeV) followed by two $\alpha$'s, a nearly pure Gamow-Teller transition $2^- \rightarrow 2^+$, as found at Cal. Tech. Coincidences are measured between the $\beta$-rays ($< 4$ MeV) and the two $\alpha$'s. Depending on the $\beta$-interaction, either one or the other counter will register more energetic $\alpha$-rays. Fig. 16 gives the results. The theoretical curves for $A$ and $T$ are plotted, as well as the curve

Fig. 14. Scheme of apparatus for measurement of up-down asymmetry of polarized neutron decay.

Fig. 15. Energy spectrum of recoil ions from $\text{He}^6\beta$-decay.
expected for equal mixture of $A$ and $T$. Axial vector is quite definitely favoured. A similar experiment at Cal. Tech., just finished, gives the same result.

The experiment on Na$^{24}$ has been made in Russia by Burgov and Terekhov$^{21}$. The principle is the following: Na$^{24}$ is a nearly pure Gamow-Teller $\beta$-transition followed by the emission of two $\gamma$-rays in succession (Fig. 17). The most probable angle at which resonance absorption in magnesium (the ground state nucleus) can be detected by two $\gamma$-counters in coincidence can be calculated as if there were no preceding $\beta$-emission. The effect of the $\beta$-emission is to smear out this angle. The smearing out process can be calculated for $A$ and $T$ interactions. Fig. 18 shows the experimental points and the theoretical curves. The errors are still large but the conclusion is that $\lambda$ in this case is $-0.25 \pm 0.22$, while $-0.33$ would be expected for axial vector. This result fits in with the others I have reported and strengthens somewhat the situation as we know it. We now feel that the form of the $\beta$-interaction is settled. There is so much general agreement, and sufficient understanding of what went wrong before, that we feel that we can safely state this.

About the absolute value of the $\beta$-interaction, I have received a new result from a thesis by Van der Leun$^{22}$ shown in Table III. He has remeasured some of the $f_1$ values for Fermi transitions and summarized some earlier ones and obtains an average for the Fermi interaction which is $(1.410 \pm 0.009) \times 10^{-49}$ erg cm$^3$.

Table IV is the summary of all the empirical information on $\beta$-decay, which can now be given in three lines essentially, and you are perhaps here at the end of an era of 60 years of research. We now know that the parity non-conservation is apparently complete, the odd and even couplings have the same sign, the absolute value of the Fermi interaction was already quoted in the preceding table, and the axial vector coupling constant is minus the vector coupling constant multiplied by $1.25 \pm 0.04$. Some slight
Weak interactions — Leptonic modes

TABLE III

C. van der Leun

$0^+ \rightarrow 0^+$ decays

<table>
<thead>
<tr>
<th>Transition</th>
<th>$t$ (sec)</th>
<th>$E$ (keV)</th>
<th>$f_t$ (sec)</th>
<th>$g_F 10^{-49}$ erg cm$^9$</th>
</tr>
</thead>
<tbody>
<tr>
<td>$0^{14}$</td>
<td>72.1 ± 0.4</td>
<td>1823 ± 12</td>
<td>3160 ± 12</td>
<td>1.395 ± 0.020</td>
</tr>
<tr>
<td>Al$^{26}$</td>
<td>6.36 ± 0.08</td>
<td>3218 ± 10</td>
<td>3050 ± 60</td>
<td>1.420 ± 0.014</td>
</tr>
<tr>
<td>Cl$^{34}$</td>
<td>1.54 ± 0.02</td>
<td>4495 ± 20</td>
<td>3110 ± 70</td>
<td>1.406 ± 0.016</td>
</tr>
</tbody>
</table>

Average: 1.410 ± 0.009

TABLE IV

Remarks

$C_V = + C_V'$; $C_A = + C_A'$

Parity non-conservation apparently complete

$C_V = (1.410 \pm 0.009) \times 10^{-49}$ erg cm$^9$

From $0^{14}$, Al$^{26}$ and Cl$^{34}$ $f_t$ values.

$C_A = -(1.25 \pm 0.04) C_V$

Lifetime of neutron

Up-down asymmetry of polarized $n$-decay.

No deviation from time reversal invariance observed.

changes may be expected in these numbers. The last line summarizes a lot of work which is by no means finished but which has not given any indication that time reversal invariance is in any way not conserved.

The accuracy of these statements on time reversal invariance depends on whose experiment you look at. There is one proceeding at Chalk River by Robson in collaboration with Nathans of Brookhaven. They find that it holds to within say $\pm 45^\circ$ around the phase angle of $180^\circ$. Telegdi gives a value from an experiment in progress at Argonne which he quotes as certain to $\pm 20^\circ$. Many other experiments have all accuracies somewhat closer to the accuracy I quoted for Robson’s experiment, but as I said, none has shown any sure deviation, so we can perhaps for a while assume that time reversal invariance holds, and if we do not get into trouble we will just get used to it. It is naturally not easy to find very small deviations if such exist. So we can now say that the gross features of $\beta$-decay are known. There remain fine points to be settled, of which you shall hear more in the theoretical talks, but the experimenter who wants to investigate these fine points will, I believe, have first to sharpen his tools.

Let us now consider the high energy field.

Fig. 19 is an unpublished result obtained quite a while ago by Crowe, when he was at Stanford, on the $\beta$-spectrum of the $\mu^+$ meson. As you know, the analysis of this spectrum gives $q = 0.68 \pm 0.02$, and was already quoted for several years.

![Spectrum of $e^+$ from $\mu^+$ decay.](image-url)
Table V is a summary which I believe Lee showed already at the last Rochester Conference. The particular value found for the Michel parameter \( \theta \) from this spectrum is close to the ideal case of 0.75 predicted for the case where the two neutrinos are of a different nature. It is therefore believed that in the \( \mu \) decay we have different neutrinos. If we call the negative electron just by definition a particle, the positive electron is an antiparticle and we shall then use this decay to define the \( \mu^+ \) meson as an antiparticle. So we have the simple Table VI of particles and antiparticles:

<table>
<thead>
<tr>
<th>Family</th>
<th>Particle</th>
<th>Antiparticle</th>
<th>Defining interaction</th>
</tr>
</thead>
<tbody>
<tr>
<td>( e^- )</td>
<td>( e^- )</td>
<td>( e^+ )</td>
<td>Electromagnetic</td>
</tr>
<tr>
<td>( \nu )</td>
<td>( \nu )</td>
<td>( \bar{\nu} )</td>
<td>( \beta )-decay</td>
</tr>
<tr>
<td>( \mu^- )</td>
<td>( \mu^- )</td>
<td>( \mu^+ )</td>
<td>( \mu^- e )-decay</td>
</tr>
</tbody>
</table>

The particular neutrino which is emitted together with a negative electron, we have called the antineutrino. So far we have no proof that the total number of particles plus antiparticles is always conserved. However, there are reactions which can now check whether lepton conservation is true because the \( \pi \) meson (Fig. 20) not being a lepton when it decays into a muon, say for positive charge, should then, if we have lepton conservation, emit a neutrino. This is something which can be empirically tested, namely we now know (I am again using the language of the two-component theory without taking it for granted that it is true) that the neutrino emitted together with a \( \mu^+ \) meson should have negative helicity. This would predict negative helicity for the \( \mu^- \), that means it predicts which way its spin is pointing, namely opposite to its direction of motion. In the extreme case, when the neutrino and the antineutrino go in the same direction in \( \mu \) decay, the electron which goes in the opposite direction has to carry away the spin of the \( \mu \) meson. We predicted that the

\[
\mu^+ \rightarrow e^+ + 2\nu \quad (\theta = 0)
\]

\[
\mu^+ \rightarrow e^+ + 2\bar{\nu} \quad (\theta = 0)
\]

\[
\mu^+ \rightarrow e^+ + \nu + \bar{\nu} \quad (\theta = \frac{3}{4})
\]

\[ \theta_{exp} = 0.68 \pm 0.02 \]
Fig. 22. Polarization of electron Bremsstrahlung from $\mu$ decay.

Fig. 23. Polarization of electron Bremsstrahlung from $\mu$ decay.

Fig. 24. Apparatus for measurement of Möller scattering of electrons from $\mu$ decay.
value. In general, however, one can conclude that the experiments agree with the two-component theory.

Fig. 22 shows the new measurements of the Liverpool group on the polarization of electrons and positrons from $\mu$ decay. The circular polarization of the Bremsstrahlung from electrons and positrons is measured by passing the radiation through magnetized iron. The polarization is seen to be positive for positrons and negative for electrons. The accuracy is, of course, not very great. The dotted line indicates the theoretically expected result. Fig. 23 by Crowe shows similar results obtained in Berkeley for both $e^+$ and $e^-$ from $\mu$ decay. The lower curve is a theoretical estimate from the theory of McVoy and Dyson. These two experiments illustrate very nicely that charge conjugation is not conserved in $\pi$-$\mu$-$\nu$ decay because you might say that the grand-daughters of the $\pi$ mesons produce circular polarization of Bremsstrahlung in opposite direction, but in going from the $\pi^+$ to the $\pi^-$ you have gone through a charge conjugation. If charge conjugation were conserved these two polarizations, if they existed at all, would be in the same direction. This is perhaps the neatest experimental evidence for non-conservation of charge conjugation. There is of course other experimental evidence which needs a little more theory to discuss.

Fig. 24 shows a measurement on positrons from $\mu^+$ decay done at Chicago by Anderson and his collaborators. They use Möller scattering in magnetized iron to determine the helicity of the positrons. They again confirm that the results are compatible with nearly 100\% polarization. So we can consider this question settled qualitatively but one would certainly like much more quantitative work if one feels that this is a way to pursue the question of the validity of the two-component theory.

Fig. 25 is based on the published work of Cork and Wenzel. It dates back from last year and shows how lepton conservation is obeyed in $K$ meson decay. The electron is seen to come out in the backward direction in accordance with the lepton conservation hypothesis.

Finally, there has been a search for electron or $\mu$ meson decay of hyperons. Fig. 26 shows the results of measurements done by the Columbia group. The $\pi$ mesons from $\Sigma^-$ decay have a unique momentum. The momenta expected from electrons and muons are indicated in the figure. They had 84 events of which none was in the latter class. Similar work has been done at Berkeley both by the hydrogen bubble chamber group and emulsion group. They have 500 $\Lambda$'s and 103 $\Sigma^-$ without observing any leptons in the decay. The Livermore group has observed 41 $\Sigma^-$-decays without seeing any electrons and the Barkas group has observed 34 $\Sigma^-$ without seeing any electrons. Altogether we have around 1000 hyperons with no observed electron or $\mu$ meson decay, and in addition there are many cases of decay of hyperfragments in which cases the electrons or $\mu$ mesons would have been seen if they had come out, and here there are also a few hundred. To sum up there are some 1500 hyperons, free or bound, observed, and in only one case has an electron or $\mu$ meson been reported (Fig. 27).
Weak interactions — Leptonic modes

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25. Barmin, V. V., Kanavetz, V. P., Morozov, B. V. and Pershin, I. I. (to be published)
31. Dobrokhotov, E. I., Lazarenko, Ya. and Luk'yanyov, S. Yu. (private communication)
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DISCUSSION

Telegdi: There is one aspect of weak interactions which has not been discussed. In $\mu^-$ capture one would like to establish two things: 1) that there is non-conservation of parity in $\mu$ meson capture; and 2) how universal the universal Fermi interaction is. From a naive approach one knows that the coupling is in agreement with universal Fermi interaction. There have been two experiments, one in the past at Chicago which has recently been re-evaluated, and a re-edition of the same experiment recently at Liverpool, a very nice and precise experiment. The theory underlying the capture rates which I would like to mention here has never been published. It has only been published by the experimentalists and never by the author, namely Primakoff. The expression for the capture rate is

$$A_{\text{capt}}(Z, A) = A_{\text{capt}}(1, 1) Z_{\text{eff}}^2 \gamma \left(1 - \frac{A-Z}{2A}\right).$$

First of all there is a purely atomic effect known as the $Z_{\text{eff}}^2$ and next to this there is a factor $\gamma$ to which
I will come back later. Then there is a Pauli effect which is proportional to \( \frac{A - Z}{2A} \). You create a neutron which does not know where to go, as part of the phase-space is already occupied. We shall call this neutron excess \( X \). The factor \( \gamma \) takes into account the reduction in phase space for the neutrino due to the effect that the muon is bound and the neutron has to be extracted so one has a smaller phase-space volume than one would have in the case of capture by the free proton. The capture by proton is of course the crucial experiment which one would like to see done. This factor \( \gamma \) has been evaluated by Primakoff to be 0.73 and the other factor \( \delta \) has been evaluated to be 3. You then plot the experimentally observed capture rates \( \frac{\Lambda_{\text{capt}}}{Z_{\text{eff}}} \) versus \( X \) and in fact in a marvellous fashion everything falls on a straight line very nicely within the errors. As a matter of fact at a point there may be three or four different nuclei because there are all sorts of nuclei with the same neutron excess and there is no a priori reason if you had not worked out the theory why these points should lie so well on a straight line. You could then say that after a fashion you could calculate from this the desired quantity, i.e. the capture rate in proton \( A \) (1,1). If you assume that the sum of the squares of the coupling constants governing \( \mu \) decay is the same as that in \( \beta \)-decay which is the most strong form of universality, and I do not see any theoretical reason for such a strong form of universality, then you would predict that the quantity \( \Lambda_{\text{capt}} \), (1,1) is equal to 220 sec\(^{-1}\). If you extrapolate the straight line you can obtain this quantity multiplied by the factor \( \gamma \). If you then believe Primakoff's theory you get from experiments
\[ \Lambda_{\text{capt}} \text{ (1,1)} = 255 \pm 25 \].

So far so good. We could say as Yang and Lee have pointed out that all the coupling constants are twice as big because the phase-space is half as big. We could say if the interaction was universal before the downfall of parity it is universal still now. It must involve the same neutrino so there is parity violation. Now comes the trouble: there has been a suggestion to figure out from the specific experiments on capture rates what is the Gamow-Teller to Fermi ratio through some specific experiments. Tolhoek and Luyten in Holland have derived capture rates in specific nuclei. One could say that they should get the same formula as Primakoff. Well, they work differently. Primakoff has essentially derived his formula by using closure, i.e. a sum rule. Tolhoek and Luyten have taken the approach which you take in nuclear physics. They take shell model wave functions and look to which states the transition can go, carry out the radial integrals and do all these things in much detail for a bunch of nuclei around calcium. They have for example the ratio \( \Lambda_{\text{Ca}} / \Lambda_{\text{Tl}} \) and for other nuclei in the same region. They give you the values which one should get for pure Fermi coupling or what it should be for pure Gamow-Teller interaction. Of course the measurements of the capture rates, not involving anything relativistic, can never tell axial vector from tensor and so forth. So there are two predicted ratios here and the Liverpool and the Chicago groups both measure these ratios very accurately and get for these values which agree with both theories. I would like in this discussion to raise the point as to how two people can make different theories and the experiments agree with both. If you look at the numbers they agree with Tolhoek and Luyten for pure Gamow-Teller and they agree with the theory of Primakoff. Now comes the question: what is the real composition of the interaction, once you have taken effects like those discussed by Goldberger and Treiman and so forth into account? The question is therefore what to do. Of course here in the case of parity violation you could do the experiment with the angular distribution of the emitted neutron with respect to the muon spin. At Columbia University they have measured this but I do not think it is a definitive experiment.

Lastly I would like to raise another point: Primakoff and others have pointed out that if a \( \mu \) meson is in a bound orbit its lifetime will be changed. I do not mean the disappearance rate but the decay rate will be changed. The decay rate goes like the mass to the fifth power. The \( \mu \) meson is bound so there is less energy to go. This effect has been measured at Chicago to exist but of course it has nothing to do with weak interactions.

All I wanted to point out is that there are good experimental numbers for the disappearance rates of the \( \mu^- \) mesons but there is no good theory to compare them with.

Salam: We divide the discussion into three main parts. First we discuss the \( V \) and \( A \) \( \beta \)-interaction, then we shall go on to remarks which Goldhaber made about hyperons and the lack of their leptonic decay mode, and finally we come to the points which Telegdi has raised and all the points which are also about the \( \mu \) meson and its decay. I would now invite comments about the \( \beta \)-interaction.

Nataf: From the Argonne-Chicago work I had the impression, although the accuracy is much less than the Russian work quoted by Goldhaber, that the ratio of Gamow-Teller to Fermi was rather 1.3, I mean the ratio of the squares, and not 1.5.

Telegdi: No very from the same group is here, I think, and we might catch him for one reply. Anyway, there are two numbers which we have produced. In fact, there are three, but one we had better not mention, because it was wrong. So, one number was \(-0.09 \pm 0.03\). This is in the preprints which people have. Since that preprint has gone out, the number which Goldhaber has written on the blackboard has been obtained, we have confidence in this, \(-0.11 \pm 0.02\). In the paper we made a comparison of this value with the systematics from \( \beta \)-decay where the so-called \( x^2 \)-ratio of Gamow-Teller to Fermi as has been extracted by Kofoed-Hansen, Winther and other people, who quote that this ratio is \(1.3 \pm 0.1\), i.e. with certain errors; that is the square of the ratio 1.3. Now,
assuming that this squared ratio 1.3 (which now according to this beautiful work from Russia is 1.55) was the correct ratio, one would have predicted that we should observe an effect of 0.07. Now, $-0.09 \pm 0.03$ is in agreement with 1.3. We have not used our own number to make any statements about whether this is compatible with that prediction. On the other hand, if we just take a little formula and insert the Russian value 1.55, it will come out 0.11 which is what we find. So we had to stretch our errors considerably to agree with the 1.3. But now we do not have any stretching to do, because the Russians pushed the value up.

Szalay: We developed a technique with Czikai by means of which the angular correlation in the decay of He$^6$ can be measured in an expansion cloud chamber at low pressure. We observe the recoil effect of the neutrino on the Li$^6$ residual nucleus.

The asymmetry of the angular correlations seems to point definitely to a tensor interaction in contradiction with the new results of Allen presented by the rapporteur. We carried out the evaluation of the photos with great care and criticism, but in the light of the other result we shall re-examine them again before making a final conclusion. Some systematic distortion might possibly come into the distribution, by the selection of the recoil tracks from the background.

Marshak: I just wanted to say that a lot of work has been done in $\beta$-decay on forbidden transitions. If you look back carefully at the arguments as to why so many of the first forbidden transitions show allowed shapes, it was always pointed out by Konopinski and others that the $V, A$ was as consistent with the data as $S, T$, so that there is no change in this situation.

Nakamura: It is not surprising to find that the conclusions derived from the new experiments concerning the parity business are not consistent with those derived from the old experiments such as the electron-neutrino angular correlation and the forbidden spectra. The new experiments concern essentially the internal structure of the elementary particles. In this connection I analysed the electron-neutrino angular correlation coefficients in terms of the twin neutrino theory that is the scalar, vector, tensor and axial vector. And the main point is the vector and the tensor interaction and the small part is the scalar and the axial vector interaction. I think that Feynman thought that old experiments not consistent with a $V-A$ combination were not right. I think that the problem is not settled in the situation.

Michel: What is the experimental situation on the reverse reactions; the neutrino capture or antineutrino capture?

Goldhaber: I have here a summary (kindly prepared by Oleksa) of a paper which Reines and Cowan are submitting to the September Geneva Conference. They find after re-evaluation of all efficiencies involved, especially for neutron detection in their large scintillation counter, that their new cross-section, which was previously too low by a factor of about three, is now in agreement with the two component neutrino theory. In fact they quote a cross-section of $(11 \pm 4) \times 10^{-44}$ cm$^2$ (assuming 6.1 $\beta$-rays/fission). But there has been considerable debate recently as to what is the expected cross-section for the actual pile neutrino spectrum, and there are three evaluations, one by Muehlhause and Oleksa which predicted $12 \times 10^{-44}$ cm$^2$; one by King and Perkins who calculated the expected cross-section using systematics of decay schemes in the region of the fission products and predicted in this way a cross-section of $15 \times 10^{-44}$ cm$^2$. And then Reines, Carter, Wyman and Wagner at Los Alamos have re-measured the $\beta$ spectrum and they quote a value in a somewhat different form but, if I work it out quickly, it corresponds to a prediction of $9.5 \times 10^{-44}$ cm$^2$. This may be wrong by half a unit. So, I feel that there is at present no discrepancy and one more cloud, which I suppose no one took too seriously because of the difficulty of the experiment, has now disappeared. At this point I should also like to answer a question that has been put to me in writing by Jensen; i.e. what is the status of the Davis experiment? May I do that? The Davis experiment as you may know is an attempt to see whether the pile neutrinos could also produce a reaction which is the one for which you would think you need not antiparticles but particles, namely the reaction

$$\text{Cl}^{37} + v \rightarrow \text{A}^{37} + e^- .$$

Now this reaction would need a neutrino (of the kind emitted by the sun) whereas the pile gives us by definition antineutrinos. It is an interesting question that has been discussed very much in the literature whether the pile neutrinos should make this reaction go or not. Now, this experiment is still in progress. Davis detects the argon-37 by washing it out of a very large carbon tetrachloride tank. The difficulty with the experiment is to know the background, because there is always some due to $\mu$ mesons from cosmic rays. Just before I left Brookhaven, Davis told me that if asked, I should state that there is, at present, no evidence for an effect; that he does not yet fully understand his background, and before he fully understands it, he will not know exactly how to substract it. Experiments are now in progress to measure this background. I think one can already safely say that the calculated background or, shall we say, estimated background for $\mu$ meson absorption underground, is of the same order as what he finds, and that if an effect exists it is certainly smaller than calculated for a neutrino which is the "correct" neutrino.

Segre: Since we want to know everything, what is the status of double $\beta$-decay?

Goldhaber: Well, there has been no new completed experiment to my knowledge during the last year or so. There are experiments in progress; there is a Russian one by Dobrokhotov, Lazarenko and Luk'yanov 36) in
which a Ca$^{48}$ (76.2% 423 mg) sample was placed between two scintillation counters, connected by a fast coincidence circuit. The two counters were surrounded by a large scintillator which served as an anticoincidence counter. It was shown that the half-life of the double $\beta$-decay in Ca$^{48}$ is $> 6 \times 10^{18}$ years. There is one experiment by ourselves (with der Mateosian), but the progress there is defined in a very academic way, namely we are waiting for the separated isotope Ca$^{48}$ which is supposed to be produced for us in large quantities, and if and when it arrives we believe we have a technique (involving growing a scintillating Ca crystal) which should push the limit far enough that we should be able to see the Dirac type of double $\beta$-decay which I think is the thing one should now aim for in the spirit of the present theory. Not that we might not, as a surprise, find the Majorana type of double $\beta$-decay, but if one can go far enough to see the Dirac type of double $\beta$-decay, then only will the question be stricken off the list of questions.

Cassels: I should just like to recall that the abolition of the tensor interaction has removed a difficulty with the radiative $\beta$-decay of the pion, that is

$$\pi \rightarrow e + \nu + \gamma.$$  

With tensor interaction that was supposed to be a few times $10^{-4}$ branching ratio compared to the usual decay. Experimentally this ratio is now down to a very few parts in a million depending on exactly which interaction you assume, and it was in close disagreement with the tensor interaction. With the new $V-A$ hypothesis you would expect this branching ratio to be $10^{-7}$ approximately, and the experiments are perfectly in agreement with this. There is large run on this experiment; we ran on the cyclotron for ten days and recorded one count which may have been a background event; we do not feel inclined to go looking for a $10^{-2}$ ratio.

Lyubinov: As it was shown by Alikhanov, Beristetski Geschkenborn, Rudik and myself there should be a certain polarization of the internally converted electron following the $\beta$-decay due to the polarization of the recoil nucleus. Using this idea and the results of theoretical calculations, Vishnietski and myself have shown experimentally that there exists a transversal polarization of the internally converted electron following the $\beta$-decay of Hg$^{200}$. It is necessary to stress that the value of this polarization depends on the same combination of constants as the angular correlation of polarized nuclei in $\beta$-decay.

Lipkin: I was wondering about the possible presence still of small quantities of the $S$ and $T$ interactions. Can anyone give upper limits on these on the basis of the present experimental data?

Telegdi: This is, of course, the difficulty of all these experiments, how to exclude out some small percentages of $S$ and $T$, because all experiments have errors. I am not sure that I am saying the right thing, but in the old times before we had primed constants, before we had parity violation, one of the most stringent tests, and I think the experimentally most reliable one, was that of $K$ capture to $\beta^+$-ratio and spectral shapes. I believe that the same tests as were done previously can still be used to rule out a small contribution if you are willing to assume the two-component theory and time reversal. Now the numbers on $\beta^+$ decay to capture ratio are available in the literature and they put limits of a couple of per cents at least in the Gamow - Teller cases. The situation is much less good in the case of Fermi as there are fewer decays available.

Schopper: I would just like to mention that we measured the circular polarization of internal Bremsstrahlung in the $K$ capture of $A^{37}$ and find 100% polarization of the whole spectrum. That means that even in $K$ capture parity breaks down completely within the experimental error of about 5%.

Feynman: Is there any new experimental information on decay of a $\mu$ into an electron and a $\gamma$-ray?

Goldhaber: I know of no new one. I know of experiments in progress only and we will have to wait for the next conference.

Marshak: A brief remark in connection with the STP. Telegdi essentially answered the ST part by saying that the parity breakdown experiments do not shed more light and that you simply go back to the old non-parity breakdown experiments to get the limits. In the case of the pseudo-scalar interaction and perhaps the best thing to do is to try to look at the 0—0 (yes) transitions for deviations from the $v/c$ law for the helicity of the electron as pointed out by Rose and others.

Parsey: I would like to point out that the answer given by Telegdi on the question of $S$ and $T$ versus $V$ and $A$ interactions is based on the assumption of the two-component theory with lepton conservation. Now Alder pointed out at the conference in Rehovoth last year that all the interference effects in spectrum shape and so on automatically vanish if parity effects are a maximum independently of any assumption about the two-component theory. The only way to determine the relative amount of tensor to axial vector that remains, if one accepts maximum amount of parity violation as an experimental fact, is through $\beta$-neutrino correlation or experiments that are equivalent to that. Now I want to ask if anyone has thought of analysing $\beta$-neutrino correlation results to put some limits on the amount of tensor that might be mixed with the axial vector.

Goldhaber: I have not made any particular analysis, but the errors given by Allen in his latest work 19 give limits of the order of 10 to 15% if you recall some of these correlation coefficients. It will depend on how much more one wants to push this because it is extremely hard to recognize such a deviation, if one sees one, as a real one, and not as an instrumental one, because some deviation from a particular correlation is always in the opposite direction and might show an apparent tensor interaction.
Mikaeljen: I would like to mention preliminary results which might turn out to be wrong, on measurement of longitudinal polarization of electrons emitted in some nuclei. We measured 5 nuclei and found that the longitudinal polarization was ranging from 92% for Samarium and the lowest value obtained for Indium 114 which is, I think, pure Gamow - Teller transition, is 0.75 v/c.

Tolhoek: I should like to say something about muon capture theory. Concerning Primakoff's unpublished theory I think that his calculation averages in such a way that shell structure effects would not show anyhow. What our calculations seem to show is that in case of pure Fermi interaction there should be shell structure effect especially in the f 7/2 shell. That is because the possibility of spin-flip or non-spin-flip makes a great deal of difference if f 7/2 → f 5/2 transition contributes much to the transition probability. Now for the Gamow - Teller interaction the result is that you get about the average formula because then the f 5/2 → f 3/2 as well as the f 7/2 → f 5/2 transitions are possible, hence only in case of pure Fermi interaction (or rather pure Fermi interaction) there should be a rather pronounced shell structure effect, deviating from the average formula. The experiments do not show the shell structure effects and what one can conclude is that there should be no pure Fermi interaction for the muon capture. I think one cannot conclude that it is Gamow - Teller interaction only because the results are not very sensitive to admixtures of Fermi interaction, so that, for example, an equal mixture would not be excluded. Certainly the same ratio of Fermi to Gamow - Teller as in β-interaction could not be excluded at the moment on the basis of the experiments and the preliminary theoretical calculations. Of course it is a pity that it is not easy to determine a small admixture of Fermi interaction because what one observes is a lack of shell structure effect and this might also arise because nuclear wave functions are more complicated than the one we have taken in our calculations (27) (which were mainly meant as a preliminary estimate of the magnitude of possible shell structure effects in order to show the interest of further work on this point).

I understand Ca 48 would be used for double β-decay and I might just mention that it could also be an interesting nucleus to measure muon capture for the muon capture. I think that is expensive but it may be very much worth while.

Peierls: I would like to point out that one must be very careful in drawing conclusions on this important question from shell model calculations. The difficulty is that there is a very large number of final states accessible. Now, of course, in the shell model approximation most of those do not occur but because of the interaction between the nucleons one would get small contributions in all of them. All these add up and so the answers may change appreciably. From that point of view the approach, through closure, is simpler, but it also requires a good knowledge of the wave function of the initial nucleus and there the same difficulty appears again because terms due to the couplings of the particles may make a lot of difference. I think this question can be settled, but a lot of work has to be done and until it has been done it is not easy to say which are the most promising nuclei on which to do the experiment.

Marshak: Just in connection with this I would like to point out that some people have been worrying (e.g. Fujii) about testing the universal interaction in connection with the capture of μ- by, say C 19, giving B 19 which then goes back to C 19 by means of electron decay. The point is that there is so much energy lost in this reaction that you do not have many states to worry about and that you can perhaps, by more careful study of such transitions, pin down the connection between the μ interaction and the electron interaction. I think that this is perhaps one of the most promising avenues in this particular field. An experiment along this line has been done by Godfrey from Princeton who obtained something like a 13% partial absorption rate (to the ground state of B 19) and when you do rough theoretical calculation you make estimates like 13% so that there is considerable room there for refinements of both experiment and theory.

Telegdi: Two comments about the Godfrey experiment. First of all the Godfrey experiment, as far as I recall, is based on 68 events. It makes it easy to quote the error and this experiment is now being repeated at Los Alamos in a very much improved version. The difficulty in Godfrey's original experiment, was that some of the captures may go to excited states which may be de-excited by γ emission. I personally think that if one knew how to calculate by some wave functions these rates, then this transition from C 19 up to B 19 and back might also perhaps be a check of the direct coupling to magnetic moments in the conserved vector current theory as suggested by Gell-Mann, because he, of course, loves transitions with a high energy. What has a higher energy than the neutrino that comes out of a μ capture? The effect goes like p² so it should be a nice case to test their theory.

Goldhaber: I was only going to make the point that if β-decay had been investigated with reactions that go to so many excited states we would still not be able to say much about the β-interaction and that in μ meson interactions we do not have the corresponding μ emission but only the K capture analogue. We simply must get data between well-defined states and among these nuclei is one, Li², of a well-defined spin I going to He⁶ of spin 0 which has no bound excited states which looks as if it ought to have a rate of 0.1% per μ absorbed, and which might be a conceivable nucleus which would teach us about the Gamow - Teller part of the μ meson interaction.

Telegdi: Do you want to use polarized muons for this experiment?

Goldhaber: I was not entering into the controversial question whether you could do more and study the direction of emission of the He⁶ recoils with polarized μ mesons.
and thus learn whether the helicity of the neutrino emitted there is the expected one, which I think may be possible, but the rate at least can be studied without any major difficulty.

Telegdi: All right, if that alone by itself would give you information.

Goldhaber: If I may, while I have the microphone, I had promised to report many things which I have forgotten, and there is one thing I can do in one line and that is a new $\eta$-value for the $\mu$ meson spectrum derived by Dudziak. His value is based on the old work he did with Sagane and gives a $\eta$-value of $0.736 \pm 0.022$. This is a new analysis of the old work and the UCRL report, of which I do not know the number, is available. I want to emphasize that the quoted error is a statistical one and that the data have not yet been analysed for systematic errors. A supplementary one will be issued.

Okun': I would like to make a remark which is connected with possible experiments on $\mu$ meson capture in hydrogen. As was proposed by Zel'dovich and was theoretically shown by Gershtein the $\mu$ mesons in hydrogen during their lifetime would be fully depolarized due to collisions of the $\mu$-mesic atoms with molecules and atoms of hydrogen, because in the process of these collisions $\mu$ mesons go to their lowest hyperfine structure state. So the experiments with polarized $\mu$ mesons in hydrogen are impossible for this reason. This theoretical prediction was proved in experiment by Ignatenko et al. 39) who studied the angular distribution of the electrons which appear in the decay of $\mu^-$ mesons in liquid hydrogen. The following values of the asymmetry coefficient $a_0$ for the integral spectrum have been obtained:

<table>
<thead>
<tr>
<th>Element</th>
<th>$a_0$</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\text{H}_2$</td>
<td>0.01 ± 0.01</td>
</tr>
<tr>
<td>C</td>
<td>0.04 ± 0.005</td>
</tr>
<tr>
<td>O ($\text{H}_2$)</td>
<td>0.043 ± 0.005</td>
</tr>
<tr>
<td>Mg</td>
<td>0.058 ± 0.008</td>
</tr>
<tr>
<td>S</td>
<td>0.042 ± 0.006</td>
</tr>
<tr>
<td>Zn</td>
<td>0.056 ± 0.011</td>
</tr>
<tr>
<td>Cd</td>
<td>0.055 ± 0.012</td>
</tr>
<tr>
<td>Pb</td>
<td>0.054 ± 0.013</td>
</tr>
</tbody>
</table>

Telegdi: Sorry to take your time! I do not believe that this argument about collisions transferring in the case of protons, or rather in molecular liquid hydrogen which one has to use, $\mu$ mesons of lowest hyperfine state is any way conclusive, as in very beautiful measurements from Russia recently starting with hydrogen (to be in liquid form) and going all the way up through the periodic table to lead and in all the cases the asymmetry is very low like it was found in carbon by Garwin, Ledermann, Weinrich and others. The elements in which one finds an asymmetry that is measurable are invariably spin 0 nuclei without magnetic moment, and if you have got a substance that is not hydrogen where these molecular collisions cannot occur, say a solid block of sulphur, you get the depolarization to the small value and it comes about by the spin-orbit coupling by the precession of the $\mu$ meson magnetic moment around relativistic magnetic fields that comes out of the electric field of the nucleus. In elements with nuclear magnetic moments there is further depolarization by h.f.s. So if hydrogen is one of the series of elements where this occurs and other elements which do not have molecular effects also depolarize in an understandable way, I do not see where the proof lies that hydrogen really does what one has theoretically predicted.

Überall: I would like to comment further on the neutron asymmetry from $\mu$ meson capture. Any actual experiments have not been done on the subject but I hear that an experiment is planned and in progress in Liverpool. As to the theory of the neutron asymmetry, it has been worked out for capture in hydrogen 40) in heavy nuclei using a Fermi gas 41) and in deuterium by Wolfenstein and myself. The interesting thing is that if one has exactly $V-A$ interaction with equal coupling constants, the asymmetry vanishes in hydrogen and also in heavy nuclei if one treats them by a Fermi gas. It is only non-vanishing if one has $\mu$ meson capture in deuterium. However, the experiments for capture in hydrogen and deuterium are probably very difficult.

Cassels: I would just like to remark that the experiments are even more difficult because capture of $\mu$ meson in hydrogen and deuterium gets all messed up with fusion reactions.
WEAK INTERACTIONS: LEPTONIC MODES — Theoretical I

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Let us first write the list of known leptonic decays, i.e., decays producing a neutrino \( \nu \) associated with an \( e^\pm \) or a \( \mu^\pm \):

\[
\begin{align*}
n \rightarrow p + e^- + \nu & \quad \text{(1)} \\
\mu \rightarrow e + \nu + \nu & \quad \text{(2)} \\
\mu^- + p \rightarrow n + \nu & \quad \text{(3)} \\
\pi \rightarrow \mu + \nu & \quad \text{(4)}
\end{align*}
\]

and the three modes of \( K \) decay

\[
\begin{align*}
K \rightarrow \mu + \nu & \quad \text{(5)} \\
K \rightarrow \mu + \nu + \pi^0 & \quad \text{(5')}
\end{align*}
\]

(the last two will be studied in the next session).

Let us briefly summarize the history. Fermi’s theory, formulated 25 years ago and still successful, explained (1). It also explained well the processes (2) and (3) when they were discovered 10 years ago. The strengths of the Fermi interactions in (1), (2) and (3) are remarkably alike; hence, the notion of a universal Fermi interaction. The only drawback was that the symmetry in (1), (2) and (3) between \( u \) and \( e \) was destroyed by (4): indeed the measured branching ratio \( \frac{\text{(n} \rightarrow \text{e} + \nu)}{\text{(n} \rightarrow \text{u} + \nu)} \) is astonishingly small.

The non-conservation of parity proposed by Lee and Yang at the beginning of 1957 is experimentally established. This discovery started a tremendous number of experiments on (1) and (2) (less on (3)) which have just been reported by Goldhaber and Telegdi. This has allowed some progress to be made on the theoretical views concerning these leptonic decays; it is my duty to report on them now.

The main topic I have chosen for this report is the universal Fermi interaction (U.F.I.). However, in a first part of this report I shall make a brief survey of the problems most often heard of in the leptonic decays.

Survey of Problems

Nature of the neutrino. In his report on the experimental data, Goldhaber has shown how one has been led to change the nature of the Fermi coupling in \( \beta \) radio-activity since the last Rochester Conference. The new choice

\[ a \text{ mixture of } V \text{ and } A \]

does not affect the possibility of explaining (2) and (3) by the same coupling.

What is very striking, from the presented data on asymmetry and polarization effects, is the remarkable kindness of Nature to the experimental physicists: all these effects have their maximum possible value. To be more precise and technical, it seems that

\[ \text{the violation of } C \text{ and } P \text{ is maximum.} \]

Just before the discovery of non-conservation of parity and after seven years of controversy, experts on double \( \beta \)-decay experiments had agreed on the existence of both neutrinos and anti-neutrinos. A far-reaching explanation of statement (7) on the nature of neutrinos (described by a two-component theory) has been proposed by Salam, Landau, Lee and Yang \( ^1 \).

Parity \( P \) and charge conjugation \( C \) are completely violated in leptonic decays because there are states in nature which have no charge conjugate or space symmetric corresponding states. Such states are those of the neutrino. Experiments have shown that neutrinos (i.e., those emitted in \( \beta^- \)-decay or \( K \) capture) are left-circularly polarized. Then the two-component neutrino theory says: all neutrinos are left-circularly polarized, all antineutrinos are right-circularly polarized. It has now become fashionable to replace “circular polarization” by one word: “helicity”.

Not only does this aesthetic theory explain the nuclear \( \beta \) radio-activity data well, but its prediction for \( \mu \) meson decay is precise and successful. Indeed, due to the emission of two neutrinos, this theory leads to only two possible values for the parameter \( q \) fixing the shape of the energy spectrum of the electrons from \( \mu \) meson decay at rest, through Fermi coupling:

\[
\begin{align*}
\text{either } \mu^\pm & \rightarrow e^\pm + 2\nu \text{ or } e^\pm + 2\bar{\nu} \quad \text{then } q = 0, \\
\text{or } \mu^\pm & \rightarrow e^\pm + \nu + \bar{\nu} \quad \text{then } q = \frac{1}{2}.
\end{align*}
\]

If we had statement (6), then \( q = \frac{1}{2} \) is the unique prediction. As we heard, all the published experimental \( q \) values are compatible with \( \frac{1}{2} \). However, there are \( q \) values to be published which may not agree with this statement. Of course, it is possible to fit smaller values by introducing arbitrary constants (e.g., couplings with derivative); and this can be given a deeper meaning. But must we really hurry to do that?
The two-component neutrino theory, \( V \) and \( A \) coupling, also makes a unique prediction for the energy dependence of the asymmetry of electron ejection from the decay of polarized \( \mu \) mesons. It agrees with the not yet very precise experimental data.

It is generally less difficult to disprove a theory than to prove it. So the successful two-component neutrino theory may be disproved by some of the many experiments still to be done in nuclear \( \beta \)-decay or \( \mu \) meson decay: energy dependence of the asymmetry of the electron and its polarization (see Goldhaber's report, and also several theoretical calculations \(^5\)). However, it seems proper to emphasize that the comparison of \( \beta \)-decay and its inverse reaction \( \bar{v} + p^+ \rightarrow n + e^+ \) (observed by Reines and Cowan) is a direct measurement of the number of polarization states of the neutrino. We heard that the measured rate agrees with the two-component neutrino theory and hope that this difficult experiment may be done again with the necessary precision.

But the elegant two-component neutrino theory does not give a direct explanation of the observed non-conservation of parity in the non-leptonic decay of \( K \) mesons and hyperons. This is an essential limitation and is a serious indication that something still more revolutionary than the two-component neutrino theory has to be introduced. We will come back to this point, and refer to it in the next session.

At the last conference, several questions were raised on the nature and the number of the different kinds of protons and pions, on cosmogony problems and so on, but with the hope that they would not be the only questions dealt with at the present conference. I am unable to talk on these subjects and I have not received any corresponding contribution; I suppose they would have been sent to the proposed session on fundamental ideas!

**Conservation laws of weak couplings.** I do not know how hard physicists have tried to check the conservation of energy, momentum and angular momentum, and electric charge, in weak couplings. Experimentalists have not yet ruined those laws. They have given a figure for the accuracy of the check of the nucleonic charge conservation \(^3\).

Experiments on \( T \) (time reversal) symmetry have been presented. The conclusion is that the CTP theorem also seems valid for weak couplings. An opposite conclusion would have required the rejection of non-local field theory, and thrown us into new and unknown ventures. The experimental validity of the CTP symmetry will incline physicists to be rather conservative with present field theories.

A problem which arose as soon as a U.F.I. was proposed, was to explain how to single out the sets of four fermions among which this interaction occurs, and how to exclude the other sets. This may be partly attained by the existence, beside nucleonic and electric charge conservation, of a conservation law specific to leptonic decays, and called, a few years ago \(^4\):

*lepton conservation* (by Konopinski and Mahmoud) or *neutrino charge conservation* (by Zel'dovich).

The attribution of this leptonic charge to \( \mu, e, \nu \) is arbitrary. Experimental facts and two-component neutrino theory seem to be entirely consistent with this conservation law, where \( \mu^- + e^- \) and the \( \nu \) emitted in \( \beta^- \)-decay are leptons. However, I do not think that the claim of experimental proof of this conservation law is justified. It seems to me that the measurement of the sign of the polarization of \( \mu \) meson decay is absolutely necessary. It will either spectacularly confirm the consistency of the whole scheme, or it will completely ruin it.

Since leptons have no strong couplings, it is not clear whether it is meaningful to give to them quantum numbers specific to strong couplings (isospin, strangeness, etc.). This does not mean that more symmetry cannot be given to weak couplings (e.g. the attempt of d'Espagnat and Prentki), but is it really useful in the present situation? The strong interaction quantum number, which may be of some use for weak couplings, is well known \(^5\) (although recently rediscovered). It is the product of charge conjugation \( C \) and charge symmetry \( S \) (exchange all neutrons \( \leftrightarrow \) all protons). Since \( (CS)^2 = 1 \), the \( CS \) quantum number can be considered as a \( \pm \) parity. It allows a classification of the couplings between a pair baryon-antibaryon and a pair of leptons, since for the former \( CS \) is a good quantum number. For example, in (1) the pair \( n \bar{n} \) yields

\[ C(n, \bar{n}) = (\bar{n}, n) \]

and

\[ SC(n, \bar{n}) = S(\bar{n}, p) = (p, n) = CS(n, \bar{p}). \]

Weinberg \(^6\), in a contributed paper, has studied the present situation for weak interaction, under this quantum number. He has some fine points concerning the \( \Sigma \)'s (due to the exceptionally large number — 6 — of \( \Sigma \) states). The main point of his paper, it seems to me, is to suggest where to look for experimental data which could not be explained by the Fermi types of couplings. But I must recall that this list may be inconclusive if one is not able to compute completely the electromagnetic effects, which spoil the \( S \) invariance.

I want also to recall that more specific models for all known particles (e.g. the Okun' or Sakata models) can impose definite selection rules on weak interactions. This may be an important topic of next year's conference, but for this year it belongs to another session.

The branching ratio of \( \pi \rightarrow e + \nu \) or \( e + \nu + \gamma \) as compared to \( \pi \rightarrow \mu + \nu \).

This puzzle has already been the subject of Feynman's lively report given this morning. As is known essentially from the Treiman and Wyld analysis \(^6\), and has been recalled again in the discussion by Cassels, and also in a calculation contributed by Yaks and Ioffe \(^6\) to this conference, the shift from \( S-T \) to \( V-A \) for the Fermi
interaction makes \( \pi \to \) virtual nucleon pair \( \to e + \nu + \gamma \) reasonably slow (branching ratio \( 5 \cdot 10^{-4} \) claimed by Vaks and Ioffe). However, the small experimental branching ratio \( \pi \to e + \nu \) against \( \pi \to \mu + \nu \) is not explained, except by ad hoc hypothesis explored by physicists (e.g. Huang and Low) but which most of them are reluctant to believe. As Feynman recalled, the source of the enigma may be the nature of the \( \mu \) meson and of its mass. The situation is similar for the \( K^+ \) meson. The most frequent mode of decay is \( \mu^+ + \nu \); the mode \( e^+ + \nu \) has never been reported.

**Intermediate boson theory.** In order not to be completely unfair, I have a few words to say about this before going on with more details of U.F.I. Indeed, there have been many papers following the Yukawa idea that decay occurs via an intermediate charged boson (at that time responsible for nucleon forces). It must be emphasized that until the discovery of (2) \( e + \nu \) this theory was as successful as the Fermi theory and in many ways more satisfying. Many Japanese physicists have systematically studied the possibilities of the intermediate boson (or of theories with several intermediate bosons). It is well known that the higher the intermediate boson mass, the more the indirect interaction simulates a Fermi interaction. Two papers \(^7\) (one by Byers and Peierls, one by Feinberg) have been presented on this subject. They emphasized how the intermediate boson theory can reproduce practically all the finer points of the last version we shall present of the Fermi interaction, with perhaps two more advantages: it explains perhaps more naturally some selection rules for leptonic decays and yields a \( g \) value slightly smaller than \( \frac{1}{2} \). But the intermediate boson theory is no more successful than the U.F.I. for the \( \pi \) decay (or \( K \) decay) puzzle, and it has inherent difficulties not shared by the U.F.I., giving for instance, too large a branching ratio for \( \mu \to e + \nu \), against \( \mu \to e + \nu + \gamma \). This is explained by Feynman diagrams of which the following figure is an example.

**Universal Fermi Interaction**

The nature of coupling terms. When the coupling was believed to be \( S, T \), a number of explanations of this choice were given. There are as many now which single out \( V \) and \( A \) couplings. Algebraically they all use the fact that \( \gamma^\mu \) and \( \gamma^5 \) anticommute (here we shall use \( (\gamma \psi)^2 = 1 \)).

But there is surely something in their physical interpretations which was not in the old proposals. The latter were mathematically right, but physically arbitrary. They were just aesthetical guesses. There are essentially two main physical interpretations for the choice of \( V \) and \( A \) and they are probably only two different ways of saying the same thing. Indeed, both interpretations try to extend in some way the properties of the two-component theory of the massless neutrino to the other particles interacting through the Fermi coupling. This is suggested in order to explain the non-conservation of parity in decays which do not involve neutrinos. A tentative scheme, I believe first suggested by Gell-Mann at the Pisa Conference, is to transform the Puppi triangle of Fermi interaction between pairs \((np), (ev), (pv)\) by adding \((Ap)\) and perhaps \((\Sigma N)\) pairs, although this may have consequences in contradiction with experiments if we connect all pairs by the same Fermi interaction. The theoretical branching ratio \( A^0 \to p^+ + e^- + \bar{\nu} \) against \( A^0 \to p^+ + \pi^- \) is then \( 1.5\% \) and the \( \beta \)-decay of \( A^0 \) has not yet been observed.

The two types of proposed physical explanation of a \( V \) and \( A \) universal Fermi coupling are:

1) The formalism of mass zero neutrino theory is invariant under a new type of gauge transformation: \( e^{i \psi \gamma_5} (\beta \) is an arbitrary c-number). The extension of this to all spin \( \frac{1}{2} \) particles is called mass reversal invariance and it gives the desired U.F.I. (Tiomno and Sakurai \(^9\)).

2) The circularly polarized states of the two component neutrino theory are given by the projectors \( a_\alpha = \frac{1}{2} (1 \pm \gamma_5 \psi) \).

In a reasonable way, circular polarization of zero mass spin \( \frac{1}{2} \) particles can be considered as a limiting case of longitudinal polarization of particles with mass. Both concepts are often given the name of " helicity ", though the former is relativistic invariant and the latter is not. What is evident, is that the (useful or useless) covariant generalization of helicity for spin \( \frac{1}{2} \) mass zero particles, can be done — and only done — by using the same projection operators \( a^\pm \) (invariant for the connected Lorentz group). This concept has been called (up to a sign) chirality and it is used under this name by Sudarshan and Marshak \(^9\). If you couple only the same chirality parts in the chosen sets of four fermions you get the desired \( V \) and \( A \) universal Fermi interaction.

The use of the same projectors in the case of spin \( \frac{1}{2} \) particles with non-vanishing mass leads to an interesting formulation of this theory (due to Kramers, in his book on Quantum Mechanics). One uses instead of Dirac \( \psi \) and \( \bar{\psi} \) with four components the two-component \( \chi = a \cdot \psi \),
and \( \overline{Z} = \overline{\nu} a \). This is the Feynman—Gell-Mann proposal for all fermions and the most general Fermi interaction in this new version, i.e.

\[
\overline{Z} = \sum_{i=1}^{5} (g_{1} \overline{X}_{i} O_{i} X_{2} - g_{2} O_{i} X_{1} + \text{hermitian conjugate})
\]

is just the desired one. There must be something in that! However, let us presume that none of the outlined attempts gives the ratio \( g_{A} / g_{V} \). It seems tempting to use only one coupling constant \( g_{V} = -g_{A} \). This gives a good value for the ratio of strength of Fermi interaction in \( \mu \) decay and (pure Fermi transition) in \( \text{O}^{14} \beta^{-}\)-decay. Yet the \( \beta^{-}\)-decay ratio \( g_{V} / g_{A} = -0.8 \), as we heard from Goldhaber. Could this be explained by renormalization?

To summarize, this is certainly an important step for understanding the \( V-A \) nature of the Fermi interaction. But this is the "bare" coupling. The observed effects will include radiative corrections due to the stronger couplings: nuclear and electromagnetic. Electromagnetic radiative corrections are easier to compute; they are the only ones appearing in \( \mu \) decay. According to a not yet published work of Berman, previous computations are erroneous. Berman obtains larger effects than in the previously published computations on the modification of the decay rate, and on the shape of the electron spectrum; this new computation has to be used in the analysis of all the experimental work. In terms of the \( g \) parameter (defined from a calculation without radiative corrections) it will slightly increase the "measured" value. But I have no quantitative details. Of course the electromagnetic radiative corrections are also present in nuclear \( \beta^{-}\)-decay and \( \mu \) meson capture. However, it seemed useless to study them thoroughly since nuclear radiative corrections were expected to be more important. We shall see that it might not be the case for the \( V \) coupling. Also, another Berman conclusion is that for the neutron decay, radiative corrections are somewhat larger than most physicists thought. The other main problem is the effect of nuclear forces. How do they "dress" the Fermi coupling? What is the "box" which replaces the vertex of the Fermi coupling?

For the \( V \) coupling, a very attractive answer was proposed some time ago by Gershteijn and Zel'dovitch in analogy with electrodynamics. It has been proposed again by Feynman and Gell-Mann (see the following report) and a submitted paper by Ioffe proves their claims. The starting remark is that the universality of the coupling strength \( e \) (electric charge of elementary particles) is not affected by renormalization, since the current density \( j_{\mu} \) (which includes all electrically charged particles) is conserved: \( \partial_{\mu} j^{\mu} = 0 \). So, for instance, the electric charge of a nucleus with \( Z \) protons is \( Z e \) since \( e \) is not affected by nuclear forces. But nuclei have magnetic moments, electric quadrupole moments, and so on. The corresponding \( \beta^{-}\)-interaction effects are as difficult to predict by computation as these nuclear moments are. However, from the isospin point of view, one passes from the electric current to the total strangeness conserving \( V \) current of the \( \beta^{-}\)-interaction by substituting the \( Z \) isospin component to the \( x \pm iy \) component. Thus it is possible to relate a \( Z, N \rightarrow Z + 1, N - 1 \) or \( Z - 1, N + 1 \beta^{-}\)-transition, to the \( \gamma (Z, N \rightarrow Z, N) \) transition between the corresponding states. Gell-Mann has recently studied some experimental tests for these new views and he will present them himself.

In their papers, Goldberger and Treiman, and also Blin-Stoyle, prove that it is not possible to use the same idea for the \( A \) part of the coupling (except, perhaps, if a new ad hoc meson is introduced to complete the conservation of \( A \) current?). Therefore the \( A \) coupling has to be renormalized by the nuclear (and also electromagnetic) interaction. The \( A \) vertex is to be replaced by a box, and Goldberger and Treiman have explored this box, using advanced techniques of dispersion relations. Their results are presented in two papers. In one they relate an absolute theoretical determination of the \( \pi \rightarrow \mu + \nu \) decay (via virtual nucleon pair) to an experimental measurement on nucleon anti-nucleon scattering. In the other, they can use their knowledge of the "box" (momentum dependent terms, terms simulating a \( P \) Fermi coupling) for some predictions on a detailed experimental comparison of \( \beta^{-}\)-decay and \( \mu \) capture. Goldberger will report to you on these papers.

To conclude, there has recently been some important progress: the two-component neutrino theory; a new property of all spin \( \frac{1}{2} \) particles, related to the \( \gamma_{5} \) matrix; and a more powerful attack on the problem of the nature of the "dressed" Fermi couplings. But I want to call your attention to the fact that the \( \pi \rightarrow e + \nu \) puzzle is still to be solved, and that the \( \mu \) is a strange lepton; this must excite all theorists.

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Ioffe, B. L.  On the proof of absence of renormalization of the constant in the vector variant of $\beta$-interaction. (to be published in Nuov. Cim.)

10. Blin-Stoylov, R. J.  (to be published)
Goldberger, M. L. and Treiman, S. B.  Form factors in $\beta$-decay and $\mu$ capture. (to be published in Phys. Rev.)

DISCUSSION — see p. 257.
This is not really a contribution to this Conference but a suggestion that will appear in print within a couple of weeks; however, I have been asked to talk about this matter for just a few minutes. It starts from the idea already mentioned, and given first by Gershtejn and Zel'dovitch as a speculation years ago when the V-interaction was unpopular. The idea is that one should try to form in the weak vector part of the interaction, which is not strangeness changing, a quantity coupled to leptons which should be conserved like the electromagnetic density. Of course, this is possible only if we ignore electromagnetic corrections. The usual term to which we refer is \( (\bar{p}n) \), which represents, say, conversion of a neutron to a proton (with of course the emission of leptons). Now to this term we add more terms, in particular

\[
\sqrt{2} \left( \frac{\partial \pi^0}{\partial x_\mu} - \frac{\partial \pi^+}{\partial x_\mu} \right),
\]

(1)

and so on. Then we get an expression of the type

\[
\mathcal{J}^{(\mu)} = (\bar{p}n) + \sqrt{2} \left( \frac{\partial \pi^0}{\partial x_\mu} - \frac{\partial \pi^+}{\partial x_\mu} \right) + \ldots
\]

(2)

This is the conserved quantity as long as the isotopic spin is conserved. In other words, for all three components we have

\[
\frac{\partial \mathcal{J}_\mu}{\partial x_\mu} = 0.
\]

The z-component represents simply charge and strangeness conservation. So, by adding a specific coupling of the pions to leptons, we have a new conservation law by which we are assured that coupling constant in the V part of the \( \beta \)-decay is not modified by renormalization effects. This is similar to the situation we have in the electromagnetic case in which the electric charge is not modified by renormalization effects due to the strong coupling between the nucleon and pion. The main object of this work was to find some experiment to test the preceding hypothesis. The most elementary prediction of this hypothesis is, of course, the one we have already noticed in the published paper; that is, the vector coupling constant observed in \( \beta \)-decay, where strong interactions are present, is just about the same to within the 2% as the coupling constant in \( \mu \)-decay where there are no strong interactions. But we would like to find some other experiments to test the hypothesis, and possibly one where we can make an exact prediction on the basis of the theory and be reasonably sure that the prediction is different from what we would obtain with the old-fashioned theory in which the term (1) is not present.

As already mentioned by Michel, the nucleus with Z protons has an electric charge \( Ze \), and correspondingly in our theory the nucleus has an exactly predictable vector matrix element for perfectly allowed \( 0 \rightarrow 0 \) Fermi transition (e.g. \( O^{14} \)). This is no longer true in the old theory. The difference between these two predictions is not very great so that it is difficult to detect their difference experimentally. Therefore, we have to find a more suitable case to test our new theory. Such a case is given by the \( \beta \)-transition whose vector matrix element is one component of isotopic spin current. Then one can construct, through a charge independence of nuclear forces, exact parallel between electric transition matrix element and vector matrix element. For instance, the particular case that I will consider in a moment is the following transition:

\[
\gamma \text{-transition in } C^{12} \text{ and }
\]

\[
\beta^- \text{-transition from } N^{12} \text{ and corresponding } \beta^- \text{-transition from } B^{12}, \text{ where these three levels are an isotopic multiplet with } I = 1. \text{ The main contribution to these } \beta^-\text{-transitions are, as is well known, G. T. allowed matrix element coming from axial vector interaction. There is still a further small contribution from the vector interaction which is usually referred to as the second forbidden terms. Since the}
lifetime of $\gamma$-transition in $C^{14}$ has been measured $^3$ one can calculate exactly this forbidden vector matrix element. We also note that what is $V-A$ for $e^-$ is $V+A$ for $e^+$, so that one finally finds:

$(\beta^2$-transition matrix element) 

= (allowed G. T. term, coming from $A$ interaction) 

+ (other small contributions from $A$ interaction) 

± (forbidden matrix elements from $V$ interaction).

It should be noted that there arises a spectral anomaly due to the interference between actual vector matrix element and small forbidden vector matrix element. Such anomaly in the electron energy distribution will be a most easy thing to detect. The predicted energy spectrum would be:

$[I \pm aE + ($other small corrections due to the interference between the allowed G. T. and forbidden matrix elements coming from $A$ interaction$)] \times ($usual allowed spectrum$).

Where $a$ is a constant, $a \approx 2/M$ ($M$ = nucleon mass) (as for complete expression for $a$ see reference $^3$). "$aE$" represents the interference effect just mentioned. From the measurement of $\beta^2$ energy spectra in both $N^{14}$ and $B^{12}$, one can pick up the term $(aE)$ and one can test our new theory.

**LIST OF REFERENCES**


**DISCUSSION — Michel and Gell-Mann**

**Oppenheimer:** I had one point to make after Michel's talk and I may have one after Gell-Mann's. I am also not happy with the intermediate bosons, but there is a rule in $\beta$-decay which we all take for granted and which is not embodied in the rules Michel gave. It is the one, for instance, which forbids the muon going over into three electrons, a decay scheme which satisfies all conservation laws. In general, this rule is formulated by Marshak and Sudarshan, and Gell-Mann and Feynman, as requiring that one builds up the four fermion fields in two brackets of two each, and that the charge changes in each bracket. This is a most peculiar sort of rule; and although a charged vector boson is a horrible object, it would at least serve the purpose of explaining that rule. I think one must not forget this rule because, when one says universal, one not only means not $\pi \rightarrow e + \nu$ for reasons not fully understood and not $\Sigma$ and $A$ for reasons not fully understood. These may have to do with renormalization effects; but one also very deeply means that these charge changes are built in.

Okun' said this morning that in his model in which heavy particles are all built up out of lambdas, protons and neutrons, the conserved vector interaction was inevitable, because, if the proton decays in a pion, that pion itself has a proton in it, and if it decays into an antihyperon pair they are also built out of these universal particles. I think, quite generally, that such a conserved current is a natural consequence, though not an inevitable one, of any view that there is a minimum number of three heavy fields. Thus, if Gell-Mann's terms are not found, it may give the notion that there are only three elementary fields a little harder to maintain, though I will not say impossible.

**Marshall:** I think it is perhaps worth while trying to spell out one point about chirality invariance. Some of us have been living with this business for a while and maybe it is worth emphasizing some points for others who have not. Okun's presentation this morning, which made the first serious attempt to use this chirality invariance for progress with the strong interactions, was based very importantly on the idea that you insist on chirality invariance of strong interactions for each separate field. I emphasize this because Pauli pointed out to me the other day that Stech and Jensen, in a paper a couple of years ago, looked at what we now call chirality invariance of the four fermion interaction for two fields simultaneously. Of course, at that time they wanted to preserve parity and that was the natural thing to do, and they did not arrive at the unique choice of $V-A$. The same thing happened when people looked at the problem from the point of view of mass reversal invariance which is, of course, completely equivalent to chirality invariance. In the old days, Tiomno considered mass reversal for several fields simultaneously, and he arrived at a small number of possible interactions including some others besides $V-A$. Then Sakurai, taking the lead from the hypothesis of chirality invariance for each field separately, insisted on mass reversal for each field separately and in this way he got $V-A$. I emphasize this because the fact that the $V-A$ theory is so successful may mean that there is really something deep about chirality invariance...
for each separate field and maybe some other people will have more useful ideas than either Okun' or we have had in connection with a generalization of the concept.

**Treiman:** Just one slight word of caution, it is not $V-A$, but $V-1.25A$.

**Marshak:** It is $V-A$ for the lepton currents. The strong interactions renormalize the $V$ and the $A$ for the baryon strangeness-conserving current and the baryon strangeness-non-conserving current. It turns out that, according to the ideas of Gershtein and Zel'dovitch, and Feynman and Gell-Mann, the $V$ part is not renormalized. If we use the latest value of the neutron lifetime, $1.25$ is the renormalization factor for the $A$. I might point out that in connection with the strangeness-non-conserving interactions, the large asymmetry from the $\beta$-decay also indicates that the renormalization effects for the $V$ and $A$ are comparable.

**Goldhaber:** I would like to ask Gell-Mann whether he has considered the polarization of the $\beta$-rays in these $B^{13}$, $N^{12}$ cases, whether this will be any criterion for these extra currents.

**Gell-Mann:** I have none, I do not see at the moment any effect but I shall certainly look at it. Is it not true that it is usually $\nu/e$ unless there are Coulomb effects of some kind?

**Goldhaber:** In forbidden transitions one can find deviations, and they have been theoretically investigated by the Russian group and also at Brookhaven by Bincer, Church and Weneser.

**d'Espagnat:** I would like to make two small remarks. First of all, at the beginning of his talk Michel made the point that quantum numbers of strong interactions should presumably not be used in weak interaction, especially for leptons which have no strong interactions, and in this form this is most probably quite correct. Indeed, in the work that Michel mentioned, Prentki and I insisted that isotopic spin and strangeness should have no meaning at all for leptons. However, the baryons and mesons that take place in weak interactions are, after all, the same as those that take place in the strong interactions so that — at least it seems to me — what we would like is a representation of baryons, mesons and electrons by means of which both strong and weak interactions should look nice and symmetric. The $\chi$ type of representation I mentioned yesterday could be an example of such a thing. In that case, it would be conceivable that the quantum numbers effective in weak and strong interactions, without being at all the same, should bear some kind of connection with each other. Then, of course, in many phenomena the quantum number of weak interaction would be blurred out by strong interactions. But this we expect.

My second remark is that as far as $\pi\rightarrow e + \nu$ absence is concerned I have a feeling that this conference is perhaps somewhat biased in favour of the universal Fermi coupling in the usual sense. It is true that in this hypothesis a difficulty occurs but, after all, some aspects of this hypothesis are based just on aesthetics. Now, other hypotheses are certainly conceivable which, from this point of view, look just as nice and which do not lead to this difficulty (they, for instance, allow for some cancellation between intermediate nucleon pairs and intermediate hyperon pairs in $\pi-e$ decay). It is not very difficult to construct such aesthetical schemes, and with some imagination one can certainly construct many of them, so that perhaps one should not put too much emphasis on this particular difficulty that we seem to have with the Fermi universal interaction in the usual sense.

**Wentzel:** I should like to point out that according to Gell-Mann’s theory, $\pi^+\rightarrow e^+ + \nu$ and I would guess it has a chance of about one in a million to do this. So, for the experimentalists who look into the $\pi\rightarrow e + \nu$ decay, they might just as well also look at this type of decay.

**Salam:** I think this is stated in your paper.

**Gell-Mann:** Yes.

**Cassels:** Somebody has calculated this and it is $10^{-8}$, which is a completely hopeless thing to look for.
The work I am going to describe was carried out in collaboration with Treiman. Since it will shortly appear in Physical Review I shall not go into much detail. The problem we have studied is the normal decay of the \( \pi \) meson \( \pi \to \mu + \nu \). The question is whether the lifetime can be accounted for by ordinary \( \beta \)-interactions and pion-nucleon interactions. In lowest order of perturbation theory one considers the process

\[
\begin{align*}
\pi & \to \mu + \nu \\
\text{If the } \beta \text{-coupling is taken to be axial vector, one finds a logarithmically divergent answer. If the log is replaced by unity, one finds for the rate an expression about 60 times larger than experiment. This has generally been interpreted to mean that everything is essentially all right and that a better calculation would give a reasonable convergent value. I might add that if the } \beta \text{-interaction is pseudoscalar, one finds a quadratically divergent answer.}
\end{align*}
\]

We have attempted a quantitative calculation using dispersion methods which I will now describe. This is an example of an attempt to use dispersion relations as a dynamical tool. We start with a basic Lagrangian for the weak interaction

\[
\mathcal{L} = f_A (\bar{\psi} \gamma_\mu \gamma_2 \gamma_5 \psi) N [\bar{\psi} \gamma_\mu \gamma_2 (1 + \gamma_5) \psi] N_l + f_V (\bar{\psi} \gamma_2 \gamma_5 \psi) N [\bar{\psi} \gamma_2 (1 + \gamma_5) \psi] N_l
\]

Only the axial vector term plays any role in \( \pi \) decay. The relevant \( S \)-matrix element (to lowest order in the weak interaction) is clearly

\[
\langle o | P_A | \pi \rangle = i \langle o | P_A | \pi \rangle \bar{u}(p_\mu) i \gamma_\mu \gamma_5 (1 + \gamma_5) u(p_\nu)
\]

The quantity of interest is clearly

\[
\langle o | P_A | \pi \rangle = \langle o | f_A (\bar{\psi} \gamma_\mu \gamma_2 \gamma_5 \psi) N | \pi \rangle
\]

which must have the general structure

\[
\langle o | P_A | \pi \rangle = -i (p_\mu) \frac{F(p_\mu^2)}{(2p_\sigma \omega)^1}\]

where of course \( F (-\mu^2) \) is the quantity of experimental interest.

It is trivial to prove that \( F(p_\mu^2) \) satisfies a dispersion relation

\[
F(p_\mu^2) = \frac{1}{\pi} \int d\sigma \frac{\text{Im} F(-\sigma)}{\sigma^2 + p_\nu^2 - i\epsilon}.
\]

Clearly since we are calculating a number we can tolerate no constants. A discussion of \( \text{Im} F(-\sigma) \) may be given and one would be led to consider processes like

\[
\langle o | P_A | 3\pi \rangle \langle 3\pi | j | o \rangle + \cdots
\]

These are uncomputable and we assume hopefully that perhaps the leptonic link \( \langle o | j | 3\pi \rangle \) might be small. We then consider \( \langle o | P_A | N\bar{N} \rangle \langle N\bar{N} | j | o \rangle \). The first factor is the pion vertex discussed this morning, whereas the second corresponds to \( N + \bar{N} \to \mu + \nu \). We neglect other baryons for two reasons. There is no strong evidence for baryon \( \beta \)-decay and also we want to stay close to the old model of perturbation theory. That should not affect orders of magnitude.

Now the strong vertex \( \langle o | P_A | \bar{N}\bar{N} \rangle \) must be discussed. This, too, we do by dispersion theory. I shall indicate graphically what is taken into account

\[
\begin{align*}
\text{The first intermediate state is again proportional to the pion lifetime and the second involves the } 1S_0 \text{ nucleon-nucleon scattering amplitude.}
\end{align*}
\]

It is clear we have

\[
F = F f \cdots + f \cdots
\]

and we find finally

\[
(*) \text{ Reported by M. L. Goldberger.}
\]
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Session 8

We have tried a number of models and in all cases \( J \) was so large that the one in the denominator can be neglected. Then \( J \) drops out and we get a precise prediction for the lifetime independent of \( J \). Experimentally \( J < 0.126 \); in the limit of \( J \) large we get about 0.11 which is remarkably close agreement. With the new axial coupling constant, the experimental value decreases to 0.115 !!

DISCUSSION

Polkinghorne: I just wanted to ask if it is true to say: the assumption that there is no subtraction in the dispersion relation is equivalent to the assumption that there is no direct \( \pi \) decay interaction so that the decay has to go through the loop.

Goldberger: I believe that this is true. I do not think that anyone has ever made a convincing demonstration of a one to one association of subtractions and primeval interactions in Lagrangians, but I have a sort of a feeling that there is a close connection between those two.

Chairman: There is one important point about the Michel-parameter which Goldhaber wishes to bring out.

Goldhaber: Panofsky has asked me to add a few remarks to this new \( \rho \)-value which I quoted by Dudziak which is available in this Berkeley report. In order to avoid the feeling that the question of the \( \rho \)-value is settled once and for all, I should like to point out that the error given is purely statistical and that no systematic corrections have yet been applied. They are at present being worked out and there will be an addendum to this UCRL report if and when they are done.

Good: Michel said that he was not sure whether the experiments ruled out the 1.5% of \( \beta \)-decay rate for the \( \Lambda \)-hyperon that one gets in a simple universal Fermi interaction. I think the experiments do. The Berkeley group has 500, the Columbia group 300 or 400 \( \Lambda \)'s, and I know of other unofficial compilations that bring it up to 1000 \( \Lambda \)'s which have not \( \beta \)-decayed; in our own 500 there are two of the \( \frac{1}{2} \) events of the type that Goldhaber mentioned. Those cases are possible, but rather unlikely, \( \beta \)-decays. So I think it is fair to say that the rate is about one in a thousand and is certainly consistent with zero. So I would like to inquire whether this does or does not rule out any universal Fermi interaction.

Michel: How are those \( \Lambda \)'s recognized? By their mode of decay?

Good: In two ways: those \( \Lambda \)'s that decay into a charged particle are analyzed for decay into \( p + \pi^- \) with conservation of energy and momentum, and those that decay into a neutral mode are recognized by the presence of a \( K^0 \) in the chamber, and they also have not undergone \( \beta \)-decay.

Marshak: I shall try to answer Good's question. The interaction that you are assuming, say \( V-A \), is, if you wish, for the undressed fermions. Now, when you just consider \( \mu \) decay then dress is the same as lack of dress, and so you expect \( V-A \) to be present. Now whenever you consider the currents involving the baryons then, of course, you have strong interactions coming in. The question is, what effect do they have? The reason that the \( \pi-e \) to \( \pi-\mu \) ratio has been so much part of our discussion is that in this case, the strong interactions affect the baryon current in exactly the same way so that the ratio does not depend on strong interaction effects and hence becomes a critical number. Of course, in the case of \( \Lambda \)-decay, you not only have the problem of whether you should extend the theory to strange particle decays but also the problem that it is an absolute rate which you are talking about and the strong interaction effects on the strangeness-non-conserving current may alter this rate. However, I believe that at this stage the ratio \( \pi-e \) to \( \pi-\mu \) is a more critical test of the universal theory than the absolute rate for the \( \Lambda \)-decay, although of course both experiments are very important to pursue.

Gell-Mann: I think that everybody will agree that the absence of hyperon \( \beta \)-decay and the absence of pion \( \beta \)-decay are two great mysteries. I should like to make a couple of remarks about something that was mentioned by Michel. The question of loops to me sounds like a technical matter to be discussed only in the privacy of a theoreticians's office, but I think that it has some real importance; at least we should settle it. Michel had a diagram like this:

We are talking here about the \( (\mu, e) \) interaction that leads to the \( \mu \) decay and which can conceivably lead to a
process like this (Fig. 1) in which the neutrino of the $\mu$-$\nu$ and the neutrino of the $e$-$\nu$ have eaten each other to form a loop of this kind.

$$i\gamma(p-eA),$$
in here, and then you get $\gamma$-ray decay. This first term, linear in $i(\gamma p)$, can contribute nothing to $\gamma$-decay and therefore we cannot absolutely draw the conclusion from the absence of the $\mu$ into $e + \gamma$ that this loop is totally zero. We can draw the conclusion that the $i(\gamma p)^3$ term is smaller than it would be for an intermediate boson model. We cannot draw the conclusion that the loop is absolutely non-existent, and if we had some kind of a crazy model in which these higher terms were not important but there was a big term with just $(ai(\gamma p))$ in it (such as could give no $\gamma$-ray decay here), that thing would in turn be important for another problem (which is the problem of hyperon-decay which, of course, I must not mention today). Hyperon-decay into strongly interacting particles is something like this: imagine we have an interaction, say $(Ap)$ with $(np)$. Then if this loop really should happen to exist in some weird model, it would also come in here and would give us a direct two-Fermion $(An)$ interaction. I think that we should really either bury this thing or else make use of it. I just want to add that these remarks should be attributed to Feynman and me, if he will agree. Is he here?

Chairman: Not here.

Gell-Mann: All right, then he will agree.

Källén: I would like to ask a very simple question to which I am sure the experts must know the answer. Is no one worried about the fact that the renormalized coupling constant that comes out here is bigger than the bare coupling constant? Normally we expect it to be just the other way round — what do the experts say?

Gell-Mann: I am not necessarily an expert, but I think that what you say is true of simple models and not necessarily of complicated ones, that is, if you just have, say, a static model of a nucleon and you allow for the fact that it has a pion cloud, then the axial vector renormalization represents the fraction of the time that the spin of the core points in the direction of the spin of the whole particle and is obviously less than one; but in a model where you include pair effects, besides the pions in the cloud, I think this is no longer true. Whether we have to rely on that, or whether there is some deep misunderstanding of the situation here, I do not know, but I think that you cannot prove from the existing situation with strong couplings involved and so on, that there is anything wrong.
Källén: I quite agree that there is no general mathematical theorem that will tell you it must be smaller but practically in all cases that we know about it happens anyhow.

Goldberger: I think that practically all cases that we know about that Källén is referring to is the case of quantum electrodynamics. There is also the example that Gell-Mann points out of the static meson theory. It is very easy to have the opposite behaviour and in fact Treiman and I pointed out a very simple model involving nucleon-antinucleon pairs and which is not at all unreasonable. In fact, it is exceedingly likely that the renormalized coupling constant must be larger than the unrenormalized one in our model.

Okun': Are the radiative corrections included in the $\varrho$-value which was given by Goldhaber?

Goldhaber: Perhaps Crowe would be a better man to answer this. Is he here?

Crowe: The value that Dudziak quotes does include the radiative correction as indicated by Berman. The Stanford value should be corrected up by perhaps 0.005, well within the error.

Chairman: We had a very long session but I think we can all be sure that we have participated in a more or less historical creation and I do not think that there will be another chance that weak interactions will be discussed in such detail and on the theoretical side with such deep ideas as those with which we are leaving this room today.

Thank you very much.
SESSION 9
Saturday, 5th July, 1958

Weak interactions — Other modes

Chairman R. E. MARSHAK

EXPERIMENTAL
Rapporteur D. A. GLASER
Secretaries G. MACLEOD
D. R. O. MORRISON
G. WEBER

THEORETICAL
Rapporteur S. B. TREIMAN
Secretaries B. d’ESPAGNAT
M. FROISSART
J. G. TAYLOR
WEAK INTERACTIONS: OTHER MODES — EXPERIMENTAL RESULTS

D. A. GLASER, Rapporteur
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Introduction

Non-leptonic modes of decay produced by weak interactions can be studied by observing the decays of hyperons and K mesons. Two years ago the events were counted in tens, last year in hundreds, and this year over a thousand strange particle decays of some types have been observed. The information I shall summarize concerns:

1. Lifetimes of \( \Lambda^0, \Sigma^+, \Sigma^0, \theta_3^0, \theta_3^-, K^- \).
2. Decay branching ratios of \( \Lambda^0 \) and \( \theta_3^0 \).
3. Spins of \( \Lambda^0, \Sigma^0, \) and \( \theta_3^0 \).
4. Parity violating asymmetries in the decays of \( \Lambda^0, \Sigma^+ \) and \( \Sigma^- \).
5. Determination of the mass of \( \Sigma^0 \).
6. Recent experimental information on the \( \theta_1 - \theta_2 \) problem.
7. Properties of artificially produced \( \Sigma^- \) hyperons.

It is only because of the excellent help and collaboration of Good, Macleod and Morrison in combining data received from many laboratories that it is possible to present a reasonably complete compilation of this data. In view of the large size of most of the experimental groups whose work is included in this report, I shall make reference to laboratories rather than individuals and a list of authors is given at the end of this report. These groups represent emulsion, bubble chamber and cloud chamber work, there being to date no counter experiments that I have to report.

1. Lifetimes

\( \Lambda^0 \). In Table I (*) are collected data leading to improved values for the lifetime of \( \Lambda^0 \). Results obtained at various laboratories using different techniques agree reasonably well. In calculating the mean lifetime, the value from each laboratory was weighted according to the number of events observed. All these results are consistent with a single lifetime. The Princeton cloud chamber group contributed 176 \( \Lambda^0 \) which gives a lifetime consistent with our average. Their results arrived too late for inclusion in this compilation.

\( \Sigma^- \). Bubble chamber results on the lifetime of \( \Sigma^- \) agree (Table II (*)). Emulsion results disagree and give both smaller and larger values. It should be noted that the decays in flight measured in emulsion give a lifetime considerably shorter than those measured in bubble chambers.

\( \Sigma^+ \). When decays in flight are lumped together with decays at rest by the emulsion workers they obtain a lifetime in fair agreement with that obtained in bubble chambers, see Table III (*). Decays in flight in emulsion yield discordant results usually considerably smaller than the accepted mean value. The Livermore group also applied a “cut-off” at \( 5 \times 10^{-11} \) sec to be sure of only observing decays in flight.

\( \theta_3^0 \). Those decays in flight in emulsion yielding a pion of unknown sign also give a low value for the lifetime, see Table IV (*). These low values are generally less than the accepted value for either \( \Sigma^- \) or \( \Sigma^+ \).

Does the charged \( \Sigma \) have two lifetimes?

Some of the emulsion workers feel that these “anomalous” lifetimes are due to experimental bias, but many experimentalists can find no source of error and believe the effect may be real. If the emulsion observations were taken seriously, they would indicate that about 15% of the charged \( \Sigma \)’s have the short lifetime. It is generally agreed by the bubble chamber workers that a short lifetime would not be observed for those charged \( \Sigma \)’s produced by \( \pi + N \rightarrow \Sigma + K \) because of scanning biases. On the other hand, rapid \( \Sigma \)-decays resulting from \( K^- + p \rightarrow \Sigma^+ + \pi^\pm \) would be seen in a hydrogen bubble chamber and would look like the emission of two fast pions following absorption of a \( K^- \). Present data rule out the existence of more than 15% short-lived charged \( \Sigma \)’s among the total sample. As statistics accumulate, this limit will become much lower during the next year, if the charged \( \Sigma \)’s have a single lifetime.

\( \theta_1^0 \). Values of the lifetime of the decay \( \theta_3^0 \rightarrow \pi^\pm + \pi^\mp \) are shown in Table V (*). Agreement among various laboratories is excellent, including 84 decays reported by the Princeton cloud chamber group too late for inclusion in this summary.

(*) To be found at the end of the report.
K°. The Columbia cloud chamber group has measured the lifetime of K° by observing the decay rate at two different distances from the producing target. Their result is

\[ \tau_{K^0} = 9.0^{+3.5}_{-2.3} \times 10^{-8} \text{ sec}. \]

K°. After considerable discussion among the emulsion workers who have observed decays in flight of K° mesons, it was conservatively agreed that the K° and K+ have the same lifetime within the present rather large errors of K° lifetimes.

2. Branching ratios

A°. Although the decay A° → π° + p is easily observed in cloud chambers and bubble chambers, the neutral mode A° → π° + n is rarely seen by techniques used until now. By assuming associated production of strange particles, it is possible to calculate the branching ratio from the observed charged decays alone. The results of a number of experiments measuring this branching ratio are summarized in Table VI (a). If the selection rule \( \Delta I = \frac{1}{2} \) is valid for the A°-decay, \( \frac{1}{2} \) of all A°'s should decay via the charged mode. The observed branching ratio is in good agreement with this value. In propane bubble chambers and multi-plate cloud chambers, γ-rays associated with the neutral mode are occasionally seen. They do not seem to appear quite often enough to account for all the neutral decays, but the errors are large and there is no serious discrepancy.

K°. In Table VII (a) is summarized data pertaining to the decay modes of K°. If the rule \( \Delta I = \frac{1}{2} \) applies to the decay of K°, we expect \( \frac{1}{2} \) of all K° to decay via the charged mode. The observed branching ratio is in agreement with this value. In propane bubble chambers and multi-plate cloud chambers, γ-rays associated with the neutral mode are occasionally seen. They do not seem to appear quite often enough to account for all the neutral decays, but the errors are large and there is no serious discrepancy.

3. Spins of Λ°, Σ°, Κ°

Two methods have been used for the experimental determination of the spins of Λ°, Σ° and Κ°. Both of these methods depend on observing the angular distribution of production and decay of strange particles resulting from π-p collisions.

In the method of Adair one selects a sample of hyperons, for example those which are produced nearly forward and backward with respect to the direction of the bombarding pion. If we assume the spins of K and π are zero, and choose the z-axis for quantization of the angular momentum to lie along the direction of the incident pion, then the hyperons have magnetic quantum numbers \( \pm \frac{1}{2} \) with equal probability and no phase correlation if the target protons are unpolarized. For hyperons in such an unpolarized state, the distribution is \( \cos \gamma \), where \( \gamma \) is the angle between a decay product and the z-axis which is a unique function of the spin. Since the hyperons produced by \( π^- + p \rightarrow Y + K \) are peaked in the forward and backward directions, a large fraction of all the events can be used in the analysis without disturbing the essential condition that \( m_Y = \pm \frac{1}{2} \)-states are the only ones that are appreciably involved. Since the detailed description of this analysis by the Bologna, Columbia, Michigan and Pisa groups is already in the literature, we will just quote the result.

In terms of a single critical parameter, A, the data for A° were about one standard deviation away from spin \( \frac{1}{2} \) and 7 standard deviations away from spin \( \frac{3}{2} \). For Σ°, the data were 2 standard deviations away from spin \( \frac{1}{2} \) and 6 standard deviations away from spin \( \frac{3}{2} \). Larger spins are completely inadmissible.

Recently the Berkeley hydrogen bubble chamber group has applied the Adair analysis to the K° spin, assuming that the A° spin is \( \frac{1}{2} \). The best fit to spin 2 yielded \( P_2 (x^2) = 2\% \) while for spin zero \( P_0 (x^2) = 25\% \). Odd spin is ruled out by the existence of K° → 2π°. This is evidence against spin 2 for K°, although it is not yet conclusive. The difference between the experimental results and the prediction for spin 2 is shown in Fig. 1.

Another method suggested by Lee and Yang can be used to determine the Λ°-spin by taking advantage of the large up-down asymmetry observed for Λ°'s produced in \( π^- + p \rightarrow Λ° + 0° \). In the Lee-Yang method, the angular distribution of the decay products with respect to the direction of maximum asymmetry (the normal to the production plane in this case) is expanded in Legendre polynomials in \( \cos \gamma \). Certain inequalities must hold among the coefficients of the expansion for each spin assignment of the hyperon.

400 A-decays contributed by Berkeley, Columbia and Michigan were analysed in this way by Crawford at Berkeley. Crawford modified the procedure of Lee and Yang slightly in order to permit a \( x^2 \) estimate of the probability that the data e consistent with a given spin.

(*) To be found at the end of this report.
4. Asymmetries in the angular distribution of the decay

In the past year it has been discovered, following a suggestion of Lee and Yang, that $\Lambda$'s produced by high energy $\pi^-$ mesons are polarized and do not conserve parity in their decay processes. The result is that the decay pion goes “UP” more often than “DOWN” with respect to the plane of production. In the reaction

$$\pi^-_{\text{in}} + p \rightarrow Y + K,$$

followed by the decay $Y \rightarrow \pi^-_{\text{decay}} + N$, we define the quantity $\xi = (\pi^-_{\text{in}} \times y) \cdot \pi^-_{\text{decay}}$, and take $|\xi| > 0$ to mean UP. For hyperon spin $\frac{1}{2}$ and $K$ meson spin 0,

$$W(\xi) d\xi \sim (1 + aP\xi) d\xi$$

where $P$ is the polarization averaged over all production angles. Integrating over $\xi$ we have

$$aP = \frac{N_{UP} - N_{DOWN}}{\frac{1}{2} (N_{UP} + N_{DOWN})},$$

The quantity directly measured is the asymmetry $aP$. The asymmetry has been measured at several bombarding energies and at a number of angles. $a$ is the measure of the degree of parity violation. It is necessary to make some estimate of the polarization $P$ by studying the angular distribution of the production process. For the Berkeley experiments this was done very simply by studying the value of $aP$ as a function of production angle. Since $|P|$ is always less than 1, one can put a lower limit on $|a|$ by observing the maximum value of $aP$. For 1.00 GeV $\pi^-$, they find $|a| > 0.73 \pm 0.14$. In the Bologna, Columbia, Michigan, Pisa data the polarization was estimated from the observed production angular distribution, and for $\pi^-$ energies between 0.91 and 1.10 GeV they obtain $|a| > 0.67 \pm 0.13$.

A similar study of the “UP-DOWN” asymmetry in the $\Sigma^-$ decay shows no asymmetry. The Berkeley group find for 1.10 GeV $\pi^- : |aP| = 0.04 \pm 0.13$.

It is not known whether the asymmetry does not appear because of the properties of the $\Sigma$-decay, or because $\Sigma$ is not polarized. So far no measurement of the polarization of charged $\Sigma$'s is available. However, an attempt has been made by the Berkeley group to observe the polarization of $\Sigma^0$-hyperons by looking for “UP-DOWN” asymmetries in the decay of their daughter $\Lambda$'s. In 114 cases no strong effect was found $|aP| \sigma = 0.17 \pm 0.17$. The polarization of the $\Sigma^0$ is somewhat difficult to observe because its daughter $\Lambda^0$ will have a polarization only $\frac{1}{3}$ that of the parent $\Sigma^0$.

The preceding results all represent events observed in hydrogen and propane bubble chambers. The M.I.T. group has observed a smaller asymmetry for the decay of $\Lambda^0$'s produced in iron plates by 1.5 BeV $\pi^-$ mesons in a multi-plate cloud chamber. The total sample shows no asymmetry, but by choosing events whose production angles were close to those expected for free proton events a small asymmetry was found. The Princeton group, using a multi-plate cloud chamber at the same energy found no asymmetry for hyperon production in lead, iron and carbon, although the statistics were very weak.

A large number of emulsion groups have looked for “UP-DOWN” asymmetries in the decay of charged $\Sigma$'s produced by the absorption of $K^-$ mesons in emulsion. In the reaction $K^- + p \rightarrow \pi^- + \Sigma^+$, at rest in emulsion, the $\pi$ and $\Sigma$-particles are not collinear due to the Fermi momentum of the proton in the nucleus. Therefore we can define UP with respect to a production plane. As before, we define the quantity

$$\xi = (\Sigma \times \pi_{\text{out}}) \cdot \pi_{\text{decay}}$$

and take $|\xi| > 0$ to mean UP. The combined results of the Berkeley, Bologna, Göttingen, Livermore, NRL, Paris, Parma, and Rochester emulsion groups are:

for the decay $\Sigma^+ \rightarrow \pi^0 + p$ at rest and in flight $U/D = 143.5 \pm 149.5$,

for the decay $\Sigma^+ \rightarrow \pi^+ + n$ at rest, $U/D = 103 \pm 95$,

and for the decay $\Sigma^0 \rightarrow \pi^0 + n$ in flight $U/D = 60.5 \pm 38.5$. 

Fig. 1. The histogram represents the observed angular distribution of the decay products of $K^0$ mesons, where $\gamma$ is the angle between the decay product and the z-axis. The smooth curve represents the theoretical prediction for spin 2.
Within the experimental errors, the present data indicate no asymmetry.

Hyperons produced by $K^-$ interaction in flight in a hydrogen bubble chamber $K^- + p \rightarrow \Sigma^\pm + \pi^\mp$, have been studied by the Berkeley group for a possible “UP-DOWN” asymmetry. None was found for a sample of 32 $\Sigma^-$ and 22 $\Sigma^+$. In the decay of an unpolarized sample of $\Lambda^0$'s the decay protons can possess a longitudinal polarization in the $\Lambda^0$ rest system if parity is not conserved in the decay. This longitudinal polarization is equal to $\alpha$ (the same $\alpha$ is used above to describe the “UP-DOWN” asymmetries). When the decays of $\Lambda^0$'s in flight are observed in the laboratory, the longitudinal proton polarization gives rise to a transverse proton polarization as a result of the transformation from the $\Lambda^0$ rest system to the laboratory system. The transverse polarization of the decay protons can be measured by observing a right-left asymmetry in their nuclear scattering. Knowing the momentum of the proton, it is possible to calculate its original longitudinal polarization, knowing the asymmetry produced by nuclear scattering for protons of that momentum. The M.I.T. group has observed 54 scatterings of decay protons on iron plates in their cloud chamber. At the 68% confidence level they observe the proton longitudinal polarization,

$$a = -\left(0.85 \pm 0.15 \right).$$

Since the bubble chamber lower limit on $\alpha$ is quite certain, it can be used as a cut-off on the likelihood function for $\alpha$ obtained in this experiment; the M.I.T. group obtains in this way odds of 24 to 1 favouring $\alpha < 0$, which means that the decay protons have negative helicity.

Preliminary results obtained from 21 nuclear scatterings of $\Lambda^0$-decay protons in the Berkeley propane bubble chamber give odds 9.5 to 1 favouring $\alpha > 0$! The sign of $\alpha$ is therefore still uncertain.

5. Determination of the mass of $\Sigma^0$

About 15 $\Sigma^0$-decays have been seen in hydrogen and propane bubble chambers by the Columbia-Brookhaven and Berkeley groups in which it has been possible to measure the mass of $\Sigma^0$ as a consequence of the materialization of the decay $\gamma$-ray, or the direct formation of a Dalitz pair. Stevenson of the Berkeley group has summarized this information with the result

$$Q_{\Sigma^0} = 75.3 \pm 0.9 \text{ MeV} \quad \text{and} \quad M_{\Sigma^0} = 1190 \pm 0.9 \text{ MeV}.$$  

6. Recent experimental information on the $K^*_2$ problem

A number of strange particle production decay and interaction events have been observed recently to support the particle mixture theory of the $K^*_2$. In two cases in the Berkeley hydrogen bubble chamber a $K^*_2$ produced in association with a neutral hyperon travelled some distance in the chamber and interacted producing a $\Sigma^+$ whose decay was also observed. An example of this process is shown in Alvarez' contribution to the discussion of Session VI. In addition to this, three $K^*_2$ have also been seen. In the Columbia propane chamber a $\theta_s$ decay was seen in association with a $\Sigma^0$. In the M.I.T. multi-plate chamber, 12 cases of production of hyperons by $K^*_2$'s have been observed. Of these, 4 pictures show strange particles produced in association with the interacting $K^*_2$.

From particle mixture theory of the $K^*_2$ the time variation of the amplitude of the $\bar{K}$ can be predicted for various values of the mass difference between $K_1$ and $K_2$. On comparing this time variation with the observed distribution in the cloud chamber of the 12 observed $\bar{K}$ interactions, one finds that the $K_1^* - K_2^*$ mass difference is likely to be greater than 0. Many more events must be collected before this conclusion is decisive.

7. Properties of artificially produced $\Xi^-$ hyperons

The propane bubble chamber group at Berkeley has observed at the Bevatron two particles with properties consistent with those of the negative cascade particles found in cosmic rays. The tracks were observed in a 30 in. propane bubble chamber exposed to a $5.0 \pm 0.5$ GeV/c $\pi^-$ beam. Preliminary measurements give a $Q$ value of $50 \pm 10$ MeV for one case and $59 \pm 20$ MeV for the other. One particle lives for about $4.8 \times 10^{-10}$ sec and the other for about $1.1 \times 10^{-10}$ sec. Based on these two artificially produced cascade particles, the production cross-section at this energy is in the neighbourhood of 1 or 2 microbarns. An example of this decay is seen in Fig. 2.
Fig. 2. $\Sigma^-$-decay observed by the Berkeley propane chamber group.
Weak interactions — Other modes

| Michigan Propane Bubble Chamber $(\Sigma^-, A^0)$ $(\Sigma^+)$ | J. Brown | D. Glaser | C. Graves |
| Michigan Propane Bubble Chamber $(\Sigma^-, A^0)$ $(\Sigma^+)$ | M. Perl | J. Cronin | D. Glaser |
| Michigan Propane Bubble Chamber $(\Sigma^-, A^0)$ $(\Sigma^+)$ | J. Vandervelde | |
| Princeton Cloud Chamber | T. Bowen | J. Hardy Jr. | G. T. Reynolds |
| Princeton Cloud Chamber | G. Tagliaferri | A. E. Werbrouck | W. H. Moore |
| Brookhaven, Columbia, Bologna, Pisa collaboration (Propane Bubble Chamber) (Hydrogen Bubble Chamber) | F. Eisler | R. Plano | Columbia |
| Brookhaven, Columbia, Bologna, Pisa collaboration (Propane Bubble Chamber) (Hydrogen Bubble Chamber) | A. Prodel | N. Samios | and |
| Brookhaven, Columbia, Bologna, Pisa collaboration (Propane Bubble Chamber) (Hydrogen Bubble Chamber) | M. Schwartz | J. Steinberger | Brookhaven |
| Brookhaven, Columbia, Bologna, Pisa collaboration (Propane Bubble Chamber) (Hydrogen Bubble Chamber) | P. Bassi | V. Borelli | Bologna |
| Brookhaven, Columbia, Bologna, Pisa collaboration (Propane Bubble Chamber) (Hydrogen Bubble Chamber) | G. Tanaka | P. Woloschek | V. Zobuli |
| Brookhaven, Columbia, Bologna, Pisa collaboration (Propane Bubble Chamber) (Hydrogen Bubble Chamber) | M. Conversi | P. Franzini | I. Manelli |
| Brookhaven, Columbia, Bologna, Pisa collaboration (Propane Bubble Chamber) (Hydrogen Bubble Chamber) | R. Santangelo | V. Silvestrini | Pisa |
| M. I. T. Cloud Chamber | B. Boldt | D. Caldwell | Y. Pal |
| Jungfraujoch | W. A. Cooper | H. Filthuth | L. Montanet |
| Jungfraujoch | G. Petrucci | R. A. Salmeron | J. A. Newth |

**Emulsion groups**

| Bologna | W. Alles | N. N. Bisnas | M. Ceccarelli | R. Gessaroli |
| Göttingen | H. Going | K. Gottstein | W. Puschel | J. Tietge |
| Paris | J. Crussard | J. Hennessy | |
| Parma | G. Dascola | G. Mora | |
| N. R. L. | R. G. Glaser | N. Seeman | G. A. Snow |
| Rochester | T. F. Hoang | M. Kaplon | T. Yamanouchi |
| Livermore | S. White | C. Gilbert | S. C. Freden |
| Berkeley (Barkas group) | W. H. Barkas | J. N. Dyer | P. C. Giles |
| Berkeley (Goldhaber group) | G. and S. Goldhaber | N. A. Nickols | F. M. Smith |
| Wisconsin | W. F. Fry | J. Schneps | G. A. Snow |
| Wisconsin | D. C. Wold | | M. S. Swami, |

**European collaboration**

| B. Bhowmik, D. Evans, D. Falla, F. Hassan, A. A. Kamal, K. K. Nagpaul, D. J. Prowse | Bristol |
| M. Rene | Bruxelles |
| D. Keefe | Dublin (U. C.) |
| E. H. S. Burhop, D. H. Davis, R. C. Kumar, W. B. Lasich, M. A. Shaukat, F. R. Stannard | London (U. C.) |
| M. Bacchella, A. Bonetti, C. Dilworth, G. Occhialini, L. Scarsi | Milano |
| M. Grilli, L. Guerrieri, L. von Lindern, M. Merlin, A. Saldandin | Padova |

**DISCUSSION** — see p. 274.
TABLES OF $\Lambda$, $K^0$, $\Sigma^\pm$ LIFETIMES AND $\Lambda^0$, $K^0$ DECAY BRANCHING RATIOS

Compiled by D.A. GLASER, M.L. GOOD and D.R.O. MORRISON

These tables of lifetimes and branching ratios are not necessarily a comprehensive list but are based on the information supplied to the Rapporteur of Session 9 (D. A. Glaser) of the 1958 Annual International Conference on High Energy Physics.

In calculating the mean values of $\Lambda^0$ and $K^0$ lifetimes, and the $\Lambda^0$-decay branching ratio, the individual values are weighted in proportion to the number of events upon which they were based. The errors given to the mean values are discussed by Glaser.

In computing mean values for the $\Sigma^+$- and $\Sigma^-$-lifetimes and the $K^0$ decay branching ratios, the errors were symmetrised by taking the inverse of the quoted values and errors. The squares of these new "inverse errors" were used to weight the reciprocals of the quoted values in obtaining a mean value. The errors on this mean were formed from the r.m.s. of the "inverse errors".

The following abbreviations are used:
- H.B.C. Hydrogen Bubble Chamber
- P.B.C. Propane Bubble Chamber
- C.C. Cloud Chamber
- C.R. Cosmic Ray measurement

$(p) F$ $\Sigma$-decays in Flight were taken, the proton track was measured.

$(p, \pi) F$ $\Sigma$-decays in Flight were taken, the proton and/or pion tracks were measured.

$(p) F, R$ $\Sigma$-decays in Flight or at Rest were taken, the proton track was measured.

<table>
<thead>
<tr>
<th>TABLE I</th>
</tr>
</thead>
</table>

$\Lambda^0$-Lifetime

<table>
<thead>
<tr>
<th>Group</th>
<th>Technique</th>
<th>No. of events</th>
<th>Lifetime $\times 10^{\text{10}}$ sec.</th>
</tr>
</thead>
<tbody>
<tr>
<td>BERKELEY ($K^-$ capture)</td>
<td>H. B. C.</td>
<td>76</td>
<td>2.95 $\pm$ 0.4</td>
</tr>
<tr>
<td>BERKELEY (Assoc. prodn.)</td>
<td>H. B. C.</td>
<td>340</td>
<td>3.05 $\pm$ 0.35</td>
</tr>
<tr>
<td>COLUMBIA</td>
<td>H. B. C.</td>
<td>454</td>
<td>2.29 $\pm$ 0.15 $\pm$ 0.13</td>
</tr>
<tr>
<td>PISA</td>
<td>P. B. C.</td>
<td>454</td>
<td></td>
</tr>
<tr>
<td>BOLOGNA</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>COLUMBIA</td>
<td>C. C.</td>
<td>74</td>
<td>2.75 $\pm$ 0.45 $\pm$ 0.38</td>
</tr>
<tr>
<td>JUNGFRAUJOCH</td>
<td>C. C.</td>
<td>40</td>
<td>3.04 $\pm$ 0.78 $\pm$ 0.51</td>
</tr>
<tr>
<td>MICHIGAN</td>
<td>P. B. C.</td>
<td>61</td>
<td>2.08 $\pm$ 0.46 $\pm$ 0.31</td>
</tr>
<tr>
<td>M. I. T.</td>
<td>C. C.</td>
<td>200</td>
<td>2.4 $\pm$ 0.2</td>
</tr>
<tr>
<td>MEAN LIFETIME</td>
<td></td>
<td></td>
<td>2.60 $\pm$ 0.16 $\pm$ 0.14</td>
</tr>
</tbody>
</table>
### TABLE II

$\Sigma^{-}$ Lifetime

<table>
<thead>
<tr>
<th>Group</th>
<th>Technique</th>
<th>Measurement</th>
<th>Lifetime $\times 10^{9}$ sec</th>
</tr>
</thead>
<tbody>
<tr>
<td>BERKELEY</td>
<td>H. B. C.</td>
<td>$(\pi) \ F$, $R$</td>
<td>$1.60 \pm 0.2$</td>
</tr>
<tr>
<td>COLUMBIA PISA</td>
<td>P. B. C.</td>
<td>$(\pi) \ F$</td>
<td>$1.89 \pm 0.33 \pm 0.25$</td>
</tr>
<tr>
<td>BOLOGNA</td>
<td>P. B. C.</td>
<td>$(\pi) \ F$</td>
<td>$1.67 \pm 0.40 \pm 0.28$</td>
</tr>
<tr>
<td>MICHIGAN</td>
<td>P. B. C.</td>
<td>$(\pi) \ F$</td>
<td>$1.72 \pm 0.17 \pm 0.10$</td>
</tr>
<tr>
<td>LIVERMORE</td>
<td>Emulsion</td>
<td>$(\pi) \ F$</td>
<td>$0.34 \pm 0.37 \pm 0.07$</td>
</tr>
<tr>
<td>WISCONSIN</td>
<td>Emulsion</td>
<td>$(\pi) \ F$, $R$</td>
<td>$2.5 \pm 0.8$</td>
</tr>
</tbody>
</table>

### TABLE III

$\Sigma^{-}$ Lifetime

<table>
<thead>
<tr>
<th>Group</th>
<th>Technique</th>
<th>Measurement</th>
<th>Lifetime $\times 10^{9}$ sec</th>
</tr>
</thead>
<tbody>
<tr>
<td>BERKELEY</td>
<td>H. B. C.</td>
<td>$(p, \pi) \ F$</td>
<td>$0.69 \pm 0.1$</td>
</tr>
<tr>
<td>MICHIGAN</td>
<td>P. B. C.</td>
<td>$(p, \pi) \ F$</td>
<td>$0.95 \pm 0.37 \pm 0.23$</td>
</tr>
<tr>
<td>LIVERMORE</td>
<td>Emulsion</td>
<td>$(p) \ F$, $R$</td>
<td>$0.94 \pm 0.23 \pm 0.15$</td>
</tr>
<tr>
<td>EUROPEAN COLLAB.</td>
<td>Emulsion</td>
<td>$(p) \ F$, $R$ selected</td>
<td>$0.84 \pm 0.20 \pm 0.15$</td>
</tr>
<tr>
<td>LIVERMORE</td>
<td>Emulsion</td>
<td>$(p) \ F$, $R$</td>
<td>$0.61 \pm 0.18 \pm 0.11$</td>
</tr>
<tr>
<td>N. R. L.</td>
<td>Emulsion</td>
<td>$(p) \ F$, $R$</td>
<td>$1.25 \pm 0.51 \pm 0.28$</td>
</tr>
<tr>
<td>ROCHESTER</td>
<td>Emulsion</td>
<td>$(p) \ F$, $R$</td>
<td>$0.95 \pm 0.16 \pm 0.11$</td>
</tr>
<tr>
<td>WISCONSIN</td>
<td>Emulsion</td>
<td>$(p) \ F$, $R$</td>
<td>$0.96 \pm 0.37 \pm 0.21$</td>
</tr>
<tr>
<td>EUROPEAN COLLAB.</td>
<td>Emulsion</td>
<td>$(p) \ F$</td>
<td>$0.86 \pm 0.57 \pm 0.25$</td>
</tr>
<tr>
<td>LIVERMORE</td>
<td>Emulsion</td>
<td>$(p) \ F$</td>
<td>$0.54 \pm 0.58 \pm 0.11$</td>
</tr>
<tr>
<td>LIVERMORE</td>
<td>Emulsion</td>
<td>$(p) \ F$, cut-off</td>
<td>$0.12 \pm 0.07 \pm 0.04$</td>
</tr>
<tr>
<td>LIVERMORE</td>
<td>Emulsion</td>
<td>$(\pi) \ F$</td>
<td>$0.16 \pm 0.08 \pm 0.03$</td>
</tr>
<tr>
<td>ROCHESTER</td>
<td>Emulsion</td>
<td>$(p) \ F$</td>
<td>$0.81 \pm 0.75 \pm 0.15$</td>
</tr>
<tr>
<td>WISCONSIN</td>
<td>Emulsion</td>
<td>$(p) \ F$</td>
<td>$0.47 \pm 0.44 \pm 0.15$</td>
</tr>
</tbody>
</table>
### Table IV

**$\Sigma^+$-Lifetime**

<table>
<thead>
<tr>
<th>Group</th>
<th>Technique</th>
<th>Measurement</th>
<th>Lifetime $\times 10^9$ sec.</th>
</tr>
</thead>
<tbody>
<tr>
<td>BERKELEY</td>
<td>Emulsion</td>
<td>$\pi \pm$ $F$</td>
<td>$0.51 \pm 0.2$</td>
</tr>
<tr>
<td>EUROPEAN COLLAB.</td>
<td>Emulsion</td>
<td>$\pi \pm$ $F$</td>
<td>$0.81 \pm 0.22$</td>
</tr>
<tr>
<td>LIVERMORE</td>
<td>Emulsion</td>
<td>$\pi \pm$ $F$</td>
<td>$0.33 \pm 0.11$</td>
</tr>
<tr>
<td>DAVIES ET AL.</td>
<td>Emulsion (C. R.)</td>
<td>$\pi \pm$ $F$</td>
<td>$0.35 \pm 0.15$</td>
</tr>
<tr>
<td>N. R. L.</td>
<td>Emulsion</td>
<td>$\pi \pm$ $F$</td>
<td>$0.31 \pm 0.32$</td>
</tr>
<tr>
<td>WISCONSIN</td>
<td>Emulsion</td>
<td>$\pi \pm$ $F$</td>
<td>$0.32 \pm 0.11$</td>
</tr>
</tbody>
</table>

### Table V

**$K^0$ Lifetime**

<table>
<thead>
<tr>
<th>Group</th>
<th>Technique</th>
<th>No. of events</th>
<th>Lifetime $\times 10^9$ sec.</th>
</tr>
</thead>
<tbody>
<tr>
<td>BERKELEY</td>
<td>H. B. C.</td>
<td>228</td>
<td>$0.93 \pm 0.10$</td>
</tr>
<tr>
<td>COLUMBIA PISA BOLOGNA</td>
<td>P. B. C.</td>
<td>259</td>
<td>$1.06 \pm 0.08$</td>
</tr>
<tr>
<td>COLUMBIA</td>
<td>C. C.</td>
<td>39</td>
<td>$1.15 \pm 0.40$</td>
</tr>
<tr>
<td>JUNGFRAUJOCH</td>
<td>C. C.</td>
<td>29</td>
<td>$0.84 \pm 0.35$</td>
</tr>
<tr>
<td>MICHIGAN</td>
<td>P. B. C.</td>
<td>62</td>
<td>$0.81 \pm 0.23$</td>
</tr>
<tr>
<td>M. I. T.</td>
<td>C. C.</td>
<td>90</td>
<td>$1.07 \pm 0.13$</td>
</tr>
<tr>
<td><strong>MEAN LIFETIME</strong></td>
<td></td>
<td></td>
<td>$0.99 \pm 0.08$</td>
</tr>
</tbody>
</table>
### TABLE VI

$A^0$-Decay branching ratio, $A^0 \rightarrow p + \pi^-$

<table>
<thead>
<tr>
<th>Group</th>
<th>Technique</th>
<th>No. of events</th>
<th>Branching ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>BERKELEY ($K^-$ CAPTURE)</td>
<td>H. B. C.</td>
<td>130</td>
<td>$0.61 \pm 0.05$</td>
</tr>
<tr>
<td>BERKELEY (ASSOC. PRODN.)</td>
<td>H. B. C.</td>
<td>450</td>
<td>$0.59 \pm 0.04$</td>
</tr>
<tr>
<td>COLUMBIA</td>
<td>P. B. C.</td>
<td>528</td>
<td>$0.68 \pm 0.05$</td>
</tr>
<tr>
<td>PISA</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>BOLOGNA</td>
<td>C. C.</td>
<td>200</td>
<td>$0.69 \pm 0.06$</td>
</tr>
<tr>
<td>MICHIGAN</td>
<td>P. B. C.</td>
<td>91</td>
<td>$0.57 \pm 0.10$</td>
</tr>
<tr>
<td><strong>MEAN BRANCHING RATIO</strong></td>
<td></td>
<td></td>
<td>$0.63 \pm 0.03$</td>
</tr>
<tr>
<td><strong>EXPECTED BRANCHING RATIO IF $\Delta I = \frac{1}{2}$</strong></td>
<td></td>
<td></td>
<td>$\frac{1}{3} = 0.67$</td>
</tr>
</tbody>
</table>

### TABLE VII

$K^0$ decay branching ratios

<table>
<thead>
<tr>
<th>Group</th>
<th>Technique</th>
<th>No. of events</th>
<th>$\frac{\pi^+ + \pi^-}{\text{All } \theta^0}$</th>
<th>$\frac{\pi^0 + \pi^0}{\text{All } \theta^0}$</th>
<th>$\frac{\theta_1^+}{\text{All } \theta^0}$</th>
</tr>
</thead>
<tbody>
<tr>
<td>BERKELEY</td>
<td>H. B. C.</td>
<td>450</td>
<td>$0.35 \pm 0.03$</td>
<td></td>
<td></td>
</tr>
<tr>
<td>COLUMBIA</td>
<td>P. B. C.</td>
<td>528</td>
<td>$0.42 \pm 0.05$</td>
<td>$0.07 \pm 0.03$</td>
<td>$0.49 \pm 0.075$</td>
</tr>
<tr>
<td>PISA</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>BOLOGNA</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>MICHIGAN</td>
<td>P. B. C.</td>
<td>91</td>
<td>$0.46 \pm 0.10$</td>
<td></td>
<td></td>
</tr>
<tr>
<td>M. I. T.</td>
<td>C. C.</td>
<td>(0.47)</td>
<td></td>
<td>$0.03$</td>
<td></td>
</tr>
<tr>
<td><strong>MEAN BRANCHING RATIO</strong></td>
<td></td>
<td></td>
<td>$0.39 \pm 0.03$</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>EXPECTED RATIOS IF $\Delta I = \frac{3}{2}$</strong></td>
<td></td>
<td></td>
<td>$\frac{1}{3} = 0.33$</td>
<td>$\frac{1}{6} = 0.17$</td>
<td>$\frac{1}{2} = 0.50$</td>
</tr>
</tbody>
</table>


DISCUSSION

Discussion of Lifetimes

Fowler: I have a question about the $\Lambda^0$-lifetime. Those of us who have small bubble chambers live in the hope that we will some day get hundreds of events. If I take your table and combine the Cosmotron bubble chamber results I see a lifetime of $2.26 \times 10^{-10}$ sec but if I take the Berkeley Bevatron results I see a lifetime of $3.04 \times 10^{-10}$ sec, based on 515 and 416 events respectively. This seems to be a significant difference. I wonder if, for the sake of completeness, we could hear some details?

D. A. Glaser: As can be seen from Table I, the lowest value of $2.29 \times 10^{-10}$ sec comes from the Columbia, Pisa, Bologna chamber, and the highest, $3.05 \times 10^{-10}$ sec, from the Berkeley chamber, which is indeed quite a variation. Those chambers were the same size. The Berkeley chamber was 10 inches and the other one 12 inches. The Michigan chamber was also 12 inches, and the cloud chambers were quite different in size, the M.I.T. one being a few feet. The difficulty with the experiment is that one must take into account wall effects which result from escape of $\Lambda^0$'s, and I regret to say that there has not been a very precise meeting between us as to how one should do that. Each of us did this quite independently and I think, that if the discrepancy is more than statistical, then that is its origin. I have no answer except that we have to get together and argue out how we should correct for the fact that the chambers are not really 12 inches but only 8 inches, because you have to produce the $\Lambda^0$ and then see it. It is an experimental question which has to be settled; I am sure we would have settled it if there was some theoretical prediction that the value is $2.3 \times 10^{-10}$ sec!

Crussard: About the short value of $\Sigma$-lifetime found in emulsion from decays in flight only, I want to mention values which were found last year in Göttingen. I say "mention", because unfortunately I do not have them here but they have been given at the 1957 Venice - Padua Conference. They are comfortably large values — something like 2 or $3 \times 10^{-10}$ sec with a large error; the large error is quite normal for $\Sigma$'s resulting from $K^-$ capture due to the fact that the time of flight of the hyperons is short compared with their lifetime. I also wanted to remark that the $\Sigma$'s from $K^-$ capture are slow, and have therefore an average time of flight shorter than the "normal" $\Sigma$-lifetimes; so that any loss of decays near the end of the range should result in a calculated lifetime of the order of the mean time of flight of such hyperons; that is $\sim 0.5 \times 10^{-10}$ sec.

D. A. Glaser: I know about this difficulty because you told me! I am not an expert on this question, but I asked other emulsion people about it. They were aware of this trouble and in many cases corrected their data by putting in a cut-off.

Crussard: Of course this is not true when a cut-off has been made.

Rochester: Could I ask what errors are quoted in the tables? May I ask that the type of error will be stated in the final report of the conference?

D. A. Glaser: That is a very embarrassing question because in compiling the data we did not know in many cases what errors were being given to us. In most cases the experimenters had used the maximum likelihood method of Bartlett and therefore asymmetric errors were given, but in some cases symmetric errors were given. In computing the mean values of the $\Lambda^0$ and $K^0$ lifetimes, and the $\Lambda^0$-branching ratio, we were therefore afraid to try to weight the values according to the errors quoted on the individual results, and instead we weighted them according to the number of events. This is really not more scientific but the answer seemed more reasonable to us. The final error quoted was put roughly at the least error quoted from all the contributions but was not made smaller than that, because, as you see by the question of Fowler, there is the possibility of a systematic error which is more serious at this moment, I think, than the statistical error.

For the $\Sigma^+$- and $\Sigma^-$-lifetimes and the $K^0$ branching ratio, the errors on the mean values were calculated from the errors quoted by the experimental groups.

W. Wenzel: Glaser stated that in measurements on the lifetime of the $K^-$ there were no counter experiments. I would like to mention that there was one counter measurement done two years ago at the Bevatron. The result is in agreement with the measurements by other techniques.

Okun: Are there any new results on the spectra and angular distribution in leptonic decay of $K^+$ and $K^0$ mesons?

D. A. Glaser: No contributions were given to me on these subjects, and that is why I omitted them.

Discussion on Branching Ratios

Feld: Once when the experimental situation on the $\theta_{\gamma}^\circ$ decays seemed a little more definite, I tried to estimate an upper limit for the $2\pi^0$ decay fraction of the $\theta_{\gamma}^\circ$ by assuming that the same fraction of $\gamma$'s were missed in detecting the neutral decay mode of the $\Lambda^0$ as for the $\theta_{\gamma}^\circ$. In those days, that estimate agreed fairly reasonably with one-third. Now, again, you tell me that $\pi^0$'s seem to be missed in the $\Lambda$-decay. If one makes the same kind of estimate (that is, assumes that one gets a reasonable upper limit for the $2\pi^0$ decay mode of the $\theta_{\gamma}^\circ$ by taking the efficiency of detection of $\pi^0$'s from the $\Lambda^0$ experiment, assuming that the $\Lambda^0$-decay ratio is known), do you have any idea whether one would get agreement with $\Delta T = \frac{1}{2}$ or whether one would still be off?
D. A. Glaser: I do not know exactly how your correction would go. There is a difficulty in doing that owing to the fact that the $\theta^0$ generally makes a wider angle in its decay than the $\Lambda^0$. In a multiparticle chamber I would think the efficiency for seeing the $\gamma$ from the $\Lambda$, might be rather good. However, in the case of the $\theta^0$ it may be a very wide angle decay and the $\gamma$'s may escape between the plates. The geometric argument would be different for the bubble chamber, so I am not sure that an efficiency calculation, based on that assumption, is more useful than that we have already.

Chairman: I would just like to ask one question. What is the best statement one can make now about the branching ratios from $K^0_s$ into the muon mode, the electron mode and the $\tau$ mode?

D. A. Glaser: I would like to beg off; that is a leptonic mode and I have not studied the situation!

Ledermann: The nicest statement one can make is about the things you do not see. Out of 180 $\theta^0$ events, no two-body modes were observed so you can say the branching ratio into $2\pi$'s, or any other two-particle mode you like, are absent to better than 1%. As to the modes you do see, very little is known. You do know that there are 3 modes (i.e. the $\tau^+\tau^-$ mode, and counterparts of the $K^+$ mode, namely $\pi^-\mu^-\nu$ and $\pi^-e^-\nu$), and the ratios are not really known. Probably they are consistent with equal amounts $\pi^-\mu^-\nu$ and $\pi^-e^-\nu$ but the $3\pi$ mode is present less than 20% of the time $^1$.

Gatto: About branching ratios, may I know about the $\tau^+/\tau^-$ ratio?

D. A. Glaser: I do not know anything new about it either.

Chairman: Does anyone have any information on the ratio $\tau^+/\tau^-$? I guess that just means there is no new information.

Discussion of spins

Good: I would like to say one thing which may have been said in the talk, but I am not quite sure whether it was or not, and that is on the Adair analysis of the spin of the $\Lambda^0$. This is subject to the assumption that the spin of the $K^0$ is zero; conversely the Adair analysis of the spin of the $K^0$ is subject to the assumption that the spin of the $\Lambda^0$ is $\frac{1}{2}$. This is more or less the experimental situation with respect to the Adair analysis; if you can assume one of the spins then you can use all the events in which you have the other particle. You do not even have to see the particle whose spin has been assumed. This enables you to use single $V$'s as well as double $V$'s and to say something with a moderate degree of confidence about the spin of one particle subject to this assumption about the spin of the other, so the nice thing about the Lee-Yang trick is that it enables you to say something about the spin of the $\Lambda^0$ without any assumption whatever. So I think a nice approach to follow is to nail down the $\Lambda^0$-spin and go after the $K^0$ spin by the Adair analysis.

Chairman: Any further questions on the spins? If not, we can go on to the parity violation problem.

Burhop: Just to get the record straight, there was one point about an event that you showed of a $\Sigma \rightarrow p$ decay with a Dalitz pair coming from it. This is not the first example of this kind of event as was stated. The European collaboration had such an event and they showed it at the Padua conference last year.

Nataf: About the $(a\bar{P})_{\theta^0}$ you mentioned, I have not understood the connection between the polarization of $\Sigma^0$ and $\Lambda^0$.

D. A. Glaser: This was the experimentally observed asymmetry of $\Lambda^0$ which came from $\Sigma^0$.

Telegdi: There is a semi-experimental point concerning the sign of the $\alpha$ in the $\Lambda^0$-decay. Dalitz has an argument where I believe he can extract this sign from certain branching ratios in the decay of hyperfragments, and maybe he cares to tell you what he gets.

Chairman: Are you sure it is the sign of $\alpha$, or the ratio of the $p$ to $s$ amplitudes?

Telegdi: As far as I recall he gets both but I could not give you the argument.

Treiman: Yes, I think it is just $p$ to $s$.

LIST OF REFERENCES

WEAK INTERACTIONS : NON-LEPTONIC PROCESSES — Theoretical

S. B. TREIMAN, Rapporteur
Princeton University, Princeton (N. J.)

I. Introduction

As elsewhere in particle physics, the problems posed by the weak interactions come in two parts: What are the basic couplings and what symmetries, if any, do they possess? Given these things, how are reliable computations to be carried out? Despite the weakness of the weak interactions, the second problem is of course not a trivial one. With the sole exception of $\mu$ meson decay, all known weak reactions involve at least one particle which can also participate in strong interactions; and it is notorious that strong interactions have not as yet yielded to human computations. We have already encountered these difficulties in our discussions of the leptonic interactions. The troubles are compounded for the non-leptonic interactions.

Despite this, it seems to be the case that all of the weak interactions, leptonic and non-leptonic, are about equally weak. As far as we know they all violate parity and charge conjugation invariance. For most workers in the field these observations provide a kind of boundary condition on attempts to construct a theory of the weak interactions: namely, that at bottom they must all be contained in some sort of more or less universal set of basic interactions.

In this spirit, the idea of a universal, parity non-conserving, $V-A$ Fermi interaction has been much discussed recently. The precise details of such schemes have not yet been fully spelled out—especially as regards strange particle processes. Qualitatively, however, one can certainly construct a reasonable picture of all weak processes in terms of simple sets of Fermi couplings. In fact, lowbrow computations based on such schemes yield remarkably good results in some cases—results which are better than they have a right to be. This is both tantalizing and exasperating.

The weak non-leptonic reactions share in common another feature: they of course all violate isotopic spin conservation. This suggests a more modest goal than a search for a full dynamical scheme. Namely, does $I$-spin violation occur in a capricious way? Or can we discover any surviving isobaric symmetries? From a practical point of view any supposed symmetry principles are of course useless unless they are shared, in good enough approximation, by the strong interactions which unfortunately intervene. On the empirical side, we know that the weak reactions seem to be governed by the $|\Delta S| = 1$ rule, hence $|\Delta I| = \frac{1}{2}$. This has suggested to many people a stronger rule, $|\Delta I| = \frac{1}{2}$. This rule is still with us; it holds up with only moderate success. In recent months a number of attempts have been made to provide a theoretical basis for this rule—and its violation.

A rather separate chapter under the heading of weak interactions concerns the $K_\ell^0-K_\mu^0$ complex. This field is rich with interesting and curious experimental possibilities: interference effects, charge asymmetries, regeneration phenomena, etc. As for new developments in this area, one of the most interesting is a proposal that has been made for a practical way to measure the $K_\ell^0-K_\mu^0$ mass difference, something which appears feasible if this has the expected size $\sim 10^{-40}$ g.

The newest and most striking developments in the field of non-leptonic weak interactions concern, of course, parity non-conservation in $\Lambda$-particle decay. Let us then start our detailed review with this subject.

II. Hyperon decay

The analysis of asymmetries and polarization effect in hyperon decay divides itself into three stages: (a) the kinematics; (b) the implications of $C, P, T$ invariance; (c) underlying dynamical theory.

Parts (a) and (b) have been discussed by a number of people; we follow here the analysis by Lee and Yang. Since that is where most of the data lies, let us consider in particular $\Lambda \to p + \pi^-$ decay.

(a) Assuming only invariance under proper Lorentz transformations, we have for the most general structure of the matrix element

$$\mathcal{M} = F \bar{U}(p) \left[ 1 - \gamma_5 \right] U(\Lambda),$$

where $F$ and $\gamma$ are constants, $U(p)$ and $U(\Lambda)$ are proton and $\Lambda$-spinors. The corresponding rate for $\Lambda \to p + \pi^-$ decay is

$$w = \frac{1}{8\pi} \left| F \right|^2 \left\{ [(M+m)^2 - \mu^2] + \phi \left( (M-m)^2 - \mu^2 \right) \right\} \frac{p_\pi}{M^2},$$

where $M$ is the mass of the $\Lambda$, $m$ is the mass of the $p$, and $\phi$ is a phase constant.

$$w = \frac{1}{8\pi} \left| F \right|^2 \left\{ [(M+m)^2 - \mu^2] + \phi \left( (M-m)^2 - \mu^2 \right) \right\} \frac{p_\pi}{M^2},$$

(2.1)
where $M, m, \mu$ are $A$, proton, and pion masses respectively, and $p_\Sigma$ is the pion momentum.

In Pauli spin space—in the $A$ rest frame—the matrix element (2.0) has the structure (the $\chi$'s are Pauli spinors):

$$\mathfrak{M} = \gamma_{p} \{ A_{s} + A_{p} \sigma \cdot \hat{p} \} \chi_{A}$$

(2.2)

with $\hat{p}$ a unit vector along the proton's line of flight. $A_{s}$ and $A_{p}$ are, up to a common factor, the amplitudes for $s$- and $p$-wave final states; and from (2.0) we have

$$A_{p}/A_{s} = -\varrho \left( \frac{p}{E_{p} + m} \right) \approx -0.052 \varrho.$$  (2.3)

It is now a straightforward matter to show the following things. Let $\Sigma$ be the polarization of the $A$ in its own rest frame; and let $\theta$ be the angle between the polarization direction and the proton's line of flight. Then:

(i) The angular distribution of the proton is:

$$w(\theta) d\cos \theta = \frac{1}{2} (1 - a \Sigma \cos \theta) d\cos \theta$$

$$a = -2 \frac{\text{Re} A_{s} A_{p}^{*}}{|A_{s}|^{2} + |A_{p}|^{2}}.$$  (2.4)

(2.5)

(ii) The expectation value $\langle \sigma \rangle$ of the proton spin in the proton rest frame is given by

$$\langle \sigma \rangle = (1 - a \hat{p} \cdot \Sigma)^{-1} \{ - (a - \hat{p} \cdot \Sigma) \hat{p} + \beta (\hat{p} \times \Sigma)
$$

$$+ \gamma (\hat{p} \times \Sigma) \times \hat{p} \}.$$  (2.6)

with

$$\beta = 2 \frac{\text{Im} A_{s} A_{p}^{*}}{|A_{s}|^{2} + |A_{p}|^{2}},$$

$$Y = \frac{|A_{s}|^{2} - |A_{p}|^{2}}{|A_{s}|^{2} + |A_{p}|^{2}}.$$  (2.7)

If the $A$-particle is unpolarized we have

$$\langle \sigma \rangle = -a \hat{p}.$$  (2.8)

The current experiments indicate that in fact $a \gg 0.7$.

(b) Let us now turn to the implications of $C, P, T$ invariance. It need hardly be said that $P$ invariance implies $A_{s} = 0$ or $A_{p} = 0$, in either case, $a = 0$. As for the other symmetries, we first note that the amplitudes for $A$-decay into $p + \pi^{-}$ can be expressed in terms of $I = \frac{1}{2}$ and $I = \frac{3}{2}$ amplitudes according to

$$A_{s, p} = \sqrt{2} A_{s, p} (^{1/2}) + \sqrt{2} A_{s, p} (^{3/2}).$$  (2.9)

If time reversal invariance is valid, then (up to a common over-all phase factor) the amplitudes for each state of definite $I$-spin and parity is a real number times a factor $e^{i\delta}$, where $\delta$ is the pion-nucleon scattering phase-shift for the state in question. If charge conjugation invariance is valid, the above remarks hold for the $s$-wave amplitudes, but now the $p$-wave amplitudes are real numbers times $i e^{i\delta}$. In this case, to the extent that the small phase-shifts can be neglected, the $s$- and $p$-waves are out of phase and no asymmetries can occur. Taking into account the phase-shift effects, Gatto [1] has shown for $A \rightarrow p + \pi^{-}$ decay that if $C$ invariance is valid the asymmetry parameter $a$ is restricted by $|a| \leq 0.18$. This is well below the lower experimental limit so we can safely conclude that charge conjugation invariance is violated in $A \rightarrow p + \pi^{-}$ decay.

(c) For the rest of the discussion let us suppose that time reversal invariance is valid; and let us neglect the small final state phase-shifts in hyperon decay. Then $A \rightarrow p + \pi^{-}$ decay is characterized by two real constants, $F$ and $\varrho$ (Eq. (2.0)), where $\varrho$ is related to the ratio of $p$- and $s$-wave amplitudes by (2.3). With

$$a = A_{p}/A_{s},$$

(2.10)

we may rewrite (2.3) as

$$\kappa = -0.052 \varrho,$$  (2.11)

and from (2.5) we have

$$a = -2 \frac{\kappa}{1 + \kappa^{2}}.$$  (2.12)

Unfortunately this is a double-valued relationship as regards the determination of $\kappa$ from the experimentally measurable parameter $a$. To illustrate the range of uncertainty that still exists, given say that $a \approx 0.7$, we can only conclude that $0.42 < -\kappa < 2.4$. This means that the basic parameters $F$ and $\varrho$ are still not known with any precision; and in fact a precise determination of $a$ will always lead to two possible sets $(F, \varrho)$—unless $a = 1$.

Nevertheless, we at least know (or at least, so we thought before this conference) that $a$ is positive, and that it is large. We of course also know the $A \rightarrow p + \pi^{-}$ decay rate and the branching ratio $(A \rightarrow p + \pi^{-})/(A \rightarrow n + \pi^{0}) \approx 2$. This is already quite a lot and the question is : what are the theoretical implications?

We should of course discuss such matters in the fuller context of all the weak interactions: but the general situation is well enough known so that we can start testing specific ideas on $A$-decay, where the most experimental information is available.

As a beginning, let us consider not $A$-decay but rather $\pi \rightarrow \mu + \nu$ decay. In the customary view, in so far as one neglects small electromagnetic corrections, one pictures the leptons here as being produced at a single space time point.
— in the sense of Feynman diagrams, at a single vertex. This corresponds to the general belief that the leptons couple together at a single point in the interaction Lagrangian, and that the weak interaction is adequately treated in lowest order perturbation theory. If we further suppose that the lepton coupling is of the V, A type, as in \( \beta \)-decay and \( \mu \)-decay, then the structure of the matrix element is fully specified in the form

\[
\mathcal{M}_\mu = -i \frac{f_\mu}{\mu} \bar{U}_\mu \gamma^\mu (1 + \gamma_5) U_\nu
\]

\[
= f_\mu m_\mu \bar{U}_\mu (1 + \gamma_5) U_\nu , \quad (2.13)
\]

with \( \mu \) the pion mass.

Let us now suppose for \( A \rightarrow p + \pi^- \) decay that here too the external baryons emerge from the weak vertex (an assumption for which there is now no justification). Suppose also, as in \( \pi \) decay, that the coupling type is V-A. Then the matrix element has the structure

\[
\mathcal{M}_A = -i f_A \bar{U}_\mu \gamma^\mu (1 + \gamma_5) U_A . \quad (2.14)
\]

This can be written in the form (2.0) with

\[
F = \frac{f_A}{\mu} (M-m) , \quad \varphi = \frac{M+m}{M-m} = 11.7 . \quad (2.15)
\]

where \( M \) and \( m \) are \( A \) and nucleon masses respectively. According to (2.11) and (2.12) this corresponds to an asymmetry parameter

\[
\alpha \approx 0.9 . \quad (2.16)
\]

This is consistent with the experimental results, as regards magnitude.

An even more remarkable observation is the following one, which has been noted by many people. If we suppose that the \( \pi \rightarrow \mu + \nu \) and \( A \rightarrow p + \pi^- \) decay mechanisms are similar, whatever they are, we would expect that in (2.13) and (2.14) we could set \( f_\mu = f_A \) (at least in the same approximation—unjustified—where we picture the \( A \) and \( p \) as emerging from the weak vertex). If this is done we can, knowing the pion lifetime, predict the \( A \rightarrow p + \pi^- \) decay rate. This comes out to be about one-half the experimental value, which is a very remarkable agreement indeed.

As for the underlying mechanisms, one customarily pictures pion decay as proceeding through pion disintegration into a nucleon-antinucleon pair, the latter annihilating via the \( \mu \) capture Fermi interaction to produce the leptons. This suggests a Fermi interaction as the basic mechanism for \( A \)-decay—more generally, for all weak processes, in so far as we are looking for a unified picture. It is not essential, for our qualitative purposes, to worry about whether Fermi interactions can be truly basic (they are not, conventionally, renormalizable). In the present case one might imagine, say, a "direct" Fermi coupling of the sort \((\pi \Lambda) (\bar{\eta} p)\); and in analogy with \( \beta \)-decay one might suppose that in the interaction Lagrangian the coupling types take on the V-A form, as we assumed above. With such a picture, and adopting the same approximations discussed above, one finds for the \((A \rightarrow p + \pi^-)/(A \rightarrow n + \pi^-)\) branching ratio just the experimental value \( 2:1 \). This simulates the results of the \( A I = 1/2 \) rule; but here in fact both \( A I = 1/2 \) and \( A I = 1/1 \) contribute, a point emphasized by Marshak. 3)

What this all comes to is a remarkably successful picture of \( A \)-decay based on the notion of a Fermi coupling comparable in strength and similar in structure (V-A) to the \( \beta \)-decay Fermi couplings—provided we make the seemingly unjustified assumption that the strongly interacting particles \( A \) and \( p \) emerge directly from the weak vertex. To my knowledge, no one has given an adequate explanation of how such an assumption can be justified; so we are left in doubt as to whether the remarkable agreement with experiment really argues for the model. As for the other strange particle decay processes, there is no point in entering into detailed discussion. It is enough to note that they can all be understood qualitatively in terms of similar Fermi couplings.

III. Non-mesonic hyperfragment decay

The interaction

\[
A + \text{nucleon} \rightarrow \text{nucleon} + \text{nucleon} \quad (3.0)
\]

which we considered above as a possible basic mechanism in strange particle decay, in fact shows up essentially as an observed reaction, though unaccountably it is rarely included in lists of observed weak particle processes. It is just the reaction, whether considered basic or otherwise, which presumably accounts for the bulk of non-mesonic hyperfragment decay. It is the analogue of the \( \mu \) capture reaction \( \mu^- + p \rightarrow n + \nu \). One now believes that the \( A - p \) system does not bind; if hyperdeuterons did form, we would presumably observe the process (3.0) in its pure form.

The most general possible structure for the matrix element describing (3.0) is very complicated. Even if a direct Fermi coupling \((\pi \Lambda) (\bar{\eta} N)\) of the V-A sort is assumed, the matrix element—as distinguished from the interaction Lagrangian—can be expected to be complicated, owing to the fact that all four particles involved are strongly interacting. In the Ruderman - Karplus discussion of non-mesonic hyperfragment decay the process (3.0) is pictured as proceeding via

\[
A + N \rightarrow N' + \pi + N \rightarrow N' + N'' .
\]

The first step describes virtual \( A \)-decay and this part has the structure (2.0). For this, even though we are off the mass shell, it may not be unreasonable to adopt for the
parameters $F$ and $g$ the free decay values. As for the second step, the propagation of the virtual pion and its absorption by the nucleon is usually treated in perturbation theory. The matrix element thus obtained, let us call it $\mathfrak{M}_I$, is

$$\mathfrak{M}_I = \sqrt{2} G \frac{1}{m(M-m)+\mu^2} F \bar{U}_p \gamma_5 U_A \bar{U}_n \gamma_5 U_{P_1},$$

(3.1)

where we are here considering

$$\Lambda + p_1 \rightarrow p_2 + n;$$

the initial particles being supposed at rest.

Let us now suppose that the parameter $g$ is given by the simple-minded theory leading to (2.15). Then from the known $\Lambda \rightarrow p + \pi^-$ rate we can determine $F$. The result can be expressed as

$$|F| \approx \frac{1}{\sqrt{2}} g \beta \mu^3$$

(3.2)

where $G$ is the pion-nucleon coupling constant ($G^2/4\pi \approx 15$) and $g\beta$ is the weak coupling constant of $\beta$-decay (taken as positive here). This leads to the expression

$$\mathfrak{M}_I \approx \pm g \beta \bar{U}_p \gamma_5 U_A \bar{U}_n \gamma_5 U_{P_1}.$$  

(3.3)

The extent to which this result represents a good approximation to the true matrix element depends on the mechanism assumed for $\Lambda$-decay. If, for example, $\Lambda$-decay is supposed to proceed through a direct Fermi coupling, say of the $V$-A type, then there is no reason to omit direct coupling terms in the matrix element—not to mention the various radiative corrections, of which (3.3) is just one example. In lowest order in the strong interactions, the matrix element would be

$$\mathfrak{M}_{II} = g U_p \gamma_5 \left(1 + \gamma_5 \right) U_A \bar{U}_n \gamma_5 \left(1 + \gamma_5 \right) U_{P_1}.$$  

(3.4)

As mentioned, (3.3) represents just one subclass of radiative corrections.

If we suppose that the direct coupling coefficient has about the same strength as the $\beta$-decaying coupling $g\beta$—and the comparison of $\Lambda$- and $\pi$ decay rates suggests this—then we see that $\mathfrak{M}_I$ and $\mathfrak{M}_{II}$ are of comparable importance. The customary treatment, in which only $\mathfrak{M}_I$ is retained, would in the present view seem to be quite unjustified. The true situation could be far more complicated still. To illustrate the relative importance of $\mathfrak{M}_I$ and $\mathfrak{M}_{II}$ we note the result

$$\frac{\sum_{\text{spins}} |\mathfrak{M}_I + \mathfrak{M}_{II}|^2}{\sum_{\text{spins}} |\mathfrak{M}_I|^2} \approx 1 + \left( \frac{g}{g\beta} \right) \left[ \left( \frac{g}{g\beta} \right) \pm 1 \right].$$  

(3.5)

The implications of (3.5) are evident.

About the only thing we can conclude then is that, since the simple model of Ruderman and Karplus seems to predict the right order of magnitude for non-mesonic hyperfragment decay, the direct coupling strength $g$ cannot be too much larger than the $\beta$-decaying coupling strength. But attempts to obtain detailed information about the free $\Lambda$-decay, by studying hyperfragment decay on the Ruderman-Karplus model, seem to be unjustified. We have in mind here attempts to determine the parameter $g$ on the basis of data on the ratio of non-mesonic to mesonic hyperfragment decay.

There is, however, another way that hyperfragment decay can be brought to bear on the question of determining the relative amplitudes for $p$- and $s$-wave $\Lambda$-decay. This has been discussed by Dalitz $^b$, and an outline of the argument has already been presented here at an earlier session. In brief, Dalitz considers mesonic decay of the fragment $\Lambda$H$^4$, which, in an appreciable fraction of the cases, decays via

$$\Lambda$H$^4 \rightarrow \pi^- + \text{He}^4.$$  

If the fragment has spin $J = 0$ this process occurs through the $s$-wave channel; if $J = 1$, through the $p$-wave channel. On the other hand the total $\pi^-$-mesonic decay rate depends on both the $s$- and $p$-wave amplitudes. Let $R$ be the ratio of the above two-body mode relative to all $\pi^-$ decay modes; and let $\kappa = A_p/A_s$ be the $p$- to $s$-wave ratio. Experimentally $R \approx 13/27$. From his detailed analysis Dalitz concludes that for $J = 1$, $R$ cannot exceed 0.25, for any value of $\kappa$. This is 2.5 standard deviations from the experimental value of $R$, hence the conclusion $J \neq 1$. For $J = 0$, the correct value of $R$ can be obtained with $\kappa$ very small. Allowing for an error of two standard deviations Dalitz finds $|\kappa| < 1$. Together with the evidence from up-down asymmetries in $\Lambda$-decay, this leads to the result

$$0.45 < |A_p/A_s| < 1.$$  

IV. Isobaric properties

In the present imperfect state of our understanding, it is often convenient—and not necessarily misleading—to deal separately with the space-time and the isobaric properties of weak interactions. What is common to both "spaces" is the breakdown of certain symmetries valid for strong interactions. Also common to both is the theoretical difficulty that the observed properties of weak processes are the combined effect of both weak and strong couplings. This means that any symmetries possessed by the weak couplings are apt to be obscured unless they are shared in good enough approximation by the strong interactions.

Let us first ask: Do the weak non-leptonic processes possess any observable isobaric regularities? A possible rule, $|\Delta I| = \frac{1}{2}$, has been much discussed. What is the evidence?

The strongest support comes from $\Lambda$-decay, where, to within fairly small errors, one finds for the ratio
(A→p+π−)(A→n+π0) almost exactly the result predicted by the |ΔI| = 1/2 rule. We have already mentioned, however, Marshak's point that this result can also be obtained with a suitable linear combination of |ΔI| = 1/2 and |ΔI| = 3/2 terms; and we have seen just this effect with a special model. For Σ-decay the situation is more complicated. At one time it appeared from the data on \( Σ^+→p+π^0; Σ^-→n+π^− \) that the \(|ΔI| = 1/2\) rule must be violated here. However, with the recognition that parity may not be conserved in these processes, sufficiently many new amplitudes enter into the analysis to permit solutions consistent with \(|ΔI| = 1/2\). But other solutions are also possible. At most then we can only say that Σ-decay does not rule out \(|ΔI| = 1/2\).

The observed branching ratio

\[
\frac{(K^+→π^++π^−+π^0)}{(K^→π^++2π^0)} \approx 4
\]

is sometimes quoted as evidence for \(|ΔI| = 1/2\), a rule which here implies that the final state has isotopic spin unity. This conclusion, however, is not entirely warranted. There are in fact three possible \(I=1\) states. One of these is totally symmetric; the others have intermediate symmetry. Only for the totally symmetric state does the above branching ratio hold. Now the final \(I\)-spin state is indeed likely to be totally symmetric; quite aside from \(I\)-spin selection rules. This is because a totally symmetric \(I\)-spin state corresponds to a totally symmetric spatial state; and the latter is expected, on kinematic grounds, to be dominant in \(K_{3π}\) decay. Now totally symmetric \(I\)-spin states are available only for \(I=1\) and \(I=3\) but not for \(I=2\). So any intrinsic \(|ΔI| = 3/2\) component in \(K_{3π}\) decay would in any case be suppressed kinematically. As for the symmetric \(I=3\) state, the predicted branching ratio is \(1/2\). We can thus only argue from the observed ratio that the \(I=3\) state is not appreciably excited, i.e. that there is no appreciable intrinsic \(|ΔI| = 3/2\) component in \(K_{3π}\) decay. But we cannot rule out an intrinsic \(|ΔI| = 3/2\) component.

Finally, in \(K_{3π}\) decay we have to do with the relative rates for \(K^+→π^++π^−\), \(K^0→π^++π^−\), \(K^0→2π^0\). The marked slowness of \(K^+\) decay relative to \(K^0\) decay, taken by itself, argues in favour of a strong predominance of the \(|ΔI| = 1/2\) component in \(K_{3π}\) decay. On the other hand the observed ratio \(\frac{(K^→2π^0)}{(K^→π^++2π^0)}\) was thought, before this conference, to be much smaller than the value \(1/2\) which follows from the \(|ΔI| = 1/2\) rule. The introduction of an appreciable \(|ΔI| = 3/2\) component seemed essential. As Gell-Mann \(^6\) has shown, if the experimental ratio is smaller than 0.26, as it seemed to be before the conference, even \(|ΔI| = 3/2\) components would have to be introduced. The pre-conference experimental value \(≈ 0.14\) for this ratio requires \(|ΔI| = 1/2\) and \(3/2\) amplitudes of order 0.1 or larger relative to \(|ΔI| = 1/2\). From the point of view of current models of the weak interactions it is especially important to determine whether in fact such a large \(|ΔI| = 3/2\) component really appears in \(K_{3π}\) decay. In any case, it seems to be clear that, although the \(|ΔI| = 1/2\) components may well dominate the weak interactions, appreciable \(|ΔI| = 3/2\) and perhaps even \(5/2\) components may also appear, with amplitudes larger than could be understood in terms of electromagnetic corrections to a basic \(|ΔI| = 1/2\) rule. It is essential here to improve the experimental situation.

In a number of recent papers the viewpoint is adopted that the weak couplings may possess certain "isobaric" symmetries (not, of course, in ordinary isotopic spin space) and that these symmetries are shared, in a certain approximation, by the strong couplings. The practical aim of such investigations is to provide some theoretical basis for the approximate validity of the \(|ΔI| = 1/2\) rule. Now observationally, the strong interactions display only the well-known symmetries in ordinary \(I\)-spin space, so that the supposed weak coupling symmetries are bound to be somewhat distorted by the strong couplings. What one imagines, however, is that the strong interactions can be classed as very strong (VS) and moderately strong (MS) and that the (VS) and the weak couplings share certain symmetries not possessed by the (MS) couplings—an unusual, but conceivable, situation. In the models discussed by d'Espagnat, Prenki and Salam \(^7\) and Takeda \(^8\) matters are so arranged that the \(|ΔI| = 1/2\) rule becomes exact (for actual weak processes) when the (MS) interactions are neglected.

Briefly summarized, what is involved is the following. The (VS) couplings are assumed to be invariant under rotations in a 4-dimensional isobaric space. This is the direct product of two 3-spaces with rotation operators \(I\) and \(K\), \(I\) being the ordinary isotopic spin; \(Q = I_3 + K_3 \equiv M\) is the charge. The irreducible representations \(D(IK)\) have degeneracy \((2I + 1)(2K + 1)\) and the assignments are:

\[
\begin{align*}
(N, Σ) & \quad D(1/2, 1/2) \\
K_3 & = 1/2 \\
K_3 & = -1/2 \\
Σ & = D(1 0) \\
π & = D(1 0) \\
A & = D(0 0)
\end{align*}
\]

The strong interactions are invariant under rotations generated by \(M = I + K\). It is now supposed that the weak interactions are also invariant in \(M\) space, though not in \(I\) space. The (MS) interactions, on the other hand, possess \(I\) but not \(M\) invariance.

In \(M\) space the particles \(Σ\), \(π\) and \(A\) have their usual representations as vector, vector, and scalar respectively. With linear combinations of \(Σ\) and \(N\) one forms a vector \(B\) and a scalar \(B_0\) in \(M\) space; likewise with \(K\) and \(K\) one forms \(K\) and \(K_0\). The postulated invariance of weak interactions means that the couplings must be scalars in \(M\) space formed from the above scalars and vectors.

One can now show the following. (1) A scalar in \(M\) space formed from \(p\) factors \(B, B_0, K, K_0\) corresponds in
ordinary isotopic spin space to \(|\Delta I|\) not larger than \(p/2\).

(2) In any physical process involving a total of \(q\) particles of the type \(N, \Sigma, K\), the isotopic spin change is at most \(|\Delta I| = \frac{1}{2} q\), provided we neglect (MS) interactions. For all physical weak processes, \(q = 1\), hence the \(|\Delta I| = \frac{1}{2}\) rule when (MS) couplings are ignored. This is of course the essential result. With (MS) couplings included \(|\Delta I|\) values up to the maximum appearing in the interaction Lagrangian may appear—this latter is governed in turn by (1) above. In this \(M\) space scheme, Fermi couplings can never produce \(|\Delta I| = \frac{1}{2}\) components.

More generally — and quite aside from these \(M\) space symmetries,— we may note that the Fermi coupling (\(pA\)) discussed earlier in this section also cannot produce \(|\Delta I| = \frac{1}{2}\). In the Fermi coupling framework, \(|\Delta I| = \frac{1}{2}\) requires Fermi couplings involving \(\Sigma\)-particles. It is for this reason that such interest attaches to the question whether \(|\Delta I| = \frac{1}{2}\) terms are really needed.

V. The \(K_s^0 - K_s^0\) complex

The general features here are well known. A wide variety of curious effects have been analysed theoretically over the past few years. Most still remain to be tested experimentally. Here we shall only mention two recent contributions concerning the \(K_s^0 - K_s^0\) situation.

The first has to do with the question of time reversal invariance. The original analysis of Gell-Mann and Pais \(^9\), coming as it did before faith in symmetry principles was shattered, was based on \(C\) invariance. Lee, Oehme, and Yang \(^10\) later presented a more general analysis, free from any restrictions of \(C, P, T\) invariance. But shortly thereafter it was realized by them and others that in fact the original scheme of Gell-Mann and Pais would remain correct, provided only that \(T\) invariance were valid. In particular, the observation of asymmetry in the rates for say \(K^0 \rightarrow e^+ + \pi^- + \nu\) versus \(K^0 \rightarrow e^- + \pi^+ + \bar{\nu}\), a matter discussed in general terms by Lee, Oehme, and Yang, would demonstrate violation of \(T\) invariance. However, since the \(K_s^0\) lifetime is so much longer than the \(K_s^0\) lifetime, and since \(K_s^0\) seems to decay at most very infrequently via two pion modes, such charge asymmetries are in any case expected to be small, independent of \(T\) invariance. The question rather is: do the aforementioned facts in themselves tell us anything about the status of \(T\) invariance in \(K\) decay? This has been discussed by Weinberg \(^11\), who concludes that the situation can hardly be understood without assuming the validity of \(T\) invariance.

The argument, in brief outline, is as follows:

The particles \(K_s^0\) and \(K_s^0\) are linear combinations of \(K^0\) and \(\bar{K}^0\) such that \(K_s^0\) and \(K_s^0\) each has a definite lifetime. Now there are two channels for two-pion decay: \(I\)-spin = 0 and \(I\)-spin = 2. To account for the supposed absence of \(K_s^0 \rightarrow 2\pi\), \(K_s^0\) must be a linear combination of \(K^0\) and \(\bar{K}^0\) for which the \(K^0 \rightarrow 2\pi\) and \(\bar{K}^0 \rightarrow 2\pi\) amplitudes just cancel. This must hold for both the \(I = 0\) and \(I = 2\) channels—

which means, as we shall see, a prescribed phase relation between the \(I = 0\) and \(I = 2\) amplitudes. This phase relationship is a consequence of \(T\) invariance; it would be hard to understand why it holds on any other basis. To be sure, we are supposed here that the \(I = 0\) and \(I = 2\) channels are both operative. The \(|\Delta I| = \frac{1}{2}\) rule in fact rules out decay in the \(I = 2\) state; but the observed branching ratio \((K_s^0 \rightarrow \pi^+ + \pi^-)/(K_s^0 \rightarrow \pi^0 + \pi^0) \neq 2\) indicates that the \(|\Delta I| = \frac{1}{2}\) rule is not valid here or so it seemed at least before this conference.

Let us define \(a_0 e^{i\theta_0}\) and \(a_2 e^{i\theta_2}\) as the amplitudes for \(K^0 \rightarrow 2\pi\) in the \(I = 0\) and \(I = 2\) states. Then the corresponding amplitudes for \(\bar{K}^0 \rightarrow 2\pi\) are \(a_0^* e^{i\theta_0}\) and \(a_2^* e^{i\theta_2}\). Here \(\delta_0\) and \(\delta_2\) are the final state scattering phase-shifts. Let \(K_s^0\) be the linear combination

\[K_s^0 = aK^0 + \beta\bar{K}^0.\]

The absence of \(K_s^0\) decay into \(I = 0\) and \(I = 2\) two-pion states implies

\[aa_0 + \beta a_0^* = 0,\]
\[aa_2 + \beta a_2^* = 0;\]

hence \((a_0/a_2) = (a_0/a_2)^*\). The reality of \((a_0/a_2)\), however, is just a consequence of \(T\) invariance.

We may remark that the arguments given here have a wider applicability. Thus, Okubo \(^12\) has shown that the rates for, say, \(\Sigma^+ \rightarrow p + \pi^0\) and \(\Sigma^- \rightarrow p + \pi^0\) would differ if \(C\) or \(T\) invariance does not hold—assuming decay occurs into both the \(I = \frac{1}{2}\) and \(I = \frac{3}{2}\) channels.

One of the important theoretical parameters which characterize the \(K_s^0 - K_s^0\) complex is the mass difference, \(\Delta m\). Crudely speaking, one expects this to be of order \(h\tau_1 \approx 10^{-40}\) g, where \(\tau_1\) is the \(K_s^0\) lifetime. This order of magnitude is estimated with the assumption that the transition \(K \rightarrow \bar{K}\) is second order in the weak coupling strength. Okun’ and Pontecorvo \(^13\) have, however, made the following interesting observation. If \(|\Delta S| = 2\), transitions are possible in lowest order, then a mass difference some 10\(^6\) times larger would be expected. In any case a measurement of \(\Delta m\) would provide a piece of significant information. It was already brought out in the basic paper of Pais and Piccioni \(^14\) that all of the various \(K_s^0 - K_s^0\) interference effects depend on this mass difference. Fry and Sachs \(^15\) have pointed out that, among the many possible effects, one is especially sensitive to \(\Delta m\) and may be experimentally detectable: namely, the curve which describes the intensity versus time of the \(\bar{K}^0\) component in a beam which at \(t = 0\) was pure \(K^0\). For times short compared to the \(K_s^0\) lifetime, the curve is described by

\[P(\bar{K}^0) = 1 + e^{-t/\tau_1} - 2\cos(\Delta\omega \cdot t) e^{-t/2\tau_1},\]

where, for small momenta, \(\Delta\omega\) is essentially the mass difference.
LIST OF REFERENCES

5. Dalitz, R. H. (to be published)
8. Takeda, G. (to be published)

DISCUSSION — see p. 286.
Treiman asked me to comment briefly on the $K_{\mu_3}$ and $K_{e_3}$ decays, but before doing that, I will take advantage of my position as a Chairman to say a few words about $\Lambda^0$-decay.

In the attempt to develop a universal theory of the weak interactions one considers first a lepton current

$$L = \begin{cases} \bar{\psi}^{(e)} \gamma_\mu (1 + \gamma_5) \psi^{(e)} & \text{or} \\ \bar{\psi}^{(\mu)} \gamma_\mu (1 + \gamma_5) \psi^{(\mu)} & \end{cases}$$

and then two other currents, a strangeness-conserving current $J$ (which could be neutron-proton or any other combination with baryons of the same strangeness) and a strangeness-non-conserving current $G$ (which could be $(\bar{\Lambda}p)$ or $(\bar{\Sigma}n)$, etc.).

All the weak interactions can be considered as due to products of two of these currents; for instance, one would write the $\mu$ decay symbolically as

$$L \uparrow L$$

because this decay involves a combination of two lepton currents. This is then the 4-fermion interaction, which is under discussion.

The $\beta$-decay can be written symbolically as

$$J \uparrow L$$

and, for example, the $K_{\mu_3}$ decay can be written as :

$$G \uparrow L$$

as can also the $K_{e_3}$ decay.

In addition, one can have the combination

$$G \uparrow J$$

which is responsible for $\Lambda$-decay, $K_{2\pi}$ decay and other weak processes not involving leptons.

The problem now is that even if one starts with a $V-A$ interaction (equal values for the two coupling constants), one is going to get effects due to the strong interactions, for processes represented by (2), (3) and (4). In $\beta$-decay the renormalization effects are presumed to be responsible for the value 1.25, which has been found for the ratio of axial vector to vector. And of course these effects will also show up in (3) and (4).

Hence, although one starts with a universal theory for the bare fermions, one should expect renormalization effects to produce deviations from the 1 to 1 ratio. The problem is then to calculate these effects and, as Treiman has just said, so far not very sophisticated methods have been used to make these calculations.

It is very desirable, however, to get some feel for the subject, even if highbrow calculations are difficult to carry out. For example, in connection with $\Lambda$-decay, if one takes $G = \bar{\psi}_p \gamma_\mu (1 + \gamma_5) \psi_A$ and $J = \bar{\psi}_p \gamma_\mu (1 + \gamma_5) \psi_n$, considers diagram (A) below:

and does perturbation calculations with this diagram, one finds the following relation between the transition amplitudes to the $I = \frac{1}{2}$ and $I = \frac{1}{2}$ states of the pion-nucleon system :

$$T_{\frac{1}{2}} = -2\sqrt{2} T_{\frac{1}{2}}.$$

This relation leads exactly to the 2 to 1 ratio for the $p+\pi^-$ to the $n+\pi^0$ mode. Moreover, if we call $\alpha_-$ the asymmetry factor for the proton in the decay $\Lambda \rightarrow p + \pi^-$ and $\alpha_0$ the asymmetry factor for the neutron in the decay $\Lambda \rightarrow n + \pi^0$

and define $\alpha$ as the ratio $\frac{\alpha_-}{\alpha_0}$, the theory predicts $\alpha = 1$.

Hence the old $\Lambda I = \frac{1}{2}$ selection rule and our lowbrow calculations give the same results. Furthermore, we predict that $\alpha_- = -0.88$ and that $\left( \frac{A_2}{A_0} \right) = 0.6$. It appears
therefore that our lowbrow predictions for \( \Lambda \)-decay are in agreement with all existing experiments (in saying this, I am expressing a preference for the M.I.T. measurement of \( \Delta \) rather than that of Berkeley).

Okubo and others at Rochester have tried to go somewhat further with the \( \Lambda \)-decay but their calculations are not completed as yet. A preliminary result of their calculation is, for example, the following: if you consider diagram (B)

\[
\Lambda \rightarrow n + K^0
\]

which is the next complicated diagram after (A), you can show that diagram (B) vanishes for the decay mode \( \Lambda \rightarrow p + \pi^- \), but not for \( \Lambda \rightarrow n + \pi^0 \). It should be remarked that diagram (B) leads to a small asymmetry factor. Okubo has also tried to take into account the final state interactions by setting up a partial dispersion theory for this purpose. Because \( \Lambda \) is in a \( J = \frac{3}{2} \) state, the final pion-nucleon interaction is not very strong in this state and the corrections to diagram (A) are not large. Corrections to diagram (B) have not been evaluated as yet.

It would be very interesting to decide whether the old \( \Delta I = \frac{1}{2} \) selection rule is correct for \( \Lambda \)-decays, or whether one has a combination of \( \Delta I = \frac{1}{2} \) and \( \Delta I = \frac{3}{2} \). To reach a decision, the ratio of the two decay modes should be measured very accurately. Moreover, in a more highbrow calculation one would not expect \( a_0 \) to be the same as \( a_- \). Only in the very lowest order calculation is \( a_0 = a_- \) and one can already see how deviations will occur through higher order diagrams. A good measurement of the asymmetry \( a_0 \) would therefore also provide a crucial test of the theory.

I would like to make a point now about the asymmetry in \( \Sigma \)-decay. If one supposes that the strangeness-non-conserving current obeys the selection rule \( \Delta I = \frac{1}{2} \), one introduces an asymmetry between the coupling of the \( \Sigma \)'s to the nucleon, and of the \( \Lambda \) to the nucleon. If one then thinks of the \( \Sigma \)'s as being coupled to the \( \Lambda \) through the \( \pi \)'s and if one accounts for the decays through these indirect interactions, one can see immediately from the Feynman diagrams that the asymmetries can be much less for the \( \Sigma \)-decays than for the \( \Lambda \)-decay.

In Okun's model, where the \( \Lambda \) is the fundamental particle, in addition to the proton and the neutron, it would also be expected that \( \Sigma \)-decays would be quite different from the \( \Lambda \)-decay. As of now, it is premature to decide if it is the polarization or the intrinsic asymmetry factor of the \( \Sigma \) that is small, but certainly in a theory such as ours there is room for understanding a difference in asymmetry factors between the \( \Sigma \)'s and \( \Lambda \).

The above serves also as an introduction to our brief discussion of the \( K_0 \) and \( K_0 \) decays. Okubo, Sudarshan, Teutsch, Weinberg and myself have investigated several aspects of these decays. We assume for the strangeness-non-conserving baryon current the rule \( \Delta I = \frac{1}{2} \). This means that we can include in this current not only \( \Delta (p) \) but also

\[
\sqrt{\frac{2}{3}} (\Sigma^- n) + \sqrt{\frac{1}{3}} (\Sigma^0 p)
\]

which satisfies the same selection rule. However, \( \Sigma^+ n \) is excluded. Indeed, the \( \Delta I = \frac{1}{2} \) condition encompasses the requirement

\[
\frac{\Delta S}{\Delta Q} = +1
\]

postulated by Feynman and Gell-Mann, in order to forbid \( \Sigma^- \rightarrow n + \pi^+ \). We believe that several good arguments exist for taking \( \Delta I = \frac{1}{2} \) for the strangeness-non-conserving baryon current, but for lack of time I cannot go into these reasons. In Okun's model, \( \Delta I = \frac{1}{2} \) follows automatically since \( \Lambda \) is the only hyperon considered as elementary.

With the \( \Delta I = \frac{1}{2} \) assumption, the decay \( K^+ \rightarrow \pi^0 + \mu^+ + \nu \) can be related to \( K^0 \) decay. The decay of \( K^0 \) is considered to be due to \( G \) taken between \( K^+ \) and \( \pi^0 \), multiplied by the lepton current, i.e.

\[
\langle \pi^0 | G | K^+ \rangle L
\]

and one imposes the \( \Delta I = \frac{1}{2} \) rule on the first factor. The decay \( K^0 \rightarrow \pi^- + \mu^+ + \nu \) is then consistent with \( \Delta I = \frac{1}{2} \) but the decay of \( K^0 \) violates this selection rule, and this mode is therefore forbidden. We find immediately the following relation between the transition probabilities:

\[
W(K^0 \rightarrow \pi^- + \mu^+ + \nu) = 2W(K^+ \rightarrow \pi^0 + \mu^+ + \nu)
\]

Now \( K_1^0 \) and \( K_2^0 \) are related to \( K^0 \) and \( \bar{K}^0 \) as follows:

\[
K_1^0 = \frac{p K^0 + q \bar{K}^0}{\sqrt{p^2 + q^2}}
\]

\[
K_2^0 = \frac{p K^0 - q \bar{K}^0}{\sqrt{p^2 + q^2}}
\]

where \( p = q = 1 \) if time reversal invariance is true; however, this assumption need not be made. Consequently:

\[
W(K_1^0 \rightarrow \pi^- + \mu^+ + \nu) = \frac{|p|^2}{|p|^2 + |q|^2} W(K^0 \rightarrow \pi^- + \mu^+ + \nu)
\]

(5)
and
\[ W(K_2^0 \rightarrow \pi^+ + \mu^- + \nu) = W(K_1^+ \rightarrow \pi^+ + \mu^- + \nu) \ . \] (6)

One can now reverse the sign of the charges and consider the decay \( K_{2,3}^0 \rightarrow \pi^+ + \mu^- + \nu \). In this case, it is \( \bar{K}_0 \) which can undergo the transition, but not \( K_0 \). This leads then to the relationships:
\[ W(K_2^0 \rightarrow \pi^+ + \mu^- + \nu) = \frac{|q|^2}{|p|^2 + |q|^2} W(\bar{K}_0^0 \rightarrow \pi^+ + \mu^- + \nu) \]
\[ = W(K_2^0 \rightarrow \pi^+ + \mu^- + \nu) \ . \] (7)

Because of the theorem by Lee, Oehme and Yang, one has
\[ W(K^0 \rightarrow \pi^- + \mu^+ + \nu) = W(\bar{K}_0^0 \rightarrow \pi^+ + \mu^- + \nu) \] (9)

since there are no strong interactions in the final state. Adding the two relations (5) and (7), one obtains the relation:
\[ W(K_1^+ \rightarrow \pi^+ + \mu^- + \nu) = 2 W(K_2^0 \rightarrow \pi^+ + \mu^- + \nu) \ . \]

One can repeat the argument for the electron modes of decay and one finds analogous identities, e.g.
\[ W(K_1^+ \rightarrow \pi^+ + e^- + \nu) = 2 W(K_2^0 \rightarrow \pi^+ + e^- + \nu) \ . \]

Knowing the decay probabilities of \( K^+ \)—which are measured experimentally—one can then predict the partial lifetimes of \( K_2^0 \). One should therefore find the same ratio for \( K_2^0 \rightarrow \pi^+ + \mu^- + \nu \) and \( K_2^0 \rightarrow \pi^+ + e^- + \nu \;\text{as for} \; K^+ \rightarrow \pi^+ + \mu^- + \nu \;\text{and} \; K^+ \rightarrow \pi^+ + e^- + \nu \),
which is about 1. Taking the lifetime of \( K^+ \) from experiment, one then predicts that the lifetime of \( K_2^0 \) must be smaller than \( 7.5 \cdot 10^{-8} \text{ sec} \). If there were no other modes than
\[ K_2^0 \rightarrow \pi^+ + \mu^- + \nu \rightarrow \pi^+ + e^- + \nu \;\text{one would expect the lifetime to be exactly} \; 7.5 \cdot 10^{-8} \text{ sec}. \]

But since there is some \( 3 \pi \) decay (not more than 20% according to Lederman), one can say that the lifetime should be \( 6-7 \cdot 10^{-8} \text{ sec} \).

D. A. Glaser wrote down \( (9 \pm 3) 10^{-8} \text{ sec} \) for this lifetime, which is consistent with our prediction. It would be most interesting to have an accurate measurement of the \( K_2^0 \) lifetime. Very interesting too would be an exact knowledge of the ratios of the different decay modes.

Kobsev and Okun' have also, on the basis of the \( \Delta I = 1/2 \) selection rule for \( G \), worked out the consequences for the \( K_{\mu 3} \) and \( K_{\mu 4} \) decays with the same results, of course. The \( \Delta I = 1/2 \) rule for \( G \) follows immediately from Okun's model.

A different type of remark concerning the process \( K^+ \rightarrow \pi^+ + \mu^- + \nu \) can be made. Gershtein and Zel'dovich and Feynman and Gell-Mann have pointed out that there is no need to renormalize the vector part of \( J \) if one defines this part of the strangeness-conserving current so that
\[ \partial_\mu G_{\nu \mu} = 0 \ . \] (10)

This condition has the consequence that the coupling constant evaluated from the \( \beta \)-transition in \( O^{14} \) should be the same as from \( \mu \) decay, since the vector parts of the two couplings (1) and (2) can be compared directly. The current \( J \) acts like \( L \) in so far as the vector part is concerned. The axial vector parts of \( J \) and \( L \) cannot be compared directly and Goldberger and Treiman have pointed out that if you write down a similar condition for the axial vector part, i.e. \( \partial_\mu J_{\nu \mu} = 0 \), you run into trouble (e.g. too much pseudoscalar interactions in the \( \beta \)-decay).

One might wish to enquire whether the vector part of \( G \) is divergenceless. Weinberg (from Columbia) and the Rochester group have therefore considered the consequences of the assumption
\[ \partial_\mu G_{\nu \mu} = 0 \ . \]

The most unique consequence we could find is the pion-neutrino correlation from \( K_{\mu 3} \) decay. Of course, the experimentalists will not measure the pion-neutrino correlation directly, but will measure the pion-muon correlation. However, the theoretical results can be expressed more simply in terms of the pion-neutrino correlation.

The most general invariant one can write down for the expectation value of \( G \) is:
\[ \langle \pi | G_{\nu \nu} (x) | K \rangle = i \left( f_v p_K^0 + g_v (p_K^0 - p_\pi^0) \right) e^{i(p_K - p_\pi) \cdot x} \]
where \( f_v \) and \( g_v \) are arbitrary functions of \( (p_K - p_\pi)^2 \) with \( p_K \) and \( p_\pi \) the four-momenta of \( K \) and \( \pi \). The most general expression for the pion-neutrino correlation function then turns out to be, except for certain factors:
\[ U(p, \theta) = \sin^2 \theta + \frac{y^2}{x^2} \left[ 1 + \frac{m_\pi - E}{m_K} \frac{g_v}{f_v} (1 + x \cos \theta) \right]^2 \]
where \( x = \frac{|p|}{m_K - E} \quad y = \frac{m_\pi}{m_K - E} \), \( p \) is the pion three-momentum and \( E \) its energy. If one now assumes a reasonable behaviour for \( g_v/f_v \), one can see that the correlation function \( U \) is constant for low momenta; for high momenta, the square bracket term is small compared to the \( \sin^2 \theta \) term. So for low \( p \) one expects a constant, for high \( p \) a \( \sin^2 \theta \) behaviour. However, if one imposes the condition \( \partial_\mu G_{\nu \mu} = 0 \), one gets a special relationship between the \( g \) and \( f \) functions which removes the \((1/x^2)\) singularity. The new predictions then become \( U \approx \sin^2 \theta \) for small momenta and \( U \sim \text{const.} \) for high momenta.
The entire situation is just inverted. This seems to be a rather striking prediction and it will be very interesting to test it experimentally.

Goldberger and Treiman have also considered the consequences of $\partial_{\mu} G_{\mu} = 0$, in terms of the ratio of the total transition probabilities for the $K_{\mu 3}$ to the $K_{e 3}$ decays. They predict that the ratio $W(K_{\mu 3})/W(K_{e 3}) > 1$ if $\partial_{\mu} G_{\mu} = 0$. This inequality depends on assuming that the muons and electrons are coupled in the same way. The present experiments yield a ratio very close to 1 and if this ratio should turn out to be less than 1, then one could make a statement about either the equality of the muon and electron couplings or the validity of the condition $\partial_{\mu} G_{\mu} = 0$.

Finally, I have been asked to place into the record of this conference three papers which have been contributed with regard to the $K_{\mu 3}$ and $K_{e 3}$ decays. These papers refer to the muon and electron spectra of these decays and propose to test various predictions of the universal $(V-A)$ theory. Gatto points out that universality requires that for a given pion energy the electron spectrum is uniquely predicted and the muon spectrum has only two possible shapes. Kobsarev suggests a method for analysing the $K_{e 3}$ spectrum analogous to Dalitz’s method for $\tau^+ \tau^-$ decay, in order to test the $V-A$ theory. Furuichi, Sawada and Yonezawa calculate the spectra from the $K_{\mu 3}$ and $K_{e 3}$ decays in lowest order perturbation theory for the most general interaction and conclude that existing experiments favour $S, T$ rather than $V, A$. However, in my opinion, the last conclusion is premature in view of the sparsity of experimental data and the approximate character of the calculations.

**DISCUSSION — Treiman and Marshak**

Chairman: The discussion is now open. As our first subject I suggest that we choose the asymmetry in $\Lambda$- and $\Sigma$-decays.

Yamaguchi: I got the same results as Marshak for $\Lambda$-decay. But I was not able to find anything sensible for the $\Sigma^+$s. I just wanted to ask Marshak what result did he find about $\Sigma$-decay?

Marshak: Well, as I said, as far as we have considered this point, we just can see that if you think of the $\Sigma^+$s as being coupled indirectly through $\Lambda$-$\pi$, the diagrams involved do not lead to large asymmetries. But a rigorous calculation has not been done.

Takeda: I would like to make a very simple remark on these asymmetry factors in $\Sigma$- and $\Lambda$-decays and perhaps it may be a too simple-minded one. If we take the Fermi-type coupling as a fundamental one, then in $\Sigma^-$-decay, $\Sigma^-$ goes first into two neutrons and an antiproton and then one of the neutrons and the antiproton go into $\pi$ by certain complicated ways. Here you find two neutrons in the same virtual state. On the other hand, if you do the same thing for the $\Lambda$-decay, there are processes where you would not find identical particles in the same virtual state. There might be a certain correlation between finding two identical particles in the virtual state and the smallness of the asymmetry factor. In fact, you can make a theory which gives this correlation. I think that there is not only one theory, but many theories of this sort, among which there are some interesting possibilities. This correlation was pointed out to me by Tati.

Chairman: $K_{\mu 3}$ and $K_{e 3}$ decays.

D. A. Glaser: I just want to give some experimental encouragement to those who are making theories of the kind we have just heard about. The xenon bubble chamber which is 12 in. in diameter is now being used at Berkeley for an experiment which can perhaps say something about the up-down asymmetry of the neutral mode of $\Lambda$, and we hope later in the summer to attack the problem of the $K_3$ and in particular that of $\pi^0\mu\nu$ angular correlation. The correlations seem to be not too difficult to do with that chamber.

Ruderman: I should like to ask Marshak whether in the same way that the conservation of electric current implies that the measured charge of the physical proton and physical pion must be identical (if the neutron is neutral), does not the assumed conservation of the non-strangeness-conserving current imply a relation between the absolute lifetime for $K^+ \rightarrow \pi^0 + \mu^+ + \nu$ and the absolute lifetime for $\Lambda \rightarrow N + \mu + \nu$? In other words, can one say that the magnitude of the coupling constant of the current $K-\pi$ is essentially the same as the magnitude of the coupling constant for the $\Lambda$-nucleon current when both are coupled to the lepton pair $(\mu, \nu)$ or $(e, \nu)$? How would this compare with the experimental absence of the $\Lambda$-lepton decay?

Treiman: As a far as I correctly understood your question, I believe that no comparison of the kind you imply can in fact be made.

Marshak: I might just point out in this connection that the hypothesis of a divergenceless strangeness non-conserving vector current is in a sense a very formal hypothesis, because it is very difficult to construct such a current and some of us have been toying with a new particle, a neutral vector meson with strangeness, which would be analogous to the photon, in order to be able to construct even approximately such a current. At this stage we have no construction for a divergenceless $G_T$. 


Takeda: I would just like to mention about the electron energy spectrum of $K_{\mu 3}$ which Marshak mentioned. Although the statistics are very poor the preliminary study by Yonezawa, Furuichi and Sawada shows that $A-V$ coupling is not favoured. They made a $\chi^2$ test on the energy spectrum of electrons and if we take the $A-V$ type coupling the $\chi^2$ probability is 0.004; there are some ways to struggle with it, and they can at best get 0.07. On the other hand if we take scalar and tensor interactions we can get nearly 1.

Yamaguchi: If I understand correctly, Furuichi and others noticed the following fact. If you consider the $K_{\mu 3}$ decay and if you look at a muon polarization along the $\pi^0$ direction, then it will be very sensitive to the type of weak interaction. Therefore if you can look at the asymmetry of the muon decay in the plane which is perpendicular to the $\pi^0$ momentum, then you can have a chance to test what the interaction type is.

Marshak: It is perhaps worth remarking that according to the general universal assumption, there should be a difference between the helicity of the muon that comes out from $K_{\mu 3}$ and the helicity of the muon from $K_{\mu 3}$. In the first case, if the antineutrino is an antiparticle you would expect the muon to have a negative helicity, but because of conservation of angular momentum you must send it out with positive helicity. In the second case, because you have a three-body decay, the angular momentum condition does not require an inversion of the helicity of the muon. The prediction therefore is that the helicities of the muons from $K_{\mu 3}$ and $K_{\mu 3}$ will be opposite, and one could perhaps make this measurement without too much trouble.

Chairman: Any question about hyperfragments decays?

Ruderman: I should like to ask Treiman whether or not the hard core repulsion between nucleons might sufficiently suppress the direct interaction in the nonmesonic decay of hyperfragments, so that the internal conversion might perhaps give some clue as to the relative amplitudes of $S$ and $P$.

Treiman: I simply do not know. This is one of the class of difficulties I mentioned. In fact, $A + N \rightarrow N + N$ is one of the most difficult cases we know of—4 strong interacting particles—and my remarks were not intended to add to your matrix element all that is needed to complete the correct picture, they were only to demonstrate that another term, which in fact lowbrow people would have put in alone, is comparable in importance. The true situation is undoubtedly complicated.

Chairman: $A I = 1/2$ "rule" and related subjects.

Sachs: I would like to ask one question for the sake, in this case, of incompleteness. In connection with the $A I = 1/2$ selection rule, I would like to know whether the Berkeley people have any number for the $2\pi^0$ decay mode of the $K^0$.

Good: The answer is "no".

In connection with Marshak's talk, I would like to inquire what is the prediction of the "new $A I = 1/2$ rule" for the branching ratio $K^0 \rightarrow \pi^+ + \pi^-$. Marshak: You mean the ratio

$$r = \frac{W(K^0 \rightarrow \pi^0 + \pi^0)}{W(K^1 \rightarrow \pi^+ + \pi^-) + W(K^0 \rightarrow \pi^0 + \pi^0)}.$$  

Well, this is a question of the combination of the currents $J$ and $G$ which involve the strong interactions, and the answer is that the simple lowbrow calculations give a rather sizeable $A I = 1/2$ contribution.

It might be worth remarking that, if you compare the $K^+$ and $K_S^0$ lifetimes, you get the following: the $K^+$ requires $A I = 3/2$ and if you put in the amplitude for this $A I = 3/2$ transition, compared to the $A I = 1/2$ transition in order to fit the lifetimes, then instead of getting $r = 0.33$ you get something like 0.27. This is really the number which should be compared with experiment rather than 0.33.

Chairman: $K^0$ and $K^S_0$.

Feld: I have a question for Treiman. Does the Weinberg analysis concerning the absence of $2\pi$ decay modes in the $K^S_0$ depend on the assumptions concerning the $\pi-\pi$ interaction?

Treiman: No, I do not think that this analysis does. The argument I gave was my own version. Perhaps Lee would know more about the details. The reality of matrix elements we are testing is the reality after the final state phase-shift is taken out, I mean, write the amplitude as a number times $e^{i\theta}$, where $e^{i\theta}$ is the conventional final state phase-shift factor. Then the reality in question is for the number that appears in front of that in this analysis. It is a little unfamiliar situation, but what comes out is correct, I believe.

Morpurgo: Just for the sake of completeness, I would like to mention that in our review article there is some estimate of the contribution of the 2-pion decay mode to the lifetime of the $K^S_0$.

Sachs: Treiman has remarked that we know little about the momentum dependence of the weak interactions. In that connection the measurement of the $\theta_1$, $\theta_2$ mass difference does shed light, or will, we hope, some day shed light on this question. If you calculate the $\theta_1$, $\theta_2$ mass difference, it depends on an integral over the weak interaction, logarithmically divergent in the simplest case, so that it certainly depends on the way the interaction behaves at high momentum transfers. This statement is only correct, though, if we have the $A S = 1$ selection rule; Okun's suggestion of the $A S = 2$ selection rule, if it turned out to be valid, would destroy this connection.
Feld: There seems to be considerable interest generated in the question of the mass difference between the $K^0_\pm$ and $K_{30}$, and I would like to make a comment in the same spirit as was adopted in the summary by D. A. Glaser of the MIT multi-plate cloud chamber experiment of Caldwell, Pal and Böldt. This work, it was stated, gives a preliminary indication that the mass difference is not zero. In the same spirit, one may conclude that there is also an indication, I think roughly as good as the other, that the mass difference is not large as compared to $h$ divided by the mean life of the $\theta_{11}$. The observation is this: had $\Delta m$ been large as compared to $h/\tau_1$, they would have seen a number of events in which two $\Lambda$'s appear to come out of a single interaction, from the same plate. They saw none of these, and one can then estimate an upper limit for $\Delta m$. Of course, it is very difficult to give an exact number, but I think the authors would probably agree that $\Delta m < 10 h/\tau_1$ might be regarded as a reasonable upper limit.

Panofsky: I would like to point out an experimental consequence of Marshak's remark which I think is as sensitive as some of the other tests. In the immediate neighbourhood of a plate of high density the ratio $K^0_\pm$ relative to $K^0$ would be very substantially depleted due to the nuclear absorption. Therefore according to Marshak's prediction the charge asymmetry of the two decay modes would be quite different for those $\theta_{29}$'s which would start, say, within a decay distance of $\theta_1$, which is of the order of an inch away from the plate, as compared to those starting further away.

Piccioni: I wanted to assess the experimental situation with regard to the $K^0_\pm - K^0_{29}$ mass difference. It is somewhat true that the experiments as of now allow you to set extreme limits to that difference. However, the fact is that while theory predicts that interference effect, the experimental situation so far is that you do not see any indication of it. Of course, if you assume that the interference must be there then you can put limits to the mass difference, but it is not quite so convincing as seeing the effect.

Chairman: Any other topics?

Morpurgo: Does somebody know about the cascade particle production, say from a statistical theory, for an energy of the pion of $5\text{ GeV}$? I mean, there are many other channels open. Is the expected branching ratio in contradiction with the present data or not? This is rather between experiment and theory, but it does not make any difference.

Marshak: It would be a little difficult to make a prediction.

Cerulus: I just want to say that if you take a statistical theory for strange particle production it is probably going to give you very wrong results as we know from the associated $\Lambda$ and $K$ production calculated from a Fermi-type statistical theory.

Oppenheimer: I just want to stress that point: the cascade particle production is orders of magnitudes smaller than that of the $\Lambda$'s and $\Sigma$'s. It is true that there is a large competition. Is this alone enough to explain this fact or is it further evidence either that the $\Xi$ is a peculiar object or that in fact the $K$ couplings which were involved are considerably smaller than we are prepared for? If you do not put in anything for the matrix elements but 1, would you estimate such rarity for the $\Xi$'s?

Marshak: Has anyone done this calculation?

Segrè: W. M. Powell said that no calculation has been done and I think that it is extremely difficult to know what is the probability of finding a $\Xi^-$ if you form it in the bubble chamber under the conditions of his experiment.
SESSION 10
Saturday, 5th July, 1958

Concluding remarks

by J. R. OPPENHEIMER

Chairman C. J. BAKKER
CONCLUDING REMARKS

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This marvellous conference is now over. Even last winter it seemed very doubtful what sort and what length of summary or comment would be appropriate at the end; and I hoped it could be left open, because even the existence of such a comment was not clearly a desirable thing. As it has worked out, in all those parts of our subject in which there is a bit of understanding and much work has been done, the rapporteurs have, it seems to me, provided rather complete and admirable summaries. There are areas where we know very little — extremely high energy collisions, for instance — where little can be done by anyone. It also seems to me that not only the rapporteurs but all participants have been very eager to point out in what directions further study or measurements would be helpful, whether this had to do with settling a controversial theory, or confirming a speculation, or simply getting straight matters which were experimentally inconsistent. I do not see that anything could be served by my coming before this tired audience this afternoon, to have you listen to a summary of what were already summaries of summaries. There may be some regret that there will not be a further talk, but I am clear that there will be relief which far outweighs regret. I believe that Pauli was much pleased, very many long years ago, when it was said of him that he was identical with his caricature. One may say of this meeting that it is identical with its summary.

Still, I will say a few words, partly to demonstrate to you that I am not concealing any great insights which would be helpful, and partly to remove the slight restriction which we have had throughout the sessions, which comes from the fact that they all had a title, and that matters which swept from one to another could not so readily be referred to.

I may be wrong; but my impression is that among the many extremely beautiful experimental results there are few which have the kind of impact, of novelty, which we had with the discovery of the $\pi$-$\mu$ phenomena, or the pion resonance in the 3,3-state or the strange particles and their slow decay. There have been no terribly pressing checks of the extent to which the conservation of strangeness and of isotopic spin hold, and the adequacy of the dispersion relations, within those regions of momentum transfer and mass ratio where they can really be established. But there has been, with one exception to which I will refer, very little indication that these general ideas are wrong. We have had new limits, and impressive — but still calling for the electron beam collisions — of the validity of quantum electrodynamics.

We have learned, I think rather firmly, of one new resonance in the isotopic spin $\frac{1}{2}$ state of the pion-nucleon system. I do not think anyone said “that’s just what I expected” and I do not think anyone said “I don’t see how we can understand that”, because we are beginning even at this energy to reach a point where the methods of describing strong interactions are extremely feeble, and will probably remain so for a while. I think that the most productive of the experimental developments, between last year’s conference and this, has been the revelation of the extent to which, within the framework of the strangeness conserving weak interactions, a reasonable universal interaction describes what is found. The beautiful report of Goldhaber made that clear. Now this has really changed the situation, because this very success has enabled one to ask a great many questions which a year ago we would have been unable to formulate. Some of them are quite crude, and because they involve the strangeness non-conserving parts, are really wide open. We have just heard of how easy it is to get a good impression of $\Lambda$-decay and its coherence with $K_{\pi\pi}$ decay and $\pi$ decay — puzzlingly and disturbingly easy. But the question of the $\Sigma$’s is to my mind not a simple one. To omit them from a universal interaction, that is to omit the two which are allowed by $\Delta I = \frac{1}{2}$, seems to me to introduce a new and unexpected note of arbitrariness in the scheme; and to leave them in leads to results which are hardly reconcilable with observation; and to criticise this prediction on the basis that one has not properly renormalized seems hard to reconcile with the success of one’s adventures with the $\Lambda$, unless one regards that as wholly accidental. I share the views which everyone here has expressed about the depth of the problem of the absence of $\pi$-$\nu$ decay, and I think that the rather unexpected though not disprovable sign of the renormalization effects in axial vector coupling all raise questions about self-energies, the transition from bare to clothed particles, with a sharpness which could not have been made evident a year ago. We have heard arguments for and against an intermediate boson; and surely the search for effects of it, if not directly, on spectra of energetic decays, will be one of the things for which we will all be watching.

But quite beyond this, quite beyond these which are problems that reach into the deep parts of un-understood physics — how does one deal with fields when they are strong, or how does one deal with fields when their effects are
strong, even electromagnetic fields? — quite apart from this, I find the universal interaction puzzling for two reasons. I mean by this only that it seems to raise two questions, the answers to which are not clear. One is: why this form? Chirality invariance and the use of a two-component wave field for spinors both seem to me really just to state the matter, and not in any way to lead us to a unique choice. I also find the limits which must be imposed on the universality of the interaction, and which were discussed yesterday — not only the conservation laws of leptons and baryons, and the conservation laws for isotopic spin as Marshak sketched them, but the requirement that certain decay modes be eliminated by factoring one’s quartic interaction into factors in which the charge always changes — I find these extremely puzzling; and these all seem questions to which we will come back again and again, maybe only by virtue of having become very used to them. I have in mind simply that one cannot substitute a pair neutrino and antineutrino for a pair electron and antielectron, though for all we know the quantum numbers of these two pairs are the same — there is nothing known by which to select one compared to the other.

In many sessions, I would say in almost all, wherever theory was discussed, there came the problem of how to deal with strong interactions. And it is still an unsolved problem in more than one sense. It is not known whether, if one takes the simple processes which are observed and which can be approached very closely by experiment, the virtual Yukawa process, for example — it is still not known whether a field theory describing this exists, and what kind of uniqueness it may have. We had a year ago several reasons to be anxious as to whether a causal theory, of which this would be one sample, could at all be right. One such reason was the very peculiar behaviour of the antinucleon cross-sections, which indicated a possibility of annihilation of antinucleons and nucleons for very remote encounters, which almost runs counter to the simplest ideas of causality. A second was in the problem of the electric structure of nucleons. The first of these has gone away for the time being; it awaits further experiment to see whether such a large range for annihilation really exists. The second has not gone away; and I share Peierls’ expression of doubt that what is observed is something that we really did expect or have reason to expect. I think that all the arguments about causality here have to do with the fact that the form factors — the vertex-functions — are given by integrals which have a lower limit, and that the lower limit is the square of the actual mass for a real process that may occur

\[ F(q^2) = \int \frac{d\sigma^2}{q^2 + q^2} q (\sigma^2). \]

This is the reason why one has supposed that antinucleon pairs do not give a big contribution; it is the reason why one supposed that one could limit the contribution of \( K \) particles and strange particles.

First, these representations have not really been derived for the relevant cases, and they may not even be right; but that seems perhaps not the source of the trouble. They have been derived in all orders of perturbation theory; and they look most plausible. The real trouble is that, if one is wild enough about the behaviour of this quite undefined density \( q \) — that is, if one supposes that a great many things can go on, then one can spread to small values of \( q \), which correspond to large distances, and which thus really constitute the main problem of nucleon structure — one can spread to small values of \( q \) the residues of things which are really violent at high values of \( q \) and about which we, in the nature of things, do not know much. I rather would say that to Peierls’ alternatives that we have a breakdown of local theory, or that there has been some over-interpretation of the experiments, we ought to add a third, and that is that there are real physical events going on of which we do not yet know much. One such has been talked about, and that is the quite unknown interaction of pions with each other. In fact, almost nothing is known directly about pion-pion, pion-\( K \), pion-hyperon or \( K \)-hyperon interactions; and the arguments of global symmetry are all we have to guide us, except for the quite indirect results of hyperfragments, and the indirect results from collisions — indirect because one does not have an adequate theory.

We have heard, and I thought it was one of the high points, of the new possibilities of coming close to the fundamental virtual processes — the Yukawa process and its analogue for \( K \) mesons. If one can extend the dispersion relations, and if one can make the measurements, one has the possibility of coming close to the fundamental processes of the Yukawa type of theory, and I suppose that the next years will see great applications of the new dispersion methods, and great zeal to show that these equations have a rigorous underpinning. At the higher energies, as I have said, and especially at the high energies where cosmic ray people study, we have no adequate theoretical basis for describing collisions. I have the impression that the work at lower energies suggests that the interactions do not increase so much in violence in this range, that one has no right to expect statistical methods or hydrodynamical methods to be very good, except in expressing a kind of ignorance that we may have for a long time as to what is going on.

To me also the logical situation is a little unclear. That I may put in the following way; people are testing dispersion relations in the domain in which they can be derived with rigour in forward scattering for reasonable mass ratios. And that means that there is a finite hope that they will not hold. I do not know what we should conclude from that; and the reason that I do not is that it is not clear to me that theories exist which are not macroscopically causal, but which are macroscopically causal, which more or less preserve the view that things that are far enough apart must not disturb each other, and that the development of a system taken over not too sharp times must unfold so that after the first development and the second you can get the final development as a product of the two components.
We talked a little about whether the introduction of an indefinite metric, and thereupon efforts to exclude the unreal physical states, would lead to trouble with regard either to microscopic or to macroscopic causality. It seems very clear that they will, with regard to microscopic causality; and Bogolyubov tells me that in all of the methods based on his suggestions, and including the work of V. Glaser, the lack of causality is fully macroscopic, and that this is thus not a possible way to go about it. This does not prove that there is no way to go about it; but it does enhance the urgency of the problem. It is further clear that there is nothing one can do about these matters that cannot also be done without an indefinite metric, excepting to think of them in the first place. That is, it may offer an heuristic method of using some elements of correspondence with a theory like quantum electrodynamics, or with Yukawa theory, and finding out how to change it — a correspondence which would be lacking if you simply threw the door open to the most general non-local theories.

Of course, if one finds that one does, in fact, have deviations from the dispersion relations, one will certainly first enquire whether there are any physical phenomena that could have been lurking in the background, as with the finite size of the $\pi$, or thresholds, or reactions, which could occur but which have been forgotten; and one will, I hope, try to see whether the characterization of the analytic behaviour of these amplitudes can be brought into a little closer correspondence, not with a Lagrangian, perhaps, but with the physical processes that can occur, real or virtual, and which should really in the end help to determine its singularities. But I would not know today what to do if, failing this, one had a clear indication that things were not local.

Now all of this is still on the basis of attempts which are quite phenomenological, which have been for the most part guided at every step by experiment, and in which we have learned to do less a priori, rather than more a priori, because of the impossibility of rigorous deductive calculations, at least until now. I believe that it is very much in all our minds that one would like to answer a wider range of questions than can possibly be answered, let us say, by a series of Yukawa theories with the various interactions determined from experiment and the various masses determined from experiment — though this is already, of course, very much further than one is at the moment. One would certainly like to have a bit of understanding of what particles have a rather high degree of stability. One would like to know something about why particles have the strangenesses they do. One would like to know something about why spins are limited, and why charges are limited. One would like to understand, not only the fine structure constant, but the enormous ratio between the coupling constants between different fields. One would like to know the strange small ratio between the electron mass and that of the baryons, or even the mesons. One would like to know a lot of other things — one would like to know why the $\mu$ does exist, and is so different from the electron in mass. It has been very, very dark. We would like to know why, if there is global symmetry, the isotopic scalar $\pi$ meson is missing from this otherwise symmetric array.

I think it is very much the problem, the intention, of Heisenberg’s work to give a basis for answering some of these questions. Apart from the question of mathematical adequacy, that is whether one has a well defined problem, a consistent formulation of a problem, which was discussed perhaps not fully but rather adequately during the sessions, there is another point; that has to do with how much the reduction of the present group of particles to a smaller structure can be made. This is not the question: “Are you going to regard $\lambda$ as built up of a $K$ and $p$, or are you going to regard a $K$ as built up of a $p$ and an anti-$\pi$?” because these may not be answerable questions. It is rather the question: “How many components would a field theory have to have — and I do not here mean by a field theory a necessarily local one — in order to embody the conservation laws that we already know?” More conservation laws will always mean more such fields.

Just to be definite I think of the field as embodying the full symmetry of the proper Lorentz group, and the symmetry of $CP$, so that it might be a Dirac field, or an electromagnetic field. Reading things as they stand today, I do not quite see how one can hope to do this with less than six fields, which are distinguished by something which is not a direct outgrowth of the proper Lorentz group, or so. You may start with the heavy particles; you need one just to formulate the conservation of heavy particles, and you need a second to formulate the conservation of isotopic spin, and you need a third to formulate the conservation of strangeness. It does not too much matter which three you pick. If it should turn out, in exploring Gell-Mann’s suggestions, that the vector part of the strangeness conserving current in weak interactions is not itself conserved, it would, I think, raise rather seriously the question of whether one could regard a $\pi$ meson as properly built up out of nucleon-antinucleon fields. You cannot simply prove it, because also the hyperons come into the picture.

With the light particles it is not the same story, but the number is the same. You need something for the conservation of leptons, let us say a neutrino; you need a categorical difference between neutral and charged leptons, because they do not at all enter in the same way in the weak couplings. If there should be any difference of an absolute sort between $\mu$ mesons and electrons, that would increase the number of fields. You in any case need the oldest field of all, which is light.

I, myself, feel that we are quite far from knowing how to approach these questions, quite far from knowing whether field theories of the sort which have been written, or can be written down, or whether wholly new methods of description, are called for. It has been like that for a long time; yet in the course of that, there has been a vast
enrichment of what we have learned, which has been re­

I would like to say, because I am a typical beneficiary and

I believe, an unparalleled job of organizing and criticizing

visitor, how much it seems to me we owe, first to the con­

and understanding their material; and to the organizers of

tributors, and above all the rapporteurs, who have done,

the conference, and to its chairman, Professor Bakker.

I would like to close with what I think is the code word

of this meeting — " that's all ".

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I believe, an unparalleled job of organizing and criticizing

and understanding their material; and to the organizers of

and above all the rapporteurs, who have done,
As shown by Landau and Pomeranchuk\(^1\), the process of Bremsstrahlung is disturbed by multiple scattering as high-energy electrons go through a condensed medium. As a result, the probability of photon radiation, especially that of low energy photons, is decreased in comparison with the results of the usual Bethe-Heitler theory. If the condition

\[
\omega/E < \left(\frac{E_e}{m}\right)^2 \cdot \frac{E}{m^2 L} = 10^{-8} \varrho E
\]  

(1)

is fulfilled, in which \(\omega\) is the frequency of the photon, \(E\) is the energy of the electron in MeV, \(\varrho\) is the density of the medium, \(E_e = 21\) MeV is the scattering constant and \(L\) is the radiation length, then the radiation intensity according to Landau and Pomeranchuk is expressed by

\[
dI_{L.P.} = \frac{e^2}{2\sqrt{6\pi}} \cdot \frac{E_e}{E} \cdot \frac{\sqrt{\omega}}{\sqrt{L}} \frac{d\omega}{d\omega}
\]

(2)

instead of the Bethe-Heitler formula, whose classical analogue can be written as

\[
dI_{B.H.} = \frac{e^2}{3\pi} \left(\frac{E_e}{m}\right)^2 \cdot \frac{d\omega}{\omega_9} \cdot \frac{L}{L}
\]

(3)

Ter-Mikaelyan\(^2\) has shown that the polarization of the medium leads to an additional weakening of the soft part of the Bremsstrahlung. This effect appears if

\[
\omega \ll \omega_9 \frac{m}{E} = 10^{-5} \sqrt{\varrho}.
\]

(4)

In this region

\[
dI_{T.M.} = \frac{e^2}{3\pi} \left(\frac{E_e}{E}\right)^2 \omega_9 \frac{d\omega}{\omega_9} \cdot \frac{L}{L},
\]

where

\[
\omega_9 = \sqrt{\frac{4\pi\varrho e^2}{m}} \approx 10^{26} \sqrt{\varrho} \text{ sec}^{-1}.
\]

(5)

Detailed formulae, taking into account both these effects and valid at \(E \gg m\) in the whole domain of photon energy, have been derived by Migdal\(^3\).

An experimental study of these effects appears possible when investigating high-energy electromagnetic cascades in the cosmic radiation. The emulsion-stack method presents several advantages for such investigations. In big stacks the registration of electron-photon showers with energies up to \(10^{12}\) eV is relatively efficient. Fig. 1 shows the curves of the Bremsstrahlung intensity \(\frac{dI}{E} = \frac{\omega d\omega}{E}\) in emulsions for \(E = 10^{11}\) eV and \(10^{12}\) eV according to Bethe-Heitler and Migdal (taking into account both effects of the medium). It will be seen that the effect of the medium will be noticeable at \(E = 10^{12}\) eV in the radiation of photons of \(\omega < 10^{9}\) eV. At small distances from the origin of the cascade it is possible to register with a great efficiency the electron pairs of the cascade starting from the energy of \(10^9 - 10^{10}\) eV and higher. Thus the method provides a way to determine experimentally the number of cascade pairs in a comparatively wide range of energy where the effects in question are due to appear. The use of the multiple scattering method gives the possibility of determining the energy spectrum of the pairs in the range up to \(10^9\) eV.

Fig. 1. Bremsstrahlung spectrum in emulsion for electrons of energy of \(10^{11}\) and \(10^{12}\) eV according to Bethe-Heitler and Migdal.
The effect produced by the medium shows all the more if the depth \( t \), in which the measurement is carried out, is small. Too small a depth is undesirable as it decreases the number of results available for statistics. The optimum depth is found to be \( 1 - 1.5 \, t_0 \) where \( t_0 (= L) \) is the radiation length, equal to 2.9 cm in the emulsion.

Experimental spectra, gathered from such depths, cannot be immediately compared with the available results of the cascade theories, not even in the usual Bethe-Heitler variant, since the use of asymptotic formulae for cross-sections of elementary processes is not justified in the soft region of the spectrum at such shallow depths. For this reason we performed a calculation of electromagnetic cascades by the Monte-Carlo method, taking into account accurate (non-asymptotic) cross-sections of the elementary processes. The calculation was performed in two variants: according to the usual Bethe-Heitler theory disregarding the influence of the medium and with a recourse to the formulae given by Migdal\(^3\), accounting for the influence of multiple scattering and medium polarization on the Bremsstrahlung.

In this calculation the primaries were assumed to be electrons with energies \( 10^{11} \) eV and \( 10^{12} \) eV. All particles with energies greater than \( E_{\text{min}} = 1.5 \times 10^9 \) eV were followed. The following elementary processes taking place in nuclear and electron fields of the emulsion matter were taken into account:

Bremsstrahlung, pair production by photons and electrons, Compton effect, photonuclear absorption of photons and ionization. Cross-sections for these processes were calculated taking into account the nuclear composition of the emulsion (which in this case was very similar to that of Ilford G-5).

Spatial distribution of the particles was disregarded, the problem being considered as unidimensional. The resulting data were referred to four depth values: \( t_1 = 1.0 \, t_0 \), \( t_2 = 1.5 \, t_0 \), \( t_3 = 2.1 \, t_0 \) and \( t_4 = 2.8 \, t_0 \). An electronic computer was used. About 100 cascades were computed for each depth and for each energy of primary electron. Fig. 2 shows the differential spectrum of pairs formed at depths up to \( t_1 = 1.0 \, t_0 \) and \( t_2 = 1.5 \, t_0 \) in a shower produced by a primary electron of energy \( E = 10^{12} \) eV. Calculation results according to Bethe-Heitler formulae are given as well as those according to the Migdal formulae. The graph shows that the number of pairs with energies \( < 10^9 \) eV is decreased by a factor 2 to 2.5 owing to the influence of the medium. Fig. 3 and 4 show the integral spectra of electrons which reach the depth \( t = 1.5 \, t_0 \) according to both computations (B-H and M). Results by Arley\(^4\) and Jánossy\(^5\) for these spectra are shown for comparison purposes. Fig. 5 and 6 show the distributions of numbers of cascades as a function of the number of electrons with energies \( > 1.5 \times 10^9 \) eV at the depth \( 1.5 \, t_0 \). From these graphs fluctuations of the number of electrons in the cascade can be derived.

These calculations are valid for a definite medium — the emulsion. Our results can, however, be used for other media if certain assumptions are made.

In certain papers dealing with investigations of electromagnetic cascades in emulsions, a discrepancy between the observed spectra with the results of cascade theories could be noticed. The most detailed study of one cascade with an energy of \( \sim 7 \times 10^{11} \) eV was made by Mięsowicz.
et al. However, the comparison made in this work is again based on the Jánossy curve (see Fig. 3 and 4).

In order to provide an experimental check concerning the influence of the medium on Bremsstrahlung we have performed an investigation of the energy spectra of pairs in electron-photon showers of high energy \(10^{11}-10^{12}\) eV. Table I gives the general data on the emulsion stacks in which the showers were registered. The exposure of the stacks was made at a high altitude in the stratosphere.

### Table I

<table>
<thead>
<tr>
<th>Symbol of shower</th>
<th>Range in one layer of emulsion mm</th>
<th>Stack</th>
<th>Volume of stack litres</th>
<th>Type of emulsion</th>
</tr>
</thead>
<tbody>
<tr>
<td>E-53</td>
<td>3.5</td>
<td>E</td>
<td>0.5</td>
<td>&quot; R &quot; NIKFI</td>
</tr>
<tr>
<td>O-209</td>
<td>12</td>
<td>O</td>
<td>3.0</td>
<td>&quot;</td>
</tr>
<tr>
<td>D-84</td>
<td>9</td>
<td>D</td>
<td>1.0</td>
<td>&quot;</td>
</tr>
<tr>
<td>D-44</td>
<td>6.5</td>
<td>D</td>
<td>1.0</td>
<td>&quot;</td>
</tr>
<tr>
<td>J-109</td>
<td>3</td>
<td>J</td>
<td>part of stack J</td>
<td>ILFORD G-5</td>
</tr>
</tbody>
</table>

In most stacks emulsions of the type " R " NIKFI were used, in which the grain density of relativistic tracks was equal to 27-30 grains/100 \(\mu\). Among the showers recorded in the experiment five were chosen, all of them produced by photons of energy \(E_\gamma > 10^{11}\) eV.

The value \(E_\gamma\) was derived from the energy spectrum of electrons at the depth \(t = 2.5 - 3.0\) \(t_0\). The method was similar to that used by Pinkau and Miśkowicz. The electron spectrum was measured by the multiple scattering method at the above mentioned depth within the radius of 200-300 \(\mu\) around the shower axis. From the number \(N\) of electrons with energies greater than \(\varepsilon = 3 \times 10^8\) eV.

![Fig. 4. As Fig. 3, but for a primary electron energy of \(10^{11}\) eV.](image)

![Fig. 5. Calculated fluctuation of the total number of electrons above 1.5 MeV in a depth 1.5 \(t_0\) for a primary electron energy of \(10^{12}\) eV according to Bethe-Heitler and to Migdal.](image)

![Fig. 6. As Fig. 5 for a primary electron energy of \(10^{11}\) eV.](image)
(after corrections for the spatial distribution of electrons) the energy $E_0 = E_{\gamma}/2$ was determined using the cascade curves, first those of Jánossy \(^5\) and then our own for the depth $t = 2.8 t_0$. The results are listed in Table II:

### Table II

<table>
<thead>
<tr>
<th>Shower</th>
<th>Jánossy $E_0$ eV</th>
<th>Our calculation $E_0$ eV</th>
<th>Chudakov $E_0$ eV</th>
</tr>
</thead>
<tbody>
<tr>
<td>E-53</td>
<td>$[8.4 \pm 7.2] \cdot 10^{11}$</td>
<td>$[1.60 \pm 3.4] \cdot 10^{12}$</td>
<td>$2 \cdot 10^{12}$</td>
</tr>
<tr>
<td>O-209</td>
<td>$[1.0 \pm 0.62] \cdot 10^{12}$</td>
<td>$[1.80 \pm 0.92] \cdot 10^{12}$</td>
<td>$2 \cdot 10^{12}$</td>
</tr>
<tr>
<td>D-84</td>
<td>$[1.8 \pm 1.44] \cdot 10^{12}$</td>
<td>$[3.24 \pm 2.6] \cdot 10^{12}$</td>
<td>$2 \cdot 10^{12}$</td>
</tr>
<tr>
<td>D-44</td>
<td>$[3.3 \pm 1.3] \cdot 10^{12}$</td>
<td>$[7.40 \pm 2.96] \cdot 10^{12}$</td>
<td>$2 \cdot 10^{12}$</td>
</tr>
<tr>
<td>J-109</td>
<td>$[2.6 \pm 0.98] \cdot 10^{11}$</td>
<td>$[4.44 \pm 17.56] \cdot 10^{11}$</td>
<td>$2 \cdot 10^{12}$</td>
</tr>
</tbody>
</table>

In the first case the errors were calculated as follows: from Jánossy's \(^5\) fluctuation curves and the experimental numbers the possible $N_{\text{max}}(> e)$ and $N_{\text{min}}(> e)$ were determined, and from these and from cascade curves it was possible to determine $E_{\text{max}}$ and $E_{\text{min}}$. In the second case, starting from the computed data according to the Migdal formulae, cascade curves $\bar{N}(\epsilon, E_0, t)$, $N_{\text{max}}(\epsilon, E_0, t)$ and $N_{\text{min}}(\epsilon, E_0, t)$ were plotted. $E_{\text{max}}$ and $E_{\text{min}}$ were determined from each curve successively. Values of $N_{\text{max}}(\epsilon, E_0)$ and $N_{\text{min}}(\epsilon, E_0)$ were determined from the curve giving the distribution of the number of cascades as a function of the number of electrons of energy $> e$. The method of determination of $N_{\text{max}}(\epsilon, E_0)$ and $N_{\text{min}}(\epsilon, E_0)$ is schematized in Fig. 7.

Density measurements on the tracks from the first pairs in the showers showed that in three showers of energy $\sim 10^{14}$ eV a decrease of ionization can be observed near the top, attributable to the mutual electron-positron screening (Chudakov-Perkins effect \(^9,10\)). Fig. 8 and 9 show the measured grain densities on the tracks from the first pairs in three showers. The broken line shows the measured densities of relativistic electron tracks near the measured segments of the pair track. The figures show a substantial diminution of the pair track density near the top compared with the track density of the doubly-ionizing particle $n(2I_{\text{min}})$. The effect is observed along several hundreds of microns, which shows that the separation angle between the electron and the positron of the pair is small. Table II gives evaluations of energy of the first pairs according to the Chudakov \(^9\) formula assuming equi-partition of energy among the components of the pair. It may be seen that these evaluations, made by different methods, are in agreement for the pairs D-84 and O-209. In the shower E-53 one of the electrons from the first pair had a considerably lower energy than the other. In fact this shower is produced by one electron with a high energy: this is why
than the zone in which the cascade electrons were recorded. Next, the energy of each pair was determined from the angle of the multiple scattering of the electrons. Basically a 250 μ cell was used. The full noise of second differences was 0.13 μ per 250 μ. Measurements were made with about 20 cells. In these conditions the error of measurement of the electron energy was about 20% up to the energy (5–7) 10^8 eV. In some single cases the determination of the pair energy was made with a cell of 500 μ (noise 0.2 μ) or by measuring the relative multiple scattering. In a few cases owing to unfavourable conditions of multiple scattering measurements, the pair energy was estimated from the separation angle according to the Borsellino formula. In any case it may be considered that the pair energy was measured with sufficient accuracy up to 10^9 eV.

Errors shown on Fig. 10 and 11 were determined in a way similar to that for the electron number \( N(e, E_0, t) \) (see above). The distribution curve of counted cascades against the number \( N_p \) at depths up to 1.5 \( t_0 \) determined the referential limits \( N_{p_{\text{max}}} \) and \( N_{p_{\text{min}}} \) corresponding to a 70% probability (see Fig. 7). For the number of pairs in a given shower we have \( N_p = \bar{N}_p + \Delta_1 \), where \( \bar{N}_p \) is the true mean number of pairs. Correspondingly for the mean number from \( s \) showers:

\[
N_{p_{\text{mean}}} = \bar{N}_p + \delta_1, \quad \delta_1 = \Delta_1 / \sqrt{s}, \quad \delta_2 = \Delta_2 / \sqrt{s}.
\]

Fig. 9. The same as in Fig. 8, but for greater distances from the apex.

Fig. 10. Integral energy spectrum of pairs in showers initiated by electrons by a primary energy of \( 10^{12} \) eV in photographic emulsion. The broken curve is the experimental one, obtained from 5 showers.

Fig. 11. As Fig. 10, but for a primary electron energy of \( 10^{11} \) eV.
Consequently:

\[ \bar{N}_p = N_{p_{\text{mean}}} + \delta_i. \]

This procedure was also used in the determination of the errors in the computed curve.

When comparing experimental and computed curves it is also necessary to take into account errors committed in the determination of shower energies. If the energy assigned to a shower is greater than its true energy there will be a discrepancy between the experimental spectrum and that calculated from the Bethe-Heitler formula, which will have nothing to do with the effects of the medium. Therefore, to prove the effects we have to be certain that the assumed shower energies are not too high. For the three showers with energy \( \approx 10^{12} \) eV, this condition is fulfilled because the Chudakov effect is present.

It may be seen from Fig. 11 that the discrepancy between the curves computed from the formulae of Bethe-Heitler and of Migdal for \( E_0 = 10^{13} \) eV is smaller than the experimental errors. For showers with \( E_0 = 10^{12} \) eV (Fig. 10) the observed discrepancy is somewhat higher than the errors. The sizeable experimental errors shown here may in fact be somewhat too large because of the selection methods of showers according to their energy. Fluctuations at the depths 1.5 \( t_0 \) and 2.8 \( t_0 \) are correlated; therefore, showers with electron numbers close to \( \bar{N} \) at \( t = 2.8 t_0 \) should not have big fluctuations in pair number at the depth \( t = 1.5 t_0 \).

For these reasons we consider that our results confirm in a qualitative way the effect of the influence of the medium on Bremsstrahlung. Our results show that a quantitative verification of the medium effect at \( E_0 \approx 10^{13} \) eV requires an increase of the statistical material by a factor of about 4.

### LIST OF REFERENCES

POLARIZATION IN p-n AND n-p SMALL ANGLE SCATTERING
AT ABOUT 600 MeV †

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V. S. NADEZHDIN, V. J. SATAROV.

Joint Institute for Nuclear Research, Dubna

As is known, results of all experiments on elastic nucleon-nucleon scattering may be expressed as different combinations of five complex scattering amplitudes. The determination of these in the general case requires nine independent experiments 1).

Due to the unitarity of the scattering matrix in the energy region below the threshold of mesons production, the number of the necessary experiments reduces to five 2). In the energy region above 300 MeV, the number of experiments necessary for the determination of the amplitudes increases to 9; that is to 18 if both amplitudes \( A_{pp} \) and \( A_{np} \) are determined in separate analysis of the data. It may be shown that the charge independence of nuclear forces allows one to decrease the number of independent experiments necessary for the determination of \( n-p \) and \( p-p \) amplitudes to 13, provided one carries out a joint analysis of \( n-p \) and \( p-p \) data.

It also follows from charge independence that both \( n-p \) and \( p-p \) scattering are described by 10 complex functions. These functions are connected with the nucleon interaction in the different isotopic spin states and defined in the angular interval \( (0 < \theta < \pi/2) \). Therefore, every pair of similar experiments with \( n-p \) system (in the angular interval \( (0 < \theta < \pi/2) \) and \( n-p \) system (in the interval \( (0 < \theta < \pi) \)) gives information about 3 real functions describing the scattering. Two functions are determined by nucleon interactions in the states with \( T = 0 \) and \( T = 1 \). The third function is the interference between these states. Therefore six pairs of identical experiments with \( n-p \) and \( p-p \) systems and one additional experiment with the \( n-p \) or \( p-p \) system should be done to determine completely (except for a phase factor common for the \( p-p \) and \( n-p \) amplitudes) the \( n-p \) and \( p-p \) amplitudes.

In order to make a combined analysis along the lines proposed above, a system of five experiments discussed by Putzikov et al. 3) must be partly carried out. Furthermore, the other experiments must be chosen carefully, so as to yield a unique solution for the scattering amplitude.(*)

Possibility of using data from \( p-d \)-scattering

In a number of papers proton scattering by neutrons in \( p-d \) collisions has been investigated. Here, however, the question about the validity of using the obtained results instead of the data on free \( n-p \) scattering arises. We have considered earlier the conditions under which the data on \( n-d \) scattering may be used for obtaining cross-sections for elastic neutron-neutron scattering 3). We have made an attempt to obtain nucleon polarization in different types of nucleon-deuteron collisions with a non-relativistic impulse approximation and to determine their connection with the polarization in free \( n-p \) scattering, using a method similar to that applied by Tamor 4).

For the case of the incident nucleon being scattered into the angle \( \theta \) of the lab. co-ordinate system and the states of the other two nucleons remaining undetermined, the following expression for the polarized cross-section \( PQ \) obtains:

\[
(PQ)_{pd}(\theta) = (PQ)_{pp}(\theta) + (PQ)_{np}(\theta) + PQ_{\text{interf.}}(\theta) \cdot I(\theta).
\]

Here \( I(\theta) \) is a function, equal to unity at \( \theta = 0^\circ \) and rapidly decreasing with the scattering angle.

† From this expression one can see that in the angular interval where the integral of \( I(\theta) \) is small the polarized cross-section for \( p-d \) collisions coincides with the sum of the polarization cross-section for \( p-p \) and \( n-p \) collisions (**).

In concluding we may remark that in approximate reconstruction of the nucleon-nucleon scattering amplitudes, the other data on nucleon-deuteron collisions may also be useful, as the corresponding expressions contain combinations of amplitudes that enter only in the most complicated experiments with free nucleons. In particular, the expression for the polarized \( p-d \) elastic cross-section obtained contains beside the usual terms \( \text{Re} \, a \text{e}^* \) also terms of the type \( \text{Re} \, b \text{e}^* \), which enter only into the expression describing the correlation of the polarization in the scattering of the polarized beam.

† Appendix to Session 2. — Experimental I.

(*) The discussion of a system of experiments for a combined analysis as well as the relevant analytical expressions will be given in our paper submitted to JETP.

(**) The measurements performed by us show that at 635 MeV the interference term is already small at \( \theta \geq 8^\circ \).
Results of the experiments and related discussion

In the experiments performed at the Laboratory for Nuclear Problems, at energies in the neighbourhood of 600 MeV, the total n-p and p-p collision cross-sections, the differential cross-section for n-p and p-p elastic scattering and the polarization in the elastic p-p scattering were measured. During the last year we studied the angular dependence of polarization in p-n scattering in p-d collisions ($E_p = 635$ MeV) and the differential cross-sections for elastic scattering of neutrons by free protons at small angles ($E_n = 600$ MeV).
1. Fig. 1 gives the scheme of the experiments on the polarization in \( p-n \) collisions at 635 MeV. Simultaneously with the measurements of the asymmetry in \( p-n \) scattering, the asymmetry in quasi free or free \( p-p \) scattering were measured. The agreement of the values obtained with the asymmetry in free \( p-p \) scattering at the same energy as in \(^9\) served as a criterion that in our experiments there was no spurious asymmetry.

The results on the angular dependence of the polarization in \( p-n \) scattering are given by Fig. 2 \(^(*)\) together with results obtained by the authors at smaller energies \(^{10, 11}\). It is to be noted that in the energy interval 100-300 MeV, the dependence quoted changes sharply. In the energy interval 300-635 MeV the change of that dependence is not so drastic. Although the polarization data in \( n-p \) scattering have a relatively low accuracy, it may be useful to extract from them the polarized cross-sections of the nucleons interacting in different isotopic spin states. The results obtained for three energies of the nucleons are given by Fig. 3. All the “partial” cross-sections are given with the weights with which they enter in the polarized \( p-n \) scattering. It may be seen that the relative contributions of the “partial” polarized cross-sections in \( PQ_{np} \) depend considerably on energy, and that these cross-sections vary differently with energy for \( T = 1 \) and \( T = 0 \) states.

\( PQ_{T = 1} \) rises with the energy, \( PQ_{T = 0} \) drops considerably when the energy rises. The fact that the polarized cross-section for \( T = 0 \) is fairly large at 635 MeV reveals a considerable amount of non-central interactions in these states. The same conclusion for energies 100-300 MeV was made by Wolfenstein \(^{12, 20}\).

The drop of the polarization cross-sections \( PQ_{T = 0} \) together with the previously obtained data on the angular dependence of the elastic scattering in \( T = 0 \) states, and the data on the decrease of the total nucleon cross-sections in these states with the rising energy \(^{13}\) may serve as an additional argument in favour of the possibility of a qualitative description of the nucleon interaction in \( T = 0 \) states by the Born approximation \(^{14}\).

2. The small angle \( n-p \) scattering at \( \sim 600 \) MeV was observed in our laboratory in two different ways. In the first experiments \(^{15}\) the neutrons were registered by a neutron telescope and cross-sections at \( \theta = 11^\circ \) and \( 23^\circ \) were obtained. The second group \(^{16}\) designed and used for this purpose a ring scatterer, containing 100 times more of scattering matter than used in conventional targets. The experimental scheme is shown in Fig. 4. This method allowed us to go into the region of smaller angles and to determine the cross-sections down to \( \theta = 5^\circ \) (see Table).

<table>
<thead>
<tr>
<th>( \theta )</th>
<th>( C. \text{ of } M. )</th>
</tr>
</thead>
<tbody>
<tr>
<td>5°</td>
<td>10 ± 2</td>
</tr>
<tr>
<td>8°</td>
<td>8.2 ± 1.4</td>
</tr>
<tr>
<td>11.5°</td>
<td>6.4 ± 0.9</td>
</tr>
<tr>
<td>23°</td>
<td>4.3 ± 0.5</td>
</tr>
<tr>
<td>35°</td>
<td>3.7 ± 0.2</td>
</tr>
</tbody>
</table>

*\( PQ_{np} (\theta) \) in \( 10^{-22} \text{ cm}^2 \text{/sterad} \)

Fig. 4. Experimental arrangements used for measurements of cross-section for \( n-p \) scattering at small angles.

\(^(*)\) When calculating the polarization in \( p-n \) collisions for 635 MeV, the value of the polarization of the primary proton beam equal to \( P = (58 \pm 3)^\% \) \(^9\) was used.
In the overlapping angular regions the results of these two papers are in good agreement. It can be seen from the data that the n-p scattering cross-section rises rapidly at small angles and already for \(\theta = 5^\circ\) the cross-section exceeds the value for zero angle, predicted by the optical theorem for the opaque sphere, which for 600 MeV is \(5.8 \times 10^{-27}\) cm\(^2\) sterad.

This gives us strong indications to believe that besides the imaginary part of the spin independent term of the neutron-proton scattering amplitude at \(\theta = 0^\circ\) there is also a large contribution from the real part of the spin independent term, as well as non-vanishing spin dependent terms of the amplitude.

This circumstance, together with the considerable polarization in n-p scattering observed by us, leads to the conclusion that it is not possible to consider the neutron-proton scattering at 600 MeV with the black sphere model for the nucleons, as this model does not give the polarization, while the complete forward scattering amplitude is given by the imaginary part of the spin independent term. The same conclusion may be drawn also for p-p scattering at these energies.

3. From the above it may be seen that in spite of some difficulties appearing in the analysis of n-p scattering, at present there is data showing some peculiarities of the n-p interaction. In this connection it is useful to compare the data obtained at various energies. As an example of such a comparison we consider the polarization cross-section for n-p scattering at 90° c.m. At this angle only one term remains in the expression for \(P_{Q_{np}}\), which is determined by the interference between the states with different isotopic spin. Fig. 5 gives the present data on \(P_{Q_{np}} (90^\circ)\) for a variety of energies. It is remarkable that this quantity changes its sign, becoming zero near 200 MeV. From the paper of Signell and Marshak it follows that at 150 MeV both the s-phases decrease, and the phases of all the other waves rise with energy. Comparison of these results with the phase-analysis of the p-p scattering at 300 MeV shows that the \(^1S\) phase changes its sign in the interval 150-300 MeV. On the other hand, according to Wolfenstein, the main contribution to the polarization in n-p scattering near 100 MeV is due to the interference of \(^3S - \, \, ^3D\) waves. If one assumes that also at higher energies there is a considerable contribution from the interference of different waves with the \(^3S\)-waves, one may come to the conclusion that the phase of the \(^3S\)-wave changes its sign near 200 MeV; this means that the phases of both S-waves behave similarly in this energy region.

The authors express their gratitude to L.I. Lapidus, R.M. Ryndin and Ya.A. Smorodinskij for valuable suggestions and discussion.

![Fig. 5. Polarization cross-section for n-p scattering at \(\theta = 90^\circ\) as a function of nucleon energy: \(\bigcirc - 19^\circ\); \(\times - 13^\circ\); \(\triangle - 11^\circ\); \(\bullet - the present work.\)](image-url)
In a first experiment, the relative intensities of protons scattered near the vertical and near the horizontal planes at the second event of a double scattering process have been studied, the first scattering having taken place in the horizontal plane. Protons of kinetic energy 980 MeV were scattered first from carbon at 4° and then from either carbon or polyethylene at 14°. They were detected by an array of scintillation counters which could be rotated about an axis along the flight path of the protons between the two scatterers, and which was arranged so as to detect preferentially the elastic proton-proton scattering from the polyethylene. The ratio of horizontal to vertical scattering was found to be 0.940 ± 0.056, in proton-proton collisions.

In the first experiment the only energy selection was by a threshold detector counting all protons of energy greater than about 500 MeV (Fig. 1). No evidence was found for an asymmetry at either energy and for angles of first scatter of 4° and 5°. The results for the 4° first scatter are shown in Fig. 2 and indicate only small asymmetry \( \frac{R-L}{R+L} = +0.02 \). For first and second scatters both at 5° the asymmetries, \( \frac{R-L}{R+L} \), were

\[-0.016 \pm 0.030 \text{ at 700 MeV and} \]
\[-0.024 \pm 0.024 \text{ at 950 MeV.} \]

Thus this experiment suggests polarization near to zero.

---

† Appendix to Session 2. — Experimental I.
In the second experiment the doubly scattered particles were detected \( a) \) by scintillation counters with no effective energy selection and \( b) \) by these in coincidence with a focusing Cherenkov detector designed to reject protons of energy more than 40 MeV lower than that of the doubly elastically scattered protons. The results are shown in Fig. 3. The energy selective counter shows a definite asymmetry rising to the order of 0.15 at about 8° for the second scattering angle, while in the absence of energy selection the asymmetry is small but significant. The first scattering angle was 4° and the energy 950 MeV.

The beam collimation conditions were different in the two experiments. It is probable that in the second experiment elastic first scattering events were more selectively focused by the fringing field of the synchrotron. The results are compatible with a reasonable extrapolation of the lower energy data from other laboratories. They are not in agreement with the larger asymmetries found in preliminary photographic plate measurements published from this laboratory by Batty and Goldsack, but later plate experiments from Birmingham give results in satisfactory agreement with those reported here.
INTERACTION OF ~ 9 GeV PROTONS WITH NUCLEONS
AND PHOTO-EMULSION NUCLEI †

This work was carried out by three groups of the Joint Institute for Nuclear Research, Dubna

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E. Tsyanov, M. Shafranova, Jao Tsyng-se.

II. B. Bannik, G. Bajatjan, I. Gramenitskij, M. Danysz, N. Kostanashvili, V. Lyubimov, A. Nomofilov,
M. Podgoretskij, E. Skshipchak, D. Tuvdendorge, O. Shahulashvili.


The emulsion stack, containing 100 pellicles of NIKFI-R photo-emulsion of 450 μ thickness and 10 × 10 cm²
surface was exposed in the Synchrophasotron of the High Energy Laboratory of the Joint Institute for Nuclear
Research to the inner proton beam, accelerated to 8.7 GeV. The work was carried out by three groups, who investigated
proton interaction with free and bound nucleons of photo-emulsion (groups II and III), mechanism of proton inter­
action with photo-emulsion nuclei (group I), generation of π⁰ mesons and “strange” particles (group II); besides that,
an attempt was made to investigate proton diffraction scattering on photo-emulsion nuclei (group II).

A. 9 GeV proton interaction with free and bound nucleons
in photo-emulsion

Emulsion pellicles have been scanned along tracks of primary protons. 2366 cases of proton interaction with
nuclei (scattering on the angle less than 5° was not included) in 871 m track length were found. The cases of primary
proton interaction with free protons and protons bound in nuclei were separated on the basis of the following criteria:

a) even number of the tracks;

b) absence of an electron, emitted from the star centre;

c) the range of the secondary particles, except protons from elastic p-p scattering, mesons and hyperons, should exceed 4 mm, which allows exclusion of interaction cases in which evaporation particles are present;

d) the angles of the secondary particles emitted and their energy should not contradict the kinematics of proton
collision with rest proton. The case was considered as proton collision with quasi-free neutron, if the star had odd
number of tracks and satisfied (c) and (d) for selection of p-p events. 205 cases have been selected in this way,
similar to p-p interactions, and 123 cases, similar to p-n interactions referred to below as p-p and p-n events.
Assuming that the number of proton collisions with quasi-free

† Appendix to Session 2. — Experimental II.
the number of fast particles $n_s$ (i.e. particles with ionization $I < 1.4 \sigma_{plateau}$) are given in Table I.

The calculations made according to the statistical theory lead, at $E = 8.7$ GeV, to $\bar{n} = 3.3$ for $p-p$ and $\bar{n} = 3.0$ for $p-n$.

The comparison with the results of the papers $^{10,12}$ indicates that the average number of charged particles emitted in $p-p$ interactions increases rather slowly in the energy range of the incident proton from 3 GeV to 9 GeV. The mean number of charged shower particles emitted in $p-p$ events at 9 GeV is less than the value $\bar{n}_s = 3.4 \pm 0.1$ obtained for proton-nucleus interaction $^{17}$.

The angular distributions in the laboratory system of all charged particles and shower particles are given in Figs. 1, 2 and 3 where on the abscissa is plotted the cosine of the polar angle and on the ordinate the relative number $f(\cos \theta)$ of particles, emitted in a given interval of $\cos \theta$.

The angular distributions of the charged particles from $p-p$ and $p-n$ events in the limits of the statistical errors coincide (Fig. 1). A half of the secondary particles is emitted in the cone with the angle $19.0^\circ \pm 1.5^\circ$ for $p-p$ events and $17.0^\circ \pm 2.4^\circ$ for $p-n$ events. The angular distributions of the prongs in inelastic $p-p$ events for different $n$ (Fig. 2) in the limits of the errors are not distinguishable.

If, in the centre of mass system, all the particles are relativistic ($E > mc^2$) in the case of $p-p$ events, in the laboratory system a half of the particles should emerge in the cone with the angle $23^\circ$. From the comparison of this value with the experimental value obtained it probably follows that there is present in the c.m.s. a number of relatively slow secondary particles. The angular distributions of

<table>
<thead>
<tr>
<th></th>
<th>$p-p$</th>
<th>$p-n$</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$N(n)$</td>
<td>$N(n_s)$</td>
</tr>
<tr>
<td>1</td>
<td>2</td>
<td>3</td>
</tr>
<tr>
<td>0</td>
<td></td>
<td>0</td>
</tr>
<tr>
<td>1</td>
<td></td>
<td>35</td>
</tr>
<tr>
<td>2</td>
<td>90</td>
<td>60</td>
</tr>
<tr>
<td>3</td>
<td></td>
<td>32</td>
</tr>
<tr>
<td>4</td>
<td>81</td>
<td>44</td>
</tr>
<tr>
<td>5</td>
<td></td>
<td>5</td>
</tr>
<tr>
<td>6</td>
<td>15</td>
<td>10</td>
</tr>
<tr>
<td>7</td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>8</td>
<td>2</td>
<td>1</td>
</tr>
<tr>
<td>9</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Mean number of tracks per star

<table>
<thead>
<tr>
<th></th>
<th>$p-p$</th>
<th>$p-n$</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$3.24 \pm 0.08$</td>
<td>$2.69 \pm 0.07$</td>
</tr>
</tbody>
</table>
Fig. 2. The angular distributions of the secondary charged particles in p-p collisions for the cases of different multiplicity (n).

Fig. 3. The angular distributions of the charged shower particles in p-p, p-n collisions and in proton collisions with photo-emulsion nuclei.

The mean free path for nuclear interaction of shower secondary particles from p-p events is equal to \((34 \pm 6)\) cm and from p-n events \((28 \pm 7)\) cm. These values do not differ from the mean free path for proton and \(\pi\) meson interactions with energy of 1 to 6 GeV, and from the preliminary range value for 9 GeV protons which were reported in paper \(^{17}\). According to the data of the present paper the mean free range for 9 GeV proton interactions with nuclei in photo-emulsion is equal to \((37.0 \pm 0.8)\) cm.

B. Proton interaction with nuclei

1. Investigation of \(~9\) GeV proton interaction mechanism with photo-emulsion nuclei.

The interactions with light and heavy photo-emulsion nuclei have been separated from the stars, found by scanning along the track of incident protons. 53 stars on light nuclei and 67 on heavy ones have been selected for

\[ \Sigma^+ \rightarrow n + \pi^+ . \]
Appendix I

Fig. 4. Star distributions by the number of shower particles $n_s$ on light and heavy nuclei. ( ) heavy nuclei $n_s = 3.5 ± 0.3$; ( ) light nuclei $n_s = 3.4 ± 0.3$.

The analysis. All the prongs were divided into three classes according to the measurement of ionization:

1. shower particles (ionization $I < 1.4 I_{plateau}$);
2. grey particles (ionization $I > 1.4 I_{plateau}$ and $R > 3.73$ mm);
3. black particles (range $R ≥ 3.73$ mm, which corresponds to $E = 30$ MeV protons).

Table II gives the mean values for shower, grey and black particles.

Star distribution by shower particles is given in Fig. 4.

The angular distributions of shower particles are given in Fig. 5. From the angular distributions the values of the median angles are obtained:

1. for $p-p$ collisions $\theta_{18} = 18^\circ$ (is given for comparison);
2. for light nuclei $\theta_{25} = 25^\circ$
3. for heavy nuclei $\theta_{28} = 28^\circ$.

The nuclear range for secondary interaction of shower particles obtained was $34 ± 6$ cm.

Fig. 6 gives the energy spectrum for grey particles.

The mean energy value per one particle and star was obtained:

<table>
<thead>
<tr>
<th>Interaction type</th>
<th>$E_{g}$ MeV/prong</th>
<th>$E_{1}$ MeV/star</th>
</tr>
</thead>
<tbody>
<tr>
<td>light nucleus</td>
<td>140 ± 20</td>
<td>400 ± 60</td>
</tr>
<tr>
<td>heavy nucleus</td>
<td>120 ± 15</td>
<td>1160 ± 120</td>
</tr>
</tbody>
</table>

The median angles of grey particle angular distributions were estimated:

for $p$-light nuclei $\theta_{67} = 57^\circ$, for heavy nuclei $\theta_{65} = 65^\circ$.

The mean black particle energy per star was determined by Fig. 5.

The angular distributions of shower particles in the stars on light and heavy nuclei. ( ) heavy nuclei $\theta_{28} = 28^\circ$; ( ) light nuclei $\theta_{25} = 25^\circ$.

<table>
<thead>
<tr>
<th>Interaction type</th>
<th>$n_s$</th>
<th>$n_g$</th>
<th>$n_b$</th>
</tr>
</thead>
<tbody>
<tr>
<td>$p$ - light nuclei</td>
<td>$3.4 ± 0.3$</td>
<td>$1.4 ± 0.2$</td>
<td>$3.2 ± 0.1$</td>
</tr>
<tr>
<td>$p$ - heavy nuclei</td>
<td>$3.5 ± 0.3$</td>
<td>$4.1 ± 0.5$</td>
<td>$6.1 ± 0.6$</td>
</tr>
<tr>
<td>$p$ - mixture of nuclei</td>
<td>$3.2 ± 0.2$</td>
<td>$3.1 ± 0.4$</td>
<td>$4.7 ± 0.5$</td>
</tr>
</tbody>
</table>
the values of the residual ranges of black tracks, taking into account binding energy and neutron emission in the disintegration of light and heavy nuclei:

for light nuclei $E_2 = 63 \pm 6$ MeV,
for heavy nuclei $E_2 = 245 \pm 25$ MeV.

From the results obtained the splitting energy of light and heavy nuclei was estimated: $W = E_1 + E_2$:

$\overline{W}_{\text{light}} = 470 \pm 70$ MeV; $\overline{W}_{\text{heavy}} = 1400 \pm 120$ MeV.

The conclusion was made in the experiments on the cosmic rays that the average splitting energy of the air nucleus is $440 \pm 160$ MeV at 3-40 GeV.

Fig. 6. The energy spectrum of grey prongs in the stars on light and heavy nuclei. (---) heavy nuclei $E = (122 \pm 12)$ MeV/prong, $(1165 \pm 120)$ MeV/star; (-----) light nuclei $E = (138 \pm 14)$ MeV/prong, $(400 \pm 40)$ MeV/star.

An attempt was made to estimate primary particle energy losses for nuclear mixture, taking the average splitting energy of the mean photo-emulsion nucleus $W = 1050 \pm 110$ MeV and shower particle average energy $E_3$. The energy value $E_3$ was obtained from the analysis of the secondary shower particle interactions by three angular intervals using dependence of $n_3$ on the energy for $\pi$ mesons and protons\(^{10}\). The results are given in Table III.

According to the magnitudes $\overline{W}$ and $E_3$ for the emulsion nucleus the average energy loss of the primary proton is $(60 \pm 25)\%$. We note for comparison that, using our average multiplicity of the mesons generated in the proton-nucleon collisions, and the average meson energy calculated theoretically\(^{21}\), the energy loss in $p-p$ collisions is 40-50\% and in paper\(^{19}\) the average energy losses on air nuclei appear to be equal to 30\%.

Going from the angular distribution obtained for the shower particles (Fig. 5), the qualitative discussion of proton interaction mechanism with nuclei is now given. The extension of the median angle, observed in $p$-nucleus interaction in comparison with $p-p$ collisions, may be in agreement with the suggestion that in the light nuclei the proton is subject to 1.5 collisions, and in the heavy nuclei more than 2.

Table IV gives the calculated values of the median angles under the assumption of primary proton collision with different groups of nucleons (tube mechanism). The calculation is carried out assuming isotropy in c.m.s.

### Table IV

<table>
<thead>
<tr>
<th>Number of nucleons in the tube</th>
<th>$\theta_{1/2}$ under different assumptions on the average $\pi$ meson kinetic energy in c.m.s.</th>
</tr>
</thead>
<tbody>
<tr>
<td>50 MeV</td>
<td>140 MeV</td>
</tr>
<tr>
<td>1</td>
<td>$17^\circ$</td>
</tr>
<tr>
<td>2</td>
<td>$24^\circ$</td>
</tr>
<tr>
<td>3</td>
<td>$29^\circ$</td>
</tr>
<tr>
<td>4</td>
<td>$33^\circ$</td>
</tr>
</tbody>
</table>

### Table III

<table>
<thead>
<tr>
<th>$\theta$</th>
<th>From secondary stars $\bar{n}_3$</th>
<th>$E_p^*$</th>
<th>$\bar{E}^*$</th>
</tr>
</thead>
<tbody>
<tr>
<td>0 - $10^\circ$</td>
<td>$1.4 \pm 0.4$</td>
<td>$5.2 \pm 1.7$</td>
<td>$4.0 \pm 0.6_{\pm 0.9}$</td>
</tr>
<tr>
<td>10 - $20^\circ$</td>
<td>$0.7 \pm 0.3$</td>
<td>$1.8 \pm 1.5$</td>
<td>$2.2 \pm 1.4_{\pm 0.9}$</td>
</tr>
<tr>
<td>20 - $180^\circ$</td>
<td>$0.35 \pm 0.15$</td>
<td>—</td>
<td>$0.5 \pm 0.9_{\pm 0.3}$</td>
</tr>
</tbody>
</table>

Note: $E_p^*$, $\bar{E}^*$ — effective energy, if secondary particles are protons or $\pi$ mesons.
Table IV shows that the tube dimensions can be chosen so that the experimental values are in accordance with calculation. However, the calculated values were obtained assuming that the mesons generated in the collision do not interact with nucleons, which contradicts our experimental data on the number of the grey particles and on the splitting energy of the nuclei. Besides that, considerable extension of the angular distribution of the shower particles due to the meson interaction inside nuclei must follow from the value of the average of the nuclear range and the angular distribution. Thus, by meson-nucleon collisions, the co-ordination of the tube model seems to be more complicated with our data than the model of nucleon-nucleon collision.

2. Generation of $\pi^0$ mesons at $\sim 9 \text{ GeV}$ proton interaction with photo-emulsion nuclei.

To study the interaction mechanism of the high energy particles the question about the rate of energy transfer to the secondary $\pi$ mesons is of great interest.

![Fig. 7. Emitted angular distribution of electron-positron pairs (----) and angular distribution of shower particles from stars, found at the scanning along the tracks of primary protons (--.--.--.) and from the stars, found at the scanning of secondary shower particles (--.--.--.--.).](image-url)

Because the direct measurement of fast charged $\pi$ meson energy in photo-emulsion is rather complicated, the average energy of $\gamma$-quanta due to $\pi^0$ meson decay was estimated. For this purpose, electron-positron pairs induced by $\gamma$-quanta were searched. The pairs were found by scanning of separate relativistic tracks.

The tracks with plane angle relative to the beam direction $1^\circ < \theta < 30^\circ$ and the projected length in one plate $l \geq 1600 \mu$ were chosen. The chosen tracks were followed back to the generation point of the pair, star or escaping point from the stack. Similar scanning of the pairs was proposed by King \(^{23}\). It is suitable for our work because it excludes the possible bias of the pairs by the energies. Excluding the inconsiderable background due to Bremsstrahlung $\gamma$-quanta, and $\gamma$-quanta incident on the stack from the outside, the number of pairs found was 93, and the number of the relativistic tracks leading to the stars was 116. In both cases the distributions of angles with respect to the beam are given in Fig. 7. The angular distribution of shower particles in stars, found by scanning along the tracks of the primary protons, was also found. All the distributions coincide within the error limits. Since in the interval of the investigated energies the angular distributions of $\gamma$-quanta and $\pi^0$ mesons are approximately the same, one may consider that the angular distributions of neutral and charged $\pi$ mesons are also close to each other.

The estimation of $\gamma$-quantum average energy can be made by the distribution of angles between pair components, because

$$E_\gamma = K \left( \frac{1}{\theta_\omega} \right).$$

The calculations, based on the data \(^{23}\) and \(^{41}\), show that $K = 4.15$, if the angles are expressed in radians and the energy in MeV.

The measurements of the angular separation were carried out by the method proposed in \(^{23}\), which reduces the influence of the multiple scattering. The mean value of $\gamma$-quanta energy is $E_\gamma = 420 \pm 100$ MeV.

The given error includes the measurement error, inaccuracy in the determination of the coefficient $K$, approximation inaccuracy and statistical error in determination of $\left( \frac{1}{\theta_\omega} \right)$.

To transit to the average energy of $\pi^0$ mesons the ratio of $f = E_{\pi^0}/E_\gamma$ must be estimated. The magnitude depends on the type of $\pi^0$ meson energy spectrum, but this dependence is rather weak. The upper limit of $f$ value under reasonable assumptions on $\pi^0$ meson spectrum equals 1.8. Therefore, the upper limit of the average $\pi$ meson energy equals $(750 \pm 180)$ MeV.

The total energy, transmitted to the all $\pi^0\pm$ mesons is

$$E_\pi = \frac{3}{2} (n_\pi - \bar{a}) E_{\pi^0},$$

where $\bar{a}$ is the average number of the secondary relativistic protons. For proton interaction of the considered energy
with photo-emulsion nuclei \( \bar{n} \) equals 3.4 ± 0.1. If one considers all the shower particles as \( \pi \) mesons then \( \bar{E}_\pi = 3.8 \) GeV.

One should bear in mind that the obtained value \( \bar{E}_\pi \) relates not to all \( \pi \) mesons but only to those which emit in the limits of the solid angle considered. Further, in the calculation one should take into account the presence of the protons among the relativistic particles.

These two considerations lead to reduction of the \( \bar{E}_\pi \) value. On the other hand, the energy connected with black and grey prongs (generally with split nuclei) is partially due to \( \pi \) meson energy and it increases \( \bar{E}_\pi \). The accurate account of the influence of all facts mentioned has not yet been done. In any case, one may think that the energy share of the primary protons transferred to mesons is less than 50\%. The total energy losses (including \( \delta \)-nucleon production) may be greater.

3. Generation of strange particles at 9 GeV proton collisions with photo-emulsion nuclei.

To detect hyperons and \( K \) mesons the tracks of secondary single charge particles produced in nucleus interactions induced by primary protons have been investigated. The mentioned tracks were followed to the rest point, decay, nuclear interaction or to escape from the stack.

The selection of strange particles was made by the decay scheme and nuclear capture. Only the particles which emitted in the forward hemisphere and which satisfied the following two conditions were followed:

- a) The ionisation exceeded that of primary protons not less than 1.6 times (\( \beta \leq 0.64 \));
- b) the projected length in one pellicle \( \geq 3 \) mm, which corresponds to the dip. angle \( \leq 7.5^\circ \).

This last condition excluded also the majority of slow protons and deuterons produced by evaporation from excited nuclei.

The results of 670 followed tracks from 1920 stars are given in the Table V.

Thus, under the conditions considered, one strange particle is approximately produced per 130 secondary particles. The great value of the ratio of the number of strange particles to the number of \( \pi \) mesons

\[
\frac{N_{\Sigma,K}}{N_\pi} \approx \frac{1}{6}
\]

is striking.

In addition to 5 mentioned strange particles, 25 strange particles were found during the scanning along the area, most of which were produced in stars induced by primary protons. The data relating to all found particles are given in Table VI. Only in one case a strange particle was found (\( K^\pm \) meson), which was emitted backwards. In the reaction \( N + N \rightarrow \Sigma + K + N \) the maximal emergence angle of \( \Sigma \) is 44°. Out of the 9 \( \Sigma \)-particles found, 4 emerge under considerably greater angles. This circumstance probably should be connected with the secondary hyperon interactions with nucleons of the parent nucleus.

The stars induced by the primary protons and containing slow strange particles possess a higher number (\( N_h \)) of grey and black tracks. In fact, for this group of the stars we have \( \bar{N}_h = 12.9 \pm 1.6 \) and \( \bar{n}_h = 3.3 \pm 0.5 \), while for the usual stars induced by primary protons \( \bar{N}_h = 8.3 \pm 0.5 \) and \( \bar{n}_h = 3.4 \pm 0.1 \).

An analogous phenomena was mentioned in some other papers. For example in the work \(^{30} \) carried out on \( \pi \) mesons (\( E_\pi \sim 4.3 \) GeV) for the stars containing strange particles, \( \bar{N}_h = 11.5 \pm 0.5 \) and \( \bar{n}_h = 0.9 \pm 0.1 \), compared with the usual values \( \bar{N}_h = 6.5 \pm 0.6 \) and \( \bar{n}_h = 1.7 \pm 0.1 \).

In the stars containing strange particles all prongs were followed. The associated production was found in three cases, in one of which two \( K \) mesons were produced.

Assuming that the ratio between the number of strange particles and usual ones does not depend strongly on the emergence angle, a rough estimation of the generation cross-section of charged strange particles with velocity \( \beta \leq 0.64 \) on photo-emulsion nuclei can be given: per one nucleon \( \sigma_{\Sigma,K} \sim 0.5 \) mb.

4. On the possibility of investigating the 9 GeV proton diffraction scattering on nuclei.

The investigation of elastic scattering of great energy particles on nucleons and on nuclei is a suitable method to study their structure, since under these conditions the quasi-classical approximation turns out to be valid. Unfortunately, the experiments demand a measurement of very small scattering angles.

The method considered allows the investigation of angular distributions up to angles \( \sim 0.2^\circ \) in a way analogous to the measurements of the multiple scattering. In this connection the distances of the track of the primary proton from some fixed straight line (viz. direction of the microscope stage) were measured at various points, and this enabled the dip angle and the scattering angle to be determined.

In the described methods of searching and measurements of small angle scatterings the main shortcoming of the usual scanning along the track, under which the efficiency of scattering detection depends on the angle, is absent.

On the other hand, one should not undertake the search for point scatterings, but should compare the angular distributions of the primary beam obtained in different places of the photoplate, i.e. after going through various thicknesses of photo-emulsion.

The comparison of these distributions gives a possibility of experimentally checking the different assumptions on the character of nuclear scattering.
### Appendix I

#### TABLE V

<table>
<thead>
<tr>
<th>The total number of tracks</th>
<th>The track number without visible phenomena at rest</th>
<th>$\pi$ meson number</th>
<th>Strange particles number</th>
<th>The secondary interaction number</th>
<th>The track number escaped from the stack</th>
</tr>
</thead>
<tbody>
<tr>
<td>670</td>
<td>474</td>
<td>19</td>
<td>5</td>
<td>53</td>
<td>97</td>
</tr>
</tbody>
</table>

#### TABLE VI

<table>
<thead>
<tr>
<th>$NN$</th>
<th>Particle type</th>
<th>Parent star type</th>
<th>Particle energy MeV</th>
<th>The angle between the emergence direction and the primary proton</th>
<th>Remarks</th>
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</table>

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$\Lambda^0$-particle decayed at $\sim 200\mu$ from generation point that permitted determination of the parent star type.
This method can be used with some modification for the investigation of the scattering not only by the photo-emulsion nuclei but also by the nuclei of any other material.

Due to the great distortion of photo-emulsion the angles were measured by a relative method. Pairs of primary protons spaced from each other not more than 50-60 μ were chosen for this purpose.

To compare the projected angle distributions between the particle pairs the measurements at the depth of 5 mm and 95 mm from the edge of the emulsion stack, faced to the beam, were made. About 1200 track pairs were measured in such a way.

The distributions obtained are given in Figs. 8 and 9. It is necessary to take into account the influence of multiple Coulomb scattering in emulsion for their comparison.

The angular distribution at 95 mm allowing only for Coulomb multiple scattering is given in Fig. 9. The difference between this distribution and the experimentally measured one at 95 mm must be considered to be due to the influence of nuclear scattering.

![Fig. 8. Distribution of projected angles of the primary protons at the depth of 5 mm.](image)

As is seen from Fig. 9 the statistics allow only rough analysis. Therefore, the model of the "black sphere" was chosen as the first approximation and the radius of the nuclei contained in the emulsion was assumed to be equal to

$$R_i = r_0 A_i^{1/5}.$$  

The value $r_0 = 1.25 \cdot 10^{-13}$ cm was determined from the mean path for inelastic interaction in photo-emulsion equal to $(34.7 \pm 1.5)$ cm (see $^{17}$).

![Fig. 9. Distribution of projected angles of the protons at the depth of 95 mm. (——) smooth curve = expected distribution calculated by black sphere model; (——) = expected angular distribution obtained allowing only for Coulomb multiple scattering.](image)

The angular distribution, calculated taking into account the nuclear scattering by the "black sphere" model, is given as the lower curve in Fig. 9, and is in a good agreement with experimental data.

When using the second of the above described methods, the angles between track pairs in two points spaced at a distance of 1 cm were measured. If this angle changed more than 0.2°, repeated measurements of the angle after every 2 mm were made.

In such a way 1132 pairs were investigated (the total length of tracks is 22 m) and 31 scattering cases at the angle $> 0.3°$ were found. The corresponding angular distribution and that calculated by the "black sphere" model are plotted in Fig. 10.

The "black sphere" model provides 24 scatterings, which within the error limits are in agreement with the experiment. The results obtained must be considered as preliminary.

![Fig. 10. Distribution of projected angles for 44 scattering cases on the angle 0.2°. Smooth curve = expected distribution calculated by "black sphere" model.](image)
Appendix I

LIST OF REFERENCES

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9. Barashenkov, V. S. et al. (private communication)
10. King, D. T. (private communication)
ANALYSIS OF NUCLEAR INTERACTIONS
OF ENERGIES BETWEEN 10 AND 1000 GeV

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Central Research Institute for Physics, Budapest

Abstract

The angular and energy distribution of shower particles of seven nuclear interactions, mostly complex collisions, were measured. The Lorentz factor $\gamma_{cm}$ of the centre of mass system (c.m.s.) was determined from the energy of secondaries and the Lorentz factor $\gamma_{sym}$ of the symmetry system was obtained from the angular distribution. It was found that $\langle \gamma_{sym} / \gamma_{cm} \rangle = 0.92 \pm 0.15$ which corresponds to a symmetrical emission of shower particles in the c.m.s. Energy distribution in the c.m.s. as well as transversal momentum distribution of shower particles were also determined.

We have measured the angular distribution of shower particles generated in nuclear interactions of cosmic ray particles in emulsion. The nuclear interactions were found by surface scanning in plates of the I stack irradiated in the 1955 Po Valley Expedition. The only requirement in selecting the events was that the energy of almost all shower particles should be measurable, irrespective of the nature of the primary particle and of the number of grey and black prongs. Thus the majority of these events corresponds to nucleon-nucleus or nucleus-nucleus collisions. Our aim was to investigate a) the symmetry of the emission of shower particles in the forward and backward cone in the c.m.s., b) the energy distribution in the c.m.s., and c) the transverse momentum distribution of shower particles.

Symmetry of the angular distribution in the c.m.s.

The velocity of the c.m.s. relative to the laboratory system (LS) was determined in the usual way \(^1\) with

$$\beta = \frac{V}{c} = \frac{\sum E_i}{\sum p_i \cos \theta_i}$$

where $p_i$, $E_i$ and $\theta_i$ are momentum, energy and angle of emission of shower particles, respectively, in the LS. \(^\text{*}\)

From $\beta$ the Lorentz factor of the c.m.s. can be calculated:

$$\gamma_{cm} = \frac{1}{\sqrt{1 - \beta^2}}.$$

As usual all particles with $l < 1.5 l_{\text{min}}$ were considered as shower particles. The energies and momenta of shower particles were determined at lower energies by simple scattering and at higher energies by relative scattering measurements.

We have found seven nuclear interactions in which the energy of almost all shower particles could be measured. However, owing to the great statistical error of the $\gamma_{cm}$ values determined, the deviation from the forward-backward symmetry of the angular distribution indicated large fluctuations in individual cases. Therefore, the best $\gamma_{sym}$ value for each event was taken as the value which transforms the angular distribution measured in the LS in a symmetrical one in the c.m.s., and the ratio $\gamma_{sym} / \gamma_{cm}$ was calculated for each interaction. (Table I.) $\gamma_{sym}$ was determined according to the method of Castagnoli et al.\(^2\)

$$-\ln \gamma_{sym} = \frac{1}{n_s} \sum \ln \tan \theta_i$$

where $n_s$ is the number of shower particles.

It can be seen from Table I that the weighted mean of the $\gamma_{sym} / \gamma_{cm}$ values is

$$\langle \gamma_{sym} / \gamma_{cm} \rangle = 0.92 \pm 0.15$$

and the fluctuation of individual values around this mean value is not significantly higher than normal.

Thus we can conclude that the angular distribution of shower particles generated in nuclear interactions, which are mostly complex collisions and have energies between 10 and 1000 GeV, is symmetrical in the forward and backward direction within the limits of error given above. This result is in good agreement with the hydrodynamical meson production theory of Landau\(^3\) which, also for complex collisions, predicts symmetrical emission of shower particles in the forward and backward directions in the c.m.s.

\(^1\) Appendix to Session 2. — Experimental II.

\(^\text{*}\) The above relation holds only if we assume that the conservation of momentum is valid for charged particles independently of uncharged particles emitted; this assumption can be made only if the number of shower particles is high.
TABLE I

<table>
<thead>
<tr>
<th>Symbol of jets</th>
<th>$\gamma_{cm}$</th>
<th>$\gamma_{sym}$</th>
<th>$\gamma_{sym}/\gamma_{cm}$</th>
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<tr>
<td>$17 + 17\alpha$</td>
<td>$4.6 \pm 1.3$</td>
<td>$3.6 \pm 0.8$</td>
<td>$0.78 \pm 0.27$</td>
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<td>$6 + 11\alpha$</td>
<td>$5.2 \pm 1.6$</td>
<td>$6.5 \pm 1.8$</td>
<td>$1.25 \pm 0.52$</td>
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<tr>
<td>$5 + 10\alpha$</td>
<td>$7.8 \pm 2.7$</td>
<td>$6.9 \pm 2.2$</td>
<td>$0.88 \pm 0.41$</td>
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<tr>
<td>$14 + 13n$</td>
<td>$5.9 \pm 2.1$</td>
<td>$7.7 \pm 1.9$</td>
<td>$1.30 \pm 0.57$</td>
</tr>
<tr>
<td>$17 + 10n$</td>
<td>$4.3 \pm 1.5$</td>
<td>$3.9 \pm 1.1$</td>
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</tr>
<tr>
<td>$26 + 18\alpha$</td>
<td>$5.5 \pm 1.6$</td>
<td>$4.7 \pm 1.0$</td>
<td>$0.85 \pm 0.32$</td>
</tr>
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<td>$1 + 8p$</td>
<td>$23.2 \pm 10.5$</td>
<td>$25.4 \pm 8.1$</td>
<td>$1.09 \pm 0.61$</td>
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</table>

Energy and transverse momentum distribution of shower particles.

Assuming that all particles are mesons, we have calculated from the energies of shower particles measured in the LS and from the values obtained for $\gamma_{cm}$ the energy distribution in the c.m.s. and the transverse momentum distribution for all particles of the seven interactions, (Figs. 1 and 2). The energy distribution has a maximum at an energy somewhat higher than $m_n c^2$ which was predicted by the Heisenberg theory of multiple meson production $^4$. Nevertheless, owing to the large spread in primary energies of individual interactions, the plotting of a composite energy distribution is not justified and thus no conclusion can be drawn from this distribution as to the validity of multiple meson production theories.

The transverse momenta have a mean value of about $0.3$ GeV/c which is in rough agreement with values found by other authors. At such relatively low primary energies, however, the energies of secondary particles are also low and the fact that we have measured only shower particles having $I < 1.5 I_{min}$ affects the form of the energy and transverse momentum distributions. Thus any conclusion drawn from the above distributions is strongly biased by the way of selecting the shower particles. Further investigation of the energy and transversal momentum distribution of shower particles is going on.

Fig. 1. Energy distribution in the c.m.s.

Fig. 2. Transverse momentum distribution.

LIST OF REFERENCES

EVALUATION OF $\gamma_0'$ FROM THE ANGULAR DISTRIBUTION OF JET PARTICLES IN LS USING CERTAIN SUPPOSITIONS ABOUT THEIR ANGULAR DISTRIBUTION IN THE C.M.S. †

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Physical Institute of the Czechoslovak Academy of Sciences.

For the determination of the energy of the primary particle in a nucleon-nucleon collision, in which secondary particles are produced, Dilworth et al.\textsuperscript{1)} on one hand and Castagnoli et al.\textsuperscript{2)} on the other hand developed methods using for the determination of $\gamma_0'$ the angle measurements of shower particles in a jet.

We attempt to find a procedure for the evaluation of $\gamma_0'$ by using for the integral angular distribution of shower particles in a jet an expression $F(u)$, giving its theoretical dependence on $u = \gamma_0' \Theta$, where $\gamma_0'$ represents the primary energy of one nucleon in rest mass units and $\Theta$ the angle of emission of a shower particle, with respect to the primary, observed in the laboratory system.

In Fig. 1 there are plotted, as an example, two distributions $F_2(u)$ and $F_3(u)$ given by von Lindern\textsuperscript{4)} as a function of $u$. In the same figure for the jet (0 + 16) a (No. 3 in Table I) the histogram $H_1$, beginning at the angle $\Theta_1$, and ending at the angle $\Theta_{13}$, and the histogram $H_2$ from $\Theta_3$ to $\Theta_1$ are drawn assuming a value $\gamma_0'$ of 309. The surfaces $P_{H_1}$ and $P_{H_2}$ formed by the histograms $H_1$ and $H_2$ can be calculated from the formulas:

$$P_{H_1} = \gamma_0' \sum_{k=1}^{n_p-1} y_k (\Theta_{k+1} - \Theta_k)$$

$$P_{H_2} = \gamma_0' \sum_{k=1}^{n_p-1} y_k (\Theta_{k+1} - \Theta_k)$$

where $n_p$ denotes the number of shower particles, $u_k = \gamma_0' \Theta_k$ is the value of $u$ corresponding to the track of the $k$-th shower particle in the jet and $y_k = k y_1$ is the $k$-th value of the ordinate corresponding to $\Theta_k$. Then the mean value of both these surfaces is equal to

$$P_M = \frac{1}{2} (P_{H_1} + P_{H_2}) = \frac{1}{2} \gamma_0' S$$

if we denote by $S$ the expression

$$S = \frac{1}{2} \sum_{k=1}^{n_p-1} y_k (\Theta_{k+1} - \Theta_k) + \frac{1}{2} \sum_{k=1}^{n_p-1} y_k (\Theta_{k+1} - \Theta_k)$$

The surface $P_T$ formed by the theoretical curve $F_i(u)$ is given by the expression

$$P_T = \int_{u_1}^{u_p} F_i(u) \, du$$

If we require equality of the mean surface $P_M$ and the theoretical surface $P_T$, we obtain the relation

$$P_M = \frac{1}{2} (P_{H_1} + P_{H_2}) = \int_{u_1}^{u_p} F_i(u) \, du = P_T$$

from which we can determine the value of $\gamma_0'$, supposing the theoretical function for $F_i(u)$ is given.

Using for $F_i(u)$ the relation $F_2(u)$, $F_3(u)$ and $F_5(u)$ given by Szymansik\textsuperscript{3)} and von Lindern\textsuperscript{4)} on the basis of Heisenberg theory\textsuperscript{5)}, we obtain for the evaluation of $\gamma_0'$ the three different equations for the three cases

1) isotropic emission of shower particles in the c.m.s.,

† Appendix to Session 2. — Experimental II.
2) isotropic emission of shower particles and Heisenberg spectrum,
3) strong anisotropic emission in the c.m.s. and Heisenberg spectrum.

From these three equations for cases 1), 2) and 3) the value of $\gamma_0'$ was determined for several jets; the obtained results for three jets with $\gamma_0'$ from 5 to 300 are given in Table I.

From these three examples it is not possible to draw any conclusions. A greater amount of experimental material, i.e. a greater number of jets, would be necessary to prove the usefulness of the described procedure; the evaluation of the error in each case is also desirable. Nevertheless the described way for the evaluation of $\gamma_0'$ gives a further possibility for its determination.

I am indebted to Dr. J. Pernegr for the experimental data of jets.

TABLE I

<table>
<thead>
<tr>
<th>Jet No.</th>
<th>Character of the jet</th>
<th>from $1/\tan \Theta \cdot \frac{1}{2}$</th>
<th>from Castagnoli form</th>
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<td>P 115</td>
<td>(0 + 14)a</td>
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<td>926.0</td>
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<td>181.0</td>
<td>309.0</td>
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LIST OF REFERENCES

\[ \pi^- + p \rightarrow \Lambda^o + \Theta^o \quad \text{ANGULAR DISTRIBUTION OF PRODUCTION} \]
\[ \Lambda^o \rightarrow p + \pi^- \]

AND DECAY at 1.12 GeV/c \(^\dagger\)

F. S. CRAWFORD, Jr., M. CRESTI, M. L. GOOD, K. GOTTSTEIN, E. M. LYMAN, F. T. SOLMITZ
M. L. STEVENSON AND H. K. TICHO

University of California Radiation Laboratory, Berkeley (Cal.)

Summary of maximum likelihood fit to the data

At 1.12 GeV/c, 236 \( \Lambda^o \)-decays arising from \( \pi^- + p \rightarrow \Lambda^o + \Theta^o \)
were studied.

The data were fitted with the expression

\[
dN = e(\theta) [A_1 + A_2 \cos \theta + A_3 \cos^2 \theta + \xi \sin \theta (A_4 + A_5 \cos \theta)] \	imes 2\pi d(\cos \theta) \frac{d\xi}{2} \tag{1}
\]

\( e(\theta) \) = probability that a \( \Lambda^o \) produced at angle \( \theta \) decays
inside the chamber

\( \theta \) = centre of mass production angle of the \( \Lambda^o \)

\( \xi \) = \( [\text{P}_{\text{inc}} \times \text{P}_A \cdot \text{P}_{\text{decay}}] / (\text{magnitude of same}) \).

This expression is what one obtains for production involving final \( s^- \) and \( p^- \)-waves only \(^1\). (The centre of mass
momentum is here 300 MeV/c.)

The likelihood function

\[ \mathcal{L} = \Pi_{ij} \left( \frac{N_{ij}}{\bar{N}_{ij}} \right)^{N_{ij}} \]

was formed and maximized by an iterative procedure.

The solution that maximises \( \mathcal{L} \) is:

\[ A_1 \quad A_2 \quad A_3 \quad A_4 \quad A_5 \]
\[ 21.892 \quad -27.008 \quad 11.944 \quad 14.065 \quad -32.278 \]

With this solution is associated a \( 5 \times 5 \) error matrix \( [\delta A_i, \delta A_j] \)

\[
\begin{array}{ccccc}
1 & 2 & 3 & 4 & 5 \\
1 & 5.6480 & -0.2248 & -9.2172 & +2.2051 & -1.8025 \\
2 & 8.8182 & -7.1432 & -2.0162 & +1.7779 & \\
3 & 30.2028 & -2.0248 & -5.4913 & \\
4 & 11.5237 & -9.5883 & \\
5 & & & & 50.3981 \\
\end{array}
\]

All five \( A \)'s are different from zero outside of experimental error.

The best fit to the data are shown in Fig. 1A, and in Fig. 5 of Steinberger’s report.

From the plot of

\[
\alpha P(\theta) = \frac{\sin \theta (A_4 + A_5 \cos \theta)}{A_1 + A_3 \cos \theta + A_3 \cos^2 \theta} \tag{2}
\]

(Fig. 5) a lower limit to \( \alpha \) can be obtained as follows:

The maximum of \( \alpha P \) is: \( \alpha P_{\text{max}} = 0.73 \pm 0.14 \), where the error is obtained by differentiating (2) and using the error matrix given above.

Then, since the polarization cannot exceed unity, we have a lower limit on \( \alpha \):

\[
|\alpha_{\text{min}}| = 0.73 \pm 0.14
\]

Our data are consistent with any value of \( |\alpha| \) lying between 0.73 and unity.

\(^\dagger\) Appendix to Session 5. — Experimental.
Appendix I

Curve = $N'(\theta) = A_1 + A_2 \cos \theta + A_3 \cos^2 \theta$

Plotted points are

$$N_i(\theta) \frac{1}{2\pi (\Delta \cos \theta = \lambda)} = \frac{0.478 N_i \pm \sqrt{N_i}}{E_i}$$

$E(\theta) = \text{Detection Efficiency}$

---

Fig. 1a: $\pi^- + p \rightarrow \Lambda^0 + \theta^0$ production angular distribution. Curve fitted to the experimental points using $s$- and $p$-waves.

Summary of $s$- and $p$-wave analysis of $\pi^- + p \rightarrow \Lambda^0 + K^0$ at 1.1 GeV/c (236 $\Lambda$-decays)

The paper of Lee et al.\(^1\) gives the theoretical distribution, $W(\theta, \xi)$, in $\xi$ ("the up-down" direction cosine) and $\theta$ of the decay product of the $\Lambda^0$. The assumptions of this paper are (a) that parity is conserved in the production process, (b) but not conserved in the decay of the $\Lambda^0$, and (c) that only $s$- and $p$-waves are present in the final $\Lambda^0, K^0$-state.

Now if $a$ were known we could use the 5 least squares constants $A_1, A_2, \ldots A_5$ of

$$dN = [A_1 + A_2 \cos \theta + A_3 \cos^2 \theta + \xi \sin \theta (A_4 + A_5 \cos \theta)] \times \frac{d\xi}{2} \frac{2\pi d\cos \theta}{N}$$

to determine $a, b, c, \phi$ and $\psi$, where

$$a = ae^{i\phi}, \quad b = be^{i\psi}, \quad c = c.$$ 

In order to obtain $c$, the spin-flip $p$-wave amplitude we must solve a cubic equation. Only two of the three solutions are real. These real solutions are called the $k = 0$ and $k = 2$ solutions. There is, for each of these two solutions, two alternative solutions for the phase angles, $\phi$ and $\psi$. The alternate solution is obtained by reflecting the "normal solution" about the imaginary axis in the complex plane.

$a, b,$ and $c$ have been normalized so that $\int I(\theta) d\Omega = 1$. $\phi$ and $\psi$ are given in radians.
### Appendix I

\[ k = 0 \quad \text{Solution (} \alpha = 0.95^* \text{)} \]

\[
\begin{align*}
\delta a^2 &= 0.001902 \\
\delta a b &= -0.001078 \\
\delta a c &= -0.001214 \\
\delta a \tilde{\psi} &= 0.008778 \\
\delta a \theta_p &= -0.02174 \\
\end{align*}
\]

<table>
<thead>
<tr>
<th>( b = 0.149 \pm 0.044 )</th>
<th>( \delta b^2 )</th>
<th>( \delta b a )</th>
<th>( \delta b c )</th>
<th>( \delta b \tilde{\psi} )</th>
<th>( \delta b \theta_p )</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.001098</td>
<td>0.0005559</td>
<td>-0.005680</td>
<td>0.01167</td>
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<table>
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<tr>
<th>( c = 0.286 \pm 0.033 )</th>
<th>( \delta c^2 )</th>
<th>( \delta c a )</th>
<th>( \delta c \tilde{\psi} )</th>
<th>( \delta c \theta_p )</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.001067</td>
<td>-0.005429</td>
<td>0.01520</td>
<td></td>
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</tbody>
</table>

<table>
<thead>
<tr>
<th>( \tilde{\psi} = 2.34 \pm 0.31 )</th>
<th>( \delta \tilde{\psi}^2 )</th>
<th>( \delta \tilde{\psi} a )</th>
<th>( \delta \tilde{\psi} c )</th>
<th>( \delta \tilde{\psi} \theta_p )</th>
</tr>
</thead>
<tbody>
<tr>
<td>(0.70 \pm 0.31 alt)</td>
<td>0.09767</td>
<td>-0.06420</td>
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</tbody>
</table>

<table>
<thead>
<tr>
<th>( \tilde{\psi} = 5.24 \pm 0.59 )</th>
<th>( \delta \tilde{\psi}^2 )</th>
</tr>
</thead>
<tbody>
<tr>
<td>(4.18 \pm 0.59 alt)</td>
<td>0.34911</td>
</tr>
</tbody>
</table>

\[
\begin{align*}
\delta a^2 &= 0.003945 \\
\delta a b &= -0.002193 \\
\delta a c &= -0.003125 \\
\delta a \tilde{\psi} &= -0.007259 \\
\delta a \theta_p &= -0.03127 \\
\end{align*}
\]

<table>
<thead>
<tr>
<th>( b = 0.212 \pm 0.033 )</th>
<th>( \delta b^2 )</th>
<th>( \delta b a )</th>
<th>( \delta b c )</th>
<th>( \delta b \tilde{\psi} )</th>
<th>( \delta b \theta_p )</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.001743</td>
<td>0.001584</td>
<td>0.004829</td>
<td>0.01608</td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>( c = 0.204 \pm 0.053 )</th>
<th>( \delta c^2 )</th>
<th>( \delta c a )</th>
<th>( \delta c \tilde{\psi} )</th>
<th>( \delta c \theta_p )</th>
</tr>
</thead>
<tbody>
<tr>
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<td>0.002810</td>
<td>0.005371</td>
<td>0.02623</td>
<td></td>
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</tbody>
</table>

<table>
<thead>
<tr>
<th>( \tilde{\psi} = 2.37 \pm 0.25 )</th>
<th>( \delta \tilde{\psi}^2 )</th>
<th>( \delta \tilde{\psi} a )</th>
<th>( \delta \tilde{\psi} c )</th>
<th>( \delta \tilde{\psi} \theta_p )</th>
</tr>
</thead>
<tbody>
<tr>
<td>(0.77 \pm 0.25 alt)</td>
<td>0.06358</td>
<td>0.01102</td>
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<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>( \tilde{\psi} = 5.09 \pm 0.58 )</th>
<th>( \delta \tilde{\psi}^2 )</th>
</tr>
</thead>
<tbody>
<tr>
<td>(4.30 \pm 0.58 alt)</td>
<td>0.3314</td>
</tr>
</tbody>
</table>

\* (*) From the plot of \( a, b, \) etc. vs \( a \) we see that these quantities are not rapidly varying and are almost independent of the value of \( a \).
Graphical representation for $a = 0.95$

$$s\text{-wave cross-section}$$

$$\sigma_{s\text{-wave}} = \sigma_{\text{tot}} \frac{\left|a\right|^2}{\left|a^2 + \frac{1}{2}b^2 + \frac{3}{4}c^2\right|}$$

\[
\begin{array}{c|c|c}
\hline
& k = 0 & k = 2 \\
\hline \frac{\sigma_{s\text{-wave}}}{\sigma_{\text{tot}}} & 0.28 \pm 0.16 & 0.33 \pm 0.25 \\
\hline
\end{array}
\]

$$\text{Non-spin-flip cross-section}$$

$$\sigma_{\text{non-spin-flip}} = \sigma_{\text{tot}} \frac{\left|b\right|^2}{\left(a^2 + \frac{1}{2}b^2 + \frac{3}{4}c^2\right)}$$

\[
\begin{array}{c|c|c}
\hline
& k = 0 & k = 2 \\
\hline \frac{\sigma_{\text{non-spin-flip}}}{\sigma_{\text{tot}}} & 0.34 \pm 0.08 & 0.33 \pm 0.06 \\
\hline
\end{array}
\]

$$\text{Spin-flip cross-section}$$

$$\sigma_{\text{spin-flip}} = \sigma_{\text{tot}} \frac{\left|c\right|^2}{\left(a^2 + \frac{1}{2}b^2 + \frac{3}{4}c^2\right)}$$

\[
\begin{array}{c|c|c}
\hline
& k = 0 & k = 2 \\
\hline \frac{\sigma_{\text{spin-flip}}}{\sigma_{\text{tot}}} & 0.38 \pm 0.12 & 0.35 \pm 0.18 \\
\hline
\end{array}
\]

Appendix: errors

$$k = 0$$

\[
\delta\left(\frac{\sigma_s}{\sigma_{\text{tot}}}\right) = 4\pi \delta(a^2) = 2 \times 4\pi a \delta a = 2 \times 0.149 (0.044) = 0.082 \times 2 = 0.164
\]

\[
\delta\left(\frac{\sigma_{\text{NSF}}}{\sigma_{\text{tot}}}\right) = \frac{4\pi}{3} \delta(b) = \frac{8\pi}{3} b \delta b = \frac{8\pi}{3} (0.286) (0.033) = 0.078
\]

$$k = 2$$

\[
\delta\left(\frac{\sigma_{\text{NSF}}}{\sigma_{\text{tot}}}\right) = \frac{8\pi}{3} \delta(c) = \frac{16\pi}{3} c \delta c = \frac{16\pi}{3} (0.212) (0.033) = 0.116
\]

LIST OF REFERENCES

Appendix I

\[ \alpha = a e^{i\phi} \quad a, b \text{ and } c \text{ are normalized such that } \int I(\theta) \, d\Omega = 1 \]

\[ b = b e^{i\psi} \quad \text{where } I(\theta) = |a + b \cos \theta|^2 + |c|^2 \sin^2 \theta, \quad d\Omega = 2\pi \sin \theta \, d\theta \]

\[ c = c \]

\[ \phi \text{ Right hand scale} \]

\[ \psi \text{ Right hand scale} \]

\[ \psi \text{ Left hand scale} \]

\[ \psi \text{ Right hand scale} \]

\[ \psi \text{ Left hand scale} \]

\[ k = 0 \text{ solution} \]

\[ k = 2 \text{ solution} \]
A RE-ANALYSIS OF THE EXPERIMENTAL DATA ON HYPERNUCLEI DECAYING BY $\pi^-$ EMISSION \(^\dagger\(^(*)\)

R. LEVI-SETTI, W. E. SLATER and V. L. TELEGDI

The Enrico Fermi Institute for Nuclear Studies and Department of Physics, University of Chicago, Chicago (III.)

The survey presented at the 1957 Rochester Conference (Proceedings VIII, pp. 9-10) has been prepared for publication and is to appear in Nuovo Cimento. The following table, extracted from this forthcoming paper, summarizes the main results and contains a comparison with the numbers as originally presented last year. The differences are due to the addition of a few events and to further recomputation, as well as to the adoption of a new Q-value for the free $Λ^0$ decay (as explained in footnote b).


<table>
<thead>
<tr>
<th>Identity</th>
<th>$B_1$ (MeV)</th>
<th>$\sigma_{e^+}$ (MeV)</th>
<th>$\delta_{BG}$ (MeV)</th>
<th>No. of events averaged</th>
<th>$B_1$ (MeV)</th>
<th>$\sigma_{e^+}$ (MeV)</th>
<th>$\delta_{BG}$ (MeV)</th>
<th>No. of events averaged</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>$^1H^3$</td>
<td>0.25</td>
<td>0.31</td>
<td>0.2</td>
<td>9</td>
<td>0.20</td>
<td>0.50 (c)</td>
<td>0.2</td>
<td>7</td>
<td>9</td>
</tr>
<tr>
<td>$^1H^4$</td>
<td>1.44</td>
<td>0.20</td>
<td>0.25</td>
<td>21</td>
<td>1.81</td>
<td>0.20 (c)</td>
<td>0.25</td>
<td>21</td>
<td>26</td>
</tr>
<tr>
<td>$^1He^4$</td>
<td>1.70</td>
<td>0.24</td>
<td>0.2</td>
<td>9</td>
<td>1.99</td>
<td>0.20 (c)</td>
<td>0.2</td>
<td>9</td>
<td>9</td>
</tr>
<tr>
<td>$^1He^5$</td>
<td>2.56</td>
<td>0.17</td>
<td>0.2</td>
<td>15</td>
<td>2.82</td>
<td>0.20 (c)</td>
<td>0.2</td>
<td>17</td>
<td>19</td>
</tr>
<tr>
<td>$^1Li^7$</td>
<td>4.17</td>
<td>0.62</td>
<td>0.2</td>
<td>2</td>
<td>4.80</td>
<td>0.50 (d)</td>
<td>0.2</td>
<td>3</td>
<td>3</td>
</tr>
<tr>
<td>$^2He^8$</td>
<td>5.2</td>
<td>1.0</td>
<td>0.3</td>
<td>1</td>
<td>5.60</td>
<td>0.40 (d)</td>
<td>0.25</td>
<td>3</td>
<td>3</td>
</tr>
<tr>
<td>$^2Li^9$</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>6.7</td>
<td>0.70</td>
<td>0.3</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>$^4Be^1$</td>
<td>5.9</td>
<td>0.5</td>
<td>0.2</td>
<td>1</td>
<td>6.25</td>
<td>0.60</td>
<td>0.2</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>$^4Be^2$</td>
<td>6.13</td>
<td>0.33</td>
<td>0.2</td>
<td>3</td>
<td>6.43</td>
<td>0.40 (d)</td>
<td>0.35</td>
<td>3</td>
<td>3</td>
</tr>
</tbody>
</table>

### Uniquely identified events

- $^1H^3$, $^1He^4$, $^1Li^7$, $^2He^8$, $^2Li^9$, $^4Be^1$, $^4Be^2$.

### Non-uniquely identified events

- $^1H^3$, $^1He^4$, $^1Li^7$, $^2He^8$, $^2Li^9$, $^4Be^1$, $^4Be^2$.

(a) Based on $Q_1 = (36.9 \pm 0.2)$ MeV. (Friedlander, Keefe, Menon and Merlin, Phil, Mag. 45, 533 (1954), recalculated value, 1957, private communication).

(b) The value of $Q_1$ used in the present computation is $(37.22 \pm 0.2)$ MeV. This value is based on a recomputation $(36.75 \pm 0.2)$ MeV of the events of Friedlander, et al. using the latest range-energy relation combined with the value $(37.45 \pm 0.17)$ MeV given by W. H. Barkas, P. C. Giles, H. H. Heckman, F. W. Inman, C. J. Mason, and F. M. Smith, Padua-Venice Conference, Sept. 1957.

(c) $\sigma_{e^+}$ obtained from the distribution of $B_1$. $\sigma_{e^+} = \frac{\sum \omega_1 (B_{A1} - \bar{B}_{A1})^2}{(n-1) \sum \omega_1}$, $\omega_1 = (dE)^{-1}$

(d) $\delta_{BG} = (\sum \omega_1)^{-1/2}$

(e) Only events in which the $\pi^-$ stops in the emulsion have been included in the averages.

\(^\dagger\) Appendix to Session 6. — Experimental.

\(^\dagger\) Research supported by the Air Force Office of Scientific Research, Contract No. AF 49 (638)-209.
HYPERON-NUCLEON INTERACTIONS

H. WEITZNER

Harvard University, Cambridge (Mass.)

For an examination of hyperfragment binding energies a potential was constructed largely on phenomenological grounds. If one assumes that pion exchange is to generate \( A-N \) forces and such forces are to conserve isotopic spin, then the simplest effective pion-\( A \) interaction would be \( \mathbf{q}_\pi(x) \cdot \mathbf{q}_n(x) \mathbf{\bar{q}}_\Lambda(x) \mathbf{\bar{\psi}}_\Lambda(x) \mathbf{\bar{\psi}}_n(x) \). The main characteristics of such a potential are apparent from the form of the coupling: first, there are three-body forces coming from the emission of two pions at each vertex; secondly, the \( A \) spin is not present in the potential, and thirdly, the range of the direct \( A-N \) force should be \( 1/2 \, m_n \) as it comes from double pion exchange. The form of the potential and the approximate ranges were obtained from the lowest order non-vanishing static potential produced by such an interaction, and the standard \( N-n \) interaction.

The interaction potential for two nucleons and a \( \Lambda \)-particle actually used in the variational calculation of the binding energies was

\[
V(r_1, r_2, r_\Lambda) = -V_1 \left[ e^{-2(t_1-t_0)^2} + e^{-2(t_0-t_\Lambda)^2} \right] - V_2 \sigma_1 \cdot \sigma_2 \tau_1 \cdot \tau_2 e^{-2[(t_1-t_0)^2+(t_0-t_\Lambda)^2]}. \]

The constant \( \lambda \) was chosen to be \( 1.05 \, f^{-2} \), so that the range of the two-body force corresponds to \( 1/2 \, m_n \). A trial function of the form \( \exp \left\{ -a (r_1 - r_2)^2 - \beta (r_1 + r_2 - 2r_\Lambda)^2 \right\} \) was used, and \( a \) and \( \beta \) were varied independently. With \( V_1 = 107 \, \text{MeV} \), \( V_2 = 37.5 \, \text{MeV} \), the non-existence of \( \Lambda \)H$^2$, \( \Lambda \)H$^3$, and the binding energies of \( \Lambda \)H$^3$, \( \Lambda \)H$^4$, \( \Lambda \)He$^4$, were successfully reproduced. For \( \Lambda \)H$^3$, states other than a \( \Lambda \) attached to a deuteron were considered but found not to bind.

From the symmetric pion-baryon couplings proposed by many authors, one can also form a potential by a perturbation expansion with static baryon sources for the pion field. Qualitatively the major difference between this potential and the former is that \( V_2 \) here becomes \( \Lambda \) spin dependent, with the \( A-N \) system more attractive in a spin singlet than triplet state. With the assumption that the strength of the two-body force of this potential for its more attractive state is also \( 107 \, \text{MeV} \), one may estimate that \( g_{\Sigma \Lambda N}/g_{NN} \sim 1 \). Since \( V_1 = 107 \, \text{MeV} \) corresponds to the \( A-N \) system just unbound, one may make another estimate of this ratio by determining the potential necessary so that \( K \cot \delta \) at zero energy for \( A-N \) scattering just becomes negative. Such a calculation will be described below: it gives the same result as the one already outlined.

Some estimates of the binding energy of a \( \Lambda \) in an "infinite" nucleus were also made. The above potential and the Fermi gas model of the nucleus were used. Because the Pauli principle does not effect the \( A \), the binding energies were very large (~50 MeV), although much of that energy might well come from potentials which were made too attractive in order to fit the light hyperfragments within the approximate variational treatment. With the spin dependent potential the binding energy could drop to about 10 MeV.

A particularly simple form of potential was considered for some estimates of the s-wave phase-shifts in the scattering of the \( \Sigma-A-N \) system. One way to look at the proposed baryon symmetry is to suppose that the two-dimensional isotopic spin matrices of the pion-nucleon interaction are replaced by four-dimensional ones, corresponding either to a \( (N\Sigma) \) or a \( (\Sigma\Lambda) \) multiplet. The \( \tau \) matrix for the \( (\Sigma\Lambda) \) system decomposes into \( t + s \), where \( t \) represents the ordinary isotopic spin matrix for a \( \Sigma \) \((T = 1)\) and \( s \) is a matrix which mixes \( A \) and \( E \), i.e.

\[
s_x = \begin{pmatrix} 0 & 0 & 0 & i \
0 & 0 & 0 & 0 \
0 & 0 & 0 & 0 \
i & 0 & 0 & 0 \end{pmatrix} \text{ etc.}
\]

In a most naïve way, if in the second order static \( N-N \) potential

\[
\tau_1 \cdot \tau_2 \frac{g_N g_N}{4 \pi m_N^2} (\mathbf{a}_1 \cdot \mathbf{\nabla}) (\mathbf{a}_2 \cdot \mathbf{\nabla}) e^{-m_N^2/\xi^2}
\]

\( \tau_2 \) is replaced by \( t + s \), one \( g_N \) by \( g_{\Sigma\Lambda} \), and one \( m_N \) by \( m_{\Sigma} \), then one has a \( \Sigma-A-N \) potential with a direct \( \Sigma-N \) interaction and with an interaction exchanging \( \Sigma \) and \( A \), but no direct \( A-N \) interaction. Such a potential is the one to be used here.

Of course, while there would be a direct \( A-N \) force coming from higher order terms, the usefulness of the concept of a potential seems sufficiently questionable that

\( \dagger \) Appendix to Session 6. — Theoretical.


(**) Now at the University of California, Berkeley, Cal.
at present a simple model without direct $A$-$N$ terms should suffice. For consider the “potential”:

$$V_{A \rightarrow \Sigma}(\rho) = \frac{g_{N\Sigma A}}{4m_N m_A} \tau_1 \cdot s \left( \frac{\sigma_1 \cdot \psi}{2} \right) \cdot \left( \int \frac{d^3 k}{(2\pi)^3} \frac{e^{ik\rho}}{E_\pi} \right)$$

formed in second order perturbation theory corresponding to the diagrams:

![Diagram](image)

where $\Delta m$ is the actual $\Sigma$-$A$ mass difference. The “potential” $V_{\Sigma \rightarrow A}$ would have the sign of $\Delta m$ reversed. Thus the two potentials are not the same, and if one were to write coupled equations for the $A$ and $\Sigma$ wave functions, the system of equations would not be self-adjoint, and therefore there would not be particle conservation. An evaluation of the integrals suggests that the difference between $V_{\Sigma \rightarrow A}$ and $V_{A \rightarrow \Sigma}$ is not too great, but the general problem remains. In the simple way the potential was constructed, such difficulties do not appear. The form of the potential will be taken as $K(r) = V_{\Sigma}(r) \cdot \mathbf{T} + V_2(r) \cdot \mathbf{s},$ where $V_1$ and $V_2$ may be spin dependent and tensor forces have been suppressed.

The standard variational principles for phase-shifts as well as those which may be used to generate effective range expansions may be readily applied to systems with mixtures of $E$'s and $\Sigma$'s. The scattering naturally divides into two regions: (i) below $\Sigma$-production threshold:

$$E_A = (K^2/2m_A) > 0; \quad E_\Sigma = (K^2/2m_\Sigma) < 0$$

(the bar refers to reduced mass); (ii) $\Sigma$-$N$ scattering and $A$-$N$ scattering where the two hyperons may interchange, or $k^2 > 0$. For $K^2 \rightarrow 0$ one has the standard effective range expansion of $K \cot \delta$, but for $k^2 \rightarrow 0$ it no longer exists. For $k^2 > 0$ let the two eigenphases $\delta_{1,2}$ for the $(E-A)$-$N$ system be defined by

$$\lim_{r \rightarrow \infty} r \psi_{\Sigma \Lambda} \sim \sin(kr + \delta_{1,2})$$

$$\lim_{r \rightarrow \infty} r \psi_{A \Lambda} \sim M_{1,2} \sin(kr + \delta_{1,2}).$$

$M_1$ and $M_2$ satisfy $k + M_1, M_2 K = 0$. Let 2 refer to that phase for which $M_{1,k=0} \neq 0$. Then

$$k \cot \delta_1 = (K \cot \delta_2) \left|_{k \rightarrow 0} \cdot \frac{k}{1 + \frac{k}{(KM_2^2)_{k=0}}}, \right.$$  

$$K \cot \delta_2 = (K \cot \delta_1) \left|_{k \rightarrow 0} \cdot \frac{k}{1 - \frac{k}{(KM_2^2)_{k=0}}}, \right.$$  

and for $A$-$N$ scattering just below $\Sigma$-production threshold

$$K \cot \delta = (K \cot \delta_2) \left|_{k \rightarrow 0} + \frac{k}{(M_2^2)_{k=0}}. \right.$$  

These expansions are exact to first order in $k$ and depend solely on the difference in wave number of the $\Sigma$ and $A$, and the existence of the part of the potential exchanging $\Sigma$ and $A$. Thus, even with a direct $A$-$N$ force added, such expansions are valid.

The explicit potentials used in the variational calculations were: (*)

$$\begin{align*}
\Sigma$$-$$N$$ potential & \quad (\Sigma/\Lambda)$$-$$N$$ potential \\
$S = 0$ & $S = 1$ \\
$S = 0$ & $S = 1$

$T = \frac{1}{2}$ & $44.0 e^{-ra} - 110 e^{-ra}$ \\
& $58.7 e^{-ra}s$ \\
& $36.7 e^{-ra}s$

$T = \frac{3}{2}$ & $-88.0 e^{-ra}$ \\
& $0$

$a = 0.796 f$ (i.e. about $1/m_\pi$ for the range).$

The values do not correspond exactly to the potential of the lowest order static potential — a slight attempt has been made to correct for some higher order effects. If $g_{N\Sigma}$ were of opposite sign to $g_{NN}$ — which would still preserve the symmetries — the potentials would have opposite sign. The computations were also done in this case.

The $s$-wave phase-shift expansions are given in Tables I and II along with an estimate of Coulomb corrections in $\Sigma^+p$ scattering.

If the lowest order static potential had been used with $(g_{\Sigma N\Lambda}, g_{\Sigma N\pi}) \approx 1, K \cot \delta|_{K=0}$ would be just negative in the spin singlet state, corresponding to a barely bound $A$-$N$ system. A resonance in $A$-$N$ scattering at $\sim 60$ MeV (all energies in c.m. system) occurs for most of the potentials, as well as a resonance in eigenphase 2 just above threshold. These seem to be more a property of the existence of a strong interaction mixing $\Sigma$-$A$ rather than of specific potential parameters.

Cross-sections from the above phase-shift estimates are given below. It has been assumed throughout that the $\Sigma$-mass differences are negligible. For the $(\Sigma^+ n)$, $(\Sigma^0 p)$ systems, such seems to be the case, so that the results apply. But as the $(\Sigma^-$ $p)$ and $(\Sigma^0 n)$ mass differences are large, no estimates are given as both the mass differences and Coulomb forces split the isotopic states. The $T = 3/2, S = 0$ potential was picked to give, in an exact calculation, a zero energy resonance; this value is of about the right magnitude and thus it is possible to test accuracy of the variational procedure. The huge cross-sections come from this choice. As data becomes available, the strength of the potential may be adjusted accordingly. Unfortunately, the tensor forces and higher angular momentum states — both omitted — probably give important contributions to some of the cross-sections.

(*) Attractive potentials are negative.
### TABLE I (*)

The \( T = \frac{1}{2} \) system (mixed \( \Sigma N \) and \( \Lambda N \))

<table>
<thead>
<tr>
<th>( V_1 )</th>
<th>( V_2 )</th>
<th>( 44.0 \text{ MeV} )</th>
<th>( -110.1 \text{ MeV} )</th>
<th>( -44.0 \text{ MeV} )</th>
<th>( 110.1 \text{ MeV} )</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \Lambda N ) scattering</td>
<td>( K \cot \delta = )</td>
<td>((2.30 + 3.00 , K^2))</td>
<td>(4.71 + 3.88 , K^2)</td>
<td>(1.85 + 2.24 , K^2)</td>
<td>(7.53 + 5.8 , K^2)</td>
</tr>
<tr>
<td>( \Lambda N ) scattering below</td>
<td>( K \cot \delta = )</td>
<td>(-12.2 + 19.3 ,</td>
<td>k</td>
<td>)</td>
<td>(-3.77 + 15.0 ,</td>
</tr>
<tr>
<td>( \Sigma ) production threshold</td>
<td>( K \cot \phi_2 = )</td>
<td>(-12.2 (1 - 17.2k))</td>
<td>(-3.77 (1 - 13.3k))</td>
<td>(-8.04 (1 - 4.22k))</td>
<td>(-126 (1 - 210k))</td>
</tr>
<tr>
<td>( \Sigma ) threshold</td>
<td>( M_2 = )</td>
<td>(-0.228)</td>
<td>(0.259)</td>
<td>(-0.459)</td>
<td>(0.0652)</td>
</tr>
<tr>
<td>( \Sigma ) scattering below</td>
<td>( K \cot \phi_1 = )</td>
<td>(6.14 (1 + 17.2k))</td>
<td>(0.154 (1 + 13.3k))</td>
<td>(0.539 (1 + 4.22k))</td>
<td>(36.1 (1 + 210k))</td>
</tr>
<tr>
<td>( \Sigma ) threshold</td>
<td>( M_1 = )</td>
<td>(3.89 , k)</td>
<td>(-3.43 , k)</td>
<td>(1.93 , k)</td>
<td>(-13.60 , k)</td>
</tr>
</tbody>
</table>

### TABLE II (*)

The \( T = \frac{3}{2} \) system

\[ V_1 \]
\[ k \cot \delta \]
\[ \Sigma^+ - p \text{ scattering} \]
\[ b_0 = \frac{k^2}{2m_N e^2} \]
\[ \frac{\pi}{b_0 (1 - e^{-\pi \eta})} \cot \delta + \frac{1}{b_0} \left[ \text{Re} \, \psi(\eta - \log \eta) \right] \]
\[ \eta = \frac{1}{2kB_0} \]

\[ b_0 = 0.00715 + 1.5 \, k^2 \]
\[ -0.628 + 0.244 \, k^2 \]

\[ \Sigma^- n \text{ scattering} \]
\[ \frac{g_{\Sigma n}}{g_{N n}} > 0 \]
\[ \frac{g_{\Sigma n}}{g_{N n}} < 0 \]

### TABLE III

Cross-sections

\( \Sigma^- n \) scattering

<table>
<thead>
<tr>
<th>( \frac{g_{\Sigma n}}{g_{N n}} &gt; 0 )</th>
<th>( \frac{g_{\Sigma n}}{g_{N n}} &lt; 0 )</th>
</tr>
</thead>
<tbody>
<tr>
<td>( 1 ) zero energy ( \sigma_T )</td>
<td>( \sim 400 \text{ b} )</td>
</tr>
<tr>
<td>( \Sigma^- n ) scattering</td>
<td>( \sim 46 \text{ b} )</td>
</tr>
<tr>
<td>( \sigma_{\Sigma^- n} )</td>
<td>( \sim 88 \text{ b} )</td>
</tr>
</tbody>
</table>

Each spin state has its own resonance : for \( (g_{\Sigma n}/g_{N n}) > 0 \) the singlet and triplet resonances are at \( E_{\Sigma} \sim 1/2 \) MeV; for \( (g_{\Sigma n}/g_{N n}) < 0 \) the singlet resonance is at \( E_{\Sigma} \sim 4 \) MeV,

\(*\) The unit of length is 0.8 \( \ell \), and momenta are measured accordingly.
SEARCH FOR ELECTRIC DIPOLE MOMENT OF THE MUON

D. BERLEY, R. L. GARWIN, G. GIDAL and L. M. LEDERMAN
Columbia University, New York (N. Y.)

We have carried out a search for possible electric dipole moment structure in the positive muon. The technique is based on the facts that:

(1) the external muon beams are produced with a high degree of longitudinal polarization by decay-in-flight of pions near the cyclotron target; and

(2) if the muon possesses an electric dipole moment, the term $\mathbf{v} \times \mathbf{H}$ (v = velocity, $\mathbf{H}$ = external magnetic field) will result in a precession of the spin vector way from the momentum and toward the (vertical) applied magnetic field. Thus, during the $\sim 135^\circ$ bending of the muon trajectory in the cyclotron field and the subsequent $-45^\circ$ bending in the steering magnet (Run I), the net precession of the spin vector is: $\theta_H \cdot \beta \cdot f$ where $\theta_H = 90^\circ$ is the trajectory deflection angle, $\beta$ is the velocity and $f$ is the electric dipole moment in units of $\frac{e\hbar}{\mu c}$.

The results of Run I: $f = 0.03 \pm 0.011$. This corresponds to a dipole moment $eD$ where $D = 6 \pm 2.2 \times 10^{-15}$ cm. At this stage we prefer to say $D < 10^{-14}$ cm. In Run II, the magnetic deflection angle is increased to $180^\circ$, increasing the sensitivity by a factor 2.

† Appendix to Session 8. — Experimental.
Appendix II
SESSION 1

NUCLEON STRUCTURE — Theoretical

Introductory Notes and Reading List suggested by
L. L. FOLDY, Case Institute of Technology, Cleveland (Ohio)

The aspect of nucleon structure which is most directly accessible to experiment and on which
the conference discussion is anticipated to centre is that of electromagnetic structure. Loosely
speaking, one deals here with distribution of electrical charge and current in a physical nucleon
and its physical origin. The most comprehensive non-mathematical review of this subject is
contained in

and the reading of this paper is recommended to all conference participants. Two further non-
mathematical review papers which were presented at the Stanford Conference on Nuclear Sizes
and Nucleon Structure are scheduled to appear in the April 1 1958 issue of the Reviews of Modern
Physics. The first is a comprehensive review of the neutron-electron interaction from both the
experimental and theoretical side by L. L. Foldy. The second is a discussion of very recent
developments involving the application of spectral representation to the problem of electromagnetic
structure written by M. L. Goldberger and J. Bernstein; both should be suitable for general back­
ground reading.

Turning now to more specific references suitable for detailed exploration of the background
in this subject, it may be remarked that the literature falls into four categories, selected papers on
which are listed below.

1. Phenomenological description of electromagnetic structure of nucleons.

2. Perturbation theory calculations.
   Bethe, H. A. and de Hoffmann, F., *Mesons and Fields*, vol. 2, Evanston, Row,
   Moorhouse, R. G., Advances in Physics, 2, p. 185, 1953, particularly pp. 189-197.

3. Static models of nucleon structure.
   Selected from the extensive literature on this subject are the following papers which deal
   specifically with electromagnetic structure problems.
Appendix II


4. Application of spectral representations to form factors.

Work on this subject is so recent that it has not yet appeared in print. The paper of Goldberger and Bernstein referred to above will probably be available before the conference date. The following pre-print has been rather extensively circulated and may be available to some conference participants.


It, as well as other papers, by Bernstein, Federbush, Goldberger, and Treiman, by Y. Nambu, and by K. Symanzik may appear in print (probably in the Physical Review) by the time of the conference.
SESSION 2

THE NUCLEON AND ITS INTERACTION WITH PIONS, PHOTONS, NUCLEONS AND ANTINUCLEONS; THROUGH 3/2, 3/2 RESONANCE — Experimental I

Reading List suggested by the Rapporteur:
G. PUPPI

SESSION 2

THE NUCLEON AND ITS INTERACTION WITH PIONS, PHOTONS, NUCLEONS AND ANTINUCLEONS; ABOVE 3/2, 3/2 RESONANCE — Experimental II

Introductory Notes and Reading List suggested by the Rapporteur:
O. PICCIONI

Even though no individual experiment published during the past year has produced a major breakthrough in understanding of high energy phenomena, a relevant amount of good new information has appeared in the literature.

A. Pions

The scattering of pions on nucleons has been studied at higher energies than previously and more accurate measurements have been made on the angular distribution of the elastic scattering well beyond the 3/2, 3/2 resonance. The optical model is generally taken as the theoretical model most suitable to describe the elastic scattering, which is interpreted as being mostly due to diffraction caused by the absorption process. By the proper adjustment of the various parameters a fairly good agreement is obtained between experiment and theory.

Similarly, the so-called statistical or thermodynamical models are used to describe collisions where particles, mainly mesons, are produced (see Serber, R., Rochester Conference on High Energy Nuclear Physics, 7th, p. V-1, 1957.). The agreement in this case is not so clearly good. The total cross sections of nuclei for incident BeV pions have also been investigated and give comfortable agreement with other studies of nuclear sizes.

B. Protons

A study of the angular distribution of p-p scattering at high energy has been completed. Again, the optical model is called in for theoretical guidance.

C. Photons

Accurate meson production studies have been extended to higher energies. The results have once more increased our confidence that the present meson theories provide a good description of these phenomena even at energies somewhat past the 3/2, 3/2 resonance.

The data on the interesting process of pair production of pions are also in good agreement with theory.

D. Antinucleons

Good measurements of the cross-sections of nuclei for incident antiprotons have been made. The results seem to agree with the high values found for the elementary antiproton-proton cross-section 13).
Another study is aimed at obtaining separately the inelastic-collision cross-section. By comparison with the total cross-section one can tentatively deduce that the antiproton-proton elastic scattering cross-section is probably less than 20% of the total. This suggests that the proton and the antiproton are transparent to each other despite the largeness of their total cross-section.

The production of antineutrons has been strongly reaffirmed by another counter experiment.

Less recently, emulsion studies have given a very good description of the phenomenology concerning the interactions of antiprotons with nuclei.

REFERENCES

SESSION 3

THE NUCLEON AND ITS INTERACTION WITH PIONS, PHOTONS, NUCLEONS AND ANTINUCLEONS — Theoretical

Introductory Notes and Reading List suggested by the rapporteur:

G. F. CHEW

A. Pions

For a fairly recent survey of the theory of pion-nucleon interactions the introductory talk by Chew at the 1957 Rochester Conference may be consulted. During the past year the subject of greatest interest has been the dispersion relations and the possibility of their violation. Various theoretical studies have confirmed the conclusions that the Golberger relations for forward scattering (Goldberger, M. L., Miyazawa, H. and Oehme. R. Phys. Rev., 99, p. 986, 1955) have a very general basis and that an experimental discrepancy of the magnitude suggested by Puppi, A. and Stanghellini, A. (Nuov. Cim., 5., p. 1305, 1957) indicates a breakdown of local field theory. Other developments include a new type of dispersion relation (Gilbert, W., Phys. Rev., 108, p. 1078, 1957), which seems to allow an unusually precise determination of the pion-nucleon coupling constant.

A basic question continues to be obscure in spite of much theoretical effort: is it possible in terms of three parameters, the pion and nucleon masses and the mutual coupling constant, to predict

1. the position of the \( (\ell/2, \ell/2) \) resonance,
2. the smallness of the non-charge-exchange S-wave scattering?

It has been apparent for some time that once these latter two facts are given the rest of low energy pion physics can be theoretically understood through the dispersion relations.

In the GeV energy range it is not yet known whether the primary pion-nucleon interaction continues to proceed through the nucleon “core”, as it does at low energies, or whether pion-pion collisions in the cloud are important. Optical model fits (see review by Serber in the notes of the Rochester Conference on High Energy Nuclear Physics, 7th, p. V-I, 1957.) of the observed data correspond to a proton radius \( \sim 1 \times 10^{-15} \) cm and suggest the presence of a pion-pion interaction, but calculations based on such a mechanism have thus far been inconclusive.

B. Photons

There have been relatively few new developments in the theory of photo-meson production during the last year. However the dispersion relation approach to the problem of photon-nucleon scattering continues to yield new results (Akiba, T. and Sato, I., Progr. theor. Phys., 19, p. 93, 1958; Capps, R. H., Phys. Rev., 108, p. 1032, 1957), and the agreement of theory with experiment seems to be improving. Unfortunately nothing in the nature of a survey of photon-nucleon scattering theory has yet been published.
C. Nucléons

The review of the low energy nucleon-nucleon problem given by Marshak at the Rochester Conference on High Energy Nuclear Physics, 7th, 1957, p. III-1, may be consulted for a survey of the situation last year. The Gartenhaus-Signell-Marshak potential has since continued to produce good predictions and has been shown to have many features in common with the phenomenological potential of Gammel and Thaler. The most accurate calculation to date of a purely meson-theoretic potential has recently been published (Konuma, M., Miyazawa, H. and Otsuki, S., Progr. theor. Phys., 19, p. 17, 1958) and the long-range part shown to correspond well to experimental requirements. No quantitative meson theory calculation of a spin-orbit force has yet been achieved. (For an exhaustive review of the meson theory of nuclear forces, see Progr. theor. Phys., Suppl. No. 3, 1956.)

The theory of nucleon-nucleon interactions in the 1-5 Gev range is almost non-existent — consisting mainly of optical model calculations, although many features of the multiplicity and distribution of the pions produced seem to be related to a final state (3/2, 3/2) resonance interaction. The Fermi statistical theory for very high energies seems to have been discredited by experiment and rival theories, such as that of Lewis, Oppenheimer and Wouthuysen are still in a very crude state. (For a review of theories of multiple meson production, see Lewis, H. W., Rev. mod. Phys., 24, p. 241, 1952.)

D. Antinucleons

There are no reviews of the theory of nucleon-antinucleon interaction published because none of the theoretical models proposed have convincingly demonstrated an ability to explain existing experimental facts. The initial feeling that the observed big total cross-sections would require a major departure from the Yukawa picture has subsided, but the large experimental ratio of annihilation to scattering continues to present a serious problem. The $N\bar{N}$ interaction is a subject where the current rate of accumulation of fundamental experimental information is so great and the theory so difficult that most theorists are at present waiting for empirical clues to a correct model.
SESSION 5

STRANGE PARTICLE PRODUCTION — Experimental

Reading List suggested by the Rapporteur:
J. STEINBERGER

SESSION 6

STRANGE PARTICLE INTERACTIONS — Experimental

Reading List suggested by the Rapporteur:

M. F. KAPLON

SESSION 8

WEAK INTERACTIONS: LEPTONIC MODES — Experimental

Reading List suggested by the Rapporteur:

M. GOLDHABER

SESSION 8

WEAK INTERACTIONS: LEPTONIC MODES — Theoretical

Reading List suggested by the Rapporteur

L. MICHEL

SESSION 9

WEAK INTERACTIONS: OTHER MODES — Theoretical

Reading List suggested by the Rapporteur:

S. B. TREIMAN

A. The $K^0_L$-$K^0_S$ complex

1. Gell-Mann, M. and Pais, A., Phys. Rev., 97, p. 1387, 1955; Pais, A. and Piccioni, O., Phys. Rev., 100, p. 1487, 1955. These are the basic papers in the field. They call attention to the fact that the “particles” $K^0_L$ and $K^0_S$ defined by strong reactions do not have definite lifetimes — that, in decay, it is certain linear combinations $K^0_L$ and $K^0_S$ which behave like particles, the latter having different decay modes, lifetimes, and masses. In the second reference, the possibility of curious interference effects is raised. The discussions are based on the assumption of charge conjugation invariance.


3. Treiman, S. B. and Sachs, R. G., Phys. Rev., 103, p. 1545, 1956. This paper deals with interference effects which would give rise to charge asymmetries in neutral $K$ decay: e.g. $e^+ + \pi^- + \nu$ vs $e^- + \pi^+ + \bar{\nu}$. It is shown that such asymmetries may depend sensitively on the $K^0_L$-$K^0_S$ mass difference.


6. Fry, W. F. and Sachs, R. G. (to be published). An interesting proposal for determining the $K^0_L$-$K^0_S$ mass difference, based on the Pais-Piccioni effect, is discussed.

B. Asymmetry in hyperon decay


3. The relations between $\Sigma$ and $\Lambda$ polarizations in the processes $\Sigma^0 \rightarrow A^0 + \gamma$ and $\Sigma^- + p \rightarrow m + A^0$ are discussed respectively in: Gatto, R., Phys. Rev., 109, p. 610, 1958; Pais, A. and Treiman, S. B., Phys. Rev. (to be published).

C. Charge asymmetries ($\triangle I = \frac{1}{2}$ Rule); models of weak interactions


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Atomnaya Energiya
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Bull. Amer. phys. Soc.
Doklady Akad. Nauk SSSR
Experientia
Fortschr. Phys.
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Zhurnal eksperimentalnoi i teoreticheskoi fiziki. Moscow.
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LE PRÉSENT OUVRAGE, COMPOSÉ EN CARACTÈRE TIMES NEW ROMAN, CORPS NEUF, À ÉTÉ ACHÈVÉ D'IMPRIMER LE DIX-HUIT SEPTEMBRE MIL NEUF CENT CINQUANTE-HUIT SUR LES PRESSES DE L'IMPRIMERIE HENRI STUDER S.A., À GENÈVE. LES CLICHÉS ONT ÉTÉ GRAVÉS PAR LA MAISON SCHWITTER S. A., À BALE.