Discussion

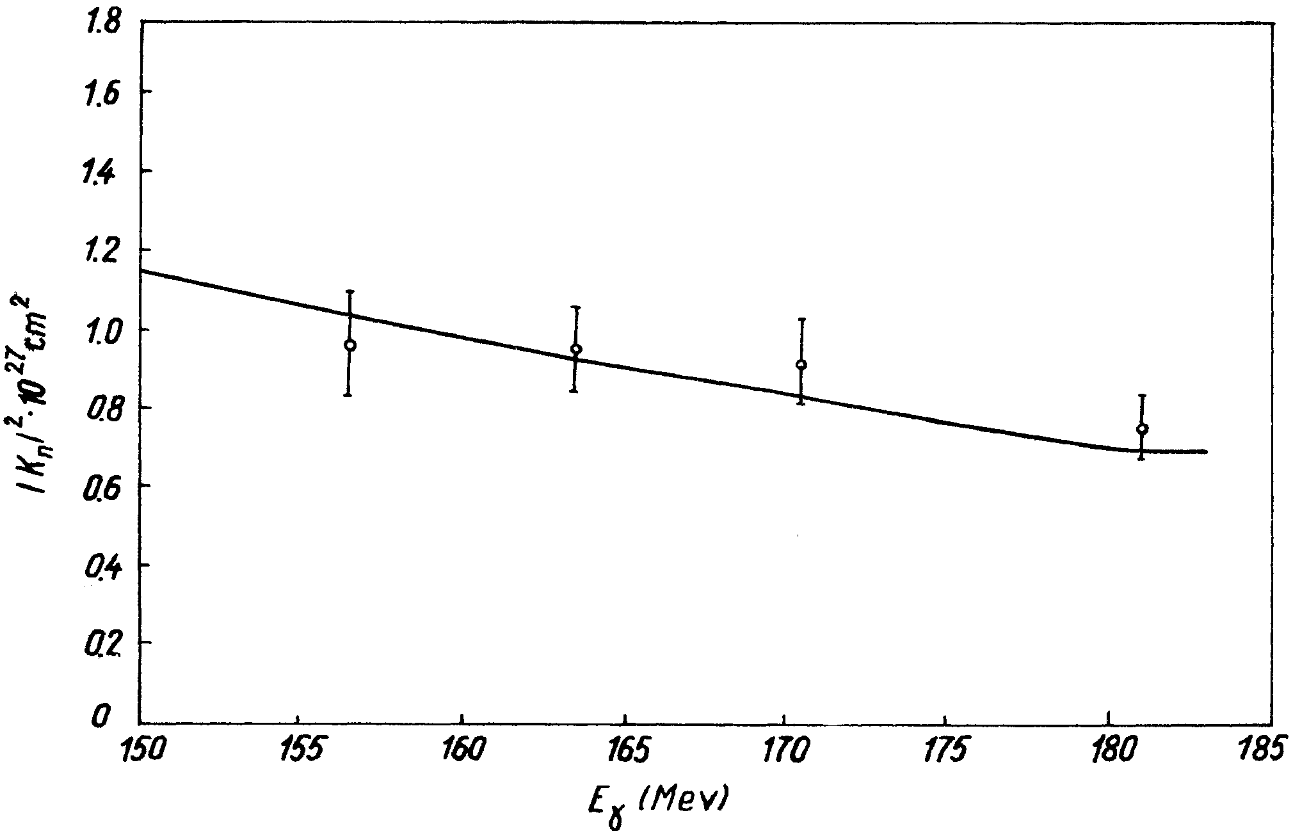
Panofsky:

The absolute value of the $\gamma + p \to \pi^+ + n$ cross-section at resonance has been measured by Raymond Alvarez and found 19.8×10^{-30} cm^2 / Ster. at θ c.m. = 123° and E = 294 MeV. Accuracy is ± 8% at present but will become 3% when the work is finished. This value is ~ 20% below the dispersion theoretical value for any reasonable choice of f^2 and ω_τ.

Adamovich:

My remark deals with the section "Low-Energy Pion Physics" of the report. For some reason, there is a tradition to compare the value of the Panofsky ratio with the photoproduction cross section of π^+-mesons using the ratio σ^- / σ^+ of π^- and π^+-meson photoproduction. Bernardini also follows this tradition. This method involves high inaccuracies, since in this case the error is due not only to errors in measuring the cross section σ^+, but also to the error in σ^- / σ^+. A correct correlation of parameters of low energy meson physics consists in a direct comparison of the Panofsky ratio with the value of the square of the matrix element of negative pion photoproduction on free neutrons. These parameters are directly related through the value $\left.\frac{\alpha_1 - \alpha_3}{\eta}\right|_{\eta \to 0}$ where $\alpha_1 \alpha_3$ = S phases, η = meson momentum.

The picture presents the values of the square of the matrix element $/K_n/^2$ of the $\gamma + n \to \pi^- + p$ process as obtained by Larionova, Kharlamov and myself in investigating the $\gamma+d\rightarrow p+p+\pi^-$ reaction. They differ from the data presented in Baldin's work (Nuovo Cim. 8, 569 /1958/) in small corrections (5%) due to the increase in statistics,



registration efficiency, etc. The solid line is a theoretical curve calculated for S-states of the meson taking into account the states from the direct interaction with the meson current. Owing to the insufficient statistical accuracy of the experimental data it is difficult to speak of a complete compliance with the theory, but it could be noted that the experimental points tend to follow the theoretical curve. It should be emphasized, however, that the experimental data do not contradict the constancy of $/K_n/^2$ in the energy range from 156 to 181 MeV, either.

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The extrapolation of experimental data to the threshold according to the theoretical law and the taking into account of the non-linearity of the variation of S-phase shifts with increasing meson momentum (mentioned by Bernardini) leads to a coincidence of the photoproduction data with the measured value of the Panofsky ratio, P = 1.46. However, the curvilinear extrapolations to $\eta \to 0$ themselves require an experimental verification. This refers to the photoproduction of both π^+ and π^- mesons. The measurement of the differential cross section of π^+ meson photoproduction at photon energies below 155 MeV is associated with great experimental difficulties. As regards the experiment on investigation of the $\gamma + n \to \pi^- + p$ process at the very threshold, in this case we encounter difficulties associated only with the theoretical calculation of the cross section σ_d of the $\gamma + d \to \pi^- + p + p$ reaction for the extraction of the value $/K_n/^2$, since the value σ_d has been measured by us for the photon energy of 150 MeV. It is believed that this program will be carried out successfully.

It is noteworthy that, considering the available experimental data, the comparison of the Panofsky ratio with the values σ^-/σ^+ and σ^+ involves much greater uncertainties and errors than the errors indicated above for $/K_n/^2$.

Finally, the very value of the ratio of the cross sections of π^- and π^+ mesons photoproduction on free nucleons is obtained by comparing $/K_n/^2$ and similar results for π^+ photoproduction on protons. According to the experiments of Popova, Yagudina, Gorzhevskaya

Kharlamov and Larionova, as well as to all the other experiments concerned with π^+ , this ratio in the range from 156 to 172.5 MeV is $\sigma^-/\sigma^+ = 1.3$ with an error of about 10%. The measurements of σ_d^-/σ_d^+ by the yield of π^- and π^+ from deuterium do not give the value of the ratio for free nucleons. In order to obtain σ^-/σ^+ for free nucleons from these data, it is necessary to know a full characteristic of the $\gamma + d \rightarrow \pi^- + p + p$ reaction.

Bernardini:

I would like to summarize what Dr. Adamovich said and attempt to answer. Adamovich pointed out in a proper way that the threshold values have been correlated in an old-fashioned manner. In principle one does not need to use the π^-/π^+ ratio. The comparison between the Panofsky ratio and the other parameters involves the pion minus-pion plus ratio, if you do not know directly the cross-section for photoproduction of negative pion by neutron. The old-fashioned method suggested was imposed by this formula.

$$P = \frac{1}{2} \frac{P_{\pi^-}^2}{P_\gamma^2} \cdot \frac{1}{R} \cdot \frac{\sigma(\pi^- + p \rightarrow n + \pi^0)}{\sigma(\gamma + p \rightarrow n + \pi^+)}$$

Now Adamovich points out that with the use of emulsions (I believe imbibed by deuterium) a direct value for the cross sections of this process can be obtained. In this case one is able to rebuild all the picture phenomenon, because it ends in the emulsion in three visible tracks: i.e. a proton, the recoiling nucleon (which is now again a proton) and the negative pion; but personally I would like to say that: first, as you have seen, the precision of the experiment - which is very difficult - is not yet good enough to regard peacefully the settlement of the threshold values. Second, the puzzle was a puzzle as a whole. In other words, there were several parameters: the π^-/π^+ ratio, the Panoffsky ratio and $(\alpha_1 - \alpha_3)/\eta$, and we wanted to fit them in a consistent scheme. Third, the real experiment is not to do the experiment by a neutron in deuterium where binding complications are unavoidable but via the reverse reaction $\pi^- + p \rightarrow n + \gamma$. When you know this reaction of course you do not need either the π^-/π^+ ratio or the cross-section $\sigma(\gamma + p \rightarrow n + \pi^+)$.

Baldin:

I would like to say a few words about the state of the theory of pion photoproduction in the near-threshold region. Until recently the amplitudes obtained by C.G.L.N. served as a basis for comparison of conclusions from the field theory with experiment. It is pointed out in the report that there are a number of indications that the amplitudes do not agree with the experiment.

The largest discrepancy (about six standard deviations) was found in analyzing the data of the work on the $\gamma + p \rightarrow p + \pi^0$ reaction near the threshold which was carried out at the Lebedev Physical Institute of the USSR Academy of Sciences. Since in deriving their amplitudes from the despersion relations C.G.L.N. used a number of specific assumptions, it appears to be important to see to what extent the revealed discrepancy is connected with these assumptions. I undertook an 3176 attempt to compare directly the dispersion relations with the available experimental data on pion photoproduction near the threshold. The picture summarizes the data of the Lebedev Institute (circles) and those of McDonald et al. (Phys. Rev. 107, 577/1957 (triangles) in the form

$$A\left[\frac{a_{33}q^2}{\sin\alpha_{33}}\right]^2\frac{1}{\omega q} \text{ and } C\left[\frac{a_{33}q^2}{\sin\alpha_{33}}\right]^2\frac{1}{\omega q}.$$

A and C = corfficients in angular distribution of the $\gamma+p \rightarrow p+\pi^0$ reaction. The factors following them tend to $\frac{1}{\omega q^3}$ when $q \rightarrow 0$.

If we represent the cross section of the reaction at $q \longrightarrow 0$ as

$$\frac{d\sigma}{d\Omega} = \sum b_{mn}\, q^m (\cos\theta)^n$$

and find b_{mn} from the data presented in the picture, then the ratio $\frac{b_{32}}{b_{30}}$ proves to be equal to -0.63 ± 0.08. From the dispersion relations we obtained for this value $+32 \pm 0.03$. It follows from this that the discrepancy not only in magnitude but also in sign remains, if we discard the assumptions of Chew et al. A similar investigation for charged mesons yields good agreement with experiment. It should be borne in mind that for charged mesons the principal contribution to the amplitude is made by the Born parts of the amplitudes and not by the dispersion integrals. This indicates that the difficulty discussed is probably associated with the evaluation of the dispersion integrals. The contribution of the first and second resonances to the dispersion integrals was taken into account when obtaining the above results.

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The contribution of the nonresonance amplitudes was not taken into account because for the $\gamma + p \rightarrow p + \pi^{0}$ reaction the imaginary parts of these amplitudes are very small. If it could be proved that the contribution to the dispersion integrals of very high energy regions is responsible for the discrepancy, this might greatly influence the attempts to develop a theory of pion photoproduction on the basis of dispersion relations. In this connection a number of specific experiments could be suggested which, for the lack of time, should rather be duscussed privately.

Cini:

Since the question of the agreement between experiments and the dispersion theoretical results of Chew, Goldberger, Low and Nambu, has been raised, I think it might be useful to anticipate briefly the results obtained for π^{+} photoproduction by Amati, Tona and Munczek in Rome on this subject. They take the dispersion relations for the four photoproduction amplitudes, and, after subtracting two of them in order to suppress the contributions of the imaginary part of the electric dipole, they use the CGLN p-wave amplitudes as an approximation to calculate the dispersion integrals without expanding the kinematic factors in powers of I/M. The result of this iteration, which differs considerably from the CGLN solution, combined with the expressions for the zero and first order coefficients of cos θ in the angular distribution a_0 and a_1, allow the computation of the five multiples (E_1, M_1 (1/2) M_1 (3/2), E_2 and d -waves which turn out to be necessary to insure the internal consistency of the data even at threshold. With these values, taking α_0 and a_1 as given by Bernardini and coworkers, they obtain the second order coefficient a_2 in satisfactory agreement with experiment. If a_0 rises at very low energies as experiments seem now to suggest, the agreement is probably still fair essentially because of the very large errors in a_2 very near threshold. There seems therefore to be no real discrepancy between this "iterated" solution of dispersion relations and experiment, at least until more precise results on the angular distributions will be available in this energy region.

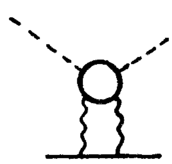
Frisch:

The interpretation of the effect of the π^0 half-life on the Compton scattering is not certain enough to set an upper limit on the π^0 half-life. It may well be greater than the value of 5.10^{-17} quoted from the dispersion theory results, and it would be wrong to stop doing experiments in the 10^{-15} - 10^{-16} second region.

Fubini:

I want to make a remark concerning the determination of the π^0 lifetime using Compton scattering. I believe such a determination depends heavily on the knowledge we have of the effect of Compton scattering on the pion cloud. Indeed these terms could compete with the one pion cloud term in creating higher multipoles.

The calculation of the two pion exchange term requires a rather good knowledge of the pion interaction which we do not yet have



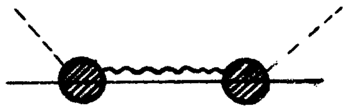
---- =photon

〰〰 = pion

——— = nucleon

Bernardini:

In the paper of Jacobs and Mathews this diagram does not seem to be explicitly included, but all diagrams as



are included.

Goldansky* :

My remark deals with the possible role of deuterium impurities in certain discrepancies between the data of different experiments on the determination of the Panofsky ratio.

Inasmuch as in the capture of π^- in deuterium the $\pi^- + d \to \pi^0 + n + n$ process is very unlikely (π^--should be in the p-, and nn, in the $^3P_1$ state), it is obvious that, qualitatively speaking, the deuterium impurities should act in the direction of decreasing the Panofsky ratio. The composition and effect of deuterium may vary depending on the target design and the conditions of hydrogen evaporation.

It is clear, however, that the quantitative effect of deuterium may be noticeable only in case the interception $(\pi H) \to (\pi D)$ prevails over the reverse process. A comparison of the data on radiative and Auger transitions in π-mesic atoms of hydrogen with the Gerstein-Zeldovich probability of interception $(\pi H) \to (\pi D)$ with a given quantum number n leads to the conclusion that when only these processes are available usual deuterium impurities could decrease the Panofsky ratio by 10 to 20% due to the interception at $n \approx 9, 10, 11$.

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* This remark is due to the post-conference discussion of the report. 3176

Whether such a decrease takes place actually or not depends on the probability of (πH) +H collisions with the variation of the principal quantum number - the role of the deuterium may be noticeable only in case the probability of such collisions is visibly suppressed as compared with the interception without a change in n. In this connection of some interest are both the calculations of the probabilities of mesoatomic transitions in hydrogen during collisions, and, particularly, an experimental study of the dependence of the Panofsky ratio on the deuterium concentration in liquid hydrogen.

Höhler*.

The predictions from the CGLN photoamplitude give a semiquantitative fit in some energy and angle regions and not even a qualitative one in others, if the effective range relations for the scattering phase shifts are used. In my opinion an essential part of the deviations result from the fact that the effective range relations do not represent the scattering data in our energy region (see Pontecorvo's report), it is even doubtful if all of them are a good approximation at the energy of the "threshold experiments". If different sets of phase shifts are used, in the limits allowed by the errors of the scattering experiments, the predictions for photoproduction show large variations, and a combined discussion of the π^0 and π^+ production is necessary to find the shortcomings of the CGLN amplitude. The B-values of Goldansky et al. fit very well with our prediction for $N^{(+)}=0$ and are far away from the prediction which corresponds to $\sigma(n\pi^0) \ll (p\pi^0)$

* See footnote on the page 80.

at threshold (Watsons Model I). The small p-phases necessary to explain the A and C values are probably not excluded by the scattering data.

Baldin*:

Dr.Höhler pointed out that it is possible to achieve the agreement between C.G.L.N. amplitudes and experimental data by making some assumptions which essentially change the magnitudes of these amplitudes. One of the results that can be obtained in this way is: $N^{(+)} = 0$ or $\sigma(n\pi^{o}) \approx \sigma(p\pi^{o})$. However, it is easy to obtain from reported value for $\sqrt{A^{o}}$ (p. 51) and from $\sigma^{(-)}$ and $\sigma^{(+)}$ near threshold that $N^{(+)} = -(0.06 \pm 0.01)$ or $\sigma(n\pi^{o}) \ll \sigma(p\pi^{o})$.

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*See footnote on the page 80.
